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The
THEORY OF PRICES

VOLUME I

The
THEORY OF PRICES

A Re-Examination
of the
Central Problems of Monetary Theory

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VOLUME I

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TO MY MOTHER AND FATHER

WHETHER a writer or a speaker undertakes to unfold principles, to set them in a novel and more striking light, or to recommend their application, he should know what has been already undertaken, what has been accomplished, and what remains for discovery and elucidation. The following work gives sundry examples of the inconveniences resulting from the want of this information, by exhibiting able men engaged in the investigation of principles and the development of laws which had been previously established and traced, and putting forward speculations as original which had been long before the public.

—J. R. McCulloch, *The Literature of Political Economy*

ARE we not . . . justified in affirming that Political Economy represents, and that in a very eminent degree, one at least of those symptoms which M. Comte has declared to be among the least equivocal evidences of really scientific conceptions—continuity of doctrine?

. . . I VENTURE to assert that a more remarkable example of continuity of doctrine, of development of seminal ideas, of original *aperçus* extended, corrected, occasionally re-cast, of new discoveries supplementing, sometimes modifying, the old—in short, of all the indications of progressive science—will not easily be found even in the history of physical speculation. . . .

—J. E. Cairnes, *M. Comte and Political Economy*

WHAT, then, remains in the attitude of the medieval masters to offend or embarrass us? Probably nothing save their docile modesty in instructing themselves in philosophy before setting out to further its progress. . . . They believed that philosophy could not possibly be the work of a single man, no matter what his genius might be, but that it progresses, like science, slowly, as the result of the patient collaboration of generations, each leaning on its predecessors in order to surpass their achievement. "We are like dwarfs," said Bernard of Chartres, "seated on the shoulders of giants. We see more things than the Ancients and things more distant, but it is due neither to the sharpness of our sight nor the greatness of our stature; it is simply because they have lent us their own."

—Etienne Gilson, *The Spirit of Medieval Philosophy*

Preface

THE fundamental proposition from which this study proceeds is that the Theory of Prices must be conceived of as an *organon*, in the sense in which Marshall's *Principles* may be said to be an organon. The Theory of Prices, for those who accept this view, is not a set of maxims of monetary policy, which can claim neither the general validity nor the authority that may be said to attach to a set of conceptual tools. Nor is it a series of speculations as to matters of fact, with respect to which the last word must be provided by investigations of the "facts" themselves. The reader, therefore, who expects a discussion of such matters as the wisdom of attempting to stabilize the price level, or a "theory" of the business cycle, in the sense of a surmise as to the facts of cyclical variation, is warned that he will be disappointed.

It will be apparent that this book, which has the dimensions and the purpose of a formal treatise, has also, by virtue of its continued concern with the writings of Mr. J. M. Keynes, many of the characteristics of a polemical tract. To the "apology for polemics" which is offered in the Introductory chapter, I have only two comments to add here. The first is the obvious one that those of us who disagree most categorically with Mr. Keynes, and insist upon subjecting his contentions to a severely critical examination, pay him the clearest of compliments as a creator of the kind of intellectual ferment out of which it is hoped truth will ultimately arise, even at the moment that we protest most emphatically against those of his utterances which we believe have introduced serious confusion into our subject.

The second comment is that by far the greater part of such criticism of Mr. Keynes as this work contains is criticism not of Mr. Keynes's positive analysis, but of his own criticism of received doctrine upon the subject with which this book is concerned. The difference is surely important.

There are, indeed, precedents for extended critiques of the work of an author of great eminence in his generation. The spirit of those parts of the present work which can be regarded as a "critique" of Mr. Keynes's writings, however, is akin to that with which John Stuart Mill insisted that his two-volume *Examination of Sir William Hamilton's Philosophy* was imbued. "My subject," said Mill, "is not Sir W. Hamilton, but the questions which Sir W. Hamilton discussed." The subject of this work is not Mr. Keynes, but the questions which Mr. Keynes has discussed. It is the importance of these questions, and not the importance of Mr. Keynes or any of his critics, which alone can justify the length at which this book treats the questions constituting its subject matter.

It follows, from the fact that by far the greater part of such polemics as are contained in this study are polemics on behalf of the Principle of Continuity as a maxim of doctrinal development, that the work as a whole is intended definitely to be constructive; and I hope that it will be so regarded by all those for whom "construction" is not necessarily construction *ex nihilo*. I am aware, to be sure, that some of my readers will regret that the line of positive analysis may appear to be obscured by what will seem to them an unnecessary degree of concern with the works of other writers, including Mr. Keynes. To this, my answer is twofold. In the first place, it is my intention to follow this treatise with a publication which will be frankly in the nature of a textbook, and which should satisfy those who would insist that it is the proper function of an author to state only the conclusions which he himself believes to be sound, and not the reasons why he prefers these conclusions to those reached by other writers.

In the second place, I happen myself to be of the opinion that if it is a vice to be concerned "excessively" with the views of other writers, it is at least equally a vice to proceed in complete disregard of what other writers have done or are doing. One is reminded of the Cockatoo in Alfred de Musset's *Histoire d'un merle blanc*, who attributed the distaste of the public for his attempts at versifying to the fact that the public also read the works of poets other than

himself, and was "distracted" thereby. I confess to the vice of opening myself to the danger of being "distracted" by a consideration of the works of others, if it is a vice; and since I am entirely prepared to forgive the trespasses of those who follow a procedure different from that which I have followed, provided only that the results that they reach are sound, I ask only that my own trespasses, if I have sinned in the opposite direction, may be forgiven also.

I am aware that it will be objected that familiarity with the work of other writers can be demonstrated in other ways than by specific citation of, or discursive comment upon, the contentions of these other writers, and that it is not necessary to bring into the exhibition gallery all the shavings of the workshop. To this I can reply only as follows: First, in all honesty, not *all* the shavings of the workshop are here presented; indeed, I am only too keenly aware that some of my readers will feel that justice is not done to the specific contributions which they have made or believe they have made. Second, there are various kinds of workshops, each producing its own kind of shavings. A tortuous concern with problems which the author himself ends by characterizing as side issues that do not strike the heart of the matter surely results in a mass of "excess" shavings, particularly when no one but the author himself seems to have been bothered by these side issues, so that no citations to the works of other authors are necessary. It is at least arguable that the fact that other writers *can* be cited would indicate that the matters under discussion are of interest to some one besides the author. Finally, I trust that I may be permitted to lay at least part of the blame, if blame is called for, upon what were undoubtedly overgenerous comments on some of my published articles in which much attention was paid to the existing literature on the subject with which they were concerned, in that these comments have encouraged me to believe that there are others who experience the same pleasure that I do in viewing the results of an adventure of exploration into the field of the history of ideas.

Familiarity with existing prejudices on the subject leads me to fear, also, that most of the obloquy which may be

directed against the form of exposition adopted in this study will concern the number and volume of the footnotes. The footnotes are chiefly of three kinds: (1) page citations to the work of authors quoted or discussed in the text; (2) detailed specification in support of a generalization advanced in the text; and (3) supplementary comments designed to reinforce or protect an argument stated in the text. For the first type of footnote, I have no apology at all. As for the second and third types, I ask the querulous reader to consider whether it is not better, assuming that the material is to be included at all—and in this matter I can only refer again to what was said above concerning the shavings of the workshop—to put it where he need look at it only in case he feels the need of further supporting argument. The same answer is offered to those who may object to my adoption of the practice—very common on the European continent, and becoming increasingly common in English-speaking countries with the translation of continental works in which the device was employed—of putting into a smaller type, material of an essentially supplementary character. There is nothing to force the reading of either footnotes or material printed in smaller type by those who dislike such devices; and if I have erred in putting into footnotes or smaller type, material which a reader thinks should have gone into the main text, or vice versa, I can only ask humbly to be forgiven for the mistake in judgment.

I fear, finally, that even such merits as may be discovered in this work will not reconcile those who have an implacable hatred for books that run to many pages. Surely, however, the physical compass of a work is not a criterion for judging its usefulness; it is something imposed by the nature of the task which the author sets himself. One of the tasks which I have set myself in this book is the illustration of the processes by which monetary theory has come to be what it is. In the nature of the case, this has involved a continuing and detailed concern with the works of earlier writers, over the whole of the field of the Theory of Money and Prices. Only those who know the vastness and the richness of that field will be aware of how much I have left out that I might have put in; and it is their forgiveness that I beg, rather than

the forgiveness of those who will not be reconciled to fat books.

The physical dimensions of the work have recommended its publication in two volumes rather than in one. The second volume, to which frequent references are made in the volume now offered to the public, will contain the material dealing with the theory of the effect of money upon output; the theory of savings and investment as related to the Theory of Prices; and a final evaluation, in the light of the received alternatives, of the Theory of Prices presented in Mr. Keynes's *General Theory of Employment, Interest, and Money*—including such elements as Liquidity Preference, the Multiplier, the so-called Method of Expectations, and so on. The relegation of the material last mentioned to the second volume will, it is believed, commend itself to most readers after the second volume will have appeared.

Dr. C. O. Hardy, who has read the first five chapters, has given me the benefit of his support where he agrees with me, and the much more valuable benefit of his frank criticism on the points—suspiciously few, so far as these chapters are concerned—on which he disagrees with me. Professor E. A. J. Johnson has read the whole manuscript with a degree of care far beyond anything that was imposed upon him by his formal duties as editor of the series in which the work appears; and he has helped me greatly in matters of presentation. I have been helped greatly in this respect also by the editorial staff of Prentice-Hall, Inc. Mr. Harold G. Russell and his assistants on the staff of the University of Minnesota Library have been extraordinarily helpful in many ways. Mr. John K. Langum of the University of Minnesota was kind enough to read the galley proof and caught several errors of content. In other cases in which specific parts of the book—including parts of the manuscript of Volume II—have been submitted to friendly critics, I have indicated, and desire here to express again, my sense of profound gratitude to the persons involved.

My obligations to the two to whom this book is dedicated, on the other hand, do not lend themselves to specification, precisely because they are measureless. The same thing must be said of my obligations to my wife, who has helped

in so many ways that any attempt at particularization would likewise represent an absurd minimization of her share in the work.

ARTHUR W. MARGET

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The THEORY OF PRICES

VOLUME I

CHAPTER ONE

Introductory

I

An Apology for Polemics

IN THE opinion of Mr. J. M. Keynes, the "more sophisticated" of recent discussions of the "Theory of Money and Prices" have brought it about that we seem, in this field, to be "lost in a haze where nothing is clear and everything is possible."[1] From many points of view, undoubtedly, the best method of demonstrating the unfairness of Mr. Keynes's statement would be to continue to use, in significant and fruitful ways, precisely that received apparatus for dealing with the "Theory of Money and Prices" which seems to its detractors little more than a "haze" of confused analysis. In the long run, indeed, this is the only way to demonstrate it.

There can be little doubt, however, that a principal reason why something less than the maximum use is made of tools lying ready to our hand is that many of those who should be using them refrain from doing so only because they are not aware of the fact that the nature of these tools has been misrepresented by writers who are themselves not in sympathy with received tradition. Whenever such misrepresentation is made, surely ordinary scruples of scholarship would demand that the misrepresentation be corrected.

There are those, to be sure, who, rightly impressed with the futility of so much of polemical discussion, would insist that it is better to pass over the misrepresentations in silence, and proceed to consider the "positive" contributions of those who are unsympathetic to the older body of doc-

[1] *The General Theory of Employment, Interest, and Money* (1936), 292. The specific context in which the statement appears will be discussed at greater length in Volume II of this study.

trine. That this is the wise procedure in many cases, one cannot deny. Yet it must be obvious that, in other cases, such a procedure may easily do more to retard the progress of our subject than would any conceivable amount of polemics.

For, in the first place, it is clearly impossible to determine the significance of "positive contributions" unless we are clear as to whether these contributions represent an accretion to, or an overthrowing of, the main body of received tradition. In either case, discussion is obviously impossible unless we are agreed as to the substance of the tradition with respect to which the "contribution" in question is to be adjudged an accretion or an antithesis.

The same proposition holds for any attempt to judge the significance of "positive contributions," when these "contributions" are regarded neither as avenues to results which demonstrate the fallaciousness of previous results obtained by the use of old techniques, nor as techniques for handling new types of problem, but are regarded rather as alternative techniques for dealing with familiar problems. It is clearly necessary, in such cases, that we should be in a position to decide whether the techniques in question are in fact superior to received techniques for dealing with those problems. This again, however, is impossible unless we are fully conversant with the substance of the techniques which the newer devices are designed to displace. Whether we like it or not, therefore, there are occasions when polemics become necessary, not only in the interest of accurate scholarship, but also in the interest of the advancement of our subject.

II

The "Theory of Prices" and Mr. Keynes

It will be immediately obvious, from the pages which follow, that this book, despite its title, is concerned with only a part of what should properly be included under the head of "The Theory of Prices"—namely, that part which has to do with the effect of *money* upon prices in a "closed" economic system. The title has been chosen in conscious imitation of

the practice indicated by Mr. Keynes in his latest book.[2] The reason for this is simply that both the form and the substance of this book—and indeed the fact that it was written at all—are due to the writings of Mr. Keynes.

It would be an injustice to Mr. Keynes to suggest that the appearance of confusion characterizing the "Theory of Money and Prices" at the present time is due solely to the impact upon contemporary monetary theory of the *Treatise on Money* and *The General Theory of Employment, Interest, and Money.* Yet there can be little doubt that this appearance of confusion became really pronounced only after the publication of the *Treatise.* Nor can there be any serious doubt that the principal reason for this was Mr. Keynes's insistence, both in the pages of the *Treatise* and in the discussion which followed its publication, not only that his analysis represented a clear break with most of what had gone before, but also that any attempt to restate the substance of his argument in terms suggested by the older analysis could lead only to an obscuring, rather than to a clarification, of that argument.[3]

It is, therefore, only reasonable that a book representing an attempt to defend the substance of received tradition on the subject of the "Theory of Money and Prices" against its detractors should center its interest upon the arguments of Mr. Keynes. The selection of Mr. Keynes for this purpose. however, can be justified by reasons other than the mere fact that it is he who is largely responsible for the feeling that the Principle of Continuity does not apply as a maxim of scientific procedure at the present stage in the development of monetary theory.

The first of these additional reasons is that Mr. Keynes holds a very distinguished place in contemporary economics. When a distinguished historian charges economists with

[2] See especially Chapter XXI of the *General Theory.*

[3] See, for example, Keynes's "Reply to Dr. Hayek," *Economica,* November, 1931, 6: "Those who are sufficiently steeped in the old point of view simply cannot bring themselves to believe that I am asking them to step into a new pair of trousers, and will insist on regarding it as nothing but an embroidered version of the old pair which they have been wearing for years." For examples of passages in the *Treatise* itself which are relevant to a judgment as to Mr. Keynes's understanding of the relationship of the apparatus presented in the *Treatise* to the older analysis, see below, pp. 13 ff.

having allowed their subject to fall into "utter confusion," or when an equally distinguished chemist sneers at the "conventions and half-truths that pass for economics," we are amused, perhaps, or mildly annoyed, but hardly deeply concerned.[4] The invasion of barbarians into fields utterly strange to them is, after all, a phenomenon to which economists, above all others, have by this time become accustomed. Mr. Keynes, however, is no barbarian. He has been called, rightly, "the most famous of living economists";[5] no one could dispute his claim to the position of one of the most eminent of living monetary theorists; and his utterances have naturally exerted an influence corresponding to this position of eminence. An attempt, therefore, to meet Mr. Keynes's strictures upon the present state of monetary theory can hardly be regarded as an unnecessary concern with the type of inconsequential criticism to which traditional economics has always been subjected, but which it has always managed somehow to survive.

Nor is this all. Mr. Keynes himself has been charged with what ought to be regarded as the most serious of crimes against scholarship, and would be so regarded if there were not so many who would stand convicted if the record were carefully kept: the crime, namely, of charging a group, vaguely described as "economists" generally, with having held notions which it is in many cases very difficult to show that any important group of economists ever did hold.[6] It would ill become the defenders of the tradition which is under such vigorous attack to adopt a similar set of tactics, and content themselves only with vague references to "the critics" of that tradition. The selection of Mr. Keynes at least makes it possible to cite chapter and verse, so that the reader himself may judge whether the position of the attackers is as seriously misrepresented by the defenders as the latter believe their position to have been misrepresented by the former.

[4] The quotations are taken from Charles A. Beard, *The Open Door at Home* (1935), 125, and Frederick Soddy, *The Rôle of Money* (1935), 4 f.

[5] So H. D. Henderson, in *The Spectator*, February 14, 1936, 263.

[6] See, in this connection, the comment of Professor Pigou, in *Economica*, May, 1936, 116, n. 1.

The purpose of the discussion that follows, then, is the restatement of the subject matter of received doctrine upon the subject of the "Theory of Money and Prices," with special reference to the criticisms levied against that doctrine by Mr. Keynes in his *Treatise on Money* and his later writings. It should hardly be necessary to add that what is to be regarded as important is the restatement and the defense, rather than such incidental counterattack upon alternatives of the type proposed by Mr. Keynes as may be associated with the defense of traditional doctrine. What follows is certainly intended in no sense as a critique of all parts of Mr. Keynes's *Treatise* or his later writings. For one thing, as Professor Pigou has put it, Mr. Keynes has gone "on a sniping expedition in a large village"; and it is doubtful whether it would be wise, even if one had the patience, to attempt to "track down the course of his every bullet."[7] The most that can be claimed for the present work is that it proceeds on the assumption that something is to be said for dividing up the village into important sectors, and for allowing different individuals to trace the bullets that have fallen within the particular sector in which they are interested. The sector to which this study is alone devoted is that which Mr. Keynes himself has demarcated under the heading of the "Theory of Prices."

[7] *Ibid.*, 116.

PART ONE

"Quantity Equations" and Their Meaning

CHAPTER TWO

"Quantity Equations" and "The Quantity Theory"

I

Quantity Equations and the Theory of Prices

IT is fair to say that, prior to the publication of Mr. Keynes's *Treatise on Money,* little objection would have been raised to the proposition that any attempt to explain the determination of general prices must, implicitly or explicitly, start with a "quantity equation" in some form—that is, with a formulation with respect to the forces determining "prices" in which the *quantity of money* was given an unmistakable place.[1] When, therefore, the analytical apparatus presented in the *Treatise* for dealing with the problem of the determination of prices was interpreted in some quarters as dispensing once and for all with the necessity for the familiar "quantity equations," the effect could not have been otherwise than disturbing. For, in substance, we were being asked to turn our backs upon a type of reasoning which goes back at least as far as the time of Jean Bodin; although algebraic "quantity equations" embodying this reasoning had not begun to appear extensively in economic literature prior to the middle of the nineteenth century or, at best, the latter part of the eighteenth.[2]

[1] The only instances that might be adduced as significant exceptions to this generalization would be represented by those cases in which (1) it was suggested that acceptance of an "income approach" to the problem of the value of money involved a rejection or supersession of the older "quantity equations," and (2) there were objections to the use of certain "quantity equations" either because of their supposedly "truistic" character, or—paradoxically—because of their "untrue" character. On the first, see below, pp. 344 ff., and on the second, pp. 88 ff. and 46 ff.

[2] Some writers have insisted that the type of reasoning involved goes back to the *Gemeine Stimmen von der Müntz* of the Albertine-Ernestine controversy (1530); others that it goes back to Julius Paulus (*ca.* 200 A.D.);

Our knowledge with respect to the history of algebraic "quantity equations" has been considerably extended in recent years. Attention has been called, for example, to a crude formulation, containing no term for "velocity of circulation," which was presented by Henry Lloyd in 1771.[3] It has also been shown that at least three algebraic formulations of the "equation of exchange" in essentially the "Fisherine" form were presented in the 1850's, namely, those of Roscher (1854), Bowen (1856), and Levasseur (1858).[4] More recently, attention has been called to the fact that K. H. Rau presented a "Fisherine" equation in 1841—i. e., some thirteen years before Roscher.[5] This last discovery has led to the suggestion that what has been called throughout this study, in conformity with current practice, the "Fisherine equation" should be called henceforth the "Rau-Fisher equation."[6] The danger of desiring prematurely to rechristen a given doctrine or analytical device in the light of the latest discovery as to its "origin" has, however, more than once been demonstrated in the history of economic doctrine; and in the present instance it has already been demonstrated by the fact that it is now possible to cite more than

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and still others that it goes back to Chinese writers of 500 B.C.! No further justification for the selection in the text of Bodin, however, is needed, for the purpose in hand, than is offered by the fact that he provided what was probably the most influential among the earlier examples of a recognition of the importance of changes in the supply of metallic money for the determination of the general level of prices. It will be noted that I have not characterized Bodin as the "founder" of "the quantity theory." Cf. what is said on this matter below, p. 96, n. 54.

[3] Cf. my article in the *Zeitschrift für Nationalökonomie,* IV (1932), 197 n. The general neglect of Lloyd's book by later English writers—a matter commented upon by Jevons (*The Theory of Political Economy,* p. xlii of the 4th [1924] edition)—prevented these writers from even considering, to say nothing of developing further, his formula. The formula was considered, however, by Lloyd's Italian contemporaries and their successors. See, e. g., the "Estratto del saggio sulla teoria della moneta del general Lloyd, stampato in Londra nel 1771," as appended by "the mathematician Paolo Frisi" to the sixth (1772) edition of Pietro Verri's *Meditazioni sulla economia politica* (reprinted in Custodi's *Scrittori classici di economia politica,* parte moderna, XVII [1804], 375 ff., and, more recently, by M. Fasiani in *Annali di statistica e di economia,* V [Genoa, 1937], 271 ff.). Frisi's discussion of Lloyd was copied almost verbatim by F. Fuoco, in the latter's "Applicazione dell' algebra all' economia politica" (1827), 108 ff. (173 ff. of Professor Fasiani's edition, *Annali di statistica e di economia,* V).

[4] See the *Journal of Political Economy,* XXXIX (1931), 574, n. 11 (the reference to Levasseur's *La Question de l'Or* there given should be p. 150 instead of p. 148).

[5] See K. F. Maier, *Goldwanderungen* (Jena, 1935), 9 n., where the fourth (1841) edition of Rau's *Lehrbuch der politischen Ökonomie,* Vol. I (*Grundsätze der Volkswirtschaftslehre*), p. 305, is cited. The equation does not appear in the corresponding passage of the French translation (1839) of the third (1837) edition of Rau, which is the only example of the editions of Rau prior to that of 1841 which I have been able to consult.

[6] So F. Lutz, "Über die Umlaufsgeschwindigkeit des Geldes," in *Jahrbücher für Nationalökonomie und Statistik,* CXLIV (1936), 387. The "Fisherine" equation has, of course, also been called the "Newcomb-Fisher equation." See, on this practice, my comments in the *Journal of Political Economy,* XLIII (1935), 149, n. 10.

one instance prior to Rau. In 1813, for example, the Italian economist Cagnazzi published an equation of the general form $MV = [P]G \cdot v$—i.e., an equation identical with that published forty-three years later by Bowen; and in 1819 Samuel Turner presented an equation which was virtually identical in form with that presented by Roscher some thirty-five years afterward.[7] Still more striking is the case of Sir John W. Lubbock, who, in a work published anonymously in 1840, wrote, as the first of his "equations of condition, which connect the quantities which occur in the theory of currency," $\Sigma a\alpha + E = lD + mB + nC$—an equation which, when rewritten in more familiar symbols, becomes nothing more nor less than $\Sigma pq + E = M'V' + M''V'' + MV$, in which Σpq, MV, and $M'V'$ have essentially the meanings given to them by Fisher; $M''V''$ is the volume of bills of exchange used as money, times

[7] For Cagnazzi's formulation, see the extracts from his *Elementi di economia politica* included by Fasiani in the latter's admirable "Note sui saggi economici di Francesco Fuoco," *Annali di statistica e di economia*, V, 112 ff., especially p. 113. The "defect" which Fasiani attributes to Cagnazzi's formula $Mc = DC$, in which c represents the "[velocity of] circulation of money," D the "quantity of commodities," and C their "[velocity of] circulation"—namely, that it does not include a term for "prices"—is at worst a merely formal one; for Cagnazzi, who regarded his equation as a formulation of the conditions for "the equilibrium of the two circulations [of money and of goods, respectively]," obviously considered a change in prices to be the criterion of "disequilibrium." Even formally, moreover, he was no more at fault than Bowen, who, though he regarded himself as translating into algebraic terms John Stuart Mill's enumeration of the forces determining the "value of money," likewise did not bother to include a special term for "prices." For Turner's formulation, see his *Letter Addressed to the Right Honorable Robert Peel, etc., etc., Late Chairman of the Committee of Secrecy, Appointed to consider of the State of the Bank of England, with Reference to the Expediency of the Resumption of Cash Payments at the Period fixed by Law* (2d ed., London, 1819), 12 ff. The a of Turner's "algebraic statement" $a = bc$, in which b was "the quantity of the precious metals circulating" in a country (i. e., M), and c was the "circulating power, or the number of times that b changes hands" within a given period of time (i. e., V), was defined as the "value of the commodities . . . exchanged in a given portion of time, as a year." It was, therefore, the exact equivalent of Roscher's u in the equation $u = ms$, in which m is the equivalent of M and s is the equivalent of V in the ordinary "equation of exchange"—u being in fact defined as the "amount of annual exchanges" (*Anzahl der jährlichen Umsätze*) for money. The case of Turner is cited by Professor Viner (*Studies in the Theory of International Trade* [1937], 249, n. 23) along with that of Henry Lloyd and the anonymous author of *The Theory of Money; or, A Practical Inquiry into the Present State of the Circulating Medium, etc.*, (London, 1811), as having provided "algebraic (or arithmetic) formulations of the equation of exchange in which the velocity of circulation of the means of payment had been expressly provided for." The citation of Lloyd in this context, however, is a slip, since, as we have seen, his algebraic formulation did not include a term for velocity; and the formulation of the author of *The Theory of Money* (pp. 41 ff.) is perhaps better regarded as an arithmetic illustration of a non-algebraic formulation of the equation of exchange. For an earlier example of such a non-algebraic formulation, see the reference to Beccaria (1769) in my "Léon Walras and the Cash-balance Approach," *Journal of Political Economy*, XXXIX (1931), 574, n. 11.

the "velocity of circulation" of such bills; and the term E has a purpose not greatly dissimilar from that assigned to the term R in M. A. Copeland's formulation of the equation of exchange as $PT + R = MV$.[8]

It is this type of formulation, we were told, which Mr. Keynes had, in the *Treatise,* "definitely abandoned . . . in favour of the 'savings and investment' analysis."[9] It is of course true that certain commentators upon Keynes's argument differed from this interpretation of its drift; but I have not been able to find any cases in which the commentators in question supposed that, in so doing, they were stating Mr. Keynes's own intention. In some instances, for example, their position seems to have been simply that Mr. Keynes's new equations could not be regarded as having superseded the older quantity equations so long as it remained true that, in the absence of a clear demonstration of the falseness or the uselessness of the older equations, the conclusions reached by Mr. Keynes through the use of these new equations were subject to continued check by the use of the older equations, which, therefore, could hardly be said to have been "abandoned."[10]

To say, in any case, that some commentators upon the *Treatise* sought to demonstrate that the use of the new equa-

[8] See [John W. Lubbock], *On Currency,* iv, n.; also 4, 6, 24, 37, and 43. (Lubbock's equation is reproduced by Viner, *Studies,* 249, n. 23; but, through some error, χ appears in place of Lubbock's α.) On some of the pages cited from Lubbock, the term which we have written lD appears as A, representing "the amount of checks drawn." Cf., however, pp. 24 and 43 of Lubbock's book. On the relation of Lubbock's E to Copeland's R, see below, pp. 57 f.

[9] So R. C. Mills and E. R. Walker, *Money* (1935), 96; cf. also J. H. Williams, "The Monetary Doctrines of J. M. Keynes," *Quarterly Journal of Economics,* XLV (1931), 549. These authors, and others that could be cited in this connection, use the expression "the quantity theory" in place of "quantity equations," which is what is called for by their context: a usage which, as must be obvious from section iii of this chapter, is copied from Mr. Keynes himself.

[10] The pointed remarks of Mr. D. H. Robertson, to which reference is made below, pp. 15 f., may be taken as typical, in this connection. When, for example, Robertson characterized the first of the "Fundamental Equations" of the *Treatise* as being based upon "that rigorous Fisherine concept of a certain flow of money in a given time-interval meeting a certain flow of goods in the same time-interval" (*Economic Journal,* XLI [1931], 401), he was presumably to be understood as having argued that, unless the first equation was so interpreted, and unless the argument based upon it was translated into the terms of equations of the general "Fisherine" form, we run the danger of obtaining results which are either wrong, or in conflict with Mr. Keynes's supposed desire to avoid the charge that the older equations would, under certain circumstances, lead us to wrong results.

tions did not necessitate, and indeed did not permit, the abandonment of the older "quantity equations" is not to say that Mr. Keynes himself would have accepted such an interpretation of the drift of his argument. The same thing must be said with respect to those interpretations of Mr. Keynes's position which held that the equations of the *Treatise* were nothing more than a "stupendous transfiguration" of the older "quantity equations." [11] If most of Mr. Keynes's readers had felt that the equations of the *Treatise* could be so regarded, and—what is more to the point—if more of them had gone on, in the manner of Mr. Robertson, to test such a contention in detail, a good part of this study need not have been written.[12] For our present purpose, in any case, it is sufficient to point to certain considerations which would make it extremely difficult to regard such a statement as an accurate interpretation of Mr. Keynes's intentions.

II

Keynes's *Treatise* and the Quantity Equations

It is noteworthy, in the first place, that, although each of the two famous "Fundamental Equations" of the *Treatise* purported to summarize the forces determining a "price-level," neither of them gave explicit place to a term representing the "quantity of money" or to any of the terms, other than "prices" and "output," which were included in the older "quantity equations." [13] This emphasis was

[11] So J. R. Hicks, "A Suggestion for Simplifying the Theory of Money," *Economica*, February, 1935, 2.

[12] For an example of Mr. Robertson's efforts in this direction, see p. 402 of the article cited above, p. 12, n. 10, where Robertson attempted to see how a given case of an "excess of savings over investment" would "work out in terms of quantity equations." The case is discussed in greater detail below, pp. 68 ff. and 525 ff.

[13] See Vol. I, pp. 135 ff. of the *Treatise;* and cf. the comment on this aspect of the "Fundamental Equations" by Lauchlin Currie, *The Supply and Control of Money in the United States* (1934), 4. On p. 185 of the same volume of the *Treatise*, to be sure, Keynes speaks of the "quantity of bank-money" and "the velocities of circulation" as if they were "elements in the Fundamental Equation"; and presumably we are to understand this statement as referring to the transformation of the first term of the second member of the Fundamental Equations (namely, E/O) into M_1V_1/O: a transformation which appeared at several points in the *Treatise* (see, e. g., the references given below, p. 134, n. 79). The "quantity of bank money"

strengthened, in the second place, by the fact that Mr. Keynes's own non-algebraic summary of what he "claimed to prove" in his *Treatise* with respect to the forces determining the "price-level of output" made no explicit reference to any of these terms.[14]

There were, to be sure, grounds for suspecting that not all these implications as to the supposed antagonism between the analytical apparatus presented in the *Treatise* and the older approach were to be taken literally. In the *Treatise* itself, for example, Mr. Keynes gave occasional evidence of a willingness to put what he regarded as his newer type of analysis in terms which were essentially those of the old "quantity equations." Yet it could not be said with any degree of assurance that these passages represented a conviction, on the part of Mr. Keynes, that such a translation was either necessary or possible. At least one of the passages in question, for example, was prefaced by a "precautionary word" designed to warn the reader against an interpretation which would make the author "appear . . . to be reverting to the old-fashioned 'quantity of money' approach to the problem of price-determination."[15] Another of the passages represented part of an attempt to demonstrate that under certain conditions the "old-fashioned quantity equations, however carefully guarded," would lead to wrong results.[16] A third was accompanied by the unenthusiastic comment that Mr. Keynes himself did not know that the equation which he presented as establishing "the relationship between the 'Fisher' equation and the Fundamental Equations" of the *Treatise* was "worth much."[17] A fourth lost most of its force by virtue of the fact that the V in the "Quantity Equation" there presented—namely, the equa-

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and "the velocities of circulation" are certainly not *explicit* elements, however, in either of the two Fundamental Equations in their original form; and this is all that matters for the present purpose. On the implications of the expression $E/O = M_1V_1/O$, see below, pp. 126 ff.

[14] See the *Treatise*, II, 345 (and cf. I, 143 f.). This aspect of Keynes's presentation was faithfully copied by certain of his popularizers. See, for example, E. M. Bernstein, *Money and the Economic System* (1935), 257, on the factors determining "the price of a unit of output."

[15] *Treatise*, II, 4.

[16] The passage referred to, which appears on pp. 149 f. of the first volume of the *Treatise*, is discussed in detail below, pp. 68 ff.

[17] See the *Treatise*, I, 240.

tion $M'V' = \Pi \cdot O$—was so defined as to sum up within itself all the forces involved in a discrepancy between "Saving" and "Investment," with the result that the relationship of the *older* "quantity equations" to the Fundamental Equations was obscured rather than illuminated.[18] In short: when one recalled the famous passage in the preface to the *Treatise* in which the author confessed that the various parts of his book "are not all entirely harmonious with one another," and that there were many "sloughed skins" still "littering these pages," it was difficult to avoid the feeling that chief among these "sloughed skins" was precisely the temptation to use "quantity equations" of the old type.[19]

It must be said, moreover, that Mr. Keynes's contributions to the controversy which immediately grew up about the central argument of the *Treatise* hardly helped to establish clarity with respect to the place that must be assigned to the older "quantity equations" in any formulation with respect to the forces determining "prices." Attention has already been called to his energetic protest against the idea that his new analytical apparatus could be regarded essentially as a refinement of the older apparatus, rather than as involving the abandonment of that apparatus.[20] Nor can it be said that the attempt by Mr. D. H. Robertson to extract from Mr. Keynes an unequivocal statement with respect to the relationship of the apparatus of the *Treatise* to the older

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[18] Cf. the *Treatise,* II, 5. It is worth noting, in this connection, that one of the commentators upon Keynes who interpreted him most enthusiastically as arguing for a complete abandonment of the older "quantity equations" (B. Josephy, "Keynes' Geldlehre," in *Zeitschrift für die gesamte Staatswissenschaft,* XCIII [1932], 59), characterized this passage as involving the use of "an equation which, in the light of his [Keynes's] own achievements, is outmoded [*rückständig*]," and went on to suggest that the appearance of the equation in question in the *Treatise* was to be attributed "presumably to the fact that this section [of the book] was written before the Fundamental Equations had been worked out."

[19] See the preface of the *Treatise,* pp. v-vi; and cf. also Keynes's comment, in his "Reply to Dr. Hayek," *Economica,* November, 1931, 6, on the difficulty he had had, when revising the manuscript of the *Treatise,* in "obliterating" certain "traces of old trains of thought."

[20] Cf. above, p. 3, n. 3. It is worth noting, in this connection, that the passage there quoted appears in connection with Mr. Keynes's emphasis upon the difficulties involved in making a "transition" to his new equations from "the old Quantity-of-Money, Velocity-of-Circulation schools of thought, whether it be Cambridge Quantity Equations or Fisher Quantity Equations."

"quantity equations" was entirely successful.[21] Mr. Keynes was, to be sure, induced to affirm that he had "never said that the older methods, strictly applied, would lead us to *wrong* results"; he had, he insisted, alleged only that "they are incapable, so applied, of leading us to certain useful results." [22] On its face, and in itself, this statement might have been interpreted as indicating that Mr. Keynes regarded his own apparatus as being, not in conflict with the older quantity equations, but supplementary to them, in the sense that his own apparatus would be invoked whenever and wherever the older quantity equations showed themselves inadequate to the task, but not otherwise. Quite apart, however, from Mr. Keynes's vigorous insistence elsewhere that his new apparatus was not to be regarded as a mere supplement to, or refinement of, the older apparatus, a number of serious considerations argued against the conclusion that Mr. Keynes's disclaimer of any intention to represent the older equations as "untrue" left the position of the older equations secure in the arsenal of weapons currently available to the monetary theorist.

Of these considerations, the one which is most important for our present purpose is that Mr. Keynes's formal profession to the effect that the older quantity equations were "true" was itself difficult to understand in the light of his actual practice, according to which it was anything but clear that he regarded the older equations as "true." Concretely, it had been assumed that both the new apparatus and the old apparatus undertook to present a "theory of prices"; they both included, that is to say, a term for "prices" among the variables selected for study. So long as this is so, a variation in the term representing "prices" must be accounted for by variations in other terms of the equations, new or old. Otherwise, either the results obtained by Mr. Keynes from the use of his newer equations were not sound, or the old equations themselves were in the nature of "untruths." It had been Mr. Robertson's contention that the identity of results which should have been obtained from

[21] Cf. Robertson, *Economic Journal,* XLI, 396 f.
[22] *Economic Journal,* XLI, 419.

the use of the two sets of equations, if the validity of the older equations was not to be impugned, was not, in fact, always obtainable; and he had adduced certain cases in support of his contention.[23]

For those, clearly, who are interested in establishing the reasons for the confused "haze" in which it has been alleged that contemporary monetary theory is enveloped, an examination of these cases is an indispensable part of any attempt to establish the relative merits of the apparatus represented by the older quantity equations and that presented in the *Treatise*. It must again be insisted, however, that the reason for this attempt is primarily the desire to reaffirm the validity of the older equations, and only secondarily to demonstrate the weakness of the equations of the *Treatise*. Otherwise, indeed, in view of the fact that Mr. Keynes himself seems no longer desirous of making use of the latter equations, a large part of this study might properly be regarded as an unwise devotion of attention to matters that it would have been wiser to allow to drop into the limbo of things well forgotten.

A single example will suffice to demonstrate the point involved. It was argued above that, so long as it is true that both sets of equations undertook to present a "theory of prices," the results obtained by the use of them in explaining given cases of price-change should be identical; it was pointed out that there was serious question whether Mr. Keynes believed that, in certain cases, this identity of results was in fact obtainable; and it was argued that an examination of these cases would provide a definite test of his attitude with respect to the validity of the older apparatus. Now it happens that Mr. Keynes himself, in the course of his discussion with Mr. Robertson, denied that the test thus proposed was relevant; and his argument on the point is of considerable importance for our present purpose.

In sum, Mr. Keynes's argument was that the syllogism upon which the proposed test is based errs in its major premise. This major premise, obviously, was that the "prices" which both the old and the new equations had pur-

[23] These cases are discussed in more detail below, pp. 68 ff. and 525 ff.

ported to explain were the same *kinds* of "prices." Mr. Keynes now argued that in fact the "prices" with which his equations were concerned were not the "prices" with which the older equations were concerned, so that a diversity in the results obtained by one set of equations or the other was not only understandable, but inevitable.[24] Clearly, what is involved is a simple question of fact, which can be tested only after there is clear understanding as to what the *content of the older equations actually is.* It will be argued, in subsequent pages, that Mr. Keynes was wrong on the particular point here involved; that, in fact, the "prices" involved in the two sets of apparatus are precisely the same kinds of "prices"; and that therefore the proposed test as to the validity of the apparatus of the *Treatise,* as compared with the validity of the older apparatus, was a fair one.[25]

Precisely the same thing is to be said, obviously, with regard to that aspect of Mr. Keynes's formal attack upon the older "quantity equations" concerning which there was no ambiguity—namely, his insistence that, although they might lead to results which would be true, they could not lead to certain results which he regarded as "useful." This is a question, clearly, which must be faced by anyone who would defend the substance of the older methods of approach; but it is difficult to see how it can be met except by testing these older methods upon the problems for which it was alleged that the equations of the *Treatise* were alone adequate. This involves, to be sure, an examination of the adequacy of the *Treatise* equations for the purpose assigned to them; but it involves also a restatement of the substance of the older equations which are alleged to be inadequate. For the purposes of this study, again, it is the latter task which is the really significant one, and which provides a justification for an examination of the argument of the *Treatise* at a length which it might be difficult otherwise to justify.

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[24] See the *Economic Journal,* XLI, 419.
[25] See especially, below, pp. 517 ff.

III

Quantity Equations versus the Quantity Theory

Our present task, then, is the demonstration of the validity and the usefulness of the older "quantity equations" in the light of the criticisms directed against these equations, on the grounds both of validity and of usefulness, in Mr. Keynes's *Treatise on Money*. A first step in such a procedure, moreover—indeed, one of its main purposes—is a demonstration of the proposition that, as a matter of fact, Mr. Keynes's arguments against the validity or usefulness of these equations are to be rejected for the simple reason that these arguments were based upon fundamental misconceptions as to the nature and purpose of these "quantity equations."

The first misconception with respect to the nature of the older "quantity equations" is best disclosed if we consider, in the light of Mr. Keynes' strictures, the implications of the following proposition:

A "quantity equation" is not the same thing as the "quantity theory." The fate of the former is, therefore, in no wise tied up with the fate of the latter.

It is undoubtedly true that, prior to the publication of the *Treatise*, instances of a confusion of "quantity equations" with "the quantity theory" were by no means uncommon in economic literature. Indeed, it is possible to find instances of this confusion not only in the writings of those unsympathetic to the use of the "quantity equations" in the study of the forces determining prices, but, unfortunately, even in the writings of those who have been most influential in popularizing the "quantity equations" as analytical tools. Thus, for example, E. W. Kemmerer, writing in 1907, referred to his own quantity equation, of the form $MV = GvP$, as "an algebraic expression of the quantity theory"; and the same sort of usage appears in his latest writings.[26]

Irving Fisher, on the other hand, can be charged only with occasional lapses from the usage which, on repeated occasions, he himself has recognized as the correct one. Thus, it is true that, in a paper published just before *The Purchasing Power of Money*, he was careless enough to

[26] See Kemmerer, *Money and Credit Instruments in their Relation to General Prices* (1907), 13. (In the quantity equation quoted in the text, above, in which symbols different from those used by Kemmerer have been inserted, G may be taken to represent the quantity of goods, and v what might be called the "velocity of circulation of goods.") Cf. also the same author's *Money* (1935), 33 f. and 64 f.

allege that the equation $MV + M'V' = PT$ "expresses the old quantity theory of money," though it is true that he regarded it as doing so "with some elaboration." [27] There were similar instances of careless expression in *The Purchasing Power of Money* itself: as when it was suggested that the quantity equations enunciated by Newcomb and his successors were to be regarded as giving "mathematical expression" to the "quantity theory," or that "the quantity theory is statistically verified to a high degree of correlation" if it can be shown that the direct statistical measures of P correspond to the statistical figures for the expression $(MV + M'V')/T$.[28] Against these lapses, on the other hand, must be set the explicit statements made by Fisher in his very earliest writings upon the subject, as well as in the main argument of *The Purchasing Power of Money*. In 1897, for example, he had been careful to point out that "the quantity theory," instead of being identical with his quantity equation $MV = PT$, is merely "one of the six theorems obtained by making any two of the four letters constant." [29] Similarly, in *The Purchasing Power of Money*, the main contention was, not that "the quantity theory" is identical with the "equation of exchange," but that the former is "made more clear" by the latter.[30] At many points in the volume, indeed, Fisher was at great pains to draw sharp limits to what the "equation of exchange" may be regarded as asserting.[31] It is not surprising, therefore, that even so unsympathetic a critic of Fisher's book, in virtually all its aspects, as B. M. Anderson, Jr., had no difficulty, at the time of his writing *The Value of Money* (1917), in keeping the "equation of exchange" separate from the "use to be made by the equation" in the development of arguments such as those involved in "the quantity theory." [32]

Instances of careless usage similar to those which may be cited from Kemmerer and, to a lesser extent, from Fisher, may be found also in the exposition of the two writers chiefly responsible for the development of "quantity equations" of the so-called "Cambridge" type. Thus Pigou, for example, was careless enough, at least on one occasion, to refer to the Fisherine equation as "the 'quantity theory' equation." [33] Similarly, Mr. Keynes, in his *Monetary Reform*, referred to his own equation $n = pk$ as "the 'Quantity Theory' equation." [34] On other occasions, however, Professor Pigou showed himself much more careful in his state-

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[27] See Fisher, "Recent Changes in Price Levels and Their Causes," *Bulletin of the American Economic Association*, 4th series, Vol. I, No. 2 (April, 1911), 38.

[28] See, for example, *The Purchasing Power of Money*, p. 25 n. and pp. 277, 295, and 297.

[29] Fisher, "The Rôle of Capital in Economic Theory," *Economic Journal*, VII (1897), 518.

[30] *The Purchasing Power of Money*, 15.

[31] See, for example, pp. 152 and 156 of the work cited; also pp. 168 f.

[32] Anderson, *The Value of Money*, 166, 171; cf. also p. 336 of the same work.

[33] Cf. Pigou, *Essays in Applied Economics*, 177.

[34] Keynes, *Monetary Reform*, 91.

ments with respect to the relationship between "quantity equations" and "the quantity theory"; and it is not without interest that Mr. Keynes should have cited one of these statements, obviously with approval, in *Monetary Reform* itself.[35]

In the light of these occasional aberrations on the part of those largely responsible for the popularization of "quantity equations," it would not have been surprising if opponents of "the quantity theory" had, in imitation of these writers in the less happy parts of their exposition, identified "the quantity theory" with "quantity equations"; and, as a matter of fact, it is possible to find instances of such a usage on the part of opponents of "the quantity theory."[36] The striking fact, however, is that, at the very time when Fisher's equation began to occupy the center of the stage in discussions of monetary theory, there were not wanting opponents of "the quantity theory" who protested against the suggestion that the fate of "quantity equations" was bound up with the fate of "the quantity theory," and insisted vigorously that it was precisely these "quantity equations" which allowed their own "anti-quantity-theory" contentions to "appear distinctly."[37]

The important thing for our purpose, however, is simply that, by the time the *Treatise* was written, the unsoundness of the practice of identifying the ordinary "quantity equations" with "the quantity theory" should have been clear to anyone who was aware of the confusion already wrought by the practice. It certainly cannot be said that the issue had gone by default; for it is possible to find emphatic statements in protest against this sort of identification even in elementary textbooks, to say nothing of discussions intended for advanced workers in the field of monetary theory.[38]

When, therefore, Mr. Keynes, in his *Treatise,* made himself guilty of identifying the issues associated with "quantity equations" with those associated with "the quantity

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[35] *Ibid.,* 81 n.

[36] See, for example, B. M. Anderson, Jr., "Commodity Price Stabilization a False Goal of Central Bank Policy," in *Chase Economic Bulletin,* Vol. IX, No. 3 (May 8, 1929), 22, where it is alleged that "the contention of the quantity theory" is "formulated in an equation" of the ordinary Fisherine form.

[37] So, e. g., J. L. Laughlin, in the *Bulletin of the American Economic Association,* pp. 67 f., cited on p. 20, n. 27, above. Cf., also, the remarks in a similar vein by M. S. Wildman and R. H. Hess, on pp. 62 and 66 of the same volume.

[38] For examples from the textbooks, see L. D. Edie, *Money, Bank-Credit and Prices,* 190 f.; R. T. Bowman, in P. F. Gemmill, *et al., Contemporary Economic Problems,* 47; R. V. Leffler, *Money and Credit,* 111 ff.; R. L. Garis, *Principles of Money, Credit, and Banking,* 236 f.; F. A. Bradford, *Money,* 214, n. 4. For particularly emphatic statements of the point in technical publications, see J. Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *Archiv für Sozialwissenschaft und Sozialpolitik,* XLIV (1917), 676, and A. F. Burns, "The Quantity Theory and Price-Stabilization," *American Economic Review,* XIX (1929), 575.

theory," he must be charged with a large share of the confusion which may still be said to exist on the subject. This was so not only because the prestige attaching to his example carried greater weight than that attaching to the example of lesser writers, but also because his own avowedly unsympathetic attitude to the older "quantity equations" made his usage more dangerous than that of others who, while they may have been guilty of looseness in identifying the "quantity equations" with the "quantity theory," accepted without question the former, and did not imply that any weaknesses of the latter could be adduced as part of the argument designed to discredit the "quantity equations."

IV

The Residuum of the Quantity Theory Controversy

That Mr. Keynes was guilty of the confusion in question will be established presently. The seriousness of the consequences of such a confusion, in any case, can hardly be doubted by those who would accept the following propositions as representing a fair distillation of the results of the battles that raged about the "quantity theory" prior, say, to 1920—by which time, one would have thought, a deep concern with the "truth" or "falsity" of the "quantity theory" could fairly be regarded as a concern with issues that had outlived their usefulness.

1. The "quantity theory," if the term is to be used to describe a doctrine which is at all open to dispute, is to be regarded as laying down a proposition, or a series of propositions, concerning the relative importance of the quantity of money among the factors which may be held to affect prices.[39] The particular "quantity equations" whose fate is at stake in the present discussion, on the other hand, affirm

[39] See, in this connection, Kemmerer, *Money and Credit Instruments*, 2, where it is stated that "the differences between the supporters of the quantity theory and its opponents" amount to "differences in the relative importance attributed to certain factors entering into the determination of the general price level." This statement is of some importance in view of the unfortunate statements with respect to the relationship between "quantity equations" and "the quantity theory" quoted above from Kemmerer, p. 19, n. 26.

merely that the "quantity of money" is *one* factor influencing prices; nothing whatever is said, in the equations themselves, concerning the importance of the influence of changes in the "quantity of money" relative to that of changes in other factors affecting prices. To identify "quantity equations" in general with "the quantity theory" is therefore to force the former to carry a burden of inadequacy which is properly to be charged only to the latter.

It goes without saying that, since any proposition with respect to the relationship between magnitudes can be given a mathematical statement, it is of course *possible* to cast even the crudest form of "the quantity theory" into the form of a "quantity equation." Thus, the "formula of the naïve quantity theory" has been expressed by some historians of monetary theory in the form $M:M_1 = P:P_1$—or, alternatively, it might be written in the form $M = kP$, in which k is a constant.[40] Similarly, it is possible to put into the form of an algebraic equation any one of the crude formulations with respect to the relationship between money and prices that have come down to us.[41] As we shall see, however, it is precisely because the particular "quantity equations" upon which we have all been "brought up"—to use a favorite expression of Mr. Keynes—are not of the absurdly simple form of, say, the equation $M = kP$ that they represent valuable tools of analysis even today.[42]

2. Debates *in abstracto* concerning the "truth" or "falsity" of the "quantity theory" have shown themselves to suffer from an initial handicap by virtue of the diversity displayed by the numerous propositions that have passed under the name of the "quantity theory."

A complete demonstration of this proposition would fill a fair-sized volume, which, at best, would have merely the negative virtue of demonstrating not only how a welter of argument might have been avoided

[40] The first of these formulations is presented by F. Hoffmann, *Kritische Dogmengeschichte der Geldwerttheorien* (1907), 40.

[41] Thus, A. de Foville in his *La Monnaie* (p. 142 of the 2d [1907] edition), stated what he regarded as Montesquieu's position with respect to the relationship between money and prices in the form of the equation $P/p = (M/m)\cdot(c/C)$, in which C represents "le nombre des choses dont il est fait commerce," and the capital letters and small letters, respectively, refer to the respective magnitudes of P, M, and C as between any two dates, although Montesquieu himself, unlike Henry Lloyd, who had presented a similar formulation (cf. above, p. 10, n. 3), had not himself put his proposition into the shape of an algebraic equation. In much the same way, F. Hoffmann undertook to translate into the form of equations the formulations of Hume, Harris, Genovesi, Beccaria, and James Mill (Hoffmann, *Kritische Dogmengeschichte*, 42 f., 50, 89, 91, 117).

[42] See especially, below, pp. 90 ff.

if the disputants had begun by agreeing as to what they meant by "the quantity theory," but also how it is possible to become so deeply involved in this most futile of all types of dispute as to lose sight completely of the issues that really matter. I have amused myself, for example, by collecting a very extensive list of instances in which the *Geldzins und Güterpreise* of Wicksell was discussed primarily, if not solely, from the standpoint of the "quantity-theory" or "anti-quantity-theory" reasoning which its argument was supposed to embody, not the least paradoxical aspect of the matter being that the critics themselves, holding different notions as to the substance of the "quantity theory," could not agree as to whether Wicksell was a supporter or an opponent of the "quantity theory." Instances could, of course, be multiplied almost *ad infinitum* to show how initial disagreement over the meaning of the "quantity theory" resulted in the expansion to an unconscionable length of arguments which would probably otherwise never have been started, or, if started, would have been brought to a speedy conclusion. How much, for example, would remain of the debate—still carried on currently in our scientific journals—as to the first appearance in economic literature of "the quantity theory" if there were preliminary agreement as to what constitutes a fair statement of "the quantity theory"?

3. Even when the parties to the dispute are agreed, moreover, as to the content of "the quantity theory," experience has shown that most *a priori* discussions of "the quantity theory," with their varying "assumptions" with respect to the "constancy" of certain of the variables included in the quantity equations, are largely fruitless. It is by this time generally agreed by virtually all competent workers in the field that these variables are true variables, in the sense that there are no compelling *a priori* reasons for assuming that they will appear as constants in a series of concrete situations.

It will be noted that the statement that the variables are "true variables" is quite distinct from the question, which has loomed so large in discussions of "the quantity theory," as to whether the variables of the more familiar quantity equations are dependent or independent. It is, of course, obvious, as Fisher himself pointed out, that some forms of the *quantity theory* require the assumption that certain of the variables of the "quantity equations" are independent.[43] It must also be obvious that if more inclusive forms of the "equation of exchange" are used in the statement of the cruder forms of "the quantity theory," we must assume, conversely, a dependent relationship of a very specific type. Thus, for example, the use of the equation $MV + M'V' = PT$ as part

[43] Cf. Fisher, "The Rôle of Capital in Economic Theory," *loc. cit.*, 518.

of a proof that price changes may be expected to be strictly proportional to changes in the quantity of money of ultimate redemption (M) would require, among other things, the assumption of a dependent relationship between M' and M of the form $M'/M = k$, in which k is a constant. Once, however, it is recognized that the use of "quantity equation" is not associated exclusively with the statement of "the quantity theory," it becomes obvious that the dependent or independent character of the variables in these equations is something which of course is deserving of study, but which could be shown to be relevant to the use of the equations themselves only if, and to the extent that, it could be demonstrated that certain of the variables other than the price variable could never under any circumstances be independent variables. If, for example, it could be demonstrated that the V of the Fisherine equation could not possibly change unless M also changes, any use of the Fisherine equation which assumed that price changes might be due solely to changes in V would lead to false conclusions; so that the equation itself could be said to be misleading insofar as it failed to indicate the dependent nature of V.[44] In fact, however, no satisfactory demonstration of a completely dependent relationship in this sense between any two of the variables (other than the price variable) of the Fisherine equation has ever been provided.

These conclusions with respect to assumptions of "dependence" or "independence" as between the variables of the familiar "quantity equations" deserve particular emphasis in the light of the recent remarks on this subject by Mr. Keynes.[45] That Mr. Keynes is right in arguing that algebraic formulations of the forces determining prices "involve just as much tacit assumption as to what variables are taken as independent . . . as does ordinary discourse" follows from the contention of the preceding paragraph. It is just as important, however, to emphasize that these algebraic formulations do not involve *more* "tacit assumption" in this respect than does "ordinary discourse." Yet this is precisely what one might be led to conclude by Mr. Keynes's statement to the effect that "it is the great fault of pseudo-mathematical methods of formalizing a system of economic analysis . . . that they expressly assume

[44] For an example of the type of situation here envisaged, see my article, "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,'" *Journal of Political Economy*, XL, 309 ff., where it is shown that the equation $V = (b/a)v$, presented by C. Bresciani-Turroni, in which V is the velocity of circulation of money, v the equivalent of "the velocity of circulation of goods," a the number of days which must pass before the whole of the money will have been spent once, and b the number of days which must elapse before the stock of goods will have been turned over once, involves the further assumptions $V=k/a$ and $v=k/b$, in which k is a constant; and that it is therefore impossible to argue, as Bresciani-Turroni argued, that we could have a situation in which a diminishes, while the "rapidity of circulation of money does not vary." It goes without saying that the relationship of V, for example, to the other variables of the familiar "quantity equations" is precisely *not* of the kind typified by the relationship $V=k/a$.

[45] See the *General Theory*, 305.

strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed."[46] If, by "all their cogency and authority," Mr. Keynes means their "cogency and authority" over the results obtained by "ordinary discourse," the answer must of course be that no one has claimed more for algebraic formulations of the type represented by the "quantity equations" than that they "exhibit the extreme complexity of the relationships between prices and the quantity of money, when we attempt to express it in a formal manner."[47] This, however, is a vastly different thing from implying that the "disallowance" of the "hypothesis of strict independence," except in the sense of "independence" given in the preceding paragraph, destroys the "authority" of these algebraic formulations to the point of making them largely useless for the purpose of analysis.[48]

4. The true goal of the Theory of Prices, therefore, is not a sterile debate in which one side affirms that certain results will follow if certain variables are held constant, while the other side affirms, with a great air of iconoclasm, that statistics show that "in fact" these variables are not constant. The goal should be rather a systematic study, on both the analytical and empirical side, of the forces which act upon each of the variables involved to give them the values they actually have. Anything else is very largely an exercise *in vacuo*.

In short: from the standpoint of one interested in the development of an analytical apparatus for dealing with the phenomena of price-change in all their complexity, debates concerning "the quantity theory" can safely be regarded as completely outmoded, in the sense that it is hardly possible to claim more for "the quantity theory" at the present day than that it represents a device of greater or less usefulness either in popular education toward the understanding of the principal causes of a given historical instance of price-change, or in stressing the fact that, in the words of Mr. Hawtrey, "it is through the quantity of money that it is possible to *regulate* the value of the currency unit."[49] Cer-

[46] *General Theory*, 297.

[47] So Keynes himself, *General Theory*, 305. Cf., also, the note prefixed by J. Marschak to his essay "Die Verkehrsgleichung," *Archiv für Soz.-wiss. u. Soz.-pol.*, LII (1924), 344.

[48] Cf., in this connection, the judicious remarks on the assumptions with respect to "independence" which are involved in the use of the familiar quantity equations by Pigou, *Essays in Applied Economics*, 188.

[49] See Hawtrey's review of Aftalion's *Monnaie, prix, et change*, in the *Weltwirtschaftliches Archiv*, XXVIII (1928), 102**.

tainly debates concerning the truth or falsity of "the quantity theory" are of no importance in the discussion as to the continuing validity and usefulness of the "quantity equations" as such. To suggest, therefore, that the fate of the latter is in any significant way tied up with the fate of the former is to make confusion doubly confounded. Yet it can be shown, without difficulty, that it is precisely this confusion of which Mr. Keynes has made himself guilty.

V

Quantity Equations and the Quantity Theory in Keynes's *Treatise*

For evidence of such confusion, in the first place, one does not have to go beyond those instances in which Mr. Keynes has used the expression "the Quantity Theory" in contexts in which only "the Quantity Equations" could possibly be relevant. This is true, for example, with respect to all those passages in which he argued for the superiority of his new equations (and of the savings-investment analysis which they were designed to summarize) over, not the older "quantity equations," which are the only things that are properly to be compared with his own savings-investment equations, but "the Quantity Theory." [50]

This would have been bad enough if all that had been involved were a mere carelessness in terminological usage. Unfortunately, however, once this identification of "the quantity theory" with the "quantity equations" was allowed to go unchallenged, further consequences followed, of which the most serious, and the least excusable, was that Mr. Keynes proceeded to charge the older approach with vices which, while they may be properly chargeable against certain forms of "the quantity theory," are certainly not chargeable against the "quantity equations" as such.

Thus, for example, we were told that the "old-fashioned" approach was "ill calculated to explain the course of events in Great Britain between 1890 and 1896." [51] "The story, on which we were brought up, tells how the decline of prices

[50] See the *Treatise,* I, 133; II, 171.
[51] *Ibid.,* II, 164.

which culminated in 1896 was due to a shortage of gold . . . "; and Mr. Keynes himself rejected this interpretation. Now this interpretation does indeed have to do with "the quantity theory" in one of its forms, since it is concerned with the relative importance, for the explanation of price changes in a given historical period, of changes in the quantity of "money" (in this case, gold) as compared with the importance of changes in other factors. The fate of this interpretation, however, and indeed of the particular form of "the quantity *theory*" involved, is in no way tied up with the fate of the "quantity *equations*." Yet one was led to believe that it was so tied up, not only by Mr. Keynes's identification of these "quantity equations" with "the quantity theory," but also by the further fact that he adduced the case under discussion as one of the "historical illustrations" designed to "illustrate the ideas" of the *Treatise*—that is, presumably, those ideas which, as summarized in the "Fundamental Equations," were supposed to provide an alternative to the "old-fashioned 'quantity of money' approach to the problem of price-determination," or, in other words, the older "quantity equations."

Precisely the same sort of thing, obviously, must be said of all the other instances in the *Treatise* in which "the Quantity Theory" is criticized for its "crudeness" and general inadequacy.[52] Taken in themselves, these instances might be regarded as innocuous examples of a concern with issues which one would have thought were safely buried at the time of the publication of the *Treatise*. Taken, however, in conjunction with the passages, indicated above, in which "the Quantity Theory" was identified outright with the "Quantity Equations," they represent an essential dispar-

[52] See, for example, the *Treatise*, II, 49, 370; cf., also, the argument on pp. 175–176 of the same volume—typical of instances in which, although the propositions involved are not discussed in the text of the *Treatise* as examples of "Quantity Theory" reasoning, they are referred to as such in the Index (II, 420). Typical also is the passage on II, 281 of the *Treatise*, where, for support of the proposition that "a stable price-level" would certainly not be secured by the mere fact that "the annual increment of new gold corresponds to the annual increment of the world's output of goods and services," Keynes appeals to his "Fundamental Equation," as if the older quantity equations, which included such terms as M', V, et cetera, would not do equally well for the purpose.

agement of the "quantity equations" on grounds which are entirely unwarranted.

VI

Quantity Equations and Keynes's *General Theory*

As we shall see in the chapters which follow, the particular type of criticism of "quantity equations" which we have been considering represented by no means the only ground on which the *Treatise* undertook to criticize these equations. Before proceeding to a consideration of these other criticisms, however, it would be well to note how the issues thus far raised have fared in Mr. Keynes's more recent *General Theory of Employment, Interest, and Money.* In the light of what was said at the beginning of this chapter with respect to the effect of the argument of the *Treatise* upon the prestige of the familiar quantity-equations, the last thing one would have anticipated in Mr. Keynes's later work was a definite recantation of the position, adopted in the *Treatise,* that it is possible to dispense entirely with "quantity equations" of the older type. Yet there are grounds for suggesting that such a recantation is precisely what Mr. Keynes now offers us.[53]

This is obvious, in the first place, from the very terms which Mr. Keynes now uses to describe the subject matter of the "Theory of Prices." The latter, it now appears, is, at least in the first instance, nothing more nor less than "the analysis of the relation between changes in the quantity of money and changes in the price-level, with a view to determining the elasticity of prices in response to changes in the quantity of money." [54] If there could be a clearer example

[53] I am discussing here only the formal presentation of the "Theory of Prices" which is to be found in Chapter XXI of the *General Theory.* It is, of course, true that, in certain other parts of his argument, Mr. Keynes's practice is not entirely consistent with his professions. The most notable instance of this kind is that having to do with those aspects of the theory of the effect of monetary expansion upon output which Mr. Keynes chooses to treat through the aid of the apparatus represented by such analytical devices as "the multiplier" and the like. These matters will be dealt with in Volume II of this study.

[54] *General Theory,* 296.

of the extent to which Mr. Keynes has retraced his steps since the days when he was anxious to warn the reader against believing that he was "reverting to the old-fashioned 'quantity of money' approach to the problem of price-determination," it is difficult to imagine what it would be.

In the second place, incredible as it appears, Mr. Keynes's "new" formulation of the "Theory of Prices," in its formal aspects—and of course it is with the formal aspects of the problem that the "quantity equations" have, in the first instance, been concerned—makes considerable use of a "quantity equation" of a particularly simple type.[55]

It would be easy, in the light of these two central facts, for respecters of traditional methods of approach to the problem included under the "Theory of Prices" to announce triumphantly that the debate between Mr. Keynes and the "traditionalists" is now closed, in one of its crucial aspects, by a complete surrender, on the part of Mr. Keynes, of the heterodox position he had assumed in the *Treatise on Money*. Yet there are a number of reasons for regarding any such procedure as unwise in the extreme.

The first of these is that when once an author launches a book as influential on the current state of monetary theory as the *Treatise on Money* has been, he does not dispose of its consequences merely by turning his back upon parts of it which had shown themselves to be particularly disturbing. There is, indeed, already abundant evidence that the most disconcerting aspect of the "Theory of Prices" presented in the *Treatise*—namely, the uncertainty of its relationship to the body of analysis summed up by the older "Quantity Equations"—has been carried over bodily into the discussion of those, among Mr. Keynes's disciples, whose primary allegiance is avowedly to the *General Theory,* rather than to the *Treatise on Money.*[56]

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[55] The implications of this particular "quantity equation" will be discussed in some detail in Volume II.

[56] For a particularly striking example, see R. F. Kahn, "Dr. Neisser on Secondary Employment," in the *Review of Economic Statistics,* XVIII (1936), 144 ff. Kahn's argument will be examined in more detail in Volume II. It is sufficient, at this juncture, to point out that, in protesting (*op. cit.,* p. 145) against Neisser's applying the "Quantity Theory of Money"—which, from the context, could mean only the older "quantity equations"—as a "touchstone" against Kahn's own analysis, and in protesting against

That Mr. Keynes's influence should persist in this most crucial respect, despite the apparent change of front in the *General Theory,* is not surprising when it is observed that nowhere in the latter work does he include a change in attitude toward the usefulness of a "quantity of money approach" among those changes, either of substance or of emphasis, which he regards as embodied in the argument of the *General Theory* as compared with the argument of the *Treatise.*[57] It was, therefore, not to be hoped that he would have presented us with a detailed account of the reasons for what we have characterized as a "change of front" with respect to the issues involved in the construction of a satisfactory apparatus for dealing with the "Theory of Prices." Yet, obviously, the inevitable result of a continued failure to dispose of these issues must be to leave undissipated a large part of the "haze" which, according to Mr. Keynes, surrounds the central problems of monetary theory, even if it remains true that this "haze" may fairly be said to have been created in large part by aspects of the argument of the *Treatise* which Mr. Keynes himself in effect now repudiates.

It must be obvious, moreover, that any fair evaluation of Mr. Keynes's latest formulation must take account not only of its relation to what may be regarded as the substance of received doctrine upon the subject of the "Theory of Prices" as it existed prior to the publication of the *Treatise,* but also of its relation to the apparatus presented by Mr. Keynes in the *Treatise* itself. To do less would be to violate not only the general principle that a set of doctrines, like a set of institutions, can be appraised fairly only in the light of what preceded, but to refuse to Mr. Keynes what he has been charged with having accorded inadequately to his own predecessors. Mr. Keynes has himself asserted that what, in

anyone's "imagining" that his (Kahn's) argument "can be expressed in terms of such concepts" as "the flow of money," "hoarding," and the like, Kahn is following to the letter the example set by Mr. Keynes when the latter protested violently against the suggestion that the argument of the *Treatise* could be expected to be translated in terms of the apparatus suggested by "the old Quantity-of-Money, Velocity-of-Circulation schools of thought, whether it be Cambridge Quantity Equations, or Fisher Quantity Equations" (cf. above, p. 3, n. 3).

[57] For examples of Keynes's explicit recognition of changes in emphasis or substance as between the argument of the *Treatise* and the argument of the *General Theory,* see below, pp. 138 f.

the *General Theory,* "may sometimes strike the reader as a confusing change of view" as compared with the argument of the *Treatise,* is in fact, despite "certain changes in terminology," nothing more nor less than "a natural evolution" in a line of thought he had been pursuing for years; and the *General Theory* has itself been regarded by others than Mr. Keynes as in reality "a further development and superior reformulation of those ideas which tantalised and vexed us in the *Treatise.*" [58] Clearly, therefore, an appraisal of the "Theory of Prices" presented in the *General Theory* requires, as a prerequisite, an appraisal of that theory presented in the *Treatise.*

A continuing concern with those aspects of the argument of the *Treatise* which cannot be regarded as having yet been systematically met is called for, moreover, even in the interest of a comparison between Mr. Keynes's latest formulation of the "Theory of Prices" and the body of received doctrine upon the subject as it existed prior to the publication of the *Treatise.* For it must be remembered that, so far as the purposes of this study are concerned, the demonstration of the weaknesses of the apparatus proposed by Mr. Keynes at various times as a substitute for more traditional methods of approach is entirely subordinate to our major purpose, which is the restatement of the substance of those more traditional approaches in the light of the criticisms levied against them by Mr. Keynes. Whatever may be said of the argument of the *Treatise* otherwise, it must be accorded the historic role of having directed an attack against the traditional methods in terms so sharply challenging that a clear restatement of the substance of these methods has become unavoidable if we are to be fully aware of the richness of received tradition as well as of its incompleteness at many points. It was hardly to have been expected that Mr. Keynes himself would have felt any responsibility to undertake such a restatement; the obligation to do so is clearly that of the defenders of that tradition, and theirs alone.

[58] So J. R. Hicks, in *Economic Journal,* XLVI (1936), 238. The statement quoted from Keynes himself appears on p. vi of the *General Theory.*

VII

Greek Gifts

There is, however, a fourth, and decisive reason for refusing to accept what we have characterized as a virtual recantation by Mr. Keynes of his earlier position as a satisfactory substitute for a clear statement of the considerations which would have made such a recantation unavoidable sooner or later. *Timeo Danaos et dona ferentes.* If, that is to say, Mr. Keynes brings with him, in his surrender, types of reasoning which were confusing when they were used against the defenders of existing tradition, there is no reason why these types of reasoning should be regarded as any less confusing when they are not so used. The point will be sufficiently illustrated if we examine, at this point, those aspects of the "Theory of Prices" presented in the *General Theory* having to do with the matter with which the preceding sections of this chapter were concerned; namely, the relationship between the "Quantity Equations," on the one hand, and "the Quantity Theory," on the other.

The first objection to the argument in the *Treatise* on this head, it will be recalled, was that Mr. Keynes used the terms "quantity equation" and "quantity theory" in such a way as to imply that they were identical. It can be only a source of profound regret, therefore, to note that, in the *General Theory,* Mr. Keynes again makes himself guilty of the same kind of usage.[59] It is good to have Mr. Keynes return to the fold of those conservatives, in matters of monetary theory, who would insist that the older "quantity equations" have not lost their usefulness; but one's rejoicing is tempered

[59] See, for example, the *General Theory,* 209, where it is alleged that the equation $MV=OP$ "is much the same as the Quantity Theory of Money in its traditional form." See also, *ibid.,* 305, where an algebraic expression to which is ascribed the purpose of exhibiting "the extreme complexity of the relationship between prices and the quantity of money, when we attempt to express it in a formal manner" (an expression the implications of which will be discussed in Volume II) is described, on the very same page, as "a generalized statement of the Quantity Theory of Money." It is to be noted also, in this connection, that certain of Mr. Keynes's disciples imitate him closely in failing to distinguish clearly between "the Quantity Theory," on the one hand, and "Quantity Equations"—of the form, say, $MV = PT$—on the other. See, *e.g.,* Kahn, "Dr. Neisser on Secondary Employment," *loc. cit.,* 146.

more than a little by the discovery that, in returning, he still carries *impedimenta* which are no more welcome as Greek gifts than they were when Mr. Keynes used them, in the *Treatise,* as straw men to be destroyed as part of his attack on the "quantity equations"—the more so since the effect of the presence of these *impedimenta* is a continuing uncertainty as to the precise nature of Mr. Keynes's own position with respect to "the Quantity Theory," on the one hand, and the "Quantity Equations," on the other.

We have been told, for example, by commentators on the argument of the *General Theory,* that it represents a "denial" of "the quantity theory of money." [60] This would be a comparatively innocuous matter if Mr. Keynes's usage, in the *General Theory* as well as in the *Treatise,* had been such as to draw a sharp distinction between "the Quantity Theory," on the one hand (with whose fate, except for purposes of defense against the most vulgar of popular errors, no one can be seriously concerned these days) and, on the other hand, the "Quantity Equations," with whose fate, as we shall see from the argument presented in the following chapters, we are very profoundly concerned. It is, for example, of some importance to know precisely what significance we are to attribute to Mr. Keynes's slighting reference, in the *General Theory,* to the "preoccupation" of the "classical school" with "the idea that prices depend on the quantity of money." [61] Is the source of Mr. Keynes's disapproval of the "classical economists," in this respect, a belief that the "classical economists" held that prices depend *exclusively* upon the quantity of money? If so, the statement, while it is of course entirely without foundation as a matter of history of doctrine, has no great importance for the construction of a satisfactory "Theory of Prices," which by this time, surely, can proceed without a "preoccupation" with the controversies that have raged in the past regarding the "truth" or "falsity" of "the Quantity Theory." The situation is, however, vastly different if the source of Mr. Keynes's disapproval is a resurgence of the position, ad-

[60] So, for example, C. O. Hardy, in the *American Economic Review,* XXVI (1936), 491.

[61] *General Theory,* 12.

vanced in the *Treatise,* that a "'quantity of money' approach to the problem of price-determination" (as represented, let us say, by any of the familiar "quantity equations") is not only "old-fashioned," but also outmoded. In short: the unfortunate continuation of a usage which had already done incalculable damage in the *Treatise* constitutes justifiable grounds for fearing a revival of a type of analysis which could be expected to contribute to the "haze" surrounding the "Theory of Prices."

VIII

The Dead Shall Rise Again

The situation is aggravated, moreover, by the fact that, if Mr. Keynes sometimes uses the expression "the quantity theory" merely as a synonym for "quantity equations," he also uses it in a sense which would identify it with those propositions which have in fact bulked so large in the controversies concerning the "truth" or "falsity" of "the quantity theory": that is, he has identified it with those propositions respecting the *relative importance* of the quantity of money among the factors which may be held to affect prices, or (what comes to the same thing) respecting the "degree of proportionality" in the price change that may be expected to follow from changes in the quantity of money.[62] He has, in fact, revived these very controversies; and, in so doing, he has resuscitated a corpse which most competent workers in the field would have regarded as well buried long since.

The principal result, indeed, of Mr. Keynes's efforts in this direction is a renewed demonstration, none the less impressive because it was unintentional, of the soundness of the position which holds these discussions of "the quantity theory," regardless of what may have been said for them in the past, to be very largely a waste of effort at the present

[62] Thus, for example, on p. 209 of the *General Theory,* "the Quantity Theory" is discussed in terms which suggest that it is concerned with the proposition that "the price level will be directly proportional to the quantity of money." Similarly, on p. 296, the "Quantity Theory of Money" is discussed as a proposition which, by making "a sufficient number of simplifying assumptions," amounts to the contention that, under certain circumstances, "prices will change in the same proportion as the quantity of money."

stage of monetary theory. It was suggested above, for example, that these discussions suffer from an initial handicap by virtue of the diversity displayed by the numerous propositions that have passed under the name of "the quantity theory." [63] This is bad enough when the current diversity of usage is found in different authors. It becomes very nearly unforgivable when it is evidenced by the same author within the covers of a single book; for the truth is that Mr. Keynes himself, even in those passages in which the expression "the quantity theory" is used in such a way as to differentiate it from "the quantity equations," uses the former expression to describe not one, but several propositions, each of which is quite distinct from the others.

In at least one passage in the *General Theory,* for example, we are told that the "crude quantity theory of money" holds that "prices rise in exact proportion to *MV*." [64] The "Quantity Theory of Money" which is "enunciated" a few pages later, however, would allege that "prices will change in the same proportion as the *quantity of money*"—though Mr. Keynes's usage elsewhere in the same volume is such as to make it clear that he regards the "quantity of money" as something quite distinct from "*MV*." [65] At still a later point, Locke is characterized as "the parent of twin quantity theories," of which the first is that "the rate of interest depended on the proportion of the quantity of money (allowing for the velocity of circulation) to the total value of trade," and the second is that "the value of money in exchange" [i.e., the "price-level"] "depended on the proportion of the quantity of money to the total value of trade." [66] It is clear that even the latter formulation, which makes no assumption with respect to the *constancy* of the "total value [i.e., the physical volume] of trade," is quite different from either of the two

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[63] Cf. above, pp. 23 f.

[64] *General Theory,* 289. It should be noted that, in this statement of "the crude quantity theory," Keynes interprets "velocity" to mean "income-velocity," in a very special definition of the latter term (cf. p. 201); yet, in a footnote on p. 209, where a different definition of "income-velocity" is considered, a formulation containing this alternative definition is likewise referred to as "the Quantity Theory"!

[65] For Keynes's "enunciation" of the "Quantity Theory of Money" in the form indicated, see p. 296 of the *General Theory.* For an example of a treatment of the "quantity of money" as something quite distinct from *MV,* see p. 209 of the same work, where the proposition that "the price level will be directly proportional to the quantity of money" is alleged to hold only if, among other things, V is taken as constant—although, on the very same page, the equation $MV = OP$ is alleged to be "much the same as the Quantity Theory of Money in its traditional form."

[66] *General Theory,* 343.

formulations of "the quantity theory" quoted above.[67] With respect to the first of the "twin quantity theories" which Keynes attributes to Locke, it should be sufficient, quite apart from the question as to the validity of the proposition itself, which is not here in debate, to ask where, outside of this passage in Keynes's *General Theory*, the title "the Quantity Theory" has ever been applied to a proposition implying that changes in the *rate of interest* will be in some degree proportional to changes in the "quantity of money"!

It was argued also, above, for reasons there given, that most *a priori* discussions of "the quantity theory," with their varying "assumptions" respecting the "constancy" of certain of the variables included in the quantity equations, have shown themselves to be largely fruitless. It was hardly necessary for Mr. Keynes to provide a further demonstration—again no less impressive because it was unintentional—of the futility of this type of exercise by asking us to follow him through a discussion of the conditions under which it will be true that "prices change in the same proportion as the quantity of money." [68]

As compared, to be sure, with the issues associated with the question of the continuing validity of the "quantity equations" as such, Mr. Keynes's relapse into a concern with issues that may have plagued monetary theorists a few decades ago, but have come to be regarded by most competent specialists in the field as of no real significance for the future development of monetary theory, can be regarded as a diversion that is harmless enough in itself, and would not be even mildly annoying if it were not for the fact that Mr. Keynes seems to imply that his concern with the issues in question is called for by a desire on his part to satisfy "tradition." [69] In point of fact, of course, this part of Mr. Keynes's discussion provides, at most, an ironical illustration of how it was possible for one who has urged us so eloquently to rid ourselves of the "old ideas" that "ramify . . .

[67] See again, in this connection, the passage on p. 209 of the *General Theory*, already cited, in which it is argued that "the Quantity Theory," in the sense of a proposition alleging that "the price-level will be directly proportional to the quantity of money," will hold only if *O*—the "quantity of output"—as well as *V*, is held constant.

[68] See, for example, pp. 209 and 296 of the *General Theory*.

[69] *General Theory*, 296.

into every corner of our minds," to make himself responsible for the reintroduction of "old ideas" of which we had regarded ourselves as fairly well rid.[70] Apart from this, the whole episode is of altogether secondary importance. What remains for discussion are those criticisms advanced in the *Treatise* against the "quantity equations" which do not proceed directly from the unwarranted identification of these equations with "the quantity theory"—although, as we shall see, the identification in question has an unfortunate way of intruding itself at most unexpected stages of the argument.

[70] The quotation is from the Preface (p. viii) to the *General Theory*.

CHAPTER THREE

Quantity Equations as "Statical Identities"

I

The Quantity Equations as "Statical" Equations

THE second point in the indictment which the argument of the *Treatise* may be regarded as having launched against the usefulness of the older "quantity equations" is represented by the allegation that these equations are strictly limited as to the range of problems to which they may be applied, by virtue of the fact that they are "statical" in character.[1]

[1] See, e.g., the *Treatise,* I, 133. The suggestion that the familiar "quantity equations" are "statical" in character had been advanced prior to the appearance of the *Treatise.* Such a position has been attributed, for example, to Cassel by H. Döring, *Die Geldtheorien seit Knapp* (1922), 137. It is only fair to Cassel, however, to point out that while his unfortunate utterances with respect to the "point-of-time" rather than "period-of-time" aspects of most of the variables of the Fisherine equation (see Cassel's *Theory of Social Economy* [1924 ed.], 424 ff. and cf. below, p. 425) would fit in with the equally unfortunate notions sometimes advanced with respect to the relation between "statics" and "dynamics," it remains true that his own loose remarks with respect to "statics" and "dynamics," in this connection (*Theory of Social Economy,* 434 f.) appear to have reference to the quantity *theory* and not to the "Quantity Equations" as such. Similarly, though Schumpeter, Walras, and Mill have been cited as having characterized the "Quantity Equations"—or their non-algebraic equivalents—as being "statical" in character, I have been unable to find in any of their works statements which would justify the attribution to them of such a position. Cf., however, M. Palyi, "Ungelöste Fragen der Geldtheorie," in *Die Wirtschaftswissenschaft nach dem Kriege, Festgabe für Lujo Brentano* (1925), II, 478. On the other hand, it is certainly possible to find unequivocal instances, prior to the publication of Keynes's *Treatise,* in which the Quantity Equations were characterized as being "statical" in character. See, for example, K. Elster, *Die Seele des Geldes* (1920), 45, 165, on the Quantity Equations as involving the "subjection of the dynamic process of price-formation to a static view." For an example of what would seem to be the same type of usage in a publication which appeared *after* Keynes's *Treatise,* see L. Mises, "Die Stellung des Geldes im Kreise der wirtschaftlichen Güter," in *Die Wirtschaftstheorie der Gegenwart* (edited by H. Mayer), II (1932), 315, where, in the course of a criticism of those who would "operate with the equation of exchange," it is alleged that "the problem of changes in the value of money has been handled by the methods of statics, whereas there should never have been any doubt about its dynamic character."

An initial difficulty in dealing with accusations of this type arises from the fact that the very meaning of the accusation is not clear so long as there is no preliminary agreement as to what is meant by "statical." We are confronted, that is to say, by another of those instances, so common in contemporary economic discussion, wherein the dichotomy "statics versus dynamics," which has shown itself to be helpful and indeed indispensable for the handling of certain types of problem when the terms are significantly defined, has shown itself to be confusing in the extreme whenever no attempt is made to define with precision the positive content of these terms.[2] Unfortunately, Mr. Keynes himself did not make such an attempt. We are therefore left with the necessity for constructing a series of propositions which might be taken as possible translations of the proposition that the older "quantity equations" are "statical" equations.

II

"Statical" Conditions as Stationary Conditions

A first set of propositions can be constructed upon the assumption that the word "statical" has to do with the *conditions* which must be present if the equations are to be regarded as (1) valid; or, granting their formal validity, (2) useful.[3] This statement, however, does not carry us far unless there is further agreement as to what "statical conditions" are.

[2] A particularly striking illustration of this proposition is again provided by certain aspects of the discussion concerning the concept of a "natural rate of interest," which has on various occasions been charged with being either misleading or actually useless because it is a "static concept." I hope, in the near future, to be able to examine this particular charge at some length.

[3] From our examination, in this chapter, of those parts of Mr. Keynes's argument in the *Treatise* which can be shown to have implied a questioning of the *validity* of the "quantity equations" under all conditions, it will be obvious that we are here dealing with a further set of instances bearing out the contention, advanced above, p. 16, that Mr. Keynes's *practice* was by no means always consistent with his profession to the effect that he had "never said that the older methods, strictly applied, would lead us to *wrong* results." Mr. Keynes could argue, at best, that the phrase "strictly applied" was intended to restrict the applicability of the older equations to cases in which they would lead to "right" results; but this is merely another way of saying that the equations would, under certain conditions, lead to results which would be definitely wrong.

Indeed, it has even been argued—and with much emphasis by some writers—that only confusion can result from the very practice of applying the adjective "statical" to *conditions*, instead of to certain types of analysis, or to certain types of theorem.[4] It can, however, be demonstrated that such significant differences in *kind* as may be held to exist between "static" analysis, on the one hand, and "dynamic" analysis, on the other, follow from a previous definition of static or dynamic problems in accordance with some criterion with respect to the nature of the *conditions* assumed in each type of problem.[5] The real argument against the use of the expression "statical conditions," therefore, would seem to be, not that there is no sense in speaking of "statical conditions," but that there is a danger (all too often exemplified) of identifying "statical conditions" with a particular set of conditions—namely, "stationary conditions"—which are by no means necessarily coextensive with those associated with what is properly to be designated as "statical analysis," when the latter is regarded as the equivalent of "equilibrium analysis."[6] Insofar, therefore, as the position of those who have objected to the use of the expression "statical conditions" amounts to a contention that "statical conditions" should not be identified with "stationary conditions," it is a position with which one can hardly disagree.[7] Still less is one inclined to disagree with that position if it be interpreted as insisting, more broadly, that the *term* "statical conditions" should be avoided wherever possible in favor of an unequivocal definition of the particular conditions assumed in each case; and that this is true even if we grant, as we must, that the differentiation between static and dynamic analysis turns upon a previous definition of static and dynamic problems on the basis of the nature of the conditions assumed in each type of problem.

On both counts, unfortunately, there is ground for criticism of Mr. Keynes. For, in the first place, his usage was often such as to suggest that "statical conditions" are identical with "stationary" conditions.[8]

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[4] This position, of which a particularly emphatic statement was made by R. Frisch in his "Statikk og Dynamikk i den økonomiske Teori" (*Nationaløkonomisk Tidskrift*, LXVII [1929], 323, 332 f.), has since been adopted by other writers. See, for example, J. Tinbergen, in *Econometrica*, II (1934), 27, and E. Schneider, in *Der Stand und die nächste Zukunft der Konjunkturforschung* (*Festschrift für Arthur Spiethoff*), 1933, 260 n.

[5] A demonstration of this proposition must be left for the study suggested above, p. 40, n. 2. See, however, in this connection, below, pp. 79 f.

[6] For a case in point, see below, pp. 76 f.

[7] It is worth noting, in this connection, that a protest against the identification of "static" with *stationary* conditions had been uttered, even before the publication of the article by Frisch referred to above, note 4, by J. Schumpeter, in his article, "The Explanation of the Business Cycle," *Economica*, VII (1927), 289 f.

[8] The identification of "statical" with "stationary" conditions is obviously implicit in his argument, examined below, p. 44, with respect to the conditions under which we may expect that "if the quantity of money were double the price levels would be double also." For an explicit identifica-

In the second place—and more unfortunately, for our present purpose—he at no point undertook to examine systematically the various possible meanings of the concept of "statical conditions," and the implication of each of these possible meanings for the purpose in hand. The result, inevitably, was not only uncertainty as to the precise content of his charge that the older "quantity equations" were "statical" in character, but also difficulty in interpreting the positive argument of the *Treatise* itself. This was particularly true because of the general uncertainty as as to Mr. Keynes's understanding of the relationship between "stationary" conditions, on the one hand, and "equilibrium" conditions, on the other. Thus, Mr. Robertson felt impelled to doubt whether Mr. Keynes had fully taken account of "the features which sharply differentiate" between an equilibrium that "envisages a considerable output of new machines" and the so-called "stationary state" [read: stationary conditions?].[9] Similarly, another critic was forced to wonder whether "the kind of equilibrium he [Keynes] has in mind" is a "moving" or a "static" [read: stationary?] equilibrium.[10] The difficulty was particularly evident in those instances in which Keynes wavered between a definition of "equilibrium" as involving *stationary* conditions (such as the condition that output remain constant) and one involving conditions such as either "full employment," which is by no means identical with *constancy* in employment, or equality between savings and investment, which is by no means identical with a *stationary* condition respecting the volume of savings and investment.[11] It is clear, in the light of this diversity of usage with respect to the meaning of the expression "statical conditions," that the proposition that the older "quantity equations" are "statical" in character because they assume "statical conditions" may mean anything and nothing.

If, therefore, we are to discuss intelligently the proposition that the familiar "quantity equations" are "statical" because they are based on the assumption of "statical conditions," we must proceed by constructing a set of propositions on the basis of each of the meanings assigned to the phrase "statical conditions" both by Mr. Keynes and by other writers of standing. Of these, the two meanings most important for our purposes are those which would define

tion of "statical" conditions with stationary or "constant" conditions, see also the *General Theory,* 48 n., where "statical"—that is, "constant"—conditions with respect to wealth or population are *contrasted* with conditions which, on the basis of the context, we should be justified in characterizing as the conditions for "equilibrium."

[9] See the *Economic Journal,* XLI, 399; cf. also p. 410 of the same article, and note 2 thereto.

[10] So S. H. Slichter, *Towards Stability* (1934), 48 n.

[11] On the first of these pairs of concepts, as it appears in the argument of the *Treatise,* see below, p. 76, n. 13; and on the second, cf. below, pp. 103 ff.

"statical conditions" as (a) stationary (that is, unchanging) conditions, or (b) conditions of equilibrium. The two are certainly not identical, despite the not uncommon practice of assuming that they must be so on the ground that non-stationary (that is, changing) conditions would necessarily disrupt equilibrium. That we may have, for example, a situation properly to be described as one of "equilibrium" but nevertheless not characterized by stationary conditions was pointed out by Marshall in terms to which nothing need be added.[12] It should be equally clear, conversely, that the simple fact that disequilibrium may exist under stationary conditions (as the result, say, of what has been called "friction or other stoppage") provides a further ground for rejecting the suggestion that "equilibrium" conditions are necessarily identical with "stationary" conditions.[13] There can be not the slightest doubt that the two are sufficiently distinct to warrant a separate construction in each case.

The first proposition calling for consideration, then, would be that the older "quantity equations" are *valid* only on the assumption that certain conditions are *stationary*. What would these conditions be? Surely, one would have thought, not the conditions summarized by any one of the variables included in the familiar quantity equations; for to assume that the use of "quantity equations" necessarily involves the holding of any of these variables "constant" is to follow the practice which we were at such pains to condemn in the preceding chapter; namely, the practice of confusing "quantity equations" with the "quantity theory."

As it happens, the "quantity *theory*" has sometimes been described as a "static theory" precisely on the grounds that it is supposed to assume that "other things" than the quantity of money and prices will "remain equal." [14] And in-

[12] See Marshall's letter to J. B. Clark on the "static state"—our "statical conditions"—in *Memorials of Alfred Marshall,* 415; and cf. p. 315 of the same volume. The situation presented by Marshall in the first of the passages cited has sometimes been described as one of "dynamic equilibrium"; but discussion of the various meanings that have been attached to the latter expression must be deferred to another occasion.

[13] For the phrase quoted, see F. H. Knight, "Issues in the Economics of Stationary States," *American Economic Review,* XXVI (1936), 395 n.

[14] Thus, J. W. Angell, in his *Theory of International Prices* (1926), 85, included, among his reasons for characterizing Mill's version of the "quantity theory" as being "static" in character, the fact that "it makes little

deed, if one were prepared to accept the practice of characterizing as "statical" any set of assumptions involving the "holding constant" of certain variables (a practice for which precedents can be found in the writings of certain "quantity theorists"), the characterization of the "quantity theory" as a "static theory" would follow as a matter of course.[15] What is worth noting, however, is that none of the writers who have characterized the "quantity *theory*" as a "static theory" ventured to suggest that the "quantity *equations*" were "statical" in character. They were not, that is to say, guilty of confusing the "quantity equations" with "the quantity theory."[16]

Yet it is precisely this confusion that Mr. Keynes introduced, in his *Treatise,* as part of his attempt to show the limited validity of "the old-fashioned quantity equations, however carefully guarded."[17] It is, he argued, only under certain conditions that "there is a unique relationship between the quantity of money and . . . price levels, . . . of such a character that if the quantity of money were double the price levels would be double also."[18] No one in his senses, surely, would have denied this; what one would deny is merely the suggestion that the question whether "price

real allowance for the element of growth in economic life." (Angell's other reasons have to do with Mill's alleged failure to deal adequately with the "phenomena of the transition periods." On the validity and usefulness of the Quantity *Equations* under "non-statical" conditions in this sense—though Angell was discussing merely the *usefulness* of Mill's form of the Quantity *Theory*—see below, Chapter IV, and especially p. 73, n. 3. For a more explicit example of a characterization of the quantity *theory* as "static" on the ground that it assumes *"ceteris paribus,"* see L. D. Edie, *Money, Bank-Credit, and Prices,* 196; and, in a similar vein, H. P. Willis, *The Economics of Inflation* (1935), 54 ff. Further examples could be provided in abundance from the continental literature.

[15] For an example of the type of precedent referred to, see Kemmerer, *Money and Credit Instruments,* 80 f., 89.

[16] It will be noted, for example, that Edie was one of those mentioned above, p. 21, n. 38, as having emphasized the distinction between "the quantity theory" and "quantity equations." Cf. also, in this connection, Willis, *Economics of Inflation,* 135 f.

[17] Cf. the *Treatise,* I, 146. In the passage in question, to be sure, Keynes spoke of "equilibrium" conditions, rather than *stationary* conditions; but it is obvious that the proposition quoted in the next sentence of the text, above, can have reference only to *stationary* conditions. Cf., moreover, the reference to the *General Theory,* given above, p. 42, n. 8. Insofar as the passage in the *Treatise* under discussion concerns "equilibrium" conditions which are not at the same time "stationary" conditions, it is discussed at greater length below, pp. 77 ff.

[18] *Treatise,* I, 147.

levels would be double" if "the quantity of money were double" has anything directly to do with the validity of the familiar "quantity equations," as such. These "quantity equations" do allege, to be sure, that the "degrees of change in the quantity of money, the velocities of circulation, . . . the volume of output" and in any other variables which are included in the "quantity equations," will, in fact, be "related to the degrees of change in . . . price-levels" in a "definite" and "predictable" ratio *when the facts with respect to the magnitude of all these variables and of any other variables included in the quantity equations are known;* and they do allege that this will be true under all conditions, "at the acute phase of a credit cycle" or at any other time.[19] This, however, is virtually the antithesis of an argument to the effect that a "definite" and "predictable" result can be obtained from the use of the quantity equations only if certain of the magnitudes in the equations are assumed to be *constant.* It is only the latter assumption that is necessary to the specific type of "unique relationship" between changes in the "quantity of money" and "price levels" which Mr. Keynes charged the "quantity equations" with assuming; and it is presumably such an assumption that he had in mind when he questioned the possibility of obtaining valid results from the use of these equations when applied to the "acute phases of a credit cycle." Again it must be insisted that assumptions with respect to the constancy of the magnitudes included in the "quantity equations" are not involved in the use of these equations and have nothing whatever to do with their validity. If this is what we are reduced to, in an attempt to attach a significant meaning to the proposition that "the quantity equations"

[19] This is precisely what was denied by Keynes, *ibid.* It is of the first importance to note, however, that his statement of what was to be denied did not include the words italicized in the text above. Mr. Keynes's argument with respect to the "predictability" of a given result from the use of the equations, it should be noted in passing, had nothing to do with the type of consideration involved in the discussion of "anticipations" (on which see below, pp. 48 f.). The "predictability" with which he was concerned was solely the "predictability" of results which follows from a correct scientific formulation. Cf., in this connection, the comment by Robertson, *Economic Journal,* XLI (1931), 396 n.; and, on the sense in which economics in general may lay claim to the "power of prediction," see J. E. Cairnes, "M. Comte and Political Economy," in Cairnes's *Essays in Political Economy,* 303 ff.

are "statical equations," in the sense that they are supposedly valid only under "statical" (unchanging) conditions, the case is a sorry one indeed.

III

The "Truth" of the Quantity Equations

If even the appearance of cogency is to be given the argument that the use of "quantity equations" involves the assumption of "statical" in the sense of "unchanging" conditions, it must be interpreted to have reference, not to a constancy of the magnitudes given an explicit place in the equations, but to a constancy of certain relationships which must be assumed in order to make the more familiar "quantity equations" true under all conditions, *even though no explicit mention of these relationships appears in the equations themselves.*

The problem has only to be stated in these terms, however, to make it clear that what is involved is another aspect of a debate which has been carried on intermittently for many years: namely, the question as to the formal "truth" of the "quantity equations," in the sense of a questioning of the mathematical equivalence of the two members of any one of these "equations." [20] It is, therefore, necessary to examine at some length the nature of the issues involved in the more significant forms of this debate.

Obviously, it is only these more significant forms of the debate that deserve to be taken with any degree of seriousness. It can hardly be worth the time of serious students of the subject, for example, to devote attention to the arguments of those who have come to the conclusion that the Fisherine equation is not strictly "true" on the basis of independent statistical computations of the magnitudes involved in both of its members.[21] Clearly, until it can be shown that something more is

[20] The discussion of the issues involved antedates by many years the popularization of the "quantity equations" in their algebraic form. See, for example, the references to J. G. Hufeland's *Neue Grundlegung der Staatswirtschaftskunst* (1813), and to Marx's *Kapital* given in my article in the *Zeitschrift für Nationalökonomie*, IV, 207 and 216 f. The American literature on the question is summarized by A. F. Burns, "The Quantity Theory," *loc. cit.*, 576 n.

[21] See, for example, Anderson, "Commodity Price Stabilization a False Goal of Central Bank Policy," *loc. cit.*, 24.

involved than the crudity of the statistical material involved in these computations, there is no ground for believing that this type of argument has any significance whatever for the question as to the mathematical validity of the "quantity equations" themselves. The most that could possibly be said, in this connection, is that certain of the sponsors of the use of "quantity equations" have opened themselves to this essentially absurd type of attack by themselves carrying on statistical investigations designed to "test" the "accuracy" of the "equation of exchange." [22]

It is also highly doubtful whether anything more than the crudity of available statistical material is responsible for a possible source of discrepancy to which attention has been called by Wesley C. Mitchell.[23] It is obvious that the "prices" (P) in, say, the Fisherine equation should always be understood as the "prices" at which the objects included in the T of the equation are actually sold. By reason of the fact that the published statistical material on prices, on the one hand, and on the objects sold at those prices, on the other, is usually derived from two separate sources, it happens that it is often impossible to be sure that the prices included in our statistical computations do not refer to goods other than those included in our computations for the total of objects sold during the period in question. This, after all, is the essence of Professor Mitchell's contention that prices agreed upon, and therefore recorded, in the present, may apply to goods to be delivered in the future. Obviously, however, to write an equation supposed to explain why the objects included in the T of the equation sell at a given level of prices, and then to insert in such an "equation" prices which apply not to the objects included in T, but to an entirely different set of objects, is to write an expression which Professor Mitchell rightly regards as "non-

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[22] See, for example, the remarks by Fisher on the "verification" by "actual statistics" of "the equation of exchange," in *The Purchasing Power of Money*, 276 ff., 292 ff. Fisher attributed a similar procedure to E. W. Kemmerer (cf. Fisher, *op. cit.*, 276 ff.). In justice, however, to Professor Kemmerer, whose comparison (*Money and Credit Instruments*, 141, 148) between the recorded values of P and the results obtained by independent computation of the magnitudes represented in the formula $(MV + M'V')/T$ —his "relative circulation"—forms the basis for Fisher's reference, it may be pointed out that Professor Kemmerer himself did not describe the operation in question as an attempt to "verify" the "equation of exchange." On the general usefulness of such attempts at "verification," see the comments of E. Petersen in his *Den moderne kvantitetsteoris gyldighet for pengeverdiens bestemmelse* ("The Validity of the Modern Quantity Theory for the Determination of the Value of Money"), Oslo, 1933, 10.

[23] See Mitchell, *Business Cycles: the Problem and its Setting*, 130 f. The present discussion has to do only with the suggestion that the Fisherine equation does not apply to the same period of time as do the MV and the T of that equation. The other cases discussed by Mitchell as possible sources of an "indeterminacy" in the relationship between the two members of the equation are virtually identical with the first group of Newcomb's "exceptions to the equation of societary circulation," discussed below, pp. 51 ff.

sense."[24] The only question worth asking, therefore, is whether a justification for such a practice has ever been attempted on any grounds other than the crudity of our statistical data. At any rate, I do not know of any such attempt.

It should hardly be necessary to add, in connection with the fact that prices are often agreed upon before either goods or money change hands, that this circumstance does not in the slightest degree invalidate the proposition that these prices are, in every significant sense, "determined" by the relative magnitude of the subsequent money- and goods-streams.[25] This is a matter which by this time, fortunately, is a commonplace of our textbooks.[26] In view of the fact, however, that the prices arrived at, in the case indicated, are based upon "anticipations" with respect to the magnitude of the money- and goods-streams, respectively, and in view of the further fact that it has been recently suggested that the "enlargement" of the "validity" of certain of our equations demands the "explicit introduction" of "anticipations," it may not be amiss to emphasize here that the point has no bearing upon the validity of the particular equations with which we are here concerned.[27] For it must be obvious that to argue, for example, that the equation of

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[24] Mitchell, *op. cit.*, p. 131 n. It should be pointed out, in justice to Mr. Keynes, that he himself was insistent upon this point in his discussion of the Fisherine equation. See the *Treatise*, I, 239. It is also true, however, that the conclusions which he drew from the point can hardly be regarded as warranted. Cf. the comment of Mr. Robertson on the conclusion in question in his "Note on the Theory of Money," *Economica*, August, 1933, 246; and, for a discussion of Keynes's conclusions themselves, see below, pp. 429 ff.

[25] The contrary has been implied by those who would insist that the prices included in the equation of exchange must include only "current market prices, *i.e.*, prices at the time when they are fixed by contract." See, for example, M. A. Copeland, "Money, Trade, and Prices—a Test of Causal Primacy," *Quarterly Journal of Economics*, XLIII (1929), 658 f.

[26] See, for example, Edie, *Money, Bank-Credit, and Prices*, 199; and cf. also Mitchell, *op. cit.*, 132, on the relation between "a man's decisions regarding prices" and "the quantity of coin, paper money, and deposit currency in his possession."

[27] For the suggestion to which reference is made in the text, see J. R. Hicks, "Mr. Keynes' Theory of Employment," *Economic Journal*, XLVI (1936), 240. The rôle of anticipations in the theory of output is discussed in Volume II of this study. Further justification for calling attention to Mr. Hicks's comment at this point is found in the fact that he cites, in this connection, C. F. Roos's *Dynamic Economics*, on p. 235 of which there appears a suggestion to the effect that one of the reasons why a quantity equation of the general form of the Fisherine equation is "no more than a first approximation" to "an equation representing the exchange of goods for money" is that it "fails to recognize" that the prices of certain types of goods depend upon certain "expectations" and that therefore these prices "are largely psychological." Cf. also pp. 239 f. of the same work, where a formulation is presented which purports to represent the forces determining the "prices of capital goods," and in which specific place is given to such elements as γ_{ij}, representing "a factor used in estimating future income," and μ_i, representing "a mortgage or bond ratio," both γ_{ij} and μ_i being described as "largely psychological."

exchange is invalidated by the fact that the actual streams of money and of goods, respectively, may turn out to be of magnitudes different from those anticipated, is to forget that, by the terms of the argument, the particular prices involved have already been set. The only effect, therefore, of a subsequent divergence of the actual from the anticipated magnitude of the two streams must be that *other* prices, which were *not* set considerably in advance of the actual passage of money against goods, will be other than they would have been if anticipations had been correct: since the parts of the two streams definitely allocated by the price agreement must be subtracted from streams of a magnitude different from that which had been anticipated. In cases, on the other hand, in which price agreements are cancelled, because of changed conditions, the prices previously agreed upon are no longer data for our problem, which is the determination of the prices at which objects actually sell; the cancelled prices must therefore be replaced by prices which will be determined by the actual magnitude of the money- and goods-streams, respectively. In all cases, however, the prices involved are the prices at which objects included under the T of the equation are actually sold; and in all cases the prices are, in a fundamental sense, determined by the magnitude of the streams of money and goods, respectively, which actually meet in the process of purchase and sale.[28]

Of the forms of the argument questioning the mathematical equivalent of the two members of a given "quantity equation" which deserve serious consideration, the most important, for our purposes, reduce to the proposition that the variables, other than "prices," which are included in the "quantity equations" do not in fact represent a complete list of the forces determining these "prices." These "equations," that is to say, are not true equations because they are incomplete. This, of course, is merely another way of putting the proposition that the expressions in question can be regarded as valid equations only if we assume the constancy of certain relationships of which no explicit mention is made in the so-called "equations" themselves.

A simple illustration should make the point clear. If it could be shown, for example, that the M and M' of the Fisherine equation are so defined as to exclude means of payment which may in fact be directed against the T of that equation, and which thus help to determine the P whose movements it is our task to explain, it is conceivable that the P could change without any change in the other variables of

[28] For a further discussion of this point, in another connection, see below, pp. 429 ff.

the Fisherine equation. We should then say that the expression generally known as the Fisherine equation is not a true equation, since it is incomplete to the extent that it omits certain essential terms from the first member.[29] Alternatively, it may be alleged that the so-called Fisherine "equation," if its terms are defined in the way indicated, is a true equation only upon certain assumptions with respect to the constancy of certain data not specifically included in the expression as it stands. Thus, if we let M_x represent the total amount of all kinds of spendable units that may conceivably act as money, of which M and M' are the only kinds that are actually functioning as such at a given time, then, in writing $MV+M'V'=PT$, we are, in effect, assuming that $M_x-(M+M')=0$. Or, more generally, we are assuming a relationship of the form $M_x/(M+M')=k_x$, in which k_x is a constant.

It is obviously easy to pass from this simple example to a generalized statement with respect to the nature of the assumptions concerning the constancy of relationships which, although not given a specific notation in a given quantity equation, may nevertheless be said to be involved in such an equation. If, for example, we have a "quantity equation" of the form $MV=PT$, in which MV is taken to represent the *total of money payments for all purposes whatsoever made during a given period of time,* and the T is taken to represent *the total of goods and services transferred during that period of time,* it is obvious that the expression $MV=PT$ will be a true equation only upon the assumptions: (1) that $(MV)_t/MV=k_1$, in which $(MV)_t$ represents the *total of money payments actually directed against goods and services for money during the period of time in question,* and k_1 is a constant; and (2) that $T_{mv}/T=k_2$, in which T_{mv}

[29] It was, in fact, precisely on this ground that some of the earlier commentators on Fisher's equation denied it anything more than an "approximate" character as an expression of "mathematical equality." See, for example, R. H. Hess, in the *Bulletin of the American Economic Association,* 4th series, Vol. I, No. 2, p. 65. It is worth pointing out, however, that, so far from its being true that objections of this kind constitute a valid objection against the general *form* of the Fisherine equation, one of the very earliest of the algebraic expressions of this type—namely, that of Sir John Lubbock—provided explicit notation for media of payment other than cash and bank deposits—for example, bills of exchange when used as media of payment. Cf. above, pp. 11 f.

represents the *total of goods and services actually sold for money during the same period,* and k_2 is also a constant; or (3) if k_1 and k_2 are variables, that $k_1 = k_2$.[30] It can, in fact, be shown that this generalized statement may be used to cover every one of the cases which, from the time of Simon Newcomb, have been advanced as representing what Newcomb himself called "exceptions to the equation of societary circulation"—that is, cases involving an asymmetry as between the two members of a given "quantity equation."[31]

The first of Newcomb's "exceptions," for example, was represented by what he called "the act of incurring debts."[32] What this means is that there will be a transfer of goods without a corresponding money payment; hence there will be a decline in T_{mv} relative to T,[33] that is, a fall in the ratio $T_{mv}/T = k_2$. Similarly, the repayment of a debt will mean that there will be a transfer of money without a corresponding transfer of goods; hence there will be a decline in $(MV)_t$ relatively to MV,[34] that is, a fall in the ratio $(MV)_t/MV = k_1$. It is obvious, therefore, that the "equation of societary circulation" of the form $MV = PT$, when the terms are defined as above, would be strictly true only when k_1 and k_2 were either not changing, or, as Newcomb himself pointed out,

[30] These conclusions follow from simple recognition of the fact that the expression $(MV)_t = P \cdot T_{mv}$ is necessarily true by definition. If k_1 and k_2 are regarded as variables, the expressions $(MV)_t/MV = k_1$ and $T_{mv}/T = k_2$ are likewise necessarily true by definition. The expression $(MV)_t = P \cdot T_{mv}$, when rewritten so as to include MV and T, the terms being defined as in the text, then becomes $MV \cdot k_1 = P \cdot T \cdot k_2$. It follows that $MV = PT$ only when k_1 and k_2 are constants, or, if they are variables, only when $k_1 = k_2$.

[31] See Newcomb, *Principles of Political Economy,* 332 ff.

[32] Newcomb, *Principles,* 332, 344 f. Cf. also Fisher, *The Purchasing Power of Money,* pp. 370 f. It will be observed that recognition of the fact that some forms of the "quantity equation" involve assumptions with respect to the constancy of certain relationships not given a specific notation in the equation itself, instead of being a discovery by those unsympathetic to the use of these equations, is to be found in the earliest writings of those most influential in the popularization of these equations. Their recognition was, indeed, so complete, even to the details respecting the factors, under certain circumstances, making for asymmetry, that it is not necessary to cite other writers than Newcomb and Fisher in order to obtain a virtually complete list of the factors cited by subsequent critics of the equation.

[33] Newcomb applied this reasoning also to the case of the incurring of debt by governments. See Newcomb, *Principles,* 333. He also pointed out that, under certain circumstances, the incurring of debt could be expected to affect what we have designated as the ratio $(MV)_t/MV = k_1$. Cf., in this connection, what is said in the text (p. 52) with respect to the "incurring of debt on a considerable scale," and note 37, thereto.

[34] This case, also, was applied by Newcomb (*Principles,* 333) to the repayment of debt by a government. It will be observed, incidentally, that the case of repayment of debt is in this respect exactly analogous to the case of prepayment of debt adduced by Mitchell, *Business Cycles: The Problem and its Setting,* 131.

were changing in equal degree.[35] The case of what Newcomb called "bankruptcy" would thus represent a further possible source of asymmetry in the "equation of societary circulation"; for, in effect, "bankruptcy"—that is, the non-payment of debts—would mean that a larger proportion of the total of money payments (MV) would be directed against the current purchase of goods than would have been the case if the same money had been used to pay debts.[36] In other words, we should have an increase in the ratio $(MV)_t/MV$. Similarly, the incurring of debt on a considerable scale, if it were accompanied by a determination on the part of the borrowers to maintain their total monetary expenditures at the same rate, would likewise mean a significant increase in the ratio[37] $(MV)_t/MV = k_1$. Conversely, the repayment of debt on a considerable scale would mean a significant decline in the ratio[38] $(MV)_t/MV = k_2$.

The second broad group of "exceptions" recognized by Newcomb, all of which really arise from the use of methods for the exchange of goods and services which do not demand a corresponding movement in the stream of money-outlay, is likewise easily translatable into the generalized formulation represented above. Thus, in the case of barter—or,

[35] See Newcomb, *Principles,* 332, 344 f.

[36] Newcomb, *Principles,* 332.

[37] See Newcomb, *Principles,* 345, on the "apparent redundancy of the monetary flow," in such cases, so that "the result will be the same as in the case of an increase of the volume of the currency; that is, a universal demand for commodities of all kinds, with a tendency towards a rise of price." However, he also pointed out the possibility—although he regarded it as "an extreme case"—that the borrowers might not choose to maintain their total monetary expenditures at the same rate, so that "the monetary flow would for the moment entirely cease," in which case, obviously, the price-raising effect of the incurring of indebtedness would follow solely from the decline in the ratio $T_{mv}/T = k_2$.

[38] See Newcomb, *Principles,* 345, 385. The argument had been anticipated a generation before by Sir John Lubbock, who had written, as the second of his "equations of condition, which connect the quantities which occur in the theory of currency," $qC = rA + pB + nC$, in which qC is essentially equivalent to an MV representing all types of money-transfer, and nC is equivalent to the MV of his first equation as transliterated on p. 11, above, while pB represents the amount of "bills paid in money." (Cf. Lubbock, *On Currency,* iv, n., where the term mB appears by mistake in place of pB; also 5 f., 43, of the same work. On the meaning of the term rA, see below, p. 54, n. 44.) The comment with which Lubbock accompanied an elaboration of his equation is worth quoting in view of the fact that the case in question is in reality nothing more nor less than one of the instances whose inclusion under the head of an "excess of saving over investment"—*when this excess is regarded as a factor determining the level of prices*—gave rise to so much confusion in the discussion arising out of the argument of Keynes's *Treatise.* "This equation," said Lubbock, "serves to show how, after a period of speculation and confidence, in which the quantity of bills generated is increased, when they become due, the transactions for money [i.e., the nC of the equation] must be curtailed in order that engagements may be met, unless the amount of money has been increased." (Lubbock, *On Currency,* 5.) The relation of the point involved to the theory of saving and investment will be discussed in more detail in Volume II.

as Newcomb called it, the "direct exchange of goods and services"—we should have a decrease in the ratio[39] $T_{mv}/T = k_2$. The same thing holds for those "great speculative transactions"—whether in commodities or securities—which, as the result of the introduction of devices for "clearing," involve very considerable transfers of ownership without any money-outlay to correspond.[40] It holds also for the cases in which, as a result of the offsetting of debts, a single money payment suffices for what in effect are two separate transfers of property.[41]

Finally, according to Newcomb, there is a group of cases involving a unilateral payment of money, as in the case of taxes levied for purposes which do not represent the provision of services by the government.[42] In this case, we should have a decrease in $(MV)_t/MV = k_1$ without any change in $T_{mv}/T = k_2$, since, by hypothesis, there would be no increase either in the total of services produced or in the total of services exchanged against money.

The cases cited by writers subsequent to Newcomb as representing instances in which "part of the total transfer, or flow" of property that is changing hands is "not balanced by a reverse flow of money," or vice versa, are likewise easily translatable into the general formula suggested above.[43] An increase in the amount of "gifts, bequests," and "charity," for example, would represent a unilateral type of money-payment, corresponding to Newcomb's third group, and hence a case of a decrease in the ratio $(MV)_t/MV = k_1$. Money payments representing merely the change of one form of currency into another would likewise represent a decrease in this ratio.[44] Again, if, on the one hand, the

[39] See Newcomb, *Principles*, 332 f. For a similar recognition, on the part of sponsors of the use of a "quantity equation," of the effect of barter upon the validity of certain forms of the quantity equation, which has seemed to some critics of these equations as a matter of very great importance for the validity of the equations, see Fisher, "The Role of Capital in Economic Theory," *loc. cit.*, 516, and J. P. Norton, *Statistical Studies in the New York Money-Market* (1902), 2.

[40] Newcomb, *Principles*, 333.

[41] Newcomb, *loc. cit.* Cf. Fisher, *Purchasing Power of Money*, 371.

[42] Newcomb, *loc. cit.* The fact that Newcomb chose, for the sake of simplicity, to assume that the government would in fact provide such services does not alter the fact that he recognized clearly that, if such service were not in fact provided, we should have "yet another . . . exception to the equation." Cf. also the reference to Sir John Lubbock's formulation below, p. 57, note 51, and p. 61, n. 59.

[43] Cf. Fisher, "The Role of Capital in Economic Theory," *loc. cit.*, 516, from which virtually all the cases which follow are taken.

[44] The cases in question were summarized by Fisher (*loc. cit.*) as representing an exchange of "deposits against deposits; as when a draft is purchased by a cheque; . . . money against money, as in 'changing' a bank-note," and "money against deposits." (Cf. also *The Purchasing Power of Money*, 47, 53.) Similarly, Norton (*Statistical Studies*, 1 ff.) characterized the passage of "Media of Exchange against Media of Exchange" as one of the "four permutations"—along with barter and the reciprocal exchange of money against commodities—of which "the two elements, commodities and media of exchange," are capable. As a commentary, however, upon the emphasis with which some critics of certain

MV and the T of a given "quantity equation" are defined in such a way as to make no distinction between domestic payments and property-transfers, on the one side, and foreign payments and property-transfers, on the other (for example, if MV is taken as representing the total of money payments made by the inhabitants of one country, and T the total of sales, domestic and foreign, made by the inhabitants of that country) and if, on the other hand, $(MV)_t$ and T_{mv} continue to be defined in such a way as to make them refer to a single national community (that is, so that $(MV)_t$ will represent the total payments made for objects sold in a given country, and T_{mv} the total of these objects), it follows that an increase in the expenditure upon foreign items will be represented by a decline in the ratio

$$\frac{(MV)_t}{MV} = k_1,$$

and a decrease in such expenditure in a rise in k_1; whereas an increase in exports, or a decline in imports, will be represented by a decline in

$$\frac{T_{mv}}{T} = k_2$$

and a decrease in exports, or a rise in imports, in a rise[45] in k_2. Again, therefore, the particular "quantity equation" represented by the expression $MV = PT$, when the terms are defined in the way indicated above, would be true only upon the assumptions that (1) no changes take place in the factors affecting the magnitude of k_1 and k_2, respectively; or (2) the changes which take place in the factors affecting k_1 and k_2, respectively, are of such a mutually compensating character as to leave k_1 and k_2 themselves unchanged; or (3) if k_1 and k_2 change, they change in equal degree.

Once the charge with respect to hidden assumptions of constancy in the familiar "quantity equations" is reduced to an allegation of incompleteness in the number of variables necessary to make the expressions involved true equations, it becomes clear that what is involved is not the validity of "quantity equations" as such, but merely the validity of

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forms of the "quantity equations" (for example, Anderson, *The Value of Money*, 365 ff.,) have seized upon this type of factor as affecting the total of MV, it is worth noting not only that Fisher insisted that "in a complete theory," account should be taken of all these factors, but also that Sir John Lubbock, who, as was pointed out above, p. 11, was one of the earliest writers to formulate an algebraic equation of exchange of the "Fisherine" form, went out of his way to include, in the second of his "equations of condition, which connect the quantities which occur in the theory of currency," a special term to take care of the type of factor later indicated by Fisher, namely, rA, representing "the amount of checks paid in money."

[45] On the "modification of the equation of exchange required by international trade," see Fisher, *The Purchasing Power of Money*, 28, 372 ff. In the cases indicated in the text, it is of course assumed that other factors—for example, the total of T—remain equal.

certain expressions which, though called "equations," are not true equations because their two members are not symmetrical. One has, however, merely to put the problem in these terms to observe that the missing symmetry can be provided, and the expressions involved converted into true equations, by one of two methods—both of which, in fact, have been applied in the treatment of the particular "quantity equations" whose validity is here under discussion.

IV

Methods of Correction: Insertion of Missing Terms

The first of these methods would establish the required symmetry by introducing into the expression under discussion a specific term for the factor whose absence makes for asymmetry. Obviously, there is no clear *a priori* reason why the introduction of the missing terms should in any way necessitate the abandonment of the general form of the more familiar "quantity equations," which, explicitly or implicitly, represent a stream of money (in the determination of whose magnitude the "quantity of money" is an important element) against a stream of objects offered for sale against this stream of money.[46]

The process involved of course amounts, in terms of the generalized formulation given above, to a writing of the expression $MV=PT$ in the form $(MV)\cdot k_1=P\cdot T\cdot k_2$, in which k_1 and k_2 are now taken to be variables rather than constants. It is clear, however, that such a procedure becomes a mere bit of formalism unless the nature of the forces affecting the magnitude of k_1 and k_2, respectively, is clearly understood and accurately described in each case. In practice, therefore, the process should take, and indeed has taken, the form of the insertion of a special notation for

[46] It is of some importance to note that, despite frequent suggestions to the contrary, the particular "quantity equations" which take the form of "cash-balance" equations provide no exception to the general rule that "quantity equations" represent a stream of money against a stream of objects offered for sale against this stream of money. It is, indeed, this fact which makes the establishment of the relationship between the two types of "quantity equation" a matter of great simplicity. On certain of Mr. Keynes's utterances respecting the nature of "quantity equations" of the "cash-balance" form, which have unfortunately tended to obscure the true nature of the issues involved, see below, pp. 425 f.

each of the specific terms whose absence had made for asymmetry.

Thus, in the first example given above, the asymmetry arose from the fact that the T—and therefore the P—of the particular "quantity equation" used included transactions which were paid for not only by the M and M' of the equation as given, but also by media of payment the magnitude of which was represented by the expression $M_x - (M + M')$. Since $M_x/(M + M') = k_x$, symmetry may be restored to the expression $(M + M')V = PT$ simply by writing $k_x(M + M')V = PT$; or, alternatively, if we let M'' represent the type of currency previously omitted from the formulation, we might write $(M + M' + M'')V = PT$.

It is easy to demonstrate that this has, in fact, been the practice of those sponsors of the use of equations of the "Fisherine" type who have taken pains to point out the possibility of asymmetry as between the two members of a given expression presented as an "equation of exchange." Newcomb, for example, proposed to correct the specific difficulty arising from the possibility of "bankruptcy," which, as we have seen, might be represented as an increase in the ratio $(MV)_t/MV = k_1$, by inserting, in an expression of the form $MV = PT$, a special term B, so that we have $MV + B = PT$, which is nothing more than a special form of the expression[47] $MV \cdot k_1 = PT$. Similarly, Fisher proposed to deal with the "effect of time credit" (which of course corresponds to Newcomb's first group of "exceptions to the equation of societary circulation") by letting E'' represent the "creation" of "time loans" and E''' their "extinguishment," and then writing $MV + M'V' + E'' - E''' = \Sigma pQ$, which is again nothing more than a special form of the equation $MV(k_1/k_2) = PT$, the conditions for the accuracy of the expressions $MV + M'V' = \Sigma pQ$ and $MV = PT$ being $E'' - E''' = 0$ and $k_1/k_2 = 1$, respectively.[48]

Essentially the same procedure was followed by Fisher in dealing with the "modification" of the equation of exchange which is supposed to be "required by international trade."[49] If we write $MV = H + O$, in

[47] See Newcomb, *Principles*, 332. The same procedure is implied in his suggestion—though he did not, as in the case of "bankruptcy," actually give the suggestion algebraic form—that, in cases when "A purchases from B and B from C, and A pays C directly, and thus cancels both debts with one payment, . . . such a payment should be counted as made from A to B and from B to C" (*ibid.*, 333). A similar procedure, finally, is implied in his suggestion—though in this case, also, he did not give his suggestion algebraic form—that "we may . . . consider the general benefit rendered by the government as . . . an industrial flow, and then the balance will hold good." (*Ibid.*)

[48] See Fisher, *The Purchasing Power of Money*, 370 f. A somewhat different notation to cover the elements designated by Fisher as E'' and E''' is used by F. Vinci, in *Econometrica*, II (1934), 130 f., but Vinci refers to Fisher's formulation as the basic one.

[49] *Ibid.*, 372 ff. The summary of Fisher's suggestion given in the text is really a free paraphrase of the original, and attempts, among other things, to simplify the algebraic notation.

which H represents "the sum of purchases at home" and O the sum of payments "outward" (that is, for imports); if we write also $T = Q_h + Q_o$, in which the subscripts refer to goods sold at home and those sold abroad, respectively; and finally, if we let Q_i represent the quantity of goods imported for domestic sale, it will be obvious that, strictly speaking, MV will be equal not to PT (which, by definition, is equal to $P(Q_h + Q_o)$) but rather to $P(Q_h + Q_i)$. Conversely, PT is equal, strictly speaking, not to MV (which, by definition, is equal to $H + O$) but to $H + I$, in which I represents the "sum of payments coming inward" from abroad. To write $MV = PT$, in other words, assumes that $H + I = H + O$, and that $P(Q_h + Q_o) = P(Q_h + Q_i)$, that is, that $I = O$ and $Q_o = Q_i$. We must write, instead, $MV + I - O = P(T + Q_i - Q_o)$. It will be seen that $I - O$ corresponds to our k_1 and that $Q_1 - Q_o$ corresponds to our k_2, in the sense that:

1. Movements in O and Q_o will be inversely correlated with movements in k_1 and k_2, while movements in I and Q_1 will be directly correlated with movements in k_1 and k_2.
2. The expression $MV = PT$ will be a true equation whenever the factors affecting I, O, Q_1, and Q_o either, first, change in such a way as to leave unchanged the magnitude of $(I - O)$ and $(Q_1 - Q_o)$—the equivalents of our k_1 and k_2, respectively—or, second, change in such a way as to cause $(I - O)$ and $(Q - Q_o)$ to change in equal degree.

With these examples before us of a readiness, on the part of the two writers principally responsible for the popularization of the best known of the "quantity equations," to "correct" any demonstrated asymmetry in these equations by the insertion of the missing variables, the proposal, by later writers, to "correct" the equation of exchange by the insertion of additional variables in one or the other side of the equation can hardly be regarded as representing a sharp break with "tradition." This would be true, for example, of the proposal, by M. A. Copeland, to insert a term R in the second member of the equation, in order to take care of transactions whose exclusion would make the expression $MV = PT$ asymmetrical if PT were to be interpreted as including only transactions representing "the sale of a specifiable volume of goods at a specifiable price."[50] Indeed, a suggestion almost identical with that of Copeland had been made by Sir John Lubbock, in one of the very earliest algebraic formulations of the "equation of exchange" that have come down to us.[51]

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[50] M. A. Copeland, "Special Purpose Indexes for the Equation of Exchange for the United States, 1919–1927," *Journal of the American Statistical Association,* XXIV (1929), 114. Cf. below, pp. 60 f., and nn. 58 and 59, thereto.

[51] The E of Lubbock's equation, which is transliterated above, p. 12, as resembling Copeland's R, was designed to cover "the payments described in the fourth and fifth categories" listed in Lubbock's "Attempt to Classify Pecuniary Transactions"—namely, (4) payments of acceptances that had become due and (5) "The payment of taxes, rates, etc., etc., etc. Gifts. The payment of the interest on loans and mortgages. Payment of rents." (*On Currency,* 1, 3.) This is not to say, of course, that there are not diver-

Insofar, moreover, as the "transactions" included by Copeland in his R are represented in the first member of the equation by unilateral money payments, such as "charity subscriptions," an analogue to the insertion of the term R in the second term of the equation is provided by Newcomb's suggestion, to which attention has already been called, that unilateral goods-transfers, in the second member—as represented by cases of "bankruptcy"—should be compensated for by a special term (B) in the first member.[52]

In view of the fact that Mr. Keynes's charge that the older "quantity equations" are "statical" in character is now being investigated on the assumption that it may be interpreted to mean that these equations involve the assumption of constancy in some relationship not given a specific notation in the equations themselves, it is worth noting that Mr. Keynes himself experienced no difficulty in finding the same sort of "remedy" that the original sponsors of the Fisherine equation had found for this type of "defect," when nothing more serious was involved than an alleged omission of what Mr. Keynes regarded as the equivalent of more generally recognized forms of money-spending power—namely, "unused overdraft facilities."[53] The case, it will be noted, is exactly comparable to the simple example given at the outset of this discussion; and it is worth observing that Mr. Keynes's "remedy" was precisely that which has always been adopted in such cases; that is, the assigning of a term to describe the relationship of the omitted variable to variables already included in the equation (the $k_x = M_x/M + M'$ of our example), and the subsequent incorporation of this term into the equa-

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gences between the list of items included in Lubbock's E and Copeland's R. In this connection, see below, p. 61, n. 59. The resemblances are, however, more striking than the differences.

[52] It should be pointed out, however, that Copeland's R, though it is designed in part to compensate for unilateral payments, is also designed to cover much more. See below, pp. 60 f., and notes 58 and 59 thereto.

[53] See the *Treatise*, I, 236. Keynes argued also—although in both cases he admitted that the importance of the correction depended upon the form of the statistics used to measure M (his "total deposits")—that the M, as so defined, was too inclusive, as well as too exclusive, and should therefore be corrected by a coefficient (w) which would represent that part of total deposits—namely, "cash deposits," as opposed to "savings deposits"—which was really relevant to the determination of the P of the Fisherine equation. This, clearly, is equivalent to arguing, as Mr. Keynes himself argued, that the use of the expression $MV = PT$ assumes that this coefficient is unity. The coefficient w thus becomes a special case of k_1 of our generalized formulation; and it is to be noted that the "remedy," as in the case of the "omission" of unused overdraft facilities, was the insertion of this coefficient into the equation $MV = PT$, which then read $Mw \cdot V = PT$. The remaining aspects of Keynes's formulation which call for comment (such as, for example, [1] the treatment of "unused overdraft facilities" as equivalent to a form of money-spending capable of serving as a cash-balance; [2] the special treatment accorded to "savings deposits," and [3] the treatment of the "velocity of cash deposits" in relation to "unused overdraft facilities") are touched upon briefly in a later chapter. See below, pp. 467 ff. and 472 ff.

tion itself.[54] It is of some importance, in short, to emphasize the fact that Mr. Keynes, who here showed himself a faithful follower of "tradition," even to the details of the way in which a traditional difficulty was traditionally handled, did not for a moment suggest that the mere fact that a given quantity equation of the Fisherine form may be charged with having assumed the constancy of certain relationships not given specific notation in the equation itself argued for the abandonment of the *general form* of the Fisherine equation.

The *necessity* of adopting this first method for correcting a demonstrated case of asymmetry as between the two members of a given "quantity equation" (that is, the method of inserting a term to represent the variable whose absence was the cause of the asymmetry) cannot be questioned as long as we insist, first, upon retaining the meaning already assigned to the other terms of the questioned expression and, second, upon presenting this expression as an "equation" which will be true under all circumstances. It is to be emphasized, however—and the point will be developed at greater length in the following chapter—that the *usefulness* of this method depends entirely upon the significance of the equation as finally formulated for what are properly to be regarded as the major problems of monetary theory.[55] If the sole effect of the operation in question is to validate an expression whose only virtue is that the definitions assigned to the terms included therein coincide with the statistics that we happen to possess, though the terms themselves may not represent magnitudes in which we are really interested, little, indeed, is accomplished.

Thus it is difficult to see anything more than an avoidance of the relevant analytical problems in the practice, sometimes followed, of introducing a special "balancing factor," for which the primary justifica-

[54] Corresponding to the k_2 of our example was Mr. Keynes's w', which was defined by the equation

$$w' = \frac{\text{Volume of overdraft facilities}}{\text{Volume of cash deposits } (Mw)}.$$

The "volume of overdraft facilities" thus becomes Mww' and is inserted as such in the Fisherine equation, which then becomes $(Mw + Mww') \cdot V' = PT$, in which V' represents "the velocity of circulation of [total] cash-facilities," as opposed to V, the "velocity of circulation of cash-deposits." The writing of the expression $MV = PT$, then, according to Keynes, assumes not only that $w = 1$ (see the preceding note), but also that $w' = 0$. Thus the full expression which is to be taken as the equivalent of a special case of our k_1 is $w(1 + w')$.

[55] See, especially, below, pp. 99 f.

tion is that it is "obviously necessary . . . on account of the employment of indices" that reflect the inadequacies of available statistical data.[56] From the standpoint of the development of a significant theory of prices, surely, such a procedure is not greatly removed from the practice, commented upon above, of denying validity to the Fisherine equation on the ground that independent statistical calculations with respect to the magnitude of the variables included in the two members of the equation show that the expression is not a true equation, nor from the converse practice of arguing that such statistical calculations demonstrate the essential "truth" of the equation.[57]

The situation is only slightly improved, moreover, when the "balancing" factor, even though its introduction be justified on more cogent grounds than the fact that, in its absence, the statistical measures of the two members of the "equation" do not result in an equality, is nevertheless the resultant quite as much of the shortcomings of our data as it is of considerations of an analytical character. If, for example, we have included in the T of our equation, as we ought, "services" as well as "goods," no great analytical significance attaches to the segregation, in a separate term, of services (such as the services of government, of insurance companies, or of agents whose service is recompensed otherwise than by a time-unit such as the hour or day), which can certainly be said to vary in magnitude as well as in cost (that is, "price") but whose magnitude is statistically measurable only through the use of various devices of approximation.[58] In any case, there are certainly objections

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[56] Cf., for example, J. H. Rogers, *The Process of Inflation in France* (1929), 284. On p. 304 of the same volume, to be sure, Professor Rogers suggested that his "balancing factor" $\bar{C}$ was the resultant not only of the failure of "estimated indices" to be "true indicators of the magnitude of their respective quantities" but also of "lags in any of the series with respect to any of the others," including "inaccuracies in the equation arising from the failure of payments to be made at the time of purchase." In practice, however, his $\bar{C}$ is simply equal to $\bar{V}'(M + M')/\bar{P}\bar{T}$; that is, a figure resulting from the failure of his statistical measures for the two members of the Fisherine equation to balance. It will be observed, in passing, that Rogers's "balancing factor" $\bar{C}$ has not the same significance as the "proportionality factor" λ, in an equation of the form $MV + M'V' = \lambda \cdot PT$, which has been proposed by C. F. Roos (*Dynamic Economics*, 235) as a means of taking care of the discrepancies arising from the use of indexes for P and T; for Roos's λ is a factor whose use becomes necessary only if, when indexes are used for P and T, absolute figures are used for MV and $M'V'$. It follows, obviously, that Roos's λ, from an analytical standpoint, has virtually no common basis with the k_1 and k_2 of our generalized formulation.

[57] Cf. above, pp. 46 f., and especially notes 21 and 22, thereto.

[58] The practice in question was followed by M. A. Copeland, on pp. 114 f. of his article, "Special Purpose Indexes," cited above, p. 57, n. 50. Cf. also the following note. By way of illustrating the proposition stated in the text, it may be pointed out that there would seem to be no convincing ground for saying, for example, that, when there is an increase in the amount expended upon the services of government, it is quite impossible to distinguish *analytically* the share in the increase which is due

to a lumping of terms of this kind, which deserve a special notation, if at all, only because of the statistical difficulties involved in their measurement, in a single term with other items—such as security transactions—which can be definitely shown, under certain circumstances, to have a type of consequence for the determination of "prices" that is quite different from the type of consequence following from an increase in the quantity of "services" sold for money.[59]

It must be admitted, finally, that in certain of the cases of asymmetry pointed out by Fisher himself, the proposal for emendation has significance chiefly for the validation of expressions whose terms were given their original definition primarily because of the nature of the available

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to an increase in the prices of the services purchased from the government and an increase in the kinds and amount of services provided. That the problem is one of great practical difficulty, calling for a very considerable amount of ingenuity upon the part of anyone essaying the task, goes without saying; but so are most of the practical problems involved in the construction of index numbers whose analytical basis seems at first sight very simple. The case of defining what is meant by "an automobile," or even "a Ford car," in an index designed to show the change in price of such objects, is as good an example as any. Yet no one has seriously suggested abandoning an attempt to distinguish between the changes in the amount spent upon automobiles which are due to variations in the price of a given amount of "service" provided, and those due to the provision of a larger amount of "service."

[59] Thus Copeland, "Special Purpose Indexes," *loc. cit.*, includes in his R not only (1) unilateral transfers, such as "charity subscriptions," and (2) *services* not sold "at a specifiable price," such as "personal and professional service enterprise revenues" and "most taxes," but also (3) such items as "capital-flotations," "short-time loans," and certain types of "bond sales." (It will be observed, therefore, that the characterization of Copeland's R by E. Lindahl [*Om förhållandet mellan penningmängd och prisnivå* ("On the Relationship between the Quantity of Money and the Price-Level"), (Uppsala, 1929), 6, n. 2] as representing "the monetary transactions which have no reference to real counter-offerings (*reala motprestationer*)" is not really accurate.) On the problems arising from the inclusion of security transactions in a general Quantity Equation, see below, pp. 576 ff. So far as the other items in Copeland's R are concerned, the case for their segregation from the items included in his T—though hardly for their inclusion in a single item along with security transactions—would be greatly improved if it could be shown that they differ from the items included in the T of his equation on some ground such as the relative flexibility of the two types of money-payment during periods of general price-change. Something of this sort seems to have been in the mind of Lubbock, who, in discussing the probability that, during periods of deflation, "the price of each article may be diminished in exactly the same proportion as the circulation," argued that "if, as is probable, the value of E is nearly constant," despite the reduction in the quantity of money, "the price of each article in mercantile operations"—that is, the prices of most of the items included in his equivalent of Σpq—"will be diminished in a greater proportion" than the reduction in the quantity of money. See Lubbock's *On Currency*, 37; and cf. the comment of Fisher ("The Role of Capital," *loc. cit.*, 519) with respect to certain items included in his T other than "commodities in the more restricted sense," such as "fixed payments, maturing debts, etc.," to the effect that "these fixed prices have a tendency to intensify either a fall or a rise of other prices."

statistics. This would seem to be true, for example, so far as the limits of the present study are concerned, even of the difficulties, dealt with by Fisher, arising from international trade—though they might be of considerable analytical significance for the theory of international prices.[60]

It is, however, anything but clear that all "corrections" of a given "quantity equation" by the insertion of the term or terms whose absence has made for asymmetry have little significance beyond the fact that it then becomes possible to obtain, from existing statistics, measures of the different variables involved. On the contrary, as we shall see, the discovery of a statistical discrepancy in the results obtained by the use of a given formula has often proved to be the method whereby variables both new and significant for the problem in hand have been discovered.[61]

Still less justification is there for the suggestion, for which Mr. Keynes and others have been responsible, that the chief virtue of all "quantity equations" of the Fisherine *form* is that they permit the use of variables that may be measured on the basis of statistics now available.[62] It is true that Sir John Lubbock, in presenting, in 1840, his "equations of condition, which connect the quantities which occur in the theory of currency," declared that there was no "reason to suppose that artifices of analysis can be employed with advantage in this subject," and suggested that "the chief difficulty which remains to be overcome appears . . . to consist in the accurate determination of the numerical values of the constants which enter into these equations"; and it is

[60] For Fisher's suggestions with respect to the modifications in the equation of exchange which are called for by the existence of international trade, see above, pp. 56 f.

[61] See below, pp. 95 ff.

[62] Thus, Keynes adduced, as "the great advantage" of the formula $MV = PT$, "the fact that one side of it, namely MV, fits in better than most [formulas] with the actually available banking statistics" (*Treatise*, I, 235), and was prepared to admit that in this respect, at least superficially, his own Fundamental Equations were at a "disadvantage" (I, 221). Even Professor Hayek has on occasion written in such terms as to imply that "the apparatus of mathematical formulae" represented by formulations of the type of the Fisherine "equation of exchange" has been primarily "constructed to admit of statistical verification" and is thus a "typical"—and presumably unflattering—"instance of 'quantitative' economics" (Hayek, *Prices and Production*, 3).

also true that some of the most notable among the later sponsors of "quantity equations"—Kemmerer and Fisher are examples—have been deeply interested in the "determination of the numerical values of the constants." [63] It is difficult, however, to see why this fact alone should lead to the conclusion that the "quantity equations" were expressly formulated with this end in view, and have little value otherwise. If this were the case, the fact that very shortly after the publication of the *Treatise* statistical calculations were presented by investigators in sympathy with the apparatus of the *Treatise* purporting to "evaluate quantitatively . . . a number of the most important factors in his [Keynes's] price equations" might likewise have been taken as *prima facie* proof that the "great advantage" of the equations of the *Treatise* was that they "fit in with" actually available statistics.[64] In actual fact, of course, others than Lubbock, Kemmerer, and Fisher, responsible equally with these three writers for the development and use of equations of the "Fisherine" form, have regarded such equations primarily as providing a starting point for further analysis, which in turn should result in a further development and elaboration of the equations themselves. It follows, therefore, that the thing to be said of any given "quantity equation" of the general Fisherine form is what was said of the general method thus far proposed for the establishment of an otherwise missing symmetry in a given "quantity equation": in all cases, the adoption of the device will show itself to be useful in proportion as the final formulation may be said to signalize our recognition of the importance of certain variables for the determination of a type of price-level which can be shown to have economic significance.

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[63] For the quotation from Lubbock reproduced in the text, see his *On Currency*, iv. On p. 43 of the same work, Lubbock, anxious "to obtain as soon as possible some approximation to the value of the different quantities" in his equation, went so far as to present a statistical table purporting to provide such an "approximation," though he himself characterized the figures there presented as "rude conjectures," offered "in the hope that others with better means of information will improve the table."

[64] For an example of such statistical investigations, see C. Clark, *The National Income, 1924–1931* (1932), 126 ff.

V

Methods of Correction: Redefinition of Terms

There is, however, a second method for rectifying instances of asymmetry in a given "quantity equation." This method, to be sure, does not commend itself to those who would insist that it is necessary to construct our analytical categories upon the basis of the available statistical material, instead of orienting our search for additional statistical material upon the basis of what is called for by significant theoretical analysis.[65] It is, nevertheless, the method which provides the most direct answer to accusations of the kind under discussion. The method in question is the following:

If we are prepared to define the terms of a given "quantity equation" without regard to whether statistics are at present available for the measurement of the terms as so defined, symmetry can be given to an otherwise asymmetrical expression simply by defining the terms in such a way that both members of the expression refer to the same thing over properly chosen periods of time. Under such conditions, the expressions in question can never be "untrue": they are necessarily true by definition.

[65] It should hardly be necessary to point out that those who would insist that our analytical categories must be developed on the basis of statistical material *now available* do a disservice to the cause of "quantitative economics" even when the latter is interpreted as demanding a conformance "to that important canon for scientific hypothesis that they should be so formulated as to be capable of empirical test"—that is, as demanding that our analytical categories must be of such a nature as to permit statistical measurement of the variables involved. (So, M. A. Copeland, "Money, Trade, and Prices—a Test of Causal Primacy," *loc. cit.*, p. 651; see also, in this connection, below, p. 90, n. 42.) Obviously, the case for restricting investigation to a study of variables which are "measurable" on the basis of currently available statistical material is vastly weaker than the case for restricting it to variables which would become "measurable" if we were prepared to spend a sufficient amount of energy in collecting the desired data. It goes without saying, of course, that even the case for restricting theoretical analysis to the latter type of variable is anything but clear, as was emphasized by Böhm-Bawerk in passages that are in many respects classic. See Böhm-Bawerk's "Grundzüge der Theorie des wirtschaftlichen Güterwerts," *Jahrbücher für Nationalökonomie und Statistik*, N. S., XIII (1886), 46 ff. (No. 11 of the *London Series of Reprints of Scarce Tracts in Economic and Political Science*). The relevant passages were inserted in the third (1914) edition of *Die positive Theorie des Kapitals*, 247 ff. Cf. also Böhm-Bawerk's discussion of "Theorizing with Unknown Magnitudes" (*Das Theoretisieren mit unbekannten Grössen*), in *Einige strittige Fragen der Kapitalstheorie*, 68 ff. (also included in Böhm-Bawerk's *Kleinere Abhandlungen über Kapital und Zins*, 224 ff.).

In the notation suggested above, the procedure would amount simply to defining the terms of the expression $MV = PT$ in such a way as to make them equivalent to the corresponding terms in the expression $(MV)_t = P \cdot T_{mv}$. It will be noted that such a formulation does not necessarily assume, as has sometimes been suggested, a "simultaneity" in "the processes of pricing, delivery, and payment."[66] The timing of the "pricing," as was shown above, has to do merely with the statistical problem of insuring that the proper prices are applied to the objects included in the T of the equation.[67] The relationship between "delivery" and "payment" in the equation as newly stated, moreover, is one of "simultaneity" only in the remote sense that only those objects shall be included in T which are actually paid for by money during the period in question, and that only those money payments shall be included in MV which go to buy the objects included in T during this period.

It will be noted, also, that it is by no means true that a procedure of this kind means, as has likewise been suggested, that "the relation between the time intervals covered by the two parts of the equation becomes indeterminate," and that the expression $MV = PT$, as newly stated, is one "which shows nothing about time."[68] The interval of time chosen in each case is the same—say, a year.[69] All that is done is to excise from the statistical measures for MV and T for the year under investigation those payments and goods-transfers, respectively, which do not conform to the criterion that the payments must be made against goods sold for money during that year and that the goods-transfers must be made against money during that year. The M will be represented by the total average cash balances held during the year, and the V will be derived by dividing the figure for M into the figure for total money-payments obtained after excision of the items just indicated.[70]

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[66] Cf. Burns, "The Quantity Theory and Price-Stabilization," *loc. cit.*, 576.

[67] This is virtually admitted by Burns when he points out (presumably with other considerations in mind) that, in establishing "an index of prices and an index of trade," a definition is "given to either of these terms," and the other is "*determined thereby*" (*loc. cit.*; italics mine). In the present case, the definition would be given to the T of the equation, and the prices would be chosen accordingly.

[68] So Mitchell, *Business Cycles: The Problem and its Setting*, 131.

[69] It is worth nothing, in this connection, that Lubbock was careful to insist that, in every case, the terms of his equation were to be defined with reference to the same "given interval of time, Δt" (*On Currency*, 3 ff.); and that several of the writers who later presented an "equation of exchange" went out of their way to use a notation that called special attention to the fact that the same "given interval of time" was involved in the definition of each of the terms included in the equation. See, for example, Norton, *Statistical Studies*, 2, 5 ff.; G. C. Evans, *Mathematical Introduction to Economics* (1930), 94 ff.; and Roos, *Dynamic Economics*, 233, 235 ff.

[70] Clearly, therefore, there is no basis for the suggestion (made, for example, by Foster and Catchings, *Money*, 163) that the redefinition of the terms, in the way indicated, causes the period of time represented by the equation of exchange to be an "imaginary period," with the result that the price-level which it includes is "never the price-level with which business has to deal." Insofar as such an allegation is based upon the argument that

So understood, the equation $MV = PT$ approaches much more closely to that which figures in the argument of its principal sponsors than is suggested by the allegation that "this truistic equation is not the equation of exchange with which monetary theorists generally work."[71] It is of course true, as we have seen, that both Newcomb and Fisher were prepared, if necessary, to follow the first method indicated above as a means of providing the missing symmetry in a given quantity equation: namely, the insertion of a specific term or set of terms designed to restore the balance. It is also true, however, that they were prepared to, and did, follow the method now suggested.

Thus, Newcomb specifically stated that, in order to "preserve the equality" between the "value" of the "industrial circulation" and the "flow of the currency," we must "exclude from the monetary flow all such transfers [of money] as . . . are not balanced by reverse transfers of wealth or services," so that, by "keeping the two flows in correspondence with each other, the flow of currency must, in the long run, remain equal to the total value of the industrial circulation as measured in money."[72] Again, he proposed specifically that there should be "excluded from our sums total" such "sales, and the payment made for them," as were represented by those "great speculative transactions" in which ownership might "pass back and forth between parties . . . without corresponding direct [money] payments."[73]

A further example of the readiness of the sponsors of the "equation of exchange" to provide symmetry by *definition*, as well as by the addition of terms representing missing variables, is represented by those instances in which T was defined in such a way as to exclude transactions (such as "barter" transactions) that do not represent a transfer of goods for money.[74] This was, indeed, explicitly recognized by these writers'

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the "price-level with which business has to deal" is in fact "affected" by transactions which are excluded from equations of exchange of the form under discussion, the allegation derives from a failure to realize that all that the "exclusion" of these factors means is that the factors in question get in their effect upon prices by affecting the magnitude of the factors which *are* included. In every case, however, the prices included in the equation of exchange are actually recorded, and not "imaginary," prices.

[71] So Burns, "The Quantity Theory, etc.," *loc. cit.*, 576.

[72] Newcomb, *Principles*, 327. The passage is strangely misquoted by Keynes, in the *Treatise* (I, 233 n.), where Newcomb is represented as having suggested that the transfers in question should be excluded, not from the "monetary flow," but from the "industrial circulation," despite the fact that for Newcomb, as Keynes himself notes (*loc. cit.*), the term "industrial circulation" meant "the volume of goods and services changing hands for money," and therefore has nothing directly to do with "the industrial circulation," in Keynes's sense of the term, as one might be led to suppose by Keynes's reference to "rapidity of circulation" in the same passage.

[73] Newcomb, *Principles*, 333.

[74] Thus Norton (*Statistical Studies*, 1 f.), not only presented, as his equivalent of Fisher's T, a term C_{ma}, representing "commodities when exchanged against the media of exchange," but emphasized the definition by including, in both members of his equation of exchange, a term C_e, repre-

critics, who used the fact to argue, not that this definition of T made the equation "untrue," but that it too greatly "limited" the usefulness of the resulting equation of exchange.[75] For our present purpose, however, it is sufficient to emphasize the point that the authors concerned not only were aware of the fact that they were "limiting" their equations, but introduced their "limitations" intentionally.[76]

The important point for the problem under discussion is that it is precisely expressions which are necessarily true by definition that are really involved in any discussion of the *validity* of "quantity equations" *as such.*[77] It must be immediately obvious, therefore, that there is no foundation whatever for any suggestion to the effect that these "quantity equations" are valid only upon the condition that certain magnitudes which are not given an explicit place in the equations are held constant. There is no question of holding anything "constant," either within or outside the equations in question. So long as the terms of these equations are defined in such a way as to make both members refer to the same things—and only to these things—over a period of time properly chosen in each case, the equation must be a true one. This, of course, is only another way of saying that all changes which can be shown to affect the "prices" in which we are interested must necessarily be reflected in one or more of the other variables in the equation.

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senting "commodities when bartered." See also Fisher, *The Purchasing Power of Money,* 251, where an increase in the proportion of trade carried on by barter in the period of extreme paper money inflation is represented as a decline in T.

[75] So, for example, Anderson, *The Value of Money,* 169 f.

[76] See, for example, Lubbock, *On Currency,* 3, where the author specifically pointed out that his "equations of condition, which connect the quantities which occur in the theory of currency" were to be understood to apply only to "the payments . . . in any limited territory, Great Britain for example."

[77] It follows that the key to the paradox that "quantity equations" should have been accused at one and the same time of being not valid under all conditions—that is, "untrue"—and of being "useless" because of their supposedly "truistic" character, lies in the fact that different forms of "quantity equation" have been involved in each case. It is perfectly possible that one specific "quantity equation" may be "untrue," and that another may be "truistic." The only thing that is not permissible is that the *same* quantity equation should be charged simultaneously with being "untrue" and with being "truistic" in character.

VI

A Test Case from Keynes's *Treatise*

It is in the light of these considerations that judgment must be passed upon the argument of Mr. Keynes, in the *Treatise,* that it is "conceivable" that "the cash-deposits may remain the same, the savings deposits may remain the same, the velocities of circulation may remain the same, the volume of monetary transactions may remain the same, and the volume of output may remain the same; and yet the fundamental price-levels may change." [78] Taken literally, what this must mean is that there is a fundamental asymmetry between the two members of the "quantity equation" which such a statement is obviously intended to summarize; and indeed this seemed to be the drift of Mr. Keynes's argument in the *Treatise.*[79] What he argued, in fact, was that the variables of the older equations—of which the list of variables included in the foregoing quotation was obviously taken as typical—did not represent a complete list of those variables which are in fact relevant for the determination of the "price-levels" in which he was interested, since they assumed tacitly the constancy of certain conditions with respect to the factors which were intended to be summarized by the relationship between "saving" and "investment." [80]

[78] *Treatise,* I, 147. This, it will be recalled, is the passage cited by Mr. Robertson as evidencing an apparent intention, on Mr. Keynes's part, to assert that, under certain conditions, the older "quantity equations" would not be true. Cf. above, pp. 16 ff.

[79] It will be recalled that, in his rejoinder to Mr. Robertson, Mr. Keynes sought an escape from the former's imputation to him of a charge that the older quantity equations would, under certain circumstances, lead to false results, by arguing that quantity equations of the "Fisherine type" were not involved in the dispute at all, since they were concerned with the price-level of "transactions," rather than of "output." (Cf. above, pp. 17 f.) On this matter, see below, especially pp. 514 ff. It is sufficient here to point out merely that the section in which the passage under discussion appears, and which is entitled "The Relation of the Price-Level to the Quantity of Money," begins with a proposition attacking the conclusions that would be reached by the use of any of the "old-fashioned quantity equations, however carefully guarded" (*Treatise,* I, 146).

[80] This was made particularly clear from the passage on p. 149 of the section of the *Treatise* under discussion, in which Keynes undertook to express his conclusions "in terms of the usual monetary factors" by the statement that a quantity equation of the form $\Pi = M_1V_1/O$ will hold true only upon the assumption that $I = I' = S$. The same argument is implicit in the second passage cited by Robertson; namely, that found on pp. 89 f. of the

From one point of view, therefore, the argument turns upon a question which we shall examine in Volume II of this study—namely, whether a discrepancy between "saving" and "investment," *when such a discrepancy is regarded as a force affecting price-levels,* can in fact take a form which not only necessitates a departure from the general form of the older equations (in which a stream of money, in the determination of whose magnitude the quantity of money plays an important role, is represented as being directed against a stream of objects offered for sale against money) but also may affect price-levels without any change occurring in the variables of the most widely used of the older "quantity equations." If it can be demonstrated—and such a demonstration will be attempted—that these conclusions are without foundation, then Mr. Keynes's strictures upon the older equations because of their supposedly "statical" character (in the sense that they are supposed to be true only upon the assumption of the constancy of certain relationships not explicitly stated in the "quantity equations") will likewise turn out to be unfounded, for it will have been demonstrated that the factors which Keynes includes in his $I—S$, instead of being "held constant," are already included in the variables of the older "quantity equations."

The approach just suggested may be regarded as being analogous to the first of the two methods suggested above for removing a supposed asymmetry in a given "quantity equation." The only difference is that, in the present instance, instead of finding it necessary to remove the asymmetry by the specific inclusion of certain factors not previously included, these factors are shown to be already included under certain of the variables already contained in the particular "quantity equation" whose validity is in ques-

second volume of the *Treatise*. Here Keynes contrasted the "actual price-level" with the price-level obtained by the use of a "quantity equation" of the form $Mw \cdot V = \Pi \cdot O$, in which Mw represents the total of "cash-deposits" (that is, the total of deposits after correction for "deposit" accounts, as opposed to "current" accounts). In all cases except those in which there was no "Profit Inflation or Deflation"—that is, no divergence of "saving" from "investment," according to the apparatus of the *Treatise*—the actual price level, by the terms of Keynes's argument, would be expected to diverge from the "theoretical price-level given by $Mw \cdot V/O$," because of the fact that, under such circumstances, the expression $Mw \cdot V = \Pi \cdot O$, in which Π represents the "actual price level," does not represent a true equation.

tion. It will be recalled, however, that there is a simpler and more direct answer to all arguments which would accuse a given "quantity equation" of asymmetry. This is merely that such asymmetry becomes impossible so long as the terms are properly defined; and that therefore any case of asymmetry may be removed by proper definition.

The application to the present instance should be perfectly clear. The particular quantity equation held by Mr. Keynes to be asymmetrical under certain conditions—summed up under the head of a "discrepancy between saving and investment"—was of the form $M_1V_1=PO$, in which M_1 is the "total of Income-deposits" and V_1 their velocity of circulation.[81] Yet if such an equation is asymmetrical, it is only because *M_1V_1 is defined in such a way that it will not, under all circumstances, represent the money-stream of payments which in fact go to purchase output (O).*[82]

All that the case under discussion proves, therefore, is that, for reasons of his own, Mr. Keynes himself chose to present a quantity equation which was not true under all

[81] Cf. the preceding note. It is of some interest to note that, in presenting, in the *General Theory* (p. 209), an equation of precisely this general form, Keynes, instead of arguing, as he had in the *Treatise*, that instances of the kind under discussion show the danger of using "the old fashioned quantity equations, however carefully guarded," merely stated that the particular quantity equation thus used involved the assumption, alleged to be characteristic of "equilibrium," that $M - M_1 = 0$, in which M represents the total of cash balances and M_1 the amount of cash balances held "to satisfy the transactions and precautionary motives" (p. 199). It will be noted, of course, that the condition $M - M_1 = 0$ is merely a special case of $M_1/M = k$, in which k is a constant. Mr. Keynes went on, moreover, in a footnote (to p. 209), to point out that if the V of the equation were defined in such a way as to represent the ratio between the total of cash balances (M) and aggregate money-income (Y), the equation $MV = OP$ would become a "truism which holds in all circumstances," though he felt it would then be "without significance." These matters will be discussed in greater detail in Volume II.

[82] What would actually happen, in the conditions laid down by Keynes, will be seen if we write $PT = P(O + T_x)$, in which T_x represents objects sold for money other than those already included in the "volume of output." Under these circumstances, with no change in M_1V_1 or in the "volume of monetary transactions" (PT), P would change in inverse proportion to any change in T_x. The significance of this case for a judgment concerning Keynes's statement that the Fisherine equation is not concerned with the "price-level of output" is discussed below, pp. 517 ff., and its significance as a possible source of an "excess of saving over investment" will be discussed in Volume II. All that it is necessary to point out here is that the discrepancy discovered by Mr. Keynes arose from the fact that his money-stream (M_1V_1) is directed against objects other than those included in the "volume of output."

circumstances.[83] It does not prove that all "quantity equations" suffer from such a limitation; and least of all does it prove that "the degrees of change in the quantity of money, the velocities of circulation, and the volume of output will not be related in any definite or predictable ratio to the degrees of change in the fundamental price-levels," as one was "led to suppose" by "the old-fashioned quantity equations, however carefully guarded." [84] A "carefully guarded" statement of these equations would have made it clear that these "old-fashioned quantity equations," in the hands of competent theorists, have been stated in such terms that there can be no asymmetry between the "degree of change in price-levels" and the changes in one or more of the variables included in such equations. They have been stated, that is to say, in such terms that their validity is in no sense confined to the limits suggested by their characterization as "statical," when the word "statical" is taken to have reference to the constancy of certain conditions under which alone the expressions involved would be true equations, whether the conditions thus assumed to be constant are supposed to be summarized by variables to which explicit place is given in these equations, or whether they are outside these equations altogether.

[83] In terms that would avoid the introduction of the notion of "an excess of saving over investment," we may say that his equation is a true one only upon the assumption that T_x, as defined in the preceding note, is equal to zero, or, more generally, that his equation assumes the constancy of the ratio T/O.

[84] Cf. the *Treatise*, I, 146, 147.

CHAPTER FOUR

Quantity Equations as "Statical Identities" *(Continued)*

I

"Statical" Conditions as Equilibrium Conditions

THE proposition that the familiar "quantity equations" are valid only under "statical" conditions takes on another set of meanings when by "statical" conditions we mean not "stationary"—that is, unchanging—conditions, but conditions of *equilibrium*. Here again, however, we encounter an initial difficulty in the fact that it is by no means clear just what is involved in the notion of "equilibrium conditions." The only possible procedure, therefore, is once more to construct a set of propositions which may be taken as representing possible translations of the proposition that the older quantity equations are valid only with reference to, or under, equilibrium conditions.

The notion of "monetary equilibrium" which is at once the most naïve and the oldest, is that of a situation in which the forces coöperating to determine prices are in balance, so that there is no tendency for prices to move upward or downward.[1] Under "equilibrium," in this sense, *prices*

[1] In some cases, the price-stability taken as the criterion of "equilibrium" was a long-period stability, which would allow prices to "oscillate between the same extremes from cycle to cycle," but would still prevent a "change in [the] value [of money] from period to period" apart from such "oscillations." (So, for example, R. Giffen, *Essays in Finance*, 2d series [1886], 101 f.; but see also below, p. 75, n. 9.) In other cases, however, the "equilibrium" was thought of merely as the situation that would be realized as soon as a factor which would be expected to change prices had exerted its full effect upon those prices, so that there would be no further reason for prices to change. For examples of the concept of "monetary equilibrium" in this sense, which would of course be perfectly compatible with trend movements, upward or downward, in the price-level, see Newcomb, *Principles*, 346, and especially Fisher, "The Rôle of Capital in Economic Theory," *loc. cit.*, 519; *The Purchasing Power of Money*, 70 f.; also the references given below, p. 74, n. 4.

would be stationary; but other factors might change without disrupting equilibrium, as long as they change in such a way as to balance one another.[2] Our problem, then, is to determine what meaning can be given to the proposition that the familiar "quantity equations" are valid only under conditions of equilibrium as so defined.

One thing should be obvious at the outset. To say that the "quantity equations" are valid only *under* conditions of equilibrium, in this sense, would be to say that they are valid only under conditions of stable prices; that, in other words, when prices are changing it would be no longer true, say, that $MV + M'V' = PT$. Clearly, however, there is no more reason for arguing that the mathematical equivalence of the two members of the equation depends upon the constancy of P than there was for arguing that it depends upon the constancy of the variables other than P. In both cases, the final answer must be that, under a correct definition of the variables of the quantity equations, the equations become automatically and necessarily true.

The only type of proposition whose validity can be regarded as being in any way *related to* the assumption of equilibrium conditions, in the sense defined, would be, not the type represented by the "quantity equations," but that represented by certain forms of the *quantity theory*.[3] It was, for example, argued by Fisher, in his *Purchasing Power of Money*, that the "quantity theory," in the sense of a proposition alleging the necessity of a movement in prices proportionate to movements in the quantity of money, was in fact true with reference to any two or more periods of

[2] We are here dealing, in other words, with one of the situations characterized by Marshall (cf. above, p. 43, n. 12) as evidencing "a position of rest due to the equivalence of opposing forces which tend to produce motion"—that is, in which "there may be no change . . . because the forces tending to make change are (or for purposes of a particular argument or illustration are supposed to be) equal and opposite." It was just such a type of "equilibrium," apparently, that Giffen envisaged when he wrote of "a state of things in which there is just enough money to keep prices at an equilibrium." (Giffen, *Essays*, II, 101.)

[3] For an example of a characterization of certain forms of *"the quantity theory"* as "static" in nature because of their concentration upon a comparison between two positions of equilibrium, rather than upon the events occurring in "transition periods," see Angell's *Theory of International Prices*, 85, and Edie, *Money, Bank-Credit, and Prices*, 196. Cf. also Anderson, *The Value of Money*, vii.

equilibrium—that is, of price-stability—although it could not be expected to hold during periods of disequilibrium ("transition periods")—that is, when prices are changing.[4] It was argued, in other words, that the degree of price change which would be recorded as between two periods during which the forces making for price change could be regarded as having worked themselves out, would be found to be strictly proportional to the change in the quantity of money that had taken place between these two periods.[5]

Clearly, however, all this has nothing whatever to do with the validity of "quantity equations" as such. There would, in fact, have been no need to mention the matter had not Mr. Keynes's limitation of the validity of the "quantity equations" to "equilibrium" conditions proceeded in part upon the strange assumption that somehow the validity of the quantity *equations* was limited to conditions under which it would be true that "if the quantity of money were double the price-levels would be double also." [6] If this is what is meant by the proposition that "the quantity equations" are valid only with reference to "statical"—that is, equilibrium—conditions, the case is again so weak as hardly to deserve consideration.

It is of course true that monetary theory, particularly that of recent years, has gone far beyond the naïve statement of the conditions for "monetary equilibrium" in terms

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[4] See, for example, Fisher, *Purchasing Power of Money,* 55 ff., 159 ff.

[5] It should hardly be necessary to point out that a proof of this proposition would require a demonstration of (1) the absence of a possibility of independent changes in the other variables of the Fisherine equation, despite the fact that secular change in the total volume of production and trade is not only possible, but virtually inevitable in a progressive society, and (2) the unlikelihood that the changes induced in one variable as the result of movements in others during the transition period may turn out to have lasting effects upon the subsequent equilibrium position. (On the last point, see L. Mises, *The Theory of Money and Credit,* 145; and cf. the remarks by Marshall in his article, "Distribution and Exchange," *Economic Journal,* VIII [1898], 38 f. [*Memorials,* 313].) It is, however, precisely such weaknesses of "the quantity theory" in its so-called "static" form which make it extremely important that these weaknesses should not be imputed to the *quantity equations* through the device of characterizing the latter as "statical" in nature.

[6] Cf. the *Treatise,* I, 147, where Keynes made this statement as part of his attempt to demonstrate that the type of "simple and direct quantitative relationship" which he alleged to be characteristic of the use of "the old-fashioned quantity *equations,* however carefully guarded," "is a phenomenon only of equilibrium" (italics mine).

which would amount to the single condition that the price-level must be stationary.

The issues involved have been brought into sharp focus in recent years by that part of the discussion concerning the concept of a "natural rate of interest" which has grown up around the interpretation of the word "natural" in the sense in which it was employed by the classical economists, that is, as an "equilibrium" rate.[7] There are grounds for believing, however, that a careful combing of the older literature will disclose that the older notions with respect to the conditions for "monetary equilibrium" were by no means always as simple as has sometimes been supposed. The whole of the literature of the bimetallic controversy, for example, awaits systematic exploitation at the hand of one conversant with developments in modern monetary theory, if for no other reason than because of the rôle played in that controversy by the discussion as to the compatibility of falling prices with industrial stability under conditions of falling costs—a problem which was of course implicit in the controversy as to whether "money" had risen, or "commodities" had fallen, in "value."[8] The thing to be said of the older literature, indeed, is that it contains glimpses of what came to be regarded later as advanced views on the subject, but that these glimpses were scattered, unarticulated, and often in contradiction with the position adopted by the same writer elsewhere. Giffen, for example, whose ideas with respect to "monetary equilibrium," as we have seen, were otherwise of the simplest, occasionally wrote as if the essential condition of "equilibrium" were not the stability of the price-level over longer periods, but—to use the words of Mr. Hawtrey—that "the price-level must be such that the stream of money buys the stream of goods," or, alternatively, that "the consumers' outlay exactly balances production."[9] Even Fisher, whose criterion for "equilibrium," as we have seen, was ordinarily that prices should be conceived of as being "stationary," implied that "equilibrium" might also be conceived of as involving stability *within* the price-structure, as when he suggested that a criterion of equilibrium alternative to the condition that prices be "stationary" would be that prices would be "moving alike upward or downward and at the same rate."[10]

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[7] There can be no question of attempting to summarize here the issues raised by this usage; but some notion of their nature, as well as of the complexity assumed by modern statements of the conditions for "monetary equilibrium," may be obtained from a study of G. Myrdal, "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse," in *Beiträge zur Geldtheorie,* edited by F. A. Hayek (1933). A brief, though admittedly fragmentary, summary in English of Myrdal's argument is to be found in B. Thomas, *Monetary Policy and Crises* (1936), 86 ff.

[8] Cf., in this connection, the comments by G. Haberler, *Der Sinn der Indexzahlen* (1927), 104 ff.

[9] The quotations from Hawtrey are to be found in his "Mr. Robertson on Banking Policy," *Economic Journal,* XXXVI (1926), 422, and *The Art of Central Banking,* 107. Cf. Giffen, *Essays,* II, 91.

[10] Fisher, *The Purchasing Power of Money,* 159.

In any case, it is a fact that Mr. Keynes's *Treatise,* whatever judgment may finally be passed upon its attempt at positive analysis with respect to the conditions of monetary equilibrium, is, from any point of view, an important landmark in the history of attempts to establish these conditions on a level that would do justice to the complexities of the problem. Fairness, therefore, would require that a proposition to the effect that "the quantity equations" are valid only under "conditions of equilibrium" should be tested in the light of the specific conditions which Mr. Keynes himself, at the time of writing the *Treatise,* regarded as characteristic of "equilibrium." [11]

Unfortunately, however, it can easily be demonstrated that Mr. Keynes's charge that "the quantity equations" are valid only under the special conditions which he regarded as characteristic of equilibrium reduces, so far as most of these conditions are concerned, to a further example of his failure to keep "the quantity equations" separate from "the quantity theory," with all the assumptions which "the quantity theory," in its cruder form, requires with respect to the nature of the movements of the variables of the "quantity equations."

We were told, for example, that one of the conditions of "equilibrium" was that "the factors of production" should be "fully employed." [12] Obviously, this has to do with assumptions concerning the extent of possible variations in the total of output.[13] Just as obviously, however, there is

[11] It is obvious that, for the purpose in hand, the question of the adequacy of Mr. Keynes's list of conditions for equilibrium does not enter into the discussion; nor does the question of the mutual consistency of the conditions for "equilibrium" which Keynes himself advanced at various points in the *Treatise.* On the latter point, see, for example, C. O. Hardy, in the *American Economic Review,* XXI (1931), 153 f.; also, and more generally, Hawtrey, *The Art of Central Banking,* 337, 350 f., 355 f., 357 f. Cf. also the discussion below, pp. 107 f., and the references there given to Meade and Durbin.

[12] See, for example, the *Treatise,* I, 146; II, 4.

[13] It should be noted, in passing, that to assume that the "factors of production" are fully employed is not necessarily the same thing as to assume that the volume of output remains unchanged—as it would have to be if we are to obtain the "unique relationship between the quantity of money and . . . price levels, of such a character that if the quantity of money were double the price-levels would be double also," which Keynes held to be "a phenomenon of equilibrium," as he had defined the conditions for the latter (*Treatise,* I, 147). As between any two periods, both of which

nothing in the "quantity equations" themselves which demands that *T*—of which the "quantity of output" is part—be held constant.

Precisely the same thing may be said with respect to the consequences of a change in the "bullishness" or "bearishness" of the public with respect to "securities" and the "proportion of its total wealth" which the public "is maintaining in the form of savings-deposits." [14] As will be shown in Volume II, all these changes can be translated with perfect ease into changes in the variables of the older quantity equations. It must be repeated that these variables are, in every significant sense of the term, true variables. To argue, therefore, that the validity of "the quantity equations" is dependent upon the assumption of the existence of "equilibrium," in the sense of an assumption that no change is taking place in these factors, is merely to repeat the error of supposing that the validity of "the quantity equations," like the validity of certain of the cruder variants of "the quantity theory," is dependent upon the assumption of constancy in the variables included in these equations.

Problems of a different order, to be sure, are raised by the inclusion, among the "conditions of equilibrium" under which alone "the quantity equations" were supposed to be valid, of the condition that the "volume of saving" must be "equal both to the cost and [to] the value of new investments"—or what would be the same thing, according to the definitions of the *Treatise,* the condition that "the price-

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are characterized by "full employment," output might increase as the result, for example, of (1) an increase in the number of employable workers, or (2) an increase in output per worker. When, therefore, in Volume II of the *Treatise* (p. 4), Keynes added a *constancy* in the "volume of output" and of "employment" to his conditions of "equilibrium," he was in effect—though the context gives no clear indication that he was aware of it—adding two further conditions of "equilibrium." The whole episode, indeed, provides a further confirmation of what was said earlier both with respect to the danger of identifying "equilibrium" conditions with "stationary" conditions and with respect to Mr. Keynes's own loose practice in this regard (cf. above, pp. 41 f.).

[14] Cf. the *Treatise,* I, 147. The same thing, also, is to be said of the assumptions mentioned on pp. 4 f. of Volume II of the *Treatise* with respect to the constancy of the relationship between the amount of cash-balances "available for the Industrial Circulation," on the one hand, and the total of cash-balances, on the other, as well as the constancy of the "velocity of circulation of the deposits," as affected by "habits and methods in the receipt and disposal of incomes."

level is in equilibrium with the cost of production." [15] The problems are of a different order precisely because "costs," as such, do not appear explicitly in the familiar "quantity equations" at all—or, more accurately, because "costs" are not differentiated from "selling prices" in those equations. Yet this in itself may be taken as *prima facie* evidence that the use of the equations is not limited to cases in which there is an equivalence of costs and selling prices. Indeed, in order to demonstrate that they are so limited, one would have to demonstrate that equations of, say, the general Fisherine form, involve a tacit assumption to the effect that costs and selling prices are equal. Mr. Keynes himself, of course, attempted no such demonstration.[16] With his failure to do so, the last possible support for a contention that "the quantity equations" are valid only under "statical" conditions falls to the ground, even when by "statical" conditions we mean "equilibrium" conditions, and even when the definition of the conditions for "equilibrium" which was given in the *Treatise* itself is accepted as a basis for discussion.

II

Quantity Equations and "Non-Statical" Analysis

Thus far we have dealt with the proposition that the older "quantity equations" are "statical" in character upon

[15] The condition was stated in terms of an equality between the "volume of saving" and "the cost and . . . value of new investments," on p. 147 of the first volume of the *Treatise;* in the correlative passage in the second volume, it was stated not only in terms of an equality between "savings" and "Investment" (II, 4) but also in terms of an "equilibrium" between "the price level" and "the cost of production" (II, 5; cf. also I, 152, 179). The latter condition was of course also stated in terms of a condition of "zero profits" (*Treatise,* I, 151 ff.).

[16] It is hardly possible to discover such a demonstration, for example, in the exposition on p. 149 of the first volume of the *Treatise,* where Keynes, writing $E = M_1V_1$, proceeded to rewrite his Fundamental Equation in the form $\Pi = M_1V_1/O$, with the warning that this would be a true equation only if $I - S = 0$—a warning which, as a result of the definitions given I and S in the *Treatise,* would amount to the same thing as saying that it would be a true equation only when selling prices would be equal to costs. It must be clear, however, that if we accept this exposition as "demonstrating" that an equation of the *form* $M_1V_1 = \Pi O$ would hold only if costs were equal to selling prices, we should be overlooking the fact that Mr. Keynes was able to obtain this result only by making M_1V_1 equal to E, *as he himself defines the latter term.* See, on this matter, pp. 126 ff., below.

the assumption that the translations of which this proposition is capable have to do with the *validity* of the older equations under *conditions* which are not "statical," in the sense that they are either not stationary, or are not such as to be characteristic of a position of equilibrium. We have now to deal with the *usefulness* of these equations for a type of *analysis* that is to be regarded as "non-statical" in character.

It is not necessary, even if it were possible here, to enter upon an extended investigation of what is meant, or should be meant, by "statical analysis." [17] For our purposes, it is sufficient to start from the proposition that the basis of differentiation between analysis which is "statical" and that which is "non-statical" in character is best regarded as turning upon the nature of the data for an understanding of which the analysis in question is being employed.[18] It is then easy to pass to the further proposition that statical analysis is concerned with statical conditions, whereas "non-statical" analysis is concerned with other types of condition. On the basis of our discussion of the principal meanings to be assigned to the concept of "statical conditions," it follows that non-statical analysis would be concerned with one of two types of problem. If by "statical" conditions we mean "stationary" conditions, "statical" analysis would proceed upon the assumption that there is no change in the specific data which are of significance in economic problems, whereas non-statical analysis would be concerned with the nature and causes of change in these

[17] In a future publication which will illustrate some of the issues involved by an examination of the treatment that has been accorded, in this connection, to the concept of a "natural rate of interest" (cf. above, p. 40, n. 2), I hope to be able to show that there is still room for an investigation as to the nature of, and the relation between, "statical" and "dynamical" analysis, despite the existence of such surveys as that of R. Streller, *Statik und Dynamik in der theoretischen Nationalökonomie* (1926), and the help that is to be obtained from such suggestive essays as those of R. Frisch (cf. above, p. 41, n. 4), and F. H. Knight (in the *Zeitschrift für Nationalökonomie*, II [1930]; reprinted in the same author's *The Ethics of Competition and Other Essays* [1935], 161 ff.).

[18] That this proposition has not been generally accepted is evidenced by the survey by Streller, cited in the preceding note—although it should be added that Streller's interpretations of the position adopted by the writers he cites are by no means always to be accepted without question. It is, however, impossible to deal here in detail with the issues involved.

data. If, on the other hand, by "statical" conditions we mean—as we should—"equilibrium" conditions, statical analysis would seek to determine the conditions necessary for the establishment of equilibrium, whereas non-statical analysis would study the factors that may be regarded (1) as causing a rupture of equilibrium, (2) as determining the path of successive equilibrium positions, or (3) as determining the nature of the process of transition from one equilibrium position toward another.

It will be observed that all of these cases reduce, in one way or another, to conditions of "change." The only objection to characterizing the subject-matter of "non-statical" analysis as being concerned with conditions of "change" is the lack of precision that must attach to such a statement unless the types of change involved are distinguished with a reasonable degree of clarity. For our present purpose, in any case, it is sufficient to point out that Keynes seems to have had in mind all three of the types of "change" indicated above as examples of the sort of situation for the analysis of which the received apparatus was inadequate, and for which some such apparatus as that provided by the equations of the *Treatise* had to be substituted. Thus, though he was particularly interested in determining "the method of transition from one position of equilibrium to another," he was also interested, more generally, in an understanding of the principles controlling "a moving system," which was to be understood as referring not merely to "an economic system which is not in static equilibrium," as the result, say, of some force disturbing that equilibrium, but as referring also to the reasons for "change" generally, and the nature of the "causal process" through which this "change" manifests itself.[19]

When the problem is put in these terms, it becomes obvious that the *validity* of the older equations for purposes of non-statical *analysis* must be regarded as having been demonstrated simultaneously with the demonstration that they are valid under all *conditions*, "statical" or "non-stati-

[19] For examples of a concern with "the method of transition from one position of equilibrium to another," see the *Treatise*, I, 133, 153, and II, 5. On the desirability of working toward an understanding of "a moving system," see *ibid.*, II, 406. This "moving system" is described (*ibid.*, II, 407) as "an economic system which is not in static equilibrium." On the need for studying the reasons for "change" and the nature of the "causal process" through which this "change" manifests itself, see *ibid.*, I, 133, and also p. 153, where it is stated that "a principal object of this Treatise is to show . . . the way in which the fluctuations of the price-level actually come to pass." Cf. also the opening statement of Book IV of the *Treatise*, entitled *The Dynamics of the Price-Level:* "We must now devote ourselves to the analysis of the *factors which tend to bring about changes* in the value of money and to their mode of operation" (I, 243; italics mine).

cal." The only issue that can be profitably discussed, so far as the relationship of the older quantity equations to "statical" and "non-statical" analysis is concerned, is, therefore, their *usefulness* for such analysis. Concretely, the issue is whether or not the older quantity equations are useful for the purpose of studying: (i) the causes and the process of price-change, and therefore the causes of change in the variables affecting prices and the processes by which these changes work themselves out; and (ii) the causes of a rupture of equilibrium, the forces determining the path of successive equilibrium positions, and the nature of the process of transition from one equilibrium position toward another.

III

Quantity Equations and the Element of Change

It may be said at once that the answer which will be given to the question thus posed provides as admirable a test as could be desired of one's understanding of the nature and purpose of the older "quantity equations," and of the body of received monetary theory which those equations are intended to summarize. For an even remotely adequate answer, therefore, the reader must be referred to the chapters that follow. At this point, it is sufficient to suggest the nature of the central issue involved by considering the implications of the following proposition:

The "quantity equations" themselves are nothing more nor less than shorthand expressions designed to indicate the nature of the variables whose operation can be shown to influence prices. Each of the variables in these equations is merely a chapter-heading—a rubric for detailed analysis designed to explain why the variable in question will be of a different magnitude under different circumstances, and to indicate the circumstances under which, and the sequence in which, changes in the magnitude of one variable may be expected to be associated with changes in other variables.

It follows, as a direct corollary of this proposition, that the "usefulness" of the older quantity equations for a study of the causes of changes in prices, and the processes by which

these changes work themselves out, cannot be tested by a blind manipulation of the set of symbols contained in these equations.[20] The symbols are, as has been pointed out explicitly by sponsors of their use, merely the "bare bones" of a body of analysis which is related to the symbols as flesh and clothing are related to the skeleton which gives them form and articulation.[21] It is this body of analysis whose "usefulness" for a study of the causes and the processes of price change must be examined. Once the usefulness of this body of analysis for the purpose indicated is demonstrated—and such a demonstration will be attempted in the following chapters—the usefulness of the algebraic skeleton underlying it follows immediately from the fact that, in every instance, the particular price-making force under discussion can be shown to be reflected in one or more terms of the algebraic skeleton itself.

It is easy to prove that this was in fact the understanding of the nature and purpose of the familiar "quantity equations" which was held by their principal sponsors. Thus Fisher, for example, though he regarded the price-level as being determined by "five, and only five, groups of causes," was careful to insist that these five causes were "themselves effects of antecedent causes lying entirely outside the equation of exchange," and that when we pursue into remoter stages the inquiry as to what determines the antecedent causes, "the number of causes would be found to increase at each stage in much the same way as the number of one's ancestors increases with each generation in the past," so that the only claim made for the formulation of the problem by means of an equation of exchange was that this equation set forth

[20] For an example of a criticism, based on this misunderstanding, of a Fisherine equation as a formulation that is not "enlightening"—although, in characteristic imitation of Mr. Keynes in his less happy moments, this equation is identified with "the Quantity Theory"—see R. F. Kahn, "Dr. Neisser on Secondary Employment," *Review of Economic Statistics*, XVIII (1936), 146, where it is alleged that the equation $MV = PT$ "simply tells us that *if* nothing is calculated to increase PT, an increase in M will fail to increase MV." On this basis, one could charge any generalized algebraic formulation with being "unenlightening"—including that of Mr. Keynes in the *General Theory*, to the argument of which Mr. Kahn acknowledges allegiance (Kahn, *loc. cit.*, 144, n. 2). In this connection, it is, indeed, not unfair to call attention to the remarks in the *General Theory* itself (p. 297) on the difference between regarding such algebraic formulations as providing "a machine, or method of blind manipulation," and as providing "an organized and orderly method of thinking out particular problems." Cf. also the quotation from Keynes on the necessity for "vitalizing" the "Fundamental Equations" of the *Treatise*, on p. 83, below.

[21] Cf. the references to Fisher and Pigou in notes 22 and 23, below.

clearly "the five proximate causes through which all others whatsoever must operate." [22] Similarly, Pigou, instead of contenting himself with the mere statement of his own "quantity equation," proceeded immediately to "clothe the dry bones of this formula by a brief separate study of each of the variables which it includes." [23] It is worth noting, moreover, that Mr. Keynes himself, after having presented the "Fundamental Equations" of the *Treatise,* hastened to say that these equations were like the older equations in that "their only point is to analyse and arrange our material in what will turn out to be a useful way of tracing cause and effect, *when we have vitalized them by the introduction of extraneous facts from the actual world.*" [24] It could hardly be otherwise; for we are dealing here with nothing more nor less than a typical instance of the way in which human knowledge has been organized and advanced. As it has been put by an historian of that branch of the natural sciences most heavily drawn upon for analogies in economics: [25]

"The task of the early inquirers, who lay the foundation of any department of investigation, is entirely different from that of those who follow. It is the business of the former to seek out and to establish the facts of most cardinal importance only; and, as history teaches, more intelligence is required for this than is generally supposed. When the most important facts are once furnished, we are then placed in a position to work them out deductively and logically by the methods of mathematical physics [read: economic theory]; we can then organize the department of inquiry in question, and show that in the acceptance of some *one* fact a whole series of others is included which were not to be immediately discerned in the first. The one task is as important as the other. We should not, however, confound the one with the other." [26]

The historian's further comment as to the effect of this subsequent elaboration in bringing, "by the insight which it furnishes, *disillusionment* as well as elucidation," is likewise strictly in point:

"It brings with it disillusionment to the extent that we recognize in it facts which were long before known and even instinctively perceived,

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[22] Fisher, *The Purchasing Power of Money,* 149 f.

[23] Pigou, *Essays in Applied Economics,* 180. Cf. also pp. 187 f. of the same work, where the analogy of the skeleton is further developed.

[24] *Treatise,* I, 138. Italics mine.

[25] E. Mach, *The Science of Mechanics,* p. 76 of the third (1907) English edition.

[26] Illustrations of this procedure are provided in the following chapters. See, for example, what is said concerning the differentiation of the various types of cash-balance and of corresponding concepts of "velocity" on pp. 404 ff., below, and also what is said concerning the breakdown of the T of the Fisherine equation on pp. 538 ff., below. Each of these cases, clearly, represents an instance in which the acceptance of "some *one* fact"—in this case, the facts that M, V, and T, respectively, are important factors in the determination of prices—involves the inclusion of other facts which are of the greatest importance for the determination of prices, and whose effect is in every case *registered* in changes in the variables of the older equations.

our present recognition being simply more distinct and more definite; and elucidation, in that it enables us to see everywhere throughout the most complicated relations the same simple facts." [27]

It must be obvious, therefore, that the mere fact that some of those who have used the quantity equations have not concerned themselves with, say, the forces determining the "laws of growth" and the "rate of change" of the variables included in these equations, would prove, not that the equations themselves are to blame, but, at most, that these writers have had a "*static conception* of the equation of exchange," in the sense that they have not been interested in that branch of "non-statical" analysis which is concerned with the determination of the forces controlling the rates of growth and change.[28] Whether one chooses to emphasize the importance of studying the nature of the forces that bring about "change" in the variables of the older equations by rewriting these variables in terms of their respective "rates of change," as has been done on occasion, is a matter of personal choice, which does not change the purpose of and the degree of usefulness attaching to the equations as they have been used by their ablest sponsors.[29] When such a rewriting has been complete, we are still left with the task of explaining what determines why these "rates of change" are what they are; and for this purpose recourse must be had, as before, to the body of analysis—*including* the analysis that has for years been called "monetary dynamics"—of which the equations themselves are a mere shorthand summary.[30]

[27] The whole sentence might serve as a text for that part of Volume II of this study in which an attempt is made to translate the argument with respect to a discrepancy between "savings" and "investment," *when regarded as a force affecting general prices,* into the terms of the older quantity equations.

[28] Cf., in this connection, E. Petersen, *Den moderne kvantitetsteoris gildighet,* 35.

[29] For an example of a rewriting of the equations in these terms, see Petersen, *op. cit.,* 51 ff.

[30] The *term* "monetary dynamics" is at least a half-century old. See, for example, A. de Viti de Marco, *Moneta e prezzi* (1885), pp. 9, 10 n. That the *type of analysis* to which the term is properly applied is very much older, however, is illustrated by the fact that historians of monetary theory have not hesitated to apply the term "monetary dynamics" to this older analysis. Cf., e.g., the usage of F. Hoffmann, *Kritische Dogmengeschichte der Geldwerttheorien,* with respect to the arguments of Vanderlint (pp. 41, 98), Cantillon (pp. 59, 98), and Hume (p. 98), as well as with

At best, therefore, Mr. Keynes's argument with respect to the shortcomings of the older equations for the purpose of analyzing the forces making for change in the variables whose magnitude helps to make prices what they are would reduce, not to a demonstration that these equations are not useful for such a purpose, but that the equations of the *Treatise* were *more* useful. A judgment, however, as to the validity of this claim must rest upon the material presented in the chapters which follow.[31]

IV

The Quantity Equations and Monetary Disequilibrium

In the light of these considerations, the method of demonstrating the "usefulness" of the older quantity equations for "non-statical" analysis, when the latter is understood to deal with the causes and the processes of price-change, should be quite clear. What is not so clear is the nature of the demonstration of their usefulness for "non-statical" analysis when the latter is understood to deal with situations other than those which might be regarded as representing a state of "equilibrium." It can easily be shown however, that to call the older equations "statical," in this sense, is to misconceive entirely the nature of the limitations to which the equations are subject within the range of problems to which the concepts of "equilibrium" and "disequilibrium" are relevant.

For it must be at once obvious that it would be much more reasonable to say of the older quantity equations that,

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respect to the arguments of writers of our own day (pp. 205 ff., 248 ff., 259 ff.).

[31] It is nevertheless worth pointing out here that Mr. Keynes himself has recently declared that, despite his claims in the *Treatise* for his apparatus as one designed to deal with the forces making for "change" in the variables (for example, the volume of "output") which determine the level of prices, and also with the "causal process" through which this "change" manifests itself, he now feels that the "fundamental equations" of the *Treatise* really represented "an instantaneous picture taken on the assumption of a given output"—as opposed to an apparatus of the kind presented in the *General Theory*, designed to aid in the "study of the forces which determine changes in the scale of output"—and that the equations of the *Treatise*, with the analysis they were designed to summarize, had left "the dynamic development, as distinct from the instantaneous picture . . . incomplete and extremely confused." See the *General Theory*, vii.

if by "equilibrium" we mean something more than a mere condition of determinateness, the older quantity equations are not useful for the problems suggested by the concept of "monetary equilibrium" precisely because they do not perform the function most generally assigned to "statical analysis"—namely, the function of describing with some precision the conditions of equilibrium. That this is so will become immediately obvious if we consider one of the simplest of the more sophisticated statements of the conditions for something properly to be described as "monetary equilibrium": namely, the statement that a condition for such "equilibrium" is that the general level of money costs should be equal to the general level of money selling-prices.[32] It has already been pointed out that the older "quantity equations" say nothing whatever about the relation between costs and selling prices.[33] All that need be added here is that this fact, so far from justifying the characterization of these older equations as "statical," in the sense of being designed only to establish the conditions for equilibrium, should have argued for characterizing them as being *per se* neither "statical" nor "dynamical."

That this circumstance, rather than the alleged circumstance that the older quantity equations are "statical" in character, constitutes a serious limitation upon the usefulness of the older quantity equations, will be granted immediately by all those who would insist that the concept of equilibrium—which is of course a prerequisite for a concept of disequilibrium—is indispensable for economic analysis.

There can be no question of entering here into an examination of the opposing position, which would deny altogether the usefulness of "static" or "equilibrium" analysis. This point of view has already developed an extensive literature of its own, ranging from the position of writers such as W. C. Mitchell and S. Kuznets to the position of those whose conten-

[32] It must be emphasized that the single criterion indicated is merely one of the simplest of the statements of the conditions for "monetary equilibrium," and is here taken merely for purposes of illustration. Even in itself, indeed, the condition indicated must be very carefully stated if it is to take care of certain obvious complications—such as those, for example, which are introduced by the presence of monopoly. For an indication of the nature of the other conditions which have been regarded as necessary for "equilibrium," see the references to Myrdal above, p. 75, n. 7, and to Hawtrey, above, p. 75, n. 9.

[33] Cf. above, p. 78.

tion may be summarized by the proposition that "the changing, progressing, fluctuating economy has to be studied on its own, and cannot usefully be referred to the norm of a static state." [34] The same position has been illustrated more narrowly within the field of the special problem of the "natural rate of interest" by such propositions as the allegation that every rate of interest is, in a significant sense, an "equilibrium" rate, or, more generally, that the very notion of "monetary equilibrium" should be abandoned.[35] Here it need be pointed out only that if the proposition quoted above with respect to the inadvisability of using in our analysis the "norm of a static state" is interpreted to mean that a formulation which makes no specific reference to the conditions for equilibrium is a better one than one which does, we should be confronted with the strange paradox that what Mr. Keynes, in the *Treatise*, found to be a shortcoming in the older quantity equations has now, in the minds of some interpreters of the argument of the *General Theory*, become a virtue!

What is really important, however, is that we should understand clearly just what is implied by the admission that the fact that the older quantity equations do not themselves undertake to establish the conditions for "monetary equilibrium" constitutes a serious limitation upon their usefulness. What it does not imply is that, in dealing with the problem of monetary "equilibrium" and "disequilibrium," we can *dispense* with the older quantity equations. All that is implied is a *shifting of the stage in our analysis at which it becomes profitable to employ these equations.*

Again the example of an "equilibrium" between "costs" and "selling prices" will serve to illustrate the point. It is agreed that nothing is said with respect to this type of "equilibrium" in the "quantity equations" themselves. As soon, however, as we reach the stage in our argument at which it is alleged that disequilibrium between costs and selling prices *becomes a factor affecting general prices,* the "quantity equations" come at once into their own. For, so

[34] See, for example, J. R. Hicks, "Mr. Keynes' Theory of Employment," *loc. cit.*, 239. I am assuming here that by "static state" Hicks means a "state of equilibrium." If, by "static state," we mean "stationary conditions," of course other issues are involved.

[35] An adequate discussion of these issues must be reserved for the publication indicated above, p. 40, n. 2. For a brief indication, in English, of the nature of the positions involved, the reader may be referred to the short summary of the argument of E. Lindahl given in B. Thomas's *Monetary Policy and Crises*, 85, and the comments of B. Ohlin in his Introduction to the English translation of Wicksell's *Geldzins und Güterpreise* ("Interest and Prices"), xv.

long as it is granted that these equations are valid under all conditions, the conclusion is inevitable that a discrepancy between costs and selling prices can be a factor affecting general prices *only insofar as such a discrepancy can be shown to affect certain variables in these quantity equations.*[36] If a connection between the relation of costs to selling prices, on the one hand, and one or more of these variables, on the other, cannot be demonstrated, there must be a flaw in the argument itself; for to reason otherwise would amount to a denial that the quantity equations are valid under all conditions. To have provided a means of protection against the admission of such flaws is an achievement of no small proportions; and it is this achievement that must be fairly credited to the "quantity equations" which were subjected, in the *Treatise,* to so sharp an attack.

V

The Quantity Equations as "Identities"

All this, of course, is merely another way of saying that a demonstration of the limited usefulness of the older "quantity equations," instead of leading to an argument for their suppression, can lead only to an argument for supplementing them by additional analytical devices. What these devices are will be indicated sufficiently in subsequent chapters. A necessary preliminary to an examination of these additional devices, however, is an investigation of the further ground upon which, in the *Treatise,* Mr. Keynes argued that the usefulness of the older "quantity equations" was severely limited. This ground was the allegation that the older equations were not only "statical" in character, but that they were "identities." [37]

The charge that the familiar "quantity equations" add virtually nothing to our understanding of the forces deter-

[36] In the first instance, for example, a positive difference between selling prices and costs would be expected to affect the amount of borrowing from banks (and therefore the M' of the Fisherine equation) and the rate at which available traders' cash balances are spent (and therefore the V' of the Fisherine equation). The converse would be expected to follow from a negative difference.

[37] Cf. the *Treatise,* I, 133.

mining prices because they are "mere identities" is as old as the popularization of the equations themselves.[38] It is also true, however, that the charge was answered almost as soon as it was made.[39] It is something of a commentary, therefore, upon the slowness with which agreement is obtained upon issues as simple as they are fundamental, that it should be necessary here to rehearse even briefly the arguments involved. Yet such a rehearsal is clearly required if we are to have an adequate understanding of the place of the older "quantity equations" among the tools currently available for the central problems of monetary theory.

From one standpoint, obviously, the charge may be regarded as a welcome recognition of a truth that has not always been recognized. For if by "identity" we mean "truism," and if by "truism" we mean a proposition that is necessarily true, then the statement that the older quantity equations are "identities" or "truisms," may be taken as an answer to the criticisms examined in the preceding chapter, to the effect that the familiar quantity equations are not true.[40] It is not surprising, therefore, that some of the writers chiefly responsible for the popularization of the better known "quantity equations" should have welcomed the

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[38] Thus Fisher found it necessary, in *The Purchasing Power of Money*, 157, to answer an "objector" who had attempted "to dispose of the equation of exchange as stated by Newcomb, by calling it a mere truism." I have not been able to identify the "objector" in question by any published material. The statement that "the 'equation of exchange' is obviously a mathematical identity" had, to be sure, been made at the December, 1910, meeting of the American Economic Association devoted to a discussion of Fisher's paper "Recent Changes in Price Levels and Their Causes," by R. H. Hess (*Bulletin of the American Economic Association*, 4th series, Vol. I, No. 2, p. 66); but the proposition had not been used to demonstrate the supposed uselessness of the equation. The number of critics who used the type of argument in question *after* the publication of *The Purchasing Power of Money* is, of course, legion. It is sufficient to call attention to the particularly violent attack along these lines by Anderson in *The Value of Money*, 157, 161, 169, etc.

[39] See, for example, in addition to the reference to Fisher in the preceding note, the remarks by J. Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *loc. cit.*, 676. The fact that the contention had been answered so early unfortunately did not prevent Anderson, for example, from advancing the same proposition more than a decade later ("Commodity Price Stabilization a False Goal of Central Bank Policy," *loc. cit.*, 23), with the result that a repetition of the standard rebuttal was felt necessary (cf. Burns, "The Quantity Theory and Price Stabilization," *loc. cit.*, 573 ff.).

[40] Cf. above, pp. 46 ff.

characterization of these equations as "truisms" or "identities," nor that Mr. Keynes should have done the same with respect to the "Fundamental Equations" of his own *Treatise.*[41]

The trouble begins, however, when an "identical" or "truistic" character is assigned to the "quantity equations" as part of a proof, not of their *validity,* but of their *uselessness.*[42] To this suggestion there can be, as there has been, only one answer:

It is possible to imagine a set of equations which would be "mere identities"; but it does not follow that the particular quantity equations which had come to command general recognition are "mere identities." Whether a particular equation is to be regarded as a meaningless "identity" depends entirely upon its form. Actually, the particular "quantity equations" which have been most widely used in monetary theory represent a summary of the slow growth, over a period of centuries, of our knowledge with respect to the forces determining prices.

That this is so will become immediately obvious if we take at their word those writers who allege that the familiar quantity equations are "mere identities" because they tell us simply that "money spent is necessarily equal to money received."[43] If we put this latter statement into algebraic

[41] See the references to Fisher (to which may be added pp. 16 and 372 of *The Purchasing Power of Money*) and Schumpeter, given above, p. 89, notes 38 and 39; and cf. Keynes, *Treatise,* I, 138, 221. Attention may be called here also to the statement of Wicksell, in a context directly relevant to the question of the relation between the "quantity theory" and the "quantity equations": "That the quantity of money, multiplied by the velocity of circulation . . . must always coincide with the total value of the goods and services turned over against money in a given period of time, is not a theory at all; it is an axiom." (*Lectures,* II, 144.)

[42] It is obvious that what is involved here, by agreement between both parties to the dispute, is the charge that the equations are "useless" for purposes of *theoretical analysis.* To the charge that an equation of exchange which is "true by definition" is "useless as a means of studying the relation between currency, velocity, trade, and prices" because the variables as defined are not "empirically determinable magnitudes" (so, for example, M. A. Copeland, "Money, Trade, and Prices—A Test of Causal Primacy," *loc. cit.,* 664), it is necessary only to repeat what was said above, p. 64, and in note 65, thereto.

[43] For examples of a paraphrasing of the quantity equations in the terms indicated—although of course without the use of algebra—see Anderson, *The Value of Money,* 161, and also the reference to the same writer on p. 89, n. 39, above.

form, we then have $M_s = M_r$. It need hardly be emphasized that this is not a "quantity equation" of the type whose usefulness is here in question.

This is the obvious answer to the argument of G. Haberler, of whose attempt to prove that the quantity equations—in this case, Schumpeter's "income" variant thereof—are "mere tautologies," it must at least be said that, unlike other reiterations of the charge that these equations merely tell us that "money spent is necessarily equal to money received," it represented an effort to meet the defense set up against this particular charge by the sponsors of the equations themselves.[44] Haberler, starting from Schumpeter's equation $MV = p_1q_1 + p_2q_2 + \ldots p_nq_n$, argued that the expression merely equated the sum of money prices with the sums of money spent in purchases at these prices; and that since the money-price of a commodity is *ex definitione* equal to the money-sum received from the sale of the commodity, the equation was indeed a "tautology" in Schumpeter's meaning of the latter term: namely, an expression in which "one side of the equation would be merely another expression of *the same* quantity as that which appears on the other side, so that a change in the one magnitude could not be said to bring about a change in the other, but *ipso facto* would *mean* the same thing as a change in the other—as, for example, in an expression which would equate the money-price of a commodity with the reciprocal of the purchasing power of money with respect to this commodity."[45]

It must be obvious, however, that Haberler's argument involved several surreptitious translations of the terms of Schumpeter's equation. To his translation of "price received" into "money received from the sale of a commodity," there can be no objection, since, as he argued, the two are necessarily identical *ex definitione*. There is, however, objection to the translation of the MV of Schumpeter's equation into "money spent in the purchase of commodities"; for the identity of the two is certainly not the result of a definition in the sense in which "price" is identical with "money received from the sale of a commodity."[46] There is

[44] G. Haberler, "Kritische Bemerkungen zu Schumpeters Geldlehre," *Zeitschrift für Volkwirtschaft und Sozialpolitik*, New Series, IV (1925), 647 ff., 653.

[45] Schumpeter, "Das Sozialprodukt," *loc. cit.*, 676.

[46] Haberler was, in fact, able to make the translation of MV into M_s appear to be a matter of simple identity only by resolving MV into a succession of purchases by money, each of these purchases representing the "price" of the goods purchased (see, for example, Haberler, "Kritische Bemerkungen," *loc. cit.*, 652). It must be obvious, however, that this amounts, in effect, merely to writing, not MV, in which M represents a given stock of money and V the number of times in which it is spent in a given period, but Σm, in which each m now means merely the amount of money passed in each purchase, and therefore the "price" of each commodity purchased. The result of the substitution of Σm for MV, clearly, is to leave unsolved the problem with which the very introduction of the concept of "velocity" was designed to deal: namely, the nature of the

objection, also, to Haberler's other surreptitious translation—namely, the translation of the second member of Schumpeter's equation into the "sum of money prices." Taken literally, the latter expression would be written Σp. In fact, however, Schumpeter wrote Σpq, which is quite a different matter, as will be seen from our examination of the second type of "tautology" into which the "quantity equations" have been translated by unsympathetic critics.[47] An algebraic translation of Haberler's "tautology" would, in short, be $M_s = \Sigma p$, not the $MV = \Sigma pq$ of Schumpeter: to say nothing of the fact that both the V and the Σq thus added were defined by Schumpeter in such a way as to result in a definition of MV which makes it a very special kind of "money spent"—namely, money-*income* spent—and in a definition of Σpq which makes it a very special kind of "money received"—namely, money received from the sale of *consumers' goods*. A proof that Schumpeter's equation, as thus stated, was a "mere tautology" would involve a demonstration that the equation in question has no more significance for the development of monetary theory than Haberler's supposed translation of it: namely, the proposition that the sum of money spent on commodities is equal to the sum of the prices paid for these commodities.

The situation is somewhat improved if, taking others of these critics at their word, we assume that the older quantity equations alleged merely that "the price of goods multiplied by the quantity of goods equals the amount of money which is spent on them."[48] We then have $M_s = P \cdot G$. The improvement, obviously, resides in the fact that, instead of writing $M/P = k$, in which k is not only a constant, but a purely arbitrary magnitude without economic significance, performing the sole function of making it possible to express the supposed fact of a strictly proportional relationship between P and M, we have now discovered a true *variable of economic significance*, the movements in whose magnitude may be just as important for the determination of general prices (P) as the "amount of money spent" on these goods. Simple as the proposition is, it represented, historically, a great advance over those formulations in which the "quantity of goods" was given no specific place, and which con-

forces determining how large a stream of money-payments (Σm) will be supported by a given stock of money (M). On the significance of this type of procedure for the theory of "velocity" generally, see my comments in the *Journal of Political Economy*, XL, 483 f.; and cf. also what is said on pp. 345 ff., below, with respect to certain forms of the "income approach" to the problem of money.

[47] Cf., e.g., the quotation from Hicks, cited in the following note.

[48] So, for example, J. R. Hicks, "A Suggestion for Simplifying the Theory of Money," *loc. cit.*, 1.

tinue to appear in economic discussion, even though they do so only implicitly, as in disputes concerning the probable effect upon prices of a given expansion in the quantity of money.

A proper appreciation of the extent of the achievement represented, historically, by recognition of the "quantity of goods" as a factor affecting general prices is lost if we insist upon searching for the first instance of such an achievement in statements to the effect that variations in the "quantity of goods" are important for the determination of "prices." The reason for this becomes obvious as soon as one recognizes the truth of Menger's comment that the explanation of changes in general prices which comes instinctively to the mind untrained in economics is one that places exclusive emphasis upon what has been happening to goods, rather than upon what has been happening to money.[49] In virtually every period which is seen in retrospect to have been a period of extreme inflation, from the earliest discussion of the causes of price-change down to those of our own day, there have always been economic illiterates to cry that the difficulty was due to a "shortage of goods," rather than to an expansion in the quantity of money; and the situation is not greatly helped by the fact that, since so many of these periods of inflation were also periods of war, which involved a serious disturbance of the apparatus of production, there has usually been some grain of truth in the argument of those who could see only the "goods side" and never the "money side" of the problem.

Recognition of the importance of the "goods side" takes on genuine historical significance only when the recognition comes from those who are fully conscious of the importance for price-change of variations in the quantity of money, so that their insistence upon taking account of variations in the "quantity of goods" becomes, not an explanation of price-rise alternative to that which would stress the importance of the quantity of money, but an explanation that is complementary to the latter. From this point of view, one must admire a writer like Bodin, who, for all his insistence that "the principal and almost the only" cause of the high prices of his day, and the cause "which no one has referred to until now," was the "abundance of gold and silver," was nevertheless careful to point out that a fair statement of the causes of the high prices would recognize that the "abundance of gold and silver" was only one among several "causes," which included the "scarcity [of commodities] caused partly by export and partly by waste."[50]

Much the same type of criterion must be invoked in attempting to appraise the extent of the achievement represented by the recognition

[49] Cf. Menger, "Geld" (*The Collected Works of Carl Menger* [London, 1936], IV, 81 f.).

[50] Cf. *La Réponse de Jean Bodin à M. de Malestroit,* pp. 9 ff., as edited by H. Hauser (1932). The passage appears in translation on pp. 127 ff. of A. E. Monroe's *Early Economic Thought* (1923).

of the importance of changes in the "quantity of goods" at the hands of later writers.[51] It is, for example, difficult to accord any great credit to those mercantilist writers who, while they insisted that "more money in circulation means more trade," and therefore added a type of consideration that was absent in writers like Bodin, for whom the changes in the "quantity of goods" were independent of changes in the quantity of money, did so with considerably less awareness than was shown by Bodin with respect to the effect upon prices of changes in the quantity of money whenever those changes were greater than the changes in the "quantity of goods."[52] The type of writer who deserves the greatest credit for a recognition of the dependent nature, under certain circumstances, of the relationship between the changes in the "quantity of money" and the "quantity of goods" is Hume, of whose awareness of the importance of changes in the quantity of money for changes in general prices there can certainly be no question. Hume insisted, in fact, not only that it is "a maxim almost self-evident that the prices of everything depend on the proportion between commodities and money, and that any considerable alteration of either of these has the same effect, either of heightening or lowering prices," so that, for example, "provided the money does not increase in the nation, every thing must become much cheaper, in times of industry and refinement, than in rude, uncultivated ages," but also that, in the interim between the addition of the new money and the rise of prices to their final level, the new money may have the effect of "exciting industry," and so increasing what other writers had called the "quantity of commodities."[53]

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[51] Of the many of these "later writers" who might be cited as having recognized the importance of changes in the "quantity of goods" for the determination of general prices, Montesquieu, perhaps, deserves special mention because of the clarity with which his statement of the point shows how the "equation of exchange" was built up historically. Cf., for example, *De l'esprit des lois* (1748), Book XXII, Chap. VIII (Vol. II, p. 50, of the Paris [Garnier Frères] edition of 1922): "If, since the discovery of the Indies, gold and silver have increased in Europe in the proportion of one to twenty, the price of commodities ought to have increased in the proportion of one to twenty. But if, on the other hand, the number of commodities has increased in the proportion of one to two, the price of these commodities must have risen . . . in consequence, only in the proportion of one to ten."

[52] For examples of mercantilist writers of the type indicated, see J. Viner, "English Theories of Foreign Trade Before Adam Smith," in *Journal of Political Economy*, XXXVIII (1930), 284 ff. (*Studies in the Theory of International Trade*, 36 ff.); and cf. E. F. Heckscher, *Mercantilism*, II, 217 ff., 231 ff. As Professor Viner demonstrates (*Studies*, 40 ff; cf. also Heckscher, *op. cit.*, 224 ff.), certain of these writers accorded some degree of recognition to the possibility that prices might rise as a result of the desired increase in the quantity of money, but their recognition was hardly clear or emphatic enough to warrant any significant qualification of the statement given in the text above.

[53] Hume, "Of Money," and "Of Interest," in *Essays, Moral, Political, and Literary*, Vol. I, 307, 309, and 314 of the 1777 edition. It should be obvious that the position of Hume, like that of Bodin, is taken here merely as the most influential example of a position of which there were certainly

It should hardly be necessary to stress the fact that Hume's emphasis upon increases in the "quantity of commodities" as a force counteracting the effects of an increase in the quantity of money is strictly relevant to the arguments of those unworthy contemporary champions of conservatism in monetary policy, whose anti-inflationist contentions take the form of an exclusive emphasis upon the disastrous effects of the price-rise which is assumed to be the inevitable consequence of an expansion in the quantity of money, rather than an insistence upon the necessity of considering the possibility that, under certain conditions, monetary expansion may take the form solely of a rise in prices, or an insistence that the mere fact of an increase in the "quantity of goods," while it may prevent price-rise, is not necessarily proof that this expansion in the "quantity of goods" is of such a form as to provide the assurance of permanence. What it is necessary to stress is that a simple recognition of the difference between a "truism" of the form "money spent is equal to money received," and a proposition even of the simple kind represented by the statement that money spent is equal to "the price of goods multiplied by the quantity of goods" amounts to a recognition of the fact that our knowledge with respect to the forces affecting general prices, and therefore the "quantity equation" which summarizes that knowledge, represents something of a development beyond the extent of our knowledge before Bodin and Hume made their contributions; and it is necessary to stress the further fact that such a recognition would have spared us both the unwanted friendship of monetary conservatives whose knowledge of tradition in monetary theory is very much less than their avowed enthusiasm for that tradition, and the supposedly iconoclastic utterances of Mr. Keynes with respect to the place to be accorded, in current monetary discussion, to "the Quantity Theory," in the sense of a proposition represented by the formula $M/P = k$, in which k is a constant.

It is, however, impossible to appreciate the significance of the particular equations whose usefulness is here under discussion (and it is important to insist upon the fact that these equations are of a very much more elaborate form that is suggested by the formula $M_s = PG$) unless we remember that, as a matter of historical development, the theory of the forces determining prices was not constructed as a result of the elaboration of statements obviously "truistic" in character. It developed, rather, as a process of correction of a series of propositions of such a character that, when translated into the form of algebraic expressions, they

adumbrations before his day, but which in no other instance has the significance which attaches to it in the case of Hume, in view of the importance of the latter in the development of the theory of the effect of money upon output, which will concern us in some detail in Volume II of this study.

were demonstrably asymmetrical, in the sense that, while they purported to provide an apparatus for the explanation of movements in general prices, they in fact omitted some element that was actually of the greatest importance for the determination of these "prices."

There was a time, for example—say, in the sixteenth century before Bodin wrote his reply to the *Paradoxes* of Malestroit—when, incredible as it seems to us nowadays, it was not generally recognized that a change in the quantity of the money metal may, under certain circumstances, be an extremely important factor affecting prices.[54] When Petty and Locke added "velocity" as a factor, monetary theory made a further advance.[55]

The same sort of step forward was made when writers of the eighteenth century recognized money substitutes as a relevant affecting force.[56] The latter step was made more

[54] It should be obvious that it is no more necessary, for our present purpose, to enter upon an extended discussion as to whether Bodin had predecessors as influential as he on this matter, than it was to debate the claims for Bodin as the "first" to advance "the quantity theory" (cf. above, p. 10, n. 2). No comment to this effect would have been necessary if it were not for the fact that these issues have recently been raised anew, with the results that might have been expected. Cf. the discussion by B. W. Dempsey and E. J. Hamilton in the *Quarterly Journal of Economics,* L. (1935), 174 ff.

[55] The rôle of the suggestions of Petty and Locke in the development of the concept of a "velocity of circulation of money" has by this time become a commonplace in histories of the development of monetary theory. See especially, however, M. W. Holtrop, "Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *Economic History,* No. 4 (*Supplement* to the *Economic Journal*) (1929), 503 ff. Cf. also the references to the argument of William Potter (1650) with respect to the speed at which money "revolves," given by Viner, *Studies,* 38, n. 75.

[56] The origins of a recognition of the fact that the use of money substitutes affects the value of money of course go back further than the eighteenth century. Petty, for example, who has sometimes been "forgiven," in the light of the rudimentary development of banking institutions in his day, for having failed to do justice to the significance of "credit" (so, for example, Hoffmann, *Kritische Dogmengeschichte,* 99) in fact declared explicitly, as was pointed out by Marshall (*Money, Credit, and Commerce,* 41 n.) that certain methods for the economizing of cash, including the use of money substitutes, would make "less money necessary to drive the Trade." It is not always certain, to be sure, from statements such as that quoted from Petty, that the writers concerned were clearly aware that the economy in the use of "cash" which was effected by the use of "credit" arose not only from the possibility of keeping smaller cash-balances *of all types*—in other words, of reducing the V of our quantity equation—which is offered by the existence of institutions for borrowing and lending, but also from the fact that money substitutes, properly so called, may serve as part of the cash-balance in place of "cash" in the form of specie—in other words, are represented by the M' of our quantity equation. Yet Petty

significant as the result of the clear recognition, in the earlier part of the nineteenth century, of the fact that these money substitutes should include not only bank notes, but also deposit-currency.[57] When, likewise in the earlier part of the nineteenth century, it was recognized that account must be taken not only of the "quantity of goods offered for sale," but also of a further factor called "the rapidity of circulation of goods," still further progress was registered; and so on.[58]

himself seems to have meant the latter (see the full text of the passage cited by Marshall in *The Economic Writings of Sir William Petty* [edited by C. H. Hull, 1899], 36); and doubtless many other examples could be cited from the literature of the seventeenth century. On the other hand, in interpreting writers such as Petty, considerable generosity is required to credit them with an awareness of the relation between the quantity of money substitutes and the "purchasing power" of money, in the degree attributed to Petty by Marshall (*loc. cit.*). On both counts, however, there was no ambiguity whatever in a writer such as Jacob Vanderlint. See the latter's *Money Answers All Things,* 1734 (pp. 14 f. of the Johns Hopkins reprint, edited by J. H. Hollander).

[57] As Professor Viner has shown, recognition of the fact that "the transfer of bank notes and the transfer of book credits at the bank were alternative means of making payments" dates from "the very beginning of paper money in England" (Viner, *Studies,* 244). Quite apart, however, from the fact that the absence of widespread recognition, in these early years, of the effect upon general prices of changes in the quantity of "means of making payment" makes it difficult to suggest that the "Theory of Prices" of these early days had already given explicit place to deposit-currency as an important affecting factor, it was not until the early years of the nineteenth century that the doctrine as to the effect upon prices of the particular "means of making payment" represented by deposit currency became generally familiar. See, in this connection, the admirable account by Viner, *Studies,* 243 ff. The doctrine had, in any case, become such a commonplace by 1840 that Sir John Lubbock was prepared to insert a term for "deposits" (D) and their "velocity of circulation" (l) in the first of his "equations of condition, which connect the quantities which occur in the theory of currency," without the slightest suggestion of apology or of a feeling that an explanation was called for. (For the full form of Lubbock's "equation of condition," see above, p. 11.)

[58] For examples of a use of the concept of a "velocity of circulation of goods" in the earlier part of the nineteenth century, see my article in the *Zeitschrift fur Nationalökonomie,* IV, 188 ff. In view of the fact that the equation published in 1813 by Cagnazzi (see above, p. 11) is the earliest equation of exchange thus far discovered in which specific algebraic notation was given to the velocity of circulation of *money,* it is worth noting that this equation provided specific algebraic notation also for the velocity of circulation of *goods.* It is worth noting also that although Lubbock's equation of 1840 did not include a specific algebraic term for the "velocity of circulation of goods," Lubbock himself gave clear evidence that he wished its influence to be taken account of in the $\Sigma a\alpha$ (that is, the Σpq) of that equation; for he pointed out that "an increase of the number of times which the same article changes hands" may bring it about that "the quantity consumed of any article is not always a criterion of its influence upon the quantity $\Sigma a\alpha$." (*On Currency,* 36 f.) These instances,

Viewed, therefore, in the light of the actual historical development of doctrine on the subject of the forces determining general prices, the importance of the "truistic" character of *the quantity equations whose usefulness is under discussion* (as opposed to that of a special set of truistic equations constructed only for the sake of providing easy targets for their originators) is that the gradual attainment of this "truistic" character, instead of providing ground for criticism of the familiar quantity equations, becomes a record of slow achievement, over centuries, of precisely the kind that is represented by the advance of knowledge in any branch of science. An earlier proposition, regarded in its own day as a "truism," is shown by later investigation to be true in fact only under certain specific conditions of which not even the nature was at first recognized, to say nothing of an explicit place being given to these conditions in the statement of the proposition itself. One may, if one wishes, describe the process of conversion of the earlier and cruder formulation into a more comprehensive, and hence more nearly accurate, statement, as the conversion of a statement not strictly true into a "truism" or "identity." With equal justice, however, and with a much greater appreciation of the nature of the process by which the frontiers of knowledge have been pushed back, one could describe the process as the conversion of a proposition which was at best true only under definite assumptions, and at worst was generally and literally false, into a proposition that can be shown to be capable of the widest possible application and of passing the most exacting scientific scrutiny.

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together with those provided by the early "Fisherine" equations of Levasseur, Bowen, and Walras (in all of which a special algebraic term was introduced to represent the "velocity of circulation of goods"), provide a commentary, particularly apt in the present connection, upon the position of those critics who have argued that the equation of exchange is not always true because, among the factors which it assumes to be constant, is "the relation between the volume of production and the volume of trade" (so Foster and Catchings, *Money*, 167). Cf. also Roos, *Dynamic Economics*, 235, where the Fisherine equation is characterized as "no more than a first approximation" because it "does not take into account the lag between the production of a good and its consumption"—that is, its sale. The factors other than the "velocity of circulation of goods" which are involved in the relation between the "volume of production" (or "output") and the "volume of trade" are discussed at greater length below, pp. 538 ff.

It will, of course, be recognized that the argument thus far developed has proceeded upon the assumption that the conversion into "truisms" of propositions not strictly true proceeds according to the first of the two methods indicated in the preceding chapter as a means of securing the validity of a given "quantity equation"—namely, the method of *adding* to a given formulation elements whose absence prevents that formulation from being regarded as strictly true. It is probable, however, that the type of "quantity equation" which is alleged to be largely useless because of its "identical" or "truistic" character is that which acquires such a character not through being made to include a more comprehensive list of variables, but through an application of the second method indicated above—that is, through the defining of the terms of the equations in question in such a way that the equations themselves must necessarily be true. It is, however, easy to show that the case for the usefulness of the "quantity equations," instead of being weakened by a procedure of this sort, may, if the procedure is carried on by competent workers, be greatly strengthened.

For it must be obvious that everything turns upon the economic significance of the variables involved in the equations thus newly formulated. Let us suppose, for example, that it is discovered that not all the money payments made in a given period are in fact directed against the goods the determination of whose prices we wish to explain. The proposed procedure amounts simply to the excision of those payments which are not in fact so directed. As we shall see in later chapters of this book, this is precisely the procedure which much of modern monetary theory has followed.[59] Why should the resulting formulation, though admittedly true, be necessarily regarded as a "mere identity," in the sense of a meaningless tautology? If, for example, the stream of money payments now segregated happens to coincide with what, in the terminology of Mr. R. G. Hawtrey, may be called "consumers' outlay," who would say that "money spent," as now defined, has no economic significance? All that we are doing, in producing a "truism" of this less inclusive type, is to use a microscope in place of the

[59] See, for example, below, pp. 511 ff.

naked eye; the mere fact that a bungling worker may direct his microscope toward an object which, under the microscope, has neither coherence nor unity, does not constitute an argument for the abandonment by really competent workers of the microscope as an instrument.

If anything is to be said against microscopic studies, it is not that the microscope will give us useless results, but that, *whenever the objects studied microscopically can be shown to be related in definite ways to magnitudes too large to be included under a microscope of a given degree of power,* the results obtained by microscopic study must be *supplemented* by further studies which will have the effect of integrating into a larger whole all the areas studied separately.[60] In both cases, we shall be operating with "identities," or "truisms," in the sense that our equations must be made to be true, either by formal definition or by the addition of specific magnitudes not previously included. In neither case, however, can the degree of usefulness that may be held to attach to these equations be established by a flippant dismissal of all "quantity equations" on the ground that they are "mere truisms" or "identities," or by the construction of absurd parodies of received "equations of exchange" and of what these equations are supposed to tell us. It must be established by a careful examination of the specific equations that have come to hold a prominent place in monetary theory, and of the body of doctrine which those equations are designed to summarize. This is precisely the task that will be attempted in later chapters of this study.

[60] For examples, see below, pp. 518 ff., and 532 ff.

CHAPTER FIVE

The "Dynamical" Equations of Keynes's *Treatise on Money*

I

KEYNES'S *Treatise* AND MONETARY "DYNAMICS"

THE present chapter represents a departure from the central purpose of this study, which aims primarily to state and defend the substance of received doctrine on the subject of the "Theory of Prices" against its principal detractors. It is not essential, for the accomplishment of that purpose, to launch a counter-attack against the apparatus which these detractors have proposed to substitute for received doctrine; and the reader who is not interested in these proposed substitutes may omit this chapter without loss.

It can hardly be denied, on the other hand, that much interest attaches to the ultimate fate of that particular apparatus which was proposed in the *Treatise on Money* by Mr. Keynes, in view of the fact that he himself was one of the most extreme critics of the older apparatus. There can be little doubt, moreover, that a particular interest attaches to the fate which has overtaken the "Fundamental Equations" of the *Treatise,* in the light of the fact that, in the *Treatise,* Mr. Keynes had not only argued that the familiar "quantity equations" were statical in character, but had claimed, for his new equations, as indeed for the whole of the argument of the *Treatise,* that, "in contrast to most older work in monetary theory," his newer type of analysis was "intended to be a contribution" to the "dynamics" of the subject.[1]

This claim for the apparatus presented in the *Treatise* was accepted at its face value by more than one commentator thereon.[2] It was not long, however, before this claim

[1] See, for example, the *Treatise,* II, 406 f.; and cf. also the Preface, p. v.

[2] This is true not only of those writers whose enthusiasm for the Funda-

was challenged. That it should have been challenged by writers such as Hayek, who made no attempt to conceal their lack of sympathy with the general argument of the *Treatise,* is, of course, hardly surprising.[3] What is striking is that it should have been challenged also by writers so much in general sympathy with the tenor of the *Treatise* that they either stated explicitly that their own work was "largely based on the work of Mr. Keynes," or acknowledged such a relationship implicitly by using much of the terminology and even certain of the definitions of Keynes, or went out of their way to insist that even their most serious criticisms of the argument of the *Treatise* did not "diminish the usefulness of Keynes's theory." [4] Such a development surely justifies at least a brief examination of the issues raised by these critics, for the sake of the issues themselves quite as much as for the sake of the irony provided by the fact that the equations whose suitability for "dynamical" analysis was thus challenged had been proposed as substitutes for a set of equations whose supposedly "statical" character we were at such pains in the two preceding chapters to deny.

II

Nemesis

There is irony, in the first place, in the circumstance that a "dynamical" character should have been denied by one critic to the equations of the *Treatise* for a reason almost identical with that for which Keynes himself had denied

mental Equations of the *Treatise* as representing "an enormous progress" because of their "dynamical" character was expressed before Keynes's own claim in this respect began to be questioned (so, for example, B. Josephy, "Keynes' Geldlehre," *loc. cit.,* 59), but also of writers who were aware that these claims had been challenged. See, *e.g.,* B. P. Adarkar, *The Theory of Monetary Policy* (1935), 57.

[3] For the arguments of Hayek, in this connection, see below, pp. 132 ff.

[4] Cf., for example, J. E. Meade, *The Rate of Interest in a Progressive State* (1933), 48 ff.; E. F. Durbin, *The Problem of Credit Policy* (1935), 248 f.; and E. M. Bernstein, *Money and the Economic System* (1935), 267. The quotations in the text are from Meade (p. 48) and Bernstein (p. 269), respectively. For examples of the use, by Durbin, of Keynes's terminology and certain of his definitions, see pp. 40 f. and 89 ff. of the work cited.

such a character to the older quantity equations. This reason was simply that the critic in question confused the assumptions implicit in certain arguments which the equations might be used to illustrate, with assumptions necessarily involved in the equations themselves. An analogy immediately suggests itself: namely, Mr. Keynes's implicit attribution, to the older quantity equations, of certain assumptions respecting the stationary character of certain of the variables in these equations, whereas in fact these assumptions, while they may be necessary for certain forms of the *quantity theory,* are not necessary for the "quantity equations" as such. Insofar as the type of argument in question is analogous to those of Keynes's arguments which were based upon a confusion of the "quantity equations" with the "quantity theory," it goes without saying, obviously, that it is to be rejected, when used against the equations of the *Treatise,* as emphatically as it was to be rejected when Mr. Keynes used it, implicitly if not explicitly, against the older "quantity equations." The cases in question are deserving of mention here only as examples of the nemesis which may overtake critics who are themselves, upon occasion, something less than generous.

Typical of the kind of criticism in question was that directed against the equations of the *Treatise* by J. E. Meade, who, if he did not explicitly challenge the formal validity of the equations as such, certainly implied such a challenge both by calling attention to cases in which the requirement that the equations must be "satisfied" would lead to results which Meade himself regarded as unsound, and by experimenting with certain "corrections" of the equations.[5] Concretely, Meade argued that the equations of the *Treatise,* even when regarded merely as statements of the *conditions of equilibrium,* rather than as devices for illustrating the nature of the forces causing change and the nature of the process of change (including the disruption of equilibrium and the process by which equilibrium may be re-established after it has been disrupted) were not valid under conditions which he described as those of "dynamic

[5] Cf. the page references to Meade in the preceding note.

equilibrium."[6] Characteristic of such a "dynamic equilibrium," specifically, would be a condition of increasing "investment." It was Meade's contention that when such increased investment is taking place, the equations of the *Treatise* would no longer hold good.

Upon its face, it is difficult to see why this charge should hold true, so far as the equations themselves are concerned; and indeed it appears, upon examination, that Meade's quarrel was not with the equations, but with the details of Mr. Keynes's argument elsewhere as to the forces leading to an expansion of investment and therefore of output.[7] Mr. Keynes had, indeed, argued that a necessary condition for the expansion of "output" is the existence of "windfall profits," whereas, by definition, an equality of "savings" and "investment" excluded the possibility of such profits. To assume a condition, therefore, in which I and S are increasing equally—the condition of "dynamic equilibrium"—is to run counter to the condition that an expansion in "investment" and "output" requires a divergence between "prices" and "costs." In terms, that is to say, of the second Fundamental Equation, a condition for expanding investment, it was alleged, is that $\Pi - W_1 >$ zero. But—so Meade's argument may be stated—whenever "savings" are equal to "investment," the condition $\Pi - W_1 >$ zero is in conflict with the Fundamental Equation itself; for since, according to this equation, $\Pi - W_1 = (I-S)/O$, and since, under "dynamic equilibrium," $(I-S)/O$ is equal to zero, we cannot assume that $\Pi - W_1$ is greater than zero and at the same time assume that $(I-S)/O$ is equal to zero. The only type of equilibrium under which the equation would hold true, therefore, according to Meade, would be a "long-period static equilibrium," which would be defined by the condition that

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[6] Again it is unnecessary to enter upon an extended discussion as to the wisdom of characterizing the case in question as one of "dynamic equilibrium"—it is characterized as a case of "moving equilibrium" by S. H. Slichter (*Towards Stability*, 48 n.)—in view of the fact that other usages have been proposed with respect to the concept of "dynamic equilibrium." Cf. what is said on this matter on p. 43, n. 12, above.

[7] It may be remarked, in passing, that Meade's assumption that an increase in "investment" necessarily means, under all conditions, an increase in *total* output is anything but a self-evident proposition. For purposes of the argument in hand, however, this difficulty may be waived.

I and S would not be increasing; for only such a situation would not be in conflict with the condition that $\Pi - W_1$ would be equal to zero.

The same argument was advanced in somewhat different terms by E. F. M. Durbin, who pointed out that Keynes's definition of "Normal Income" as that which would leave entrepreneurs "under no motive either to increase or decrease their scale of operations" implied that "the maintenance of Normal Income," as so defined (that is, the maintenance of prices and costs at such a level as to keep income "Normal," in Keynes's sense) would mean *ex definitione* that output must not increase. But this, in turn, Durbin argued, meant that Investment would not increase whenever it was accompanied by a simultaneous and equal increase in Saving; for, by the terms of Keynes's own argument, Output and Investment would increase only when there was an excess of prices above cost—that is, according to the argument of the *Treatise*, only when there was an *excess* of Investment over Saving. Since, therefore, according to this argument, no increased Investment could take place unless there was a discrepancy between prices and costs, all increases in saving would be rendered "abortive." [8] Translated into terms suggested by Meade's statement of the argument, this would mean that the condition that O (= Output), and therefore I, would vary, would be consistent with disequilibrium (that is, when $I > S$ or—the case of "abortive" saving—when $S > I$) but not with "dynamic equilibrium" (that is, when $S = I$, and both, as well as output, are changing). Put in still different terms, the argument reduced to the allegation that the Fundamental Equations would hold only under conditions of "static" equilibrium (that is, when $I = S$ and both are equal to zero) and under conditions of disequilibrium (that is, when $I > S$ or $S > I$) but not under conditions of dynamic equilibrium (that is, when $I = S$, and both are greater than zero).[9]

It must be clear, however, that this argument, if it be interpreted as questioning the formal validity of the "Fundamental Equations" in the case of "dynamic equilibrium," loses its cogency as soon as we abandon an assumption that was in no way implicit in the "Fundamental Equations" themselves: namely, that an equal increase in I and S demands the presence of the condition that $\Pi - W_1$ be greater than zero. After all, this assumption is no more necessary to the validity of the equations as they stand than the assumption that P must change in strict proportion to the "quantity of money" is necessary to the older "quantity

[8] Durbin, *The Problem of Credit Policy*, 249.
[9] Cf. also below, p. 113.

equations." So far as the equations themselves are concerned, we are free to adopt any explanation we choose with respect to the nature of the forces determining the magnitude of *I*, *S*, and *O*.

At most, therefore, the argument under discussion was a criticism, not of Keynes's equations as they stand, but of the particular theory presented in the *Treatise* with respect to the nature of the forces bringing about changes in the volume of output. The adequacy of the argument of the *Treatise* with respect to the nature of the forces bringing about changes in the volume of output—an argument with which Mr. Keynes himself has since expressed dissatisfaction—will be discussed in Volume II of this study.[10] It is necessary here to point out only that while dissatisfaction with the theory presented in the *Treatise* of the forces determining changes in the volume of output may provide a commentary on the value of the *Treatise* as a whole as a contribution to the dynamics of the subject, such dissatisfaction constitutes no more of an argument against the validity of the *Treatise* equations themselves than dissatisfaction with the theory respecting the forces determining the "volume of trade" which Fisher presented in his *Purchasing Power of Money* would constitute an argument against the equation of exchange presented in the same volume. In the case of Keynes, the objection would be that too rigorous a dependent relationship was held to exist between $\Pi - W_1$ and the volume of output; in the case of Fisher, the objection was that in many cases too rigorous an independence was assumed to exist between changes in the volume of money and changes in the "volume of trade."[11] In neither case, however, could it be fairly said that the equations were themselves at stake in the dispute, to the

[10] Cf. also, in this connection, what is said below, p. 138, with respect to the interpretation to be placed upon Keynes's recently expressed attitude toward the Fundamental Equations of the *Treatise*.

[11] See, for example, pp. 155 ff. of the *Purchasing Power of Money*. It should be noted that Fisher himself was very careful to point out that his conclusions on this head were derived, not from "the equation of exchange, of itself," but from "conditions known quite apart from that equation." See also, in this connection, what is said above, p. 24, with respect to the extent to which it is true that the familiar quantity equations, as such, involve definite assumptions with respect to the "independence" of the variables which they include.

extent of requiring a "correction" in order to make them valid under all conditions.

In view of Keynes's definition of "normal income" as that which would provide no inducement to entrepreneurs to expand or contract output, it is true that something of a case may be made for the necessity for such a correction if it is argued, on the basis of a literal adherence to Keynes's definition, that the condition Output $= \varphi\ (\Pi, W_1)$ is inherent in the E of the equations as defined by Keynes: the argument then being that the least that can be done is to "correct" the Fundamental Equations by defining E in such a way as to remove the implication that its presence in the Equations demands acceptance of the condition, Output $= \varphi\ (\Pi, W_1)$. Meade himself considered the possibility of such a correction, in the form of a redefinition of the E of the Fundamental Equations so as to include therein a return to entrepreneurs necessary to induce them to expand investment in a degree sufficient to absorb an increase in "saving."[12] It would not have been surprising if, after thus redefining our E, the results obtained from the use of these new equations turned out to be different from those obtained by the equations with the terms defined as they were by Keynes. It is, however, typical of the lack of seriousness attaching to Meade's objection, that it is not easy, despite Meade's argument to the contrary, to see what there is in the equations, even as newly stated, which gives rise to difficulties within the range of problems with which Meade was concerned. It is difficult, for example, to see what in the "new" equations is inconsistent with the suggestion that we may have a condition in which I would exceed S without prices rising.[13] Even if we grant—though the case is anything but clear—that Meade is right in arguing that the result of changing the definition of E would be that, under certain conditions, E/O would not be falling, this would mean, according to the equations, merely that *under these conditions* a positive value for $I - S$ would require a rising price-level, which is precisely the conclusion that Meade seemed to regard as inconsistent with Keynes's argument.

Precisely the same thing may be said with respect to the second of Meade's principal reasons for regarding the equations of the *Treatise* as unsatisfactory for a study of "dynamic equilibrium." In essence, the argument is that the condition that "Savings" must be equal to "Investment"—which, according to the argument of the *Treatise,* was the same thing as the condition that costs must be equal to selling prices—was consistent with a very large number of

[12] The same suggestion was also made, apparently independently, by Slichter, *Towards Stability,* 48 n.

[13] Cf. Meade, *The Rate of Interest,* 49.

levels of output, including the level of output that might be expected to prevail at the bottom of a slump.[14]

Meade implied that this result is one which is obtained only as the result of his proposed redefinition of E.[15] It must be obvious, however, that so far as the paradox of regarding a condition of general slump as a condition of "equilibrium" is concerned, the objection is, if anything, more valid against the old definition of E than it is against the new. For if we interpret Meade's contention as arguing, with that of Durbin cited above, that the condition, Output $= \varphi\ (\Pi, W_1)$, is given by the definition of E as including only such "profits" as would leave entrepreneurs "under no motive either to increase or decrease their scale of operations," it would follow that Meade's proposed redefinition of E would permit a rise in the level of output without a departure from "equilibrium" in the sense of an equality between costs and selling prices, whereas the old definition, strictly applied, would not. In fact, of course, the essential part of the old definition could be rescued by the simple device of adding, to the expression "under no motive either to increase or decrease their scale of operations," the words "as a result merely of a profit-disequilibrium due to a discrepancy between costs and selling prices," Output being thus allowed to vary for other reasons than such a discrepancy.

This, however, instead of being a criticism of the equations themselves, is rather a criticism either of Keynes's argument, apart from the equations, with respect to the conditions of monetary "equilibrium," or of the interpretation of Keynes's equations as providing, by virtue of their inclusion of a relationship between costs and selling price, a complete account of the forces determining changes in the volume of output.[16] At most, therefore, Meade's argument represents a kind of nemesis overtaking the writer who had criticized the older quantity equations for failing to do more than was claimed for them—such as providing within themselves an account of the conditions for monetary equilib-

[14] It may be noted that this was virtually admitted by Mr. Keynes himself, even in the *Treatise*. See, for example, I, 305, where the case taken for "An Exercise in the Pure Theory of the Credit Cycle" was one involving a "slump which has reached an equilibrium between prices and costs of production, but is still characterized by unemployment."

[15] Meade, *The Rate of Interest*, 50.

[16] On the adequacy of a conception of "equilibrium" that confines itself merely to an equality of costs and selling prices, see again the references to Hawtrey on p. 76, n. 11, above. On the adequacy of Keynes's equations as a method for dealing even with those changes in output which may be said to be due to a discrepancy between costs and selling prices, see below, pp. 127 ff.

rium, or providing, in and of themselves, material (such as the theory of the forces determining the total of output) explaining why the variables included in the equations are of the magnitude they are.[17] So far as the equations themselves are concerned, there is nothing to prevent the users of them from improving on Keynes's theory of the forces determining the magnitude of Output, just as there is nothing to prevent users of the Fisherine equation from improving on Fisher's theory of the forces determining the magnitude of the "volume of trade."

It may be pointed out in passing that there is indeed a sense in which the paradoxical character of a definition of "monetary equilibrium" which is consistent with the maintenance of output at a slump level can be attributed to faults of the Fundamental Equations, though Meade himself did not put his finger upon the precise nature of the difficulty involved. It was, of course, Keynes's implication that during a slump costs would be generally above selling prices.[18] It is also true that, if the nature of "costs" is properly conceived, they will almost invariably be above selling prices during a "slump."[19] For if resources are unutilized, it must be because their prices are too high, in the general price-situation actually prevailing, to permit of their profitable use in production. The difficulty, however, with using this latter proposition to support Keynes's implication that in a slump costs will generally be above selling prices is that, by virtue of his identification of "costs" with *income to the factors of production,* prices which are too high to permit the profitable utilization of the factor to which the prices apply *are not included in "costs" at all,* since these costs do not enter into anyone's "income."[20] This is, indeed, as we shall see, a major vice of the Fun-

[17] Cf. what is said, on this matter, on pp. 81 ff., above.

[18] See, for example, the *Treatise,* I, 207 f., where the case of a slump is discussed as being attributable to the fact that "costs of production" have not been reduced, and that the attainment of a "true" monetary equilibrium will be impossible until there is a "reduction of the rate of efficiency-earnings."

[19] The principal reason for describing this condition as an "almost" invariable characteristic of a slump instead of as an invariable one is the possibility of monopolistic practices capable of being described in terms that would make their operation formally consistent with the generalization ventured in the text only at the cost of a tortuous conceptual construction that would greatly limit the usefulness of the generalization itself. It is obvious, in any case, that a detailed consideration of the problems involved is not required for purposes of the issues under discussion.

[20] In this connection, it is significant that, in the passage cited in note 18 above, Keynes obviously thought of the "continuance" of these high costs as leading to entrepreneurial "losses" by virtue of the fact that the high "costs" would continue to be actually accompanied by out-of-pocket payments on the part of the entrepreneurs. No other assumption,

damental Equations when the latter are regarded as a device for dealing with the forces determining the scale of output.[21] It also constitutes a reason for objecting to that identification of E as "costs" with E as income available for expenditure upon output, which is essential in order to establish the claim that the Fundamental Equations were capable of performing simultaneously the twofold function of "stream" equations and "cost and selling price" equations—a claim which, as we shall see, is permissible only on the assumption of a very special set of "stationary" conditions.[22] If, however, this latter point was the essence of Meade's criticism, it did not appear from his actual argument, which was really directed against the internal inconsistency of Keynes's various criteria for "equilibrium." It is because the equations themselves say nothing with respect to the relationship of the *level of output* to "equilibrium," which is defined by the equations solely in terms of the relationship of "costs" (in the Keynsian sense) to prices and of "Saving" to "Investment" (likewise in the sense used in Keynes's *Treatise*), that I have argued that the significance of the criticism in question did not extend beyond its interest as an ironical commentary upon Keynes's own practice of attributing to the Quantity Equations vices inherent, not in the equations themselves, but in some argument adduced by users of the equations.

It is difficult, likewise, to see more than an example of the workings of an ironic nemesis in Meade's contention that the argument which the equations of the *Treatise* were designed to summarize "depends upon the assumption of independence" between a given variable in those equations (specifically, E) and a factor not given a specific notation in those equations: namely, "the market rate of interest."[23] If the validity of the equations depended upon the assumption of a constancy of any of the elements included in the equations (or, as in the present case, upon the constancy of one of the components of the element E, namely, interest-cost) we should be confronted with as serious a limitation upon the use of the equations as Keynes imputed to the older "quantity equations" when he implied that they involved the assumption of such constancy.[24] In fact, however, we had occasion, in our discussion of the relationship

indeed, is consistent with the Fundamental Equations. The passage in question cannot, therefore, be cited as a recognition, on the part of Keynes, of the true nature of the "costs" involved in a disequilibrium between costs and selling prices in times of slump.

[21] See especially, on this matter, pp. 127 f., below.

[22] Cf. below, pp. 126 ff.

[23] Meade, *The Rate of Interest*, 50.

[24] Cf. above, pp. 44 f.

between the Quantity Equations and the Quantity Theory, to see that questions of this type were relevant only to the cruder forms of the Quantity *Theory,* and not to the Quantity Equations as such.[25] We saw, for example, that it is of some importance to the cruder forms of the Quantity Theory that a given factor which may affect one of the variables in the Quantity Equations in a price-*raising* direction should not affect another of the variables in a price-*lowering* direction. We saw, also, however, that, so far as the formal validity of the Quantity Equations themselves was concerned, there is nothing to prevent our taking account of whatever dependent relationships may be held to exist, whether these relationships are of a mutually re-enforcing or a mutually compensating character.[26] The equations as such, in other words, could not be said to involve the assumption of stationary conditions with respect to any one of the variables included therein.

Precisely the same reasoning applies here. The argument which, according to Meade, involved the assumption of "independence" between the E of the Fundamental Equations and the "market rate of interest" was, presumably, Keynes's argument with respect to the *modus operandi* of Bank-rate. This argument, as we shall see, was primarily in terms of the effect of changes in Bank-rate upon Investment, that is, the I of the Fundamental Equations.[27] It was now argued by Meade that, according to the definitions of the terms used in the Fundamental Equations, changes in the market rate of interest should also affect E, since the latter included interest-costs along with other

[25] See especially above, pp. 43 ff.

[26] See especially above, p. 25. It is obvious that the single condition there recognized as capable of limiting seriously the applicability of the Quantity Equations—namely, the condition that a given variable could under no circumstances be an independent variable—has no application to the present discussion. The term E certainly included costs—such as wage costs—which could be expected to vary as the result of what Keynes called "spontaneous" changes in the rate of efficiency earnings (cf. the references given below, p. 113, n. 35). Indeed, as Meade himself points out (*The Rate of Interest,* 50 n.), Keynes's own usage was sometimes such as to convey the impression that changes in wage-costs were the only factors of significance causing changes in E. This is not to say, however, that Keynes actually denied that changes in interest-costs would affect E. See below, p. 112, n. 30.

[27] Cf. below, pp. 280 ff.

costs.[28] All that this could conceivably mean, however, would be that any judgment as to the effect of changes in the market rate of interest would have to take account not only of the probable effects of such changes upon I, but also upon E, and possibly upon S.[29] If, therefore, there was an assumption with respect to the "independence" of movements in E, in particular, it was not inherent in the Equations, but in an argument which bore the same relationship to those equations as the cruder variants of the Quantity Theory bear to the forms of Quantity Equation actually in use.

As a matter of fact, Keynes's own discussion of the *modus operandi* of Bank-rate actually took account of the effects of a change in Bank-rate upon the interest-costs which were included in E.[30] It is possible, to be sure, to demonstrate that the particular argument he used in this case was fallacious; and it is possible to demonstrate also that Keynes was led to his fallacious argument by the fact that his E included only costs which are accompanied by simultaneous cash-payments to the factors of production.[31] This latter fact, as we shall see, is indeed of the greatest importance for a judgment of the validity of the Fundamental Equations under non-stationary conditions. This, however, was not the fact to which attention was called by Meade, whose criticism turned entirely upon the difficulties which were supposed to derive from the fact that E, as well as I, would be expected to be affected by changes in the market rate of interest.[32]

[28] Cf. Meade, *The Rate of Interest*, 50. For examples of a specific statement on the part of Keynes that E was intended to include "interest on capital," see the *Treatise*, I, 123, and also I, 211, where interest was described as being "simply the money-rate of earnings of one of the factors of production."

[29] Meade's statement (*loc. cit.*) that the lowered rate of interest would lower S before I was affected was based upon the equation $PR = E-S$, from which it follows that if we assume that "the amount spent on consumption goods" (PR) remains unchanged, any reduction in E must necessarily mean a reduction in S. In fact, of course, Keynes was himself prepared to argue that a lowering of the rate of interest would be expected to bring about a reduction in S, though his argument was based upon considerations extraneous to the equation $PR = E-S$. Thus, on I, 154 of the *Treatise*, he stated explicitly that "the rate of saving . . . is stimulated by a high rate of interest and discouraged by a low rate." One could not find a better proof of the fact that there is no conflict between the Fundamental Equations as such and the mere circumstance that changes in the market rate may be expected to cause changes in variables other than I.

[30] See the *Treatise*, I, 211.

[31] See, on this matter, especially pp. 127 ff., below.

[32] The difference in the degree of seriousness attaching to the two types of objection will become clear from a contrast between criticism such as that of Meade, on the one hand, with that of Hayek, on the other, whose

Much the same thing may be said, finally, against the ground on which E. F. M. Durbin charged that the equations of the *Treatise* "are purely statical in type and do not provide tools for a dynamic analysis as Mr. Keynes claims for them." [33] When, for example, Durbin alleged that the equations, when regarded as "statements of the monetary determinants of the price level . . . tell us nothing about the fate of prices in periods long enough for *O* [Output] to change," he was clearly imputing to the equations an assumption with respect to the constancy of *O* that is certainly not inherent in the equations themselves.[34] So far as the equations are concerned, indeed, they could be used perfectly well to demonstrate the necessity of a proposition which Durbin regarded as inconsistent with Keynes's own analysis—namely, the proposition that "an equality between Savings and Investment would not permit of constant unit prices," if it is desired to maintain "monetary equilibrium" under an "increase in physical efficiency such as will be produced by the accumulation of capital." [35] For

criticisms respecting Keynes's treatment of interest-cost in his equations were part of a general argument designed to show the lack of justification for a simultaneous identification of the *E* of the Fundamental Equations with both the costs of production of current output and with the outlay from earnings used in the purchase of that output. See, for example, Hayek's "Rejoinder to Mr. Keynes" in *Economica* for November, 1931, p. 15; and for a discussion of the bearing of Hayek's argument upon the question as to the formal validity of the equations of the *Treatise,* see below, pp. 133 ff.

[33] Durbin, *The Problem of Credit Policy,* 248.

[34] It will be noted, also, that Durbin's attack upon the "static" character of the Equations was made more than a little cryptic by virtue of the fact that it combined two elements: first, the hypothesis that the Equations were "simply statements of the monetary determinants of the price-level," and second, the suggestion that Output was not allowed to "change." It is only the latter element in Durbin's attack that is considered here. If the first element is transformed into the allegation that the Equations, when interpreted as representing "statements of the monetary determinants of the price-level," tell us nothing "about the nature of equilibrium policy" (so Durbin, *loc. cit.*)—in the sense, say, of a policy aimed at maintaining an equality between costs and selling-prices—it can, indeed, be turned to deadly account against the equations of the *Treatise* (cf. below, pp. 133 ff). The difficulties involved, however, are of quite a different order from those suggested by the mere fact that *O* may change in the interim, although, as is pointed out below (p. 135), changes in Output are involved in the problem in an indirect and roundabout way.

[35] Durbin, *The Problem of Credit Policy,* 249. As a matter of fact, Keynes was perfectly explicit (particularly in the first volume of the *Treatise*) in arguing that certain conditions—specifically, a set of conditions characterized by what he himself called "spontaneous" changes in

if we assume, as Durbin did, that such an "increase in physical efficiency" will be reflected in an increase in output, we obtain, by the second of the Fundamental Equations—that is, $\Pi = E/O + (I - S)/O$—an increase in the O of the expression E/O, which, according to the *Treatise*, was to be taken as a measure of unit costs (W_1). Thus, if we assume "an equality between Savings and Investment," we obtain a necessary fall in Π—in other words, precisely the "fall . . . in unit price," as well as in "unit cost," which was desiderated by Durbin himself, rather than the "static prices" that he understood the argument of the *Treatise* to desiderate.[36] To argue, therefore, that the Fundamental Equations, as such, could be held responsible for the conclusion that "an equality between Saving and Investment" would demand the maintenance of "static"—that is, stable—prices is to confuse the Fundamental Equations themselves with a proposition which follows from their use only when $W_1 (= E/O)$ is assumed to be constant.[37] The point to be made here, in other words, is that which was made above in connection with Meade's criticisms: namely, that when Mr. Keynes's equations were criticized for faults

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the rate of efficiency earnings—would call for a "proportionate change in the price-level." See the *Treatise*, I, 153, 157, and especially pp. 166 f. It is true that these statements are in formal conflict with those cited from the *Treatise* in the following note; but it remains true that the statements cited here are those which follow from the Fundamental Equations themselves, whereas those cited below do not.

[36] The consistency of the Fundamental Equations, as such, with the type of result desiderated by Durbin had already been pointed out by writers who were as anxious as Durbin himself to demonstrate that the maintenance of "an equality of Savings and Investment" would by no means always call for stability in the price-level, despite Keynes's frequent implications to the contrary in the *Treatise* (for example, I, 183 and particularly II, 220, 222, 350). See, in this connection, C. O. Hardy in the *American Economic Review*, XXI (1931), 153 f.; also the same author's "Savings, Investment, and the Control of Business Cycles," in the *Journal of Political Economy*, XXXIX (1931), 398; and cf. Hayek, "Reflections on the Pure Theory of Money of Mr. J. M. Keynes," Part I, *Economica*, August, 1931, 291.

[37] The disputed thesis of Mr. Keynes with respect to the relationship between an "equality of Savings and Investment" and stability of the price-level was put in these terms by Hayek in his "Reflections," Part II, *Economica*, February, 1932, 29. It is to be noted, however, that Hayek, unlike Durbin, did not charge that it was considerations of this kind which would justify the characterization of the equations of the *Treatise* as "purely statical in type." Hayek's own grounds for advancing such a characterization were of a very much more serious kind. Cf., on this matter, pp. 133 ff., below.

which, though they might be said to inhere in other parts of the argument of the *Treatise,* were certainly not inherent in the equations themselves, the critics committed a sin identical in character with that committed by Mr. Keynes when he charged the "Quantity Equations" with sins which were fairly chargeable only to the cruder forms of the "Quantity Theory."

III

Hidden Assumptions: The Attack and Keynes's Defense

The ironical aspects of the matter, however, by no means end here. It will be recalled that, in our discussion of the various meanings which might be given to the proposition that the familiar "quantity equations" were of a "statical" character, account was taken of the possibility that such a statement could be understood to allege that the equations assumed a constancy, not of the variables explicitly included therein, but of certain relationships outside the equations themselves. It was freely admitted, moreover, that, whenever a given expression could be shown to involve such an assumption, that particular expression could not be regarded as valid under all conditions—the only further point being that it was anything but clear that all "quantity equations" were, or need be, of this type. It is, therefore, not without interest to record the fact that it was possible for Keynes's critics to demonstrate clearly that the first of Keynes's own Fundamental Equations was in fact formally invalidated for any but a highly specialized set of conditions by just such a hidden assumption of constancy in magnitudes not given a specific place in the equation itself.[38]

[38] The most emphatic statement of the point in question was given by A. H. Hansen, "A Fundamental Error in Keynes's 'Treatise on Money,'" *American Economic Review,* XXII (1932), 462 (cf. also A. H. Hansen and H. Tout, "Investment and Saving in Business Cycle Theory," *Econometrica,* I [1933], 123 ff., from which some of the notation used in the text is taken, though the statement of the point itself is here freely paraphrased). Attention had been called to the difficulty earlier, however, by F. A. Hayek, in his "Reflections," Part I, *Economica,* August, 1931, 287, where it was pointed out that the assumptions made by Keynes with respect to the relationship between unit costs of production for R and unit costs of production for C "makes them [the terms representing these unit costs]

The argument, in the form in which it was presented by Professor Hansen, amounted to a demonstration that Keynes's method of introducing the term I' into the equation $P = E/O + (I' - S)/R$, which depended upon the assumption that $E(C/O) = I'$, in which C is taken to represent the "net increment of investment" (that is, the volume of production of new capital goods), was invalid for the simple reason that the expression $E(C/O) = I'$ can be regarded as a true equation only upon the assumption that $W_c/W_r = k$, in which k is a constant and W_r and W_c are the unit costs of production of consumers' and producers' goods respectively.[39] For if we write the questioned expression in the form $C/O = I'/E$, it becomes clear that I'/E is equal, strictly speaking, not to C/O, but rather (letting $O = C + R$) to $CW_c/(CW_c + RW_r)$. Obviously C/O will be equal to I'/E if, following the tacit procedure of Keynes in the *Treatise,* we assume $W_c = W_r = W_1$, in which W_1 is the cost per unit of both kinds of goods; for, in that case, we should have

$$\frac{I'}{E} = \frac{CW_1}{(C+R)W_1} = \frac{C}{C+R} = \frac{C}{O}.$$

Just as obviously, however, $C/O \lesseqgtr I'/E$, whenever W_c and W_r do not change in equal degree. This, obviously, is merely another way of saying that the expression $E(C/O) = I'$, upon which Keynes's introduction of the term I' into his equation rested, will be true only upon the assumption that $W_c/W_r = k$.

It is worth noting that the point in question—namely, that the equation $P = E/O + (I' - S)/R$ is valid only upon the assumption that the ratio W_c/W_r is a constant—holds also against the alternative derivation for the equation suggested by Mr. Hawtrey, the essence of which was the introduction of the term I' without relying upon the expres-

absolutely unsuitable for the explanation of any dynamic process" whenever such a process involves a change in "*the relative costs of consumption goods and investment goods*" (p. 287; italics Hayek's).

[39] It will be noted that the objection, as so stated, is much more inclusive than when it is interpreted as applying only to the case that both Hayek and Hansen advanced for the purpose of illustration—namely, one in which technical progress takes place at different times and in different degree in the capital goods field and the consumers' goods field. Cf. A. G. Hart, "An Examination of Mr. Keynes's Price-Level Concepts," *Journal of Political Economy,* XLI (1933), 629 f.

sion[40] $E(C/O) = I'$. Thus, using the same symbols as before, if we write $E = RW_r + CW_c$, we may also write $E = RW_r + I'$, since $CW_c = I'$ by definition. Substituting this value for E in the equation $PR = E - S$, we obtain $PR = RW_r + I' - S$, which becomes $P = W_r + (I' - S)/R$. Unfortunately, however, we are not justified in taking the next step suggested by Mr. Hawtrey, which consisted of identifying, in the manner of Mr. Keynes, W_r with E.[41] For we are now dealing, not with average unit costs of output in general, but with the average unit cost of "consumption goods." If, like Mr. Hawtrey (again in imitation of Mr. Keynes), we assume that $W_c = W_r = W_1$, there is some justification[42] for writing $E/O = W_r$, since, on the assumption that $W_c = W_r$, the expression $E/O = W_r$ follows from a writing of the expression

$$E/O = (CW_c + RW_r)/(C + R)$$

in the form $E/O = [(C + R)W_r]/(C + R)$. If, however, with Professors Hansen and Hayek, we insist upon taking account of the possibility that W_c and W_r may change in different proportions, it is obvious that $E/O \gtrless W_r$ whenever there is a change in the ratio W_c/W_r. As before, therefore, we are forced to conclude that the first of the fundamental equations of the *Treatise*, when presented in the form $P = E/O + (I' - S)/R$, was valid only upon the assumption that the ratio W_c/W_r is constant.

The reaction of Mr. Keynes and others to this criticism, in the form in which it was presented by Professor Hansen, is of considerable interest in the light of the discussion, in Chapter III of this study, of the significance of this type of criticism when it was directed against the more familiar quantity equations. It must be said, in justice to Mr. Keynes, that he did not choose the way of escape proposed by some of his ready defenders: namely, that the criticism

[40] The account given in the text represents a free paraphrase of Hawtrey's method, for which see *The Art of Central Banking*, 335. Virtually the same method was proposed by E. Lundberg (cf. Hansen and Tout, "Investment and Saving," *loc. cit.* 124), and by Hart ("An Examination," *loc. cit.*, 630); though it is important to note that in neither of the latter two cases was the final step taken of writing $W_r = E/O$, which is the crucial one, so far as the defense of the first equation of the *Treatise*, as originally presented, is concerned.

[41] See Hawtrey, *The Art of Central Banking*, 335, and cf. the *Treatise*, I, 135 f. Keynes, of course—and therefore Hawtrey—wrote, not $P = W_r + (I' - S)/R$, but $P = W_1 + (I' - S)/R$; but since W_1, in this case, must refer to the unit cost of production of R, it is W_r that is involved.

[42] The reason for saying that there is "some" justification for the procedure in question, rather than complete justification, is that all these derivations make use of the equation $PR = E - S$ in a way which assumes that the *E of this equation* may also be regarded as the equivalent of a total of *costs*. On this matter, which is crucial for a statement of the conditions under which the equations of the *Treatise* may be regarded as valid, see below, pp. 126 ff.

did not greatly matter because the particular relationship which, in his derivation of the first of the Fundamental Equations, was assumed constant, could in fact be expected to remain constant over short periods.[43] It is not necessary to labor the point that this is a contention which is hardly in the nature of a self-evident proposition.[44] The significant thing to be said, in this connection, is that such a contention would have represented a strange type of defense by one who had charged that the usefulness of the older quantity equations was seriously limited by their supposedly "statical" character. For it would have constituted a virtual admission that, so far from its being true that the equations of the *Treatise* were not hampered by limitations with respect to the "constancy" of certain conditions to which the older equations were subject, they were in fact inferior to the older equations in one essential respect: namely, that whereas the latter, when properly stated, were valid under all conditions, the equations of the *Treatise* were valid only under the special conditions which were supposed—on grounds anything but obvious—to be characteristic of the "short period."

The point is worth more than a passing emphasis, in view of the threefold circumstance, (1) that some writers have regarded the distinction between "statics" and "dynamics" as being substantially identical with the distinction between the "short period" and the "long period," (2) that the older quantity equations of the Fisherine form have been alleged to be true in the "long period" but not in the "short period," and, (3) that Keynes's *Treatise*—and therefore the equations summarizing its argument—has been characterized as an exploration of "the little-explored territory of short-period economics."[45] If all three

[43] So, for example, B. P. Adarkar, in *American Economic Review*, XXIII (1933), 87. The possibility that the difficulty "might not be serious in short-period reasoning" was suggested also by Hart, "An Examination," *loc. cit.*, 629, though only upon the assumption that Hansen's "original objection"—namely, that the cost of production of producers' and consumers' goods could not be expected to change in equal degree—was confined to what was obviously intended as an illustration of that objection: namely, the possibility of "*technical* changes altering the relative cost of production of C and R" (italics mine). Even so, it may be remarked in passing, it is anything but clear—if, by "the short period," we mean a period as long as the business cycle—that changes of the kind indicated are of no importance. Cf. the reference given in the following note.

[44] Cf. the comment by Hansen and Tout, "Investment and Saving," *loc. cit.*, 123, n. 5.

[45] The distinction between "statics" and "dynamics" as being identical

of these propositions could be accepted without question, it would be easy to conclude that we might accept the older—supposedly "statical" —equations for "long-period" analysis and the equations of the *Treatise* —supposedly "dynamical" in character—for purposes of short-period analysis. The second part of this thesis was, indeed, advanced even by supporters of Mr. Keynes who felt that his attempt to extend the application of the Fundamental Equations "to the long period" was "only tentative and not entirely successful."[46] Some basis, moreover, was given by Keynes himself, in the *Treatise*, for the first part of the thesis —namely, that equations of the general Fisherine form might be used for "long-period analysis," though not for analysis of the "short period." He stated, for example, that "the long period movements in the equilibrium price-level" would be expected to be virtually identical with movements "in the level of efficiency earnings," whereas the "short-period movements round the long-period trend of the equilibrium price-level" are "due to the temporary disequilibrium of the investment factors."[47] This reduces algebraically to the proposition that, for the long period, an equation of the form $\Pi = E/O$ would be sufficient, whereas for the short period we should have to add the term $(I - S)/O$. It will be remembered that Keynes himself regarded the translation of the term E/O into $(M_1V_1)/O$ as one of the ways in which the equations of the *Treatise* could be related to the "usual monetary factors."[48] When, therefore, he alleged that the expression $\Pi = M_1V_1/O$ (or, as he put it, a proposition to the effect that certain types of movement in the price-level will be due to changes in the "quantities of the monetary factors relatively to the volume of output") is an adequate formulation of the forces determining "the long period movements in the equilibrium price-level," he certainly lent color to the interpretation of his position as alleging that he would grant the validity of equations of the general Fisherine form for the "long period," though not for the "short."[49]

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with the distinction between the "short period" and the "long period" is so common that a list of representative references would be very long indeed; it is sufficient, therefore, to call attention here to the critical remarks by Frisch, "Statikk og Dynamikk i den økonomiske Teori," *loc. cit.*, 338. For a suggestion to the effect that the Fisherine equation is valid in the long period, but not in the short, see W. C. Mitchell, *Business Cycles: The Problem and its Setting*, 137 (see also the reference to the *Treatise* given in n. 47, below). For a characterization of the *Treatise* in the terms suggested in the text, see Pigou, in *The Nation and Athenaeum*, XLVIII, 544.

[46] So, for example, Adarkar, *American Economic Review*, XXIII, 87.

[47] *Treatise*, II, 302.

[48] Cf. the *Treatise*, I, 149. The validity of the distinction between "investment factors" and "monetary factors" will be discussed in Volume II of this study.

[49] For the statement quoted, see the *Treatise*, II, 302. Cf. also, however, II, 206, of the same work, where it was stated that "even the long-period movements of the price-level have been influenced by the *second* term of the Fundamental Equation to a much greater extent than might have been expected." The explanation of the discrepancy may lie in a distinction between "long-period movements of the price-level" (II, 206)

In fact, however, the three propositions advanced above are either unsound as they stand, or become unsound when applied in the manner proposed. The suggestion, for example, that a given analytical device is "dynamical" in character if it applies *only* to the "short period" not only raises awkward questions for those to whom the difference between "statics" and "dynamics" is somehow concerned with the difference between stationary and nonstationary conditions, but also ignores the profound implications of the proposition that, in a fundamental sense, dynamics "include statics," and therefore should be capable of dealing with "statical" conditions as well as with "dynamical" ones.[50] The suggestion that equations of the general Fisherine form are applicable *only* to the long period is, as we have seen, simply not true.[51] The suggestion, finally, that the equations of the *Treatise* are valid *only* for the short period not only restricts greatly the nature of the claims made for them as aids in the study of the forces making for change, but ignores the fact that, so long as the type of difficulty under discussion is a real one, they are not valid *even* for the short period, unless the period is so very "short" that no changes in the type indicated can possibly occur.

The interesting thing about Mr. Keynes's own reaction to the criticism in question is that it was precisely of such a nature that, if it had been applied with equal readiness to defective forms of the older "quantity equations," it would have shown the essential fallacy of the argument that the mere fact that certain specific expressions presented as "quantity equations" were not true equations, constituted a

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and "long period movements in the *equilibrium* price-level" (II, 302). This was not made clear, however, by Mr. Keynes's own exposition; and one surmises that the ambiguity is typical of the confusion that is bound to arise from loose identifications of "equilibrium" with the "long period."

[50] Typical of the confusion engendered by the first type of difficulty is that represented by the practice of Meade, who, in arguing that the equations of the *Treatise* were suited only for the analysis of "static" rather than "dynamic" equilibrium, proceeded to identify "static" equilibrium with "long period" equilibrium, despite the fact that the particular difficulty with which he was concerned—namely, a simultaneous increase in the total of saving and investment—was precisely the kind of difficulty which would be met with in periods "long" enough for such an increase in saving and investment to take place! (See Meade, *The Rate of Interest*, 49.) On the proposition that "dynamics include statics"—a proposition the implications of which must be left for exploration to the occasion indicated on p. 40, n. 2, above—see Marshall, "Distribution and Exchange," *loc. cit.*, 37 (*Memorials of Alfred Marshall*, 312); cf. also M. Pantaleoni, *Erotemi di Economia*, II, 76.

[51] Cf. pp. 46 ff., above. The truth of the matter, of course, is that if objections of the type raised by Mitchell were really valid against equations of the general Fisherine form, there are no clear reasons for assuming that, under certain conditions, they would be any more valid in the "long period" than in the "short." Cf., in this connection, the remarks of Burns, "The Quantity Theory," *loc. cit.*, 577, on Mitchell's proposition that the "longer the interval, the better for the [Fisherine] equation."

reason for rejecting *all* expressions of the general form of the older "quantity equations." For, in fact, Mr. Keynes's own procedure with respect to the equation which had been shown to be defective followed exactly the two methods indicated as alternative ways of dealing with the type of difficulty in question.[52]

The first of these methods, it will be recalled, was the insertion of the term whose absence would make the given expression untrue under certain conditions. This is precisely the first of the methods proposed by Keynes as a means of dealing with this difficulty. The specific proposal was that, instead of writing $P = E/O + (I' - S)/R$, we should write $P = E/(R + kC) + (I' - S)/R$. For if, as before, we let $W_c/W_r = k$, but now assume that k is a true variable, and if, further to simplify the algebra, we assume that $W_r = 1$, so that $W_c = k$, we may write the expression $CW_c/(CW_c + RW_r) = I'/E$ in the form

$$\frac{kC}{kC + R} = \frac{I'}{E}. \tag{1}$$

We then obtain, by substituting the correct expression $kC/(kC + R)$ for the incorrect expression C/O in the "equation" $P \cdot R = E(C/O) + E(R/O) - S$, an equation in the form

$$P \cdot R = E \cdot \frac{kC}{kC + R} + E \cdot \frac{R}{kC + R} - S. \tag{2}$$

Since, according to equation (1), we have

$$I' = E(kC/[kC + R]),$$

we may substitute I' for $E(kC/[kC + R])$ in equation (2), and obtain $PR = E(R/[kC + R]) + I' - S$, which, upon division by R, becomes the same as Keynes's substitute equation,[53] namely

[52] For Keynes's procedure, see the *American Economic Review*, XXII (1932), 691 f.

[53] The objection has been raised to this method of correcting the first of the Fundamental Equations that it fails "to lead to an interpretable definition of P." (So, for example, A. G. Hart, "An Examination," *loc. cit.*, 630.) Since, however, the P still refers to the R in the equation, we are still dealing with a definition of P at least as "interpretable" as that in the original equation or as that in the alternative equation presented by the

$$P = \frac{E}{R + kC} + \frac{I' - S}{R}.$$

The second method of meeting this type of difficulty, as we saw, was a redefinition of the terms in such a way as to make the expression in question necessarily a true equation. Again, this was precisely the alternative proposed by Mr. Keynes, who suggested a device—namely, the definition of the unit of goods consumed (R) and units of goods invested (C) in such a way that these units would always have the same cost of production—which would undoubtedly have had the effect of making the equation $P = E/O + (I' - S)/R$ a truism as far as the particular difficulty under discussion is concerned.[54] It will be recalled, however, that, while the conferring of a truistic character upon a given expression undoubtedly establishes its formal *validity* under all circumstances, the question as to the *usefulness* of the "truism" or "identity" thus established can be answered only by a consideration of the particular "truism" involved.

The question as to the usefulness of the "truism" $P = E/O + (I' - S)/R$, when the terms are redefined in such a way as to take account of the criticism with respect to the validity of the equation as first presented, takes on particular interest in view of the two circumstances that (1) Mr. Keynes himself expressed a preference for this method over the first method of dealing with the difficulty, to the point of announcing his intention of adopting this method in later editions of the *Treatise,* and (2) that he had, in the *Treatise,* claimed that the equations of the *Treatise* were more useful for "dynamic" analysis than the older "quantity equations," because, whereas the latter "do not, any of them, have the

critic himself as evading "this difficulty," and which is of the form $P = W_r + (I-S)/R$. After all, Keynes' $E/(R + kC)$ is nothing more nor less than a rewriting of W_r—or, in Hart's notation, of F'/R. The real objection to the equation, as corrected, is that the E of the new equation, as well as of the old, cannot be regarded as representing *simultaneously* the flow of money-incomes directed against the purchase of R, however defined, and the total of costs incurred in the production of R, again however defined. On this matter—which is, after all, the crucial one—see below, pp. 127 ff.

[54] This follows from the fact that to "choose . . . our unit of goods invested in any year [that is, the goods contained in C] as the quantity which has in that year the same cost of production as a unit of goods consumed [that is, the goods contained in R]" means that we make W_c equal to W_r by definition under all circumstances.

advantage of separating out those factors through which, in a modern economic system, the causal process [by which the price-level is determined] actually operates during a period of change," his own equations did precisely this.[55] Yet it must be obvious, upon reflection, that it was precisely the "separating out" of the factors determining the price-level "during a period of change" which the truism now presented to the public by Mr. Keynes would have made virtually impossible.

Surely, for example, changes in the physical output of goods are an important factor affecting the price-level. The presence of the term O in the equations of the *Treatise* would lead one to suppose that these changes were given a "separate," and therefore adequate, place in Mr. Keynes's final formulation. It was easy, however, for Mr. Keynes's critics to show that, once O is defined in such a way that changes in it will represent not only changes in the physical output of goods, but also changes in the unit costs of these goods, the new Fundamental Equations, instead of "separating out" the factors that were held to determine the price-level "during a period of change," jumbled them together in inextricable confusion.[56] When note is taken of the fact—to be demonstrated in detail in the following chapters—that no such charge can properly be levied against the older "quantity equations," whose usefulness for precisely this purpose Mr. Keynes had called into question—particularly when the more elaborate forms of these "quantity equations" are used—the irony of the situation created by Mr. Keynes's later, and preferred, formulation of the Fundamental Equations of the *Treatise* becomes fairly complete.[57]

[55] For the expression of preference referred to, see Keynes, *American Economic Review*, XXII, 692. For the passage in the *Treatise* quoted, see I, 133, of that work.

[56] Cf. the comments on this point by Hansen and Tout, "Investment and Saving," *loc. cit.*, 125. It is obvious that the comment by Hart, cited above, p. 121, n. 53, does indeed apply to this *second* method of correcting the difficulty, though it does not apply to the first.

[57] It was, therefore, hardly necessary for Mr. Keynes to add to the irony of the situation, as he has done recently, even with respect to the particular point under discussion, by advancing against "the Quantity Theory of Money"—which he had just identified with a quantity equation of the form $MV = OP$—the contention that "for the purposes of the real world it is a great fault in the Quantity Theory that it does not distinguish between changes in prices which are a function of changes in output, and

IV

The Collapse of "Keynes's Libra"

Not entirely so, however. For we come now to what is unquestionably the most serious of the accusations against the equations of the *Treatise* as being "static" in character, both in the sense that they assume the constancy of certain relations not given a specific notation in the equations themselves, and in the sense that they are not useful for the analysis of conditions of monetary "disequilibrium."

The accusation in question has to do specifically with the validity of the algebraic analysis by which Mr. Keynes was able to convey the impression that his equations really represented, *at one and the same time* (1) a formulation designed to show the nature of the forces determining prices (the P and Π of the two Fundamental Equations) and also (2) a formulation designed to establish the relationship between selling prices and the cost of production (W_1) of the goods sold at these prices. It was, after all, the combination of these two functions—and specifically the addition of the second function to the first—that was supposed to represent the great element of novelty in the equations of the *Treatise*.[58] As we have seen, the older quantity equations made no attempt to provide a statement of the relationship between "costs" and "selling prices." In that sense, they had nothing to say about the specific condition for "monetary equilibrium" which is supposed to be represented by an equality between "costs" and "selling prices." The equations of the *Treatise*, on the other hand, purported to do not

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those which are a function of changes in the wage-unit" (*General Theory*, 209). In the light of Mr. Keynes's "preferred" form of the equations of the *Treatise*, further comment upon this statement, in the present connection, should be superfluous. The validity of Mr. Keynes's comment as it stands, moreover, will be discussed in Volume II of this study.

[58] For a particularly emphatic statement of a claim of this type on behalf of the Fundamental Equations, see Adarkar, *The Theory of Monetary Policy*, 57, where the chief value of the Equations for a "dynamic analysis of money" was alleged to reside in what Adarkar summarized as "Keynes's Libra," which was designed "to show in what ways the Keynsian equations mark a departure from the classical line of approach to monetary dynamics," and reduced to the proposition that the Equations were capable of performing simultaneously the two functions indicated in the text.

only this—and thus to provide a supposedly more useful technique for a study of the causes and consequences of monetary "disequilibrium"—but also to provide at least as adequate an account of the forces determining general prices (II) as did the older quantity equations. It can be shown, however, that the equations of the *Treatise* did nothing of the kind; that, in fact, under any but a very special set of assumptions with respect to the constancy of certain relationships not given specific notation in the Fundamental Equations themselves, they may be regarded as performing *either* the first function *or* the second, but not both simultaneously.

In our attempt to demonstrate the weakness of the assumptions underlying the algebraic analysis which was supposed to justify the interpretation of the Fundamental Equations as performing both functions simultaneously, we need not pause upon the question whether we can under all circumstances accept as "truistic" the expression $PR=E-S$, which was involved in the derivation of both Fundamental Equations, and which definitely strengthened the impression that Mr. Keynes was just as much concerned with presenting a picture of a stream of money-expenditure directed against a stream of goods as he was with presenting one that would show the relationship between costs and selling-prices.[59] The answer to this question was provided very soon after the publication of the *Treatise* by the intensive discussion to which the book gave rise: the expression $PR=E-S$ is indeed truistic, if we are prepared to acquiesce in definitions of both "income" (E) and "savings" (S) which give rise to a series of constructions that are not only almost incredibly tortuous in themselves, but actually led to con-

[59] Cf., for example, the comment by Robertson to the effect that the equation in question "is of the Fisherine type, *i.e.*, it equates the flow of money devoted to the purchase of a certain type of goods during a period with the flow of goods of that type becoming available for purchase" ("Mr. Keynes' Theory of Money," *loc. cit.*, 397). It was only natural that, since the equation $PR = E-S$ was used in the derivation of the Fundamental Equations proper, they too should have been interpreted as being "Fisherine in type," in the sense indicated. For examples of an interpretation of the first of the Fundamental Equations in this sense, see Robertson, *op. cit.*, p. 401, and Hart, "An Examination," *loc. cit.*, 632. Cf., however, what is said on this matter on p. 136, below.

clusions which seem in conflict both with the implications of ordinary usage, and with the implications of certain parts of the argument of the *Treatise* itself.[60]

The bizarre paradoxes to which Mr. Keynes was led in his effort to defend simultaneously the integrity of the expression $PR=E-S$ and the particular definitions given to the terms appearing in that expression—particularly the definition of E as "earnings"—might conceivably have been justified if he had succeeded in reaching a result which was represented by none of the received "Quantity Equations": namely, the provision, *at one and the same time,* of (1) a formulation designed to show the nature of the forces determining general prices, and (2) a formulation designed to establish the relationship between selling prices and the cost of production of the goods sold at these prices. It was obviously for the sake of accomplishing the second of these purposes that Mr. Keynes clung, in particular, to his definition of E as income out of earnings; for it was clearly his intention to use this definition as the bridge by which to pass to the inclusion, in his Fundamental Equations, of the cost-item $E/O=W_1$.[61]

The sad truth of the matter, however, is that, for all his sacrifices in the way of paradoxical conclusions for which

[60] This is a judgment which will by no means be readily accepted by all defenders of the argument of the *Treatise.* Little, however, would be gained by renewing here the type of debate involved, which engaged so large a part of the interest of commentators upon the argument of the *Treatise* at the time of its first appearance. Insofar as certain of the issues raised in this debate can be shown to be related to Mr. Keynes's subsequent conversion to the view that Savings and Investment are of necessity always equal, they will be dealt with in Volume II of this study. For the rest, it is sufficient to point to the fact that, for purposes of the argument which follows, one could accept as truistic the equation $PR = E-S$, and could regard the usages required in order to establish the truistic character of the equation as quite natural and unforced, and still be compelled to conclude that it is possible to regard the Fundamental Equations of the *Treatise* as performing simultaneously the two functions indicated in the text only upon the assumption of a highly special set of "stationary" conditions.

[61] The cost-item I', in the first of the Fundamental Equations, was, of course, introduced by the simple device of substituting it for $E \cdot C/O$. The use of the latter expression, however, was itself based upon the use of the expression E/O as a cost-item (cf. the *Treatise,* I, 135). It will therefore simplify the exposition if we confine ourselves to an examination of the conditions under which E/O may be considered to be a cost-item at the same time that it is regarded as a representation of the stream of money-expenditure out of earnings upon "output."

nothing could be said except that they protected the integrity of the expression $PR=E-S$ when E was defined as "earnings," Mr. Keynes was not able to develop a method of transition from E when E is regarded as relevant to the concept of a stream of money available for expenditure against goods (which we may write as E_e) to E as a total of relevant costs (which we may write as E_x) that would justify the statement that the Fundamental Equations, in their final form, accomplished simultaneously the two-fold purpose indicated above.

We may begin by pointing to a simple fact that is all too often overlooked: namely, that it is by no means clear that the "costs" (E_x) which are strictly relevant to a comparison of "costs" with selling-prices, for purposes of a study of the forces affecting something properly to be called "monetary equilibrium," are costs which under all circumstances may be said to enter into "money income," when the latter is regarded as a stream of money receipts immediately available for money-payments (E_e). The "costs," for example—such as "sticky" wage-rates—which are too high to enable production to proceed without loss are certainly costs which must be compared with selling prices in order to determine whether or not "equilibrium," in one of Mr. Keynes's senses of the term, exists; they will therefore make part of E_x.[62] Since, however, nobody will be employed at such wage-rates, no income will be currently received at those wage-rates, and these items will make no part of E_e.

[62] This could be denied only by those who would deny that the "costs" which enter into entrepreneurial calculations, and which are therefore relevant for a judgment as to whether we are confronted with a situation properly characterized as "equilibrium," are *prospective* costs, which are then compared with *prospective* selling-prices. That virtually the whole of our apparatus for dealing with the economizing activities of individuals assumes that such activities are *prospective* in their nature is a fact that has been obscured from the vision of only those to whom the introduction of the element of "anticipations" or "expectations" has seemed a "revolutionary" step. More will be said on the latter point in Volume II of this study. In the meantime, it is sufficient to point to the fact that Mr. Keynes, whose use of the so-called "method of expectations" in the *General Theory* has been regarded as providing one of that book's chief claims to a "revolutionary" character, based a crucial part of the argument of the *Treatise* on a type of assumption that was in direct conflict with a use of the "method of expectations" which has been regarded as one of the simplest axioms of the general "theory of value." See also, on this matter, pp. 278 f., below.

It is true that Mr. Keynes often took account, in his exposition, of the possible "stickiness" of costs: indeed, as we shall see in Volume II of this study, such "stickiness" was essential to the theory of output presented in the *Treatise*. It is significant, however, that he tended to stress instances in which entrepreneurs would be subjected to out-of-pocket losses—in other words, would continue to make payments to the factors of production at the abnormally high level of costs.[63] In such cases, obviously, costs would continue to be resolved into money-incomes of the factors of production in a form available for expenditure against output. Just as obviously, however, in all cases in which production is not accompanied by actual cash disbursements in excess of cash-receipts, "costs" in a sense which would still be strictly relevant to a description of the conditions for "monetary equilibrium" and the scale of social output—i. e., E_x—would still be high, at the same time that, since there would be no "earnings" by the factors of production at this high level of costs, these costs would not be currently converted into payments of money-income (E_e).

It is obvious, therefore, that the critical point in Mr. Keynes's algebraic manipulation involving the transformation of the term E, which on the basis of the expression $PR=E-S$ should be regarded as E_e (that is, as referring to a stream of income in the form of money which is immediately available for expenditure against goods), into the E of a cost-expression, as in $E/O=W_1$ or $E\cdot C/O=I'$, where the E involved is E_x, *will be justified only in those cases in which* $E_e=E_x$, *or, at best, in which the ratio* E_e/E_x *is a constant.* It should be clear, even from the single example given above, that these conditions are of anything but universal occurrence.[64] We are therefore confronted with a first set of

[63] See, for example, the *Treatise,* I, 125. It may be added that, as a matter of realism, it is difficult to believe that even the entrepreneurs who continue to carry on operations at a loss are not able to waive *some* of the payments that are properly to be regarded as "costs," and therefore as part of E_x. One thinks, for example, of dividend payments on preferred stock. Certainly the inability of entrepreneurs to cover the dividend on such stock would be regarded as an indication of an unsatisfactory relationship between costs and selling prices; yet if the preferred dividend is passed, E_e will fall, though E_x does not.

[64] The particular example given—namely, the case of sticky wage-rates at which few workers are employed—is, of course, by no means the only

reasons for arguing that, in contrast with the older Quantity Equations, which were found, when properly stated, to be valid literally under all conditions, the Fundamental Equations of the *Treatise,* when regarded as performing simultaneously the two tasks assigned to them, were literally formally invalid for anything but a very highly special set of conditions which are by no means likely to be found to prevail in all cases.

It may be remarked, in passing, that the reasons thus far adduced against the formal validity of the Fundamental Equations under anything but a highly special set of conditions cannot be dismissed on the ground that these reasons are like those of Meade and Durbin, cited above, in that they represent a criticism, not of the formal validity of the equations themselves, but of some proposition that is related to the Fundamental Equations in the same way as the Quantity Theory is related to our Quantity Equations. It might be argued, for example, that in pointing out that the costs which are relevant to a description of the conditions of monetary equilibrium do not always enter into money incomes, what we are questioning is not the formal validity of the equations themselves, but the validity or usefulness of Mr. Keynes's conditions for "monetary equilibrium"—specifically, his implication that it is sufficient, in order to establish "monetary equilibrium," to have equilibrium between selling prices and those costs which actually enter into money incomes.

That the cases are not parallel, however, should be obvious when it is observed that, whereas it makes no difference to the formal validity of the Fundamental Equations whether these equations are consistent with many levels of output (a consideration which, as we have seen, led to the rejection of the Equations by Meade), it does make a difference in the validity of the Fundamental Equations, when regarded in their two-fold aspect, whether $E_x = E_e$ holds under all conditions, or whether this latter equation holds only under a special set of circumstances. Nor is the difficulty avoided by arguing that this expression does hold under all circumstances if we accept Mr. Keynes's implied definition of what we have called E_x (that is, the total of "costs") as including only those "costs" that are accompanied by payments into incomes. For, obviously, this is merely another way of saying that $E_x = E_e$ will hold, and therefore the Fundamental Equations, in their two-fold aspect, will be formally valid, whenever "costs" are accompanied by payments into incomes, but *not otherwise.*

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instance that could be adduced in which a given item enters into "costs" (E_x) without simultaneously entering into money-incomes available for expenditure upon output (E_e). In this connection, see, for example, Hawtrey, *Capital and Employment* (1937), 96, on "depreciation" as differing from "other costs of production, in that it does not of itself generate incomes."

Just how special the conditions are, however, which were tacitly assumed in the construction of the apparatus of the *Treatise,* can best be seen by passing to a second, and decisive, set of reasons for arguing that the formal validity of the Fundamental Equations, when the latter are regarded as performing simultaneously a two-fold function, was severely limited. This set of reasons has to do with the *time-relationship* existing between the E in equations of the "stream" type and the E of equations of the cost-profit type. From the argument of the *Treatise,* which, as we have seen, was based upon the implication that equations of the type $\Pi = E/O + (I-S)/O$ were to be regarded simultaneously as "stream" equations and as cost-profit equations, one was led to suppose that the cost-item E/O, or W_1, was the cost of production of the goods of which Π was the selling-price. It can be shown, however, that this will be true only under a set of conditions so special in their nature as to limit the application of the Fundamental Equations, in their two-fold aspect, to a vanishingly narrow sphere.

In fairness to Mr. Keynes, it may be pointed out that part of the responsibility for his lapse into the fallacy which is about to be discussed must be laid at the door of those writers who failed to lay great stress upon the importance of keeping straight the time-relationships involved in any association of earnings as "costs" with earnings as incomes available for expenditure. The association itself was certainly not original with Mr. Keynes. On the contrary, it may be regarded almost as an earmark of certain variants of the "income-approach" to the Theory of Prices.[65]

No one, for example, could have been more explicit than Wicksell in arguing that "costs of production and money-incomes are really only two different aspects of the very same thing, and the sum of *each* must be equal to the sum of the prices of all the goods (and services) produced and consumed."[66] Whether or not such an argument is fallacious, however, depends entirely upon whether the particular author concerned reasoned as if the sum of *costs* applied to the *same goods* as those offered for sale against income out of earnings in the period during which the costs are being disbursed. Wicksell, it will be observed, did not so reason. On the contrary, the very fact that he argued that the

[65] See, for example, in addition to the quotations in the text from Wicksell and Hawtrey, J. S. Robertson, *The Income Theory of Prices* (1935), 91. Cf. also A. Aftalion, *Monnaie, Prix et Change* (1927), 174, on PQ ("the value of production") as determining R, or "income" (*revenu*), as well as *vice versa.*

[66] Wicksell, *Interest and Prices,* 45.

sum of money incomes and of "costs of production," respectively, would *necessarily* be "equal to the sum of the prices of all the goods (and services) produced" would seem clearly to indicate that he thought of the relationship between incomes and costs as having reference only to the *source* of "incomes"; the "prices" to which both costs and incomes were "necessarily" equal were, obviously, the *cost*-"prices" of the goods concerned, and not the prices which would be determined as the result of the impact of the disbursement of incomes upon goods offered for sale.[67] On the other hand, Wicksell might have made this point very much more explicit than he actually did.

That the danger of slipping into the fallacy underlying the Fundamental Equations of the *Treatise* is always present unless one bears in mind the crucial matter of the time-relation between the goods for which E_x is the sum of costs and the goods which are sold against E_e is, in fact, illustrated in the variant of the "income approach" presented by Hawtrey in his *Good and Bad Trade*.[68] Hawtrey, like Wicksell, argued that "the total cost of production of all commodities per unit of time is the aggregate of all money incomes."[69] Unlike Wicksell, however, he went on to lay down another series of propositions which can be shown to be clearly incorrect. We may say, indeed, with Haw-

[67] Wicksell's addition of the words "and consumed" is certainly unfortunate, since it would seem to imply that he regarded as of not great importance the question whether the "costs" associated with the disbursement of current income necessarily applied to the goods which were *sold* against income ("consumed") in the same period. Yet it is not clear that anything more was involved than a slip in exposition. See, at any rate, the discussion of Wicksell's treatment of "costs" and "incomes" by E. Lundberg, *Studies in the Theory of Economic Expansion* (1937), 53.

[68] Hawtrey, *Good and Bad Trade*, 6 ff. It should be noted that, so far as I have been able to discover, a similar argument does not appear in Hawtrey's later writings, though it is possible to find an occasional statement with respect to the relation between "costs" and "incomes" that is not strictly accurate. See, for example, *Currency and Credit*, 1st ed., 54 (3d ed., 61): ". . .All incomes may be assumed to arise from production . . . , and all costs of production are someone's income." On the universal validity of such propositions, see the following note.

[69] It will be observed that this statement is not strictly accurate, in view of the fact that the "aggregate of money incomes" will be greater than the "total cost of production" whenever "incomes" are generated by a dole financed out of money or credit created *ad hoc*. This was, in fact, one of the difficulties which forced Mr. Keynes, in order to protect simultaneously the integrity of the expression $PR = E - S$, and the definition of E as "earnings," to fall back upon the tortuous device of regarding such a dole as a case of "negative saving" or as a case of "investment." See Keynes's "Rejoinder" to Robertson, *loc. cit.*, 420. It may be observed, however, that Mr. Hawtrey, who, unlike Mr. Keynes, was not attempting to provide a formulation that would perform the two-fold task indicated above, and for whose argument, therefore, it was not essential that "incomes" be equal to "costs" ("earnings"), did not resort to tortuous devices of the type indicated. On the contrary, he admitted frankly that several of the statements in his exposition, while they might be taken as "true in principle," were nevertheless "subject to many qualifications." See *Good and Bad Trade*, 6 n.

trey, that "if every commodity *sold* in the unit of time is set down at its money value, the total will be the effective demand for all commodities" —this "effective demand," in turn, being related to the "total money income of the community." [70] What we may not say, however, is that "if every income is set down at its money value, the total will be the cost of production of *the same commodities*." [71] The failure to make clear the unsoundness of this latter proposition has undoubtedly been one of the reasons for other regrettable usages in economic discussion—for example, the persistent misuse of the "Law of Markets," in a context that fails to take into account the time-relations involved. It is right, therefore, to criticize those formulations of the Law of Markets which fail to make clear that the sums of money appearing as "costs," on the one hand, and those sums appearing as incomes available for expenditure, on the other, may be "heterochronous," in the sense that the *costs* may apply to goods produced over a different period from that during which these goods are actually offered against expenditure out of the incomes representing *current* costs.[72] It is at least reasonable to suppose that if this type of consideration—which, after all, is merely a special case of what has come to be called in recent years "period-" or "sequence-analysis"—had been advanced with greater emphasis in connection with these earlier problems, its application to the Fundamental Equations might have been more easily seen.

So far as I am aware, the point in question was first applied in print against the Fundamental Equations of the *Treatise* by Hayek, who used it as the basis for his contention that the central assumption on which the two-fold aspect of the Fundamental Equations rested would hold only "in a stationary state"—that is, under stationary conditions—and does not hold in a "dynamic society," despite the fact that "it is exactly for the analysis of a dynamic society that Mr. Keynes constructs his formulae." [73] The point

[70] Hawtrey, *Good and Bad Trade*, 7 f. It should hardly be necessary to point out that "total money-income" will not be *identical* with "effective demand," if we are thinking of money-income *available* for the implementing of effective demand in the next "period." Hawtrey's loose exposition on this head (cf. also *ibid.*, 6) is easily forgiven, however, in the light of his sharp distinction between "income" and "expenditure" (or, as he later called it, "consumers' income" and "consumers' outlay") and his relation of the difference between the two to changes in the size of cash balances. See *ibid.*, 11 ff., and cf. below, pp. 354 ff.

[71] Hawtrey, *Good and Bad Trade*, 7 f. (Italics mine.)

[72] See, for example, O. von Zwiedineck-Südenhorst, "Die Arbeitslosigheit und das Gesetz der zeitlichen Einkommenfolge," *Weltwirtschaftliches Archiv*, XXXIV (1931), 367 ff., 373.

[73] Hayek, "Reflections," Part I, *loc. cit.*, 282 ff. Cf. also the same author's "Rejoinder to Mr. Keynes," in *Economica*, November, 1931, 15. The passage quoted in the text appears on p. 284 of the first article cited.

was later made by other writers, although not always in a form likely to command the attention of any but the most careful readers.[74] It may therefore not be amiss to restate the point with the help of a simple algebraic notation.

What Hayek questioned, specifically, was Keynes's assumption that "the E which was *the cost of production of current output* is the same thing as the E which is earned during the period when this current output comes on to the market and which therefore is *available to buy that current output.*"[75] Reflection will, in fact, demonstrate that there is in reality no reason whatever for assuming that the money outlays from earnings available in any given period for expenditure upon the output offered or sale during that period are equal to the money costs of producing his output. For, strictly speaking, the expression $\Pi = E/O + (I-S)/O$, when the E is interpreted as a cost item, should be written $\Pi = E_{t_n}/O_{t_n} + (I-S)/O_{t_n}$, in which the subscript t_n indicates the particular time-"period" (the nth of n periods of time each of which is equal to t) during which O is produced and the cost-total E is incurred. It is obvious, that is to say, that the E involved is that representing the outlays incurred in the production of O_{t_n}. When, on the other hand, the E in the expression $\Pi = E/O + (I-S)/O$ is interpreted as earnings available for expenditure upon *output offered for sale during the given period* ($O_{s \cdot t_n}$), the expression should be written $\Pi = E_{t_n}/O_{s \cdot t_n} + (I-S)/O_{s \cdot t_n}$. In this case, E_{t_n} represents money-outlays currently in-

[74] See, for example, Hart, "An Examination," *loc. cit.*, 631 n., and Bernstein, *Money and the Economic System*, 267. See also the discussion, in Pigou's *Theory of Unemployment* (1933), 192 f., of the "period of delay between the emergence of embodied services on the market and the associated money payment made to the factors of production providing them," and the discussion by G. Haberler, *Prosperity and Depression* (1937), of the "distinction between the period in which income is earned and that in which it is available to be spent or saved" (p. 212; cf. also p. 198). Neither Pigou nor Haberler, to be sure, referred to the equations of the *Treatise* or to Hayek's criticism of these equations. The equations of the *Treatise* are, however, referred to by Lundberg (*Studies*, 53, n. 2) in connection with the latter's discussion of the relationship between Wicksell's treatment of "costs" and "incomes" and Lundberg's own proposition that the fact that "the costs of producing consumption goods sold during a certain period will have been paid during preceding periods" necessarily "implies a deviation from the static assumption that costs and income are identical" (*op. cit.*, p. 52).

[75] Hayek, "Reflections," Part I, *loc. cit.*, 282. (Italics mine.)

curred in production, *including the production of that part of total current output* (O_{t_n}) *which will be offered for sale only in later periods* (represented by the subscripts t_{n+1}, t_{n+2}, et cetera), if at all; and $O_{s \cdot t_n}$, or "output offered for sale" in the current time-period ($_{t_n}$), includes not only that part of goods produced in the current time-period (O_{t_n}), but also that part of those goods produced in earlier periods ($O_{t_{n-1}}$, $O_{t_{n-2}}$, etc.) which is offered for sale in the current period.[76]

It is obvious that we are here considering (1) some of the difficulties with which Mr. Keynes attempted to deal—though his attempt was to no avail for our present purpose—under the head of the distinction between "available" and "non-available" output, as well as (2) the difficulties suggested by the distinction between "volume of output" and "volume of goods sold," which is discussed in a later chapter.[77] That Mr. Keynes himself, however, was far from recognizing the importance of these considerations for the problem in hand is particularly clear from those passages in the *Treatise* in which, although it was recognized that O and E (when the latter is regarded as a "stream" item, represented by M_1V_1) may change in different degree in a given period, the expression M_1V_1/O continued to be characterized as representing the "rate of efficiency earnings" (that is, unit cost of production) which, when compared with the prices of output currently being sold, determines "profits," and thus the scale of output![78] The identification throughout the *Treatise*, implicitly if not explicitly, of the "flow" item M_1V_1/O with the cost-item "the rate of efficiency earnings," is, indeed, itself proof of Mr. Keynes's failure to recognize the importance of the issue under discussion.[79]

It follows, in terms of the algebraic notation given above, that the equation $\Pi = E/O + (I-S)/O$ may be regarded as representing *simultaneously* the relationship between costs (E/O) and selling prices (Π), on the one hand, and the total of money outlay directed against the purchase of output

[76] The writer chiefly responsible for the introduction of the use of time-subscripts similar to those used in the text, for the purpose of "period analysis," is of course D. H. Robertson. See Robertson's *Banking Policy and the Price-Level* (1926), Appendix to Chapter V (pp. 60 ff.); and cf. the same author's "Saving and Hoarding," *Economic Journal*, XLIII (1933), 401 ff. See also D. Hammarskjöld, "Utkast till en algebraisk metod för dynamisk prisanalys" ("Outline of an Algebraic Method for Dynamic Price Analysis"), *Ekonomisk Tidskrift*, (1932), 160 ff.; and cf. the same author's *Konjunkturspridningen* (Stockholm, 1933), 14 ff.

[77] See below, pp. 538 ff.

[78] See, for example, the *Treatise*, I, 264 f.

[79] For examples of the identification in question, see especially the *Treatise*, I, 264 ff.; cf. also I, 149, 215, 244, and II, 302.

$(E+[I-S])$, on the other, *only* when $E_{t_n}=E_{s\cdot t_n}$, in which $E_{s\cdot t_n}$ represents the costs incurred in the production of the goods included in $O_{s\cdot t_n}$. It should be obvious, however, that $E_{t_n}=W_{t_n}O_{t_n}$, in which W_{t_n} represents unit costs of production in the period during which O_{t_n} is being produced and E_{t_n} is incurred. It is equally obvious, on the other hand, that $E_{s\cdot t_n}=W_{s\cdot t_n}O_{s\cdot t_n}$, in which $W_{s\cdot t_n}$ represents unit costs of production of the goods included in $O_{s\cdot t_n}$. As we have seen, $O_{s\cdot t_n}$ is really made up not only of that part of O_{t_n} which is offered for sale during the period t_n, but also of those parts of $O_{t_{n-1}}$, $O_{t_{n-2}}$, and so on, which are offered for sale during the period t_n. It follows, therefore, that the dual implications of the Fundamental Equations hold simultaneously only when $W_{t_n}\ O_{t_n}=W_{s\cdot t_n}O_{s\cdot t_n}$: that is, either when there is no change in unit costs or in output over the periods (t_{n-2}, t_{n-1}, t_n, t_{n+1}, t_{n+2}, et cetera) during which the goods included in O_s are produced and sold, respectively, or when whatever changes in unit costs (W_{t_n}, $W_{t_{n-1}}$, $W_{t_{n-2}}$, et cetera) do occur are exactly balanced by compensating changes in the volume of output (O_{t_n}, $O_{t_{n-1}}$, $O_{t_{n-2}}$, and so on).

It is of some importance to observe that this attack upon the formal validity of the Fundamental Equations is directed solely against the step by which Keynes attempted to pass from the E of the "stream" equation $PR=E-S$ to the E of the cost-item E/O, or W_1. It is, in other words, directed solely against the interpretation of the Fundamental Equations as performing simultaneously the two functions described at the outset of this section. It is perfectly true, that is to say, that the equation $P=W_1+(I'-S)/R$, with its cost-item W_1, may be derived directly, without reference to the "stream" equation $PR=E-S$. It is of the first importance to note, however, that, in such a case, we *are* introducing the "cost" item W_1 directly, so that the equation merely states that the price of the goods included in R is made up of two items: its cost of production, W_1, and a balancing term, $(I'-S)/R$, which has no significance except as the difference between cost and selling price. The fundamental equations of the *Treatise* were indeed characterized —first by Hayek, and then, more emphatically, by Hawtrey

—as saying no more than this.[80] There can be little doubt, on the other hand, that in Keynes's own understanding the great virtue of the equations was that they not only stated this truism but combined its statement with a summary of the forces determining the general level of prices. It is precisely this virtue that the argument developed above is concerned to deny.

Similarly, it is possible, if one wishes, to regard the equation $\Pi = E/O + (I-S)/O$ as a statement of the forces determining the general level of prices (Π), by conceiving of the second member of the equation as representing the stream of money against output—this stream being made up of expenditures out of earnings (E) and of all other expenditures ($I-S$).[81] (A negative value for ($I-S$) would of course mean that not all of income out of earnings available for expenditure (E) was actually spent upon output.) The important thing to be observed, however, is that it is precisely the inclusion of cost-items, such as the I' of the first Fundamental Equation and the $W_1(=E/O)$ of both equations, in combination with Keynes's repeated translation of the term E/O into the "flow" item M_1V_1/O, *even when* E/O *was defined as the "rate of efficiency earnings,"* which show that Keynes wished his Equations to be thought of as accomplishing simultaneously that two-fold purpose which, as we have seen, they cannot be regarded as having accomplished.

It is of importance, also, that the reason for the failure of the Fundamental Equations to accomplish their dual purpose should be judged in the light of Keynes's claim that

[80] Cf. Hayek's "Rejoinder to Mr. Keynes," *loc. cit.* (1931), 16, and Hawtrey, *The Art of Central Banking* (1932), 336 ff.

[81] Some difficulties, to be sure, are involved in this interpretation of the equation $\Pi = E/O + (I-S)/O$ because of the differences that necessarily exist between the "volume of output" (O), on the one hand, and the "volume of goods sold," on the other. Cf. what is said, on this matter, on pp. 538 ff., below. There is, however, much less difficulty in regarding the equation $\Pi = E/O + (I-S)/O$ as capable of representing a "stream" equation than there is in the case of the equation $P = E/O + (I'-S)/R$, both because of the presence, in the latter equation, of I', which is specifically a cost-item, and the uncertainty attaching to the meaning of E/O, when the equation is interpreted as representing a "flow" of money against *consumers'* goods (R), rather than against output as a whole (O). Strangely enough, however, it is the equation $P = E/O + (I'-S)\ R$ that has been more often characterized as being essentially Fisherine in form. See, for example, the references to Robertson and Hart, on p. 125, n. 59, above.

those equations were "dynamical" in character. In the first place, if, by "statical" equations, we mean equations which assume the constancy of relations which are given no specific notation in the equations themselves, it must now be obvious that insofar as the equations of the *Treatise* purported to present simultaneously both an apparatus for dealing with the impact of a stream of money upon a stream of goods and an apparatus for displaying the factors making for a discrepancy between costs and selling prices, these equations involved "statical" assumptions in a degree never chargeable against the older "quantity equations."

If, on the other hand, by "dynamical" equations we mean equations that enable us to study the forces making for monetary disequilibrium—in this instance, a disequilibrium between "costs" and "selling prices" *resulting from changes in the breadth of the stream of money outlay directed against output*—it must be obvious that the equations of the *Treatise* failed by reason of the fact that, except under highly special conditions, they are not even formally valid unless we reduce them *either*, first, to the form of virtual identities telling us no more than that the price of a commodity is resolvable into its cost plus a balancing term representing the difference between cost and selling price *or*, second, to expressions designed merely to represent the flow of money against output, without reference to a possible difference between the cost and the selling price of this output. The usefulness of the apparatus presented in the *Treatise* generally, when regarded as an alternative to the received apparatus for dealing with the forces determining the magnitude of these "flows"—that is, the older "quantity equations," and the body of analysis for which they stand—can be tested only after we shall have examined, in the following chapters, certain of the more crucial parts of the body of analysis in question.

Similarly, the usefulness of the apparatus presented in the *Treatise* as a method for dealing with the nature of the forces leading to a price-cost disequilibrium and therefore to changes in output can be tested only after we shall have examined, in Volume II of this study, the received methods for dealing with this type of problem. It is sufficient here

to emphasize the conclusion that, as far as the Equations of the *Treatise* themselves are concerned, they emerge from the ordeal of a critical examination designed to test their *validity* under "dynamic" conditions—which, as we have seen, is a necessary preliminary to a discussion of their *usefulness* for the analysis of such conditions—in a vastly more battered condition than do the "quantity equations" that they were designed to supersede.

The conclusion is particularly striking in view of the type of comment that certain of Mr. Keynes's more enthusiastic disciples have permitted themselves with respect to other segments of received doctrine. The traditional "theory devoted to the explanation of unemployment," we have been informed, was "erected upon the assumption that unemployment cannot exist." [82] It is hardly out of place, surely, to point out that the equations of Mr. Keynes's *Treatise,* which were supposed to supersede the received apparatus for dealing with the nature and processes of *change,* were literally based upon the assumption that no change can take place in variables that are of crucial importance for the argument in hand. The episode provides its own commentary.

V

Epilogue

It is worth adding, as a final word, and by way of further justification of our extended examination of the claims of the Equations of the *Treatise* to a peculiar fitness for "dynamic" analysis, that, to my knowledge, Mr. Keynes has nowhere—neither in his *General Theory* nor in any of his other writings subsequent to the *Treatise*—included among his reasons for abandoning the equations of the *Treatise* the difficulties examined above. He has, to be sure, declared that these equations "were an instantaneous picture taken on the assumption of a given output." [83] It also happens that Pro-

[82] So Joan Robinson in the *Economic Journal,* XLIV (1936), 298. The absurdity of this generalization on its own account will be demonstrated in the course of our appraisal, in Volume II, of the substance of received doctrine on the subject of the effect of money upon Output.

[83] See the *General Theory,* p. vii.

fessor Hayek, in stating his objection to assuming (in the notation suggested above) that E_{t_n} is necessarily equal to $E_{s \cdot t_n}$ used a terminology which involved not only a contrast between a "stationary state" and a "dynamic society," but also a proposition to the effect that the relationship $E_{t_n} = E_{s \cdot t_n}$ will hold true only at a "moment of time."[84] The unwary reader might therefore suppose that Keynes's "admission" that the equations of the *Treatise* represented only "an instantaneous picture" amounted to a confession that Hayek's denial of the formal validity of the equations under "dynamic" conditions was well-founded. An examination of the context in which the passage under discussion appears in the *General Theory* shows quite clearly, however, that Mr. Keynes, so far from intending any such confession, merely meant to contrast the emphasis in the *Treatise* on a "profit disequilibrium" as a factor causing a change in the "level of output" with the type of factor affecting the "level of output"—in particular, the element of "demand"—that bulks so large in the *General Theory*. The "confession," in other words, had to do with the incompleteness of the argument of the *Treatise* when judged as a theory of output. If the equations of the *Treatise* themselves were involved, it was not because they were admitted to be formally invalid under "dynamic" conditions, but merely because their emphasis upon the importance of a "profit disequilibrium" bulked too large as compared with the type of factor affecting output which Mr. Keynes now wishes to stress.

The point is of some importance for the rest of our study; for it will appear, from the argument of Volume II, that, even though it is impossible to accept the method suggested in the *Treatise* for dealing with a profit-disequilibrium (that is, a discrepancy between "costs" and "selling prices") as a factor affecting the level of output, there is not the slightest doubt that emphasis on the consequences of discrepancies between "costs" and "selling prices" must remain an integral part of any satisfactory theory of the effect of money upon output. In this respect, the *emphasis* of the argument presented in the *Treatise* must be regarded as a correct one, to be defended, like the emphasis upon the importance for the

[84] See Hayek, "Reflections," I, *loc. cit.*, 282, 284.

Theory of Prices of a discrepancy between Savings and Investment, even against Mr. Keynes's later self. It is precisely because of this necessity for rescuing the emphasis of the *Treatise*—for demonstrating, in other words, that the quarrel with the *Treatise* must be not with what it tried to do, but with the way it tried to do it—that it would have been necessary to examine the claims to a "dynamical" character advanced by Keynes in behalf of his Fundamental Equations at the time the *Treatise* was published, even if such an examination had not been called for by reason of the ironical light in which it places Keynes's rejection of the older Quantity Equations on the ground that they were "statical" in character.

PART TWO

Behind the "Quantity Equations"

CHAPTER SIX

General Considerations; the Quantity of Money of Ultimate Redemption

I

Plan of the Argument

In Chapter IV of this study, it was argued that while the older "quantity equations" were designed to indicate the general nature of the variables whose operation can be said to influence prices, it is a gross libel on the substance of received monetary theory to suggest that the latter is nothing more than a list of such variables. It was argued, on the contrary, that "received tradition" with respect to the Theory of Prices is represented, not by the "quantity equations" alone, but by the vast body of doctrine lying behind these equations—a body of doctrine intended to describe the nature of the forces that give each of the variables in the equations the magnitudes they actually have, and so make prices what they are. It was further argued that a study of the forces leading to a change in the magnitude of the variables of the older equations necessarily includes a study of the *processes* of change, since a "process" is obviously nothing more than a series of changes occurring in a specific sequence—a "rational filiation in the succession of events," as Comte put it.[1]

The present chapter, like those that follow, is designed, first of all, to demonstrate these propositions by conveying some notion of the complex richness of the doctrine lying "behind" the Quantity Equations.[2] It is obvious, however,

[1] Cf. Cairnes, "M. Comte and Political Economy," *loc. cit.*, 302. The passage quoted by Cairnes appears on p. 225 of Volume IV of the fifth (1893) edition of Comte's *Cours de Philosophie Positive*.

[2] This procedure is itself in strict accordance with "tradition." It was, for example, the procedure followed by Fisher in the theoretical parts of his *Purchasing Power of Money* (for example, Chapters III–X, inclusive);

that the presentation of the substance of a body of doctrine so rich and so complex must proceed upon the basis of some principle of selection. In the present instance, the principle of selection is indicated by the secondary purpose of this study: namely, the defence of the substance of received doctrine upon the subject of the Theory of Prices against the strictures of Mr. Keynes. Generally speaking, only those portions of the received body of doctrine which Mr. Keynes has subjected to a determined attack will concern us here.[3] Despite this limitation of the sphere of inquiry, however, it is hoped that it will be possible to accomplish a third purpose: namely, a demonstration of the way in which the body of received doctrine has grown up in the past, as an indication of the way in which it may be expected to grow in the future, provided that we do not allow ourselves to be deflected by extravagant claims for specific suggestions alleged to be of so completely "revolutionary" a character as to warrant a complete break with everything that has gone before. For this third purpose, the suggestions made by Mr. Keynes himself provide as good material for discussion as any others, since they may be subjected to the three tests which can be regarded as crucial for the purpose in hand: first, a test of their claims to "novelty," in the light of the actual substance of received tradition; second (in all cases in which their claims to "novelty" are not without foundation), a test both as to the soundness of the new devices and as to their supposed superiority over received devices for handling a traditional type of problem; and third, a test as to whether acceptance of the devices in question means an abandonment of everything that has gone before, or represents merely a supplement to received tradition of the kind that has made

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and it has been the procedure of those who have built their analysis about either an equation of the Fisherine type (for example, H. Neisser, *Der Tauschwert des Geldes* [1928]), or an equation of the "cash-balance" type (see, for example, Pigou, *Essays in Applied Economics,* 180 ff.).

[3] It is hoped that the exceptions to this rule of selection will commend themselves to most readers. It should hardly be necessary to add that it is impossible, within the limits of the present study, to present anything resembling a complete treatment of the issues involved even in the parts of "received doctrine" thus selected for discussion. Cf., in this connection, what is said, for example, with respect to the concept of a "natural rate of interest" on p. 177, n. 58, below, and with respect to "velocity of circulation" on p. 290, n. 1, below.

the tradition received by one generation a richer and more adequate one than the tradition that was "received" by its predecessor.

II

A Simple Quantity Equation and its Justification

It must be obvious, however, that if the argument of the preceding chapters is sound, the whole of the discussion must be built about the skeleton represented by the received "quantity equations." For purposes of the present discussion, we may begin with what is probably the most familiar —it is certainly the oldest—of the "quantity equations": namely, the so-called "Fisherine" equation $MV=PT$, or $MV+M'V'=PT$, or, as the latter equation might preferably be written, $(M+M')V=(PT)$, in which $M'=cM_r$ (c being a coefficient expressing the ratio between the amount of money substitutes serving as cash-balances (M') and that part of the stock of money of ultimate redemption which serves as banking and currency reserves (M_r), while M, without subscript, is that part of the stock of money of ultimate redemption which is kept in the form of cash-balances).[4] By those who see no objection to substituting the preferred formulation $(M+M')V=(PT)$ for the more commonly used $MV=PT$ or $MV+M'V'=PT$, the rest of this chapter may be omitted. For others, however, it may be advisable to state, at least in outline, the reasons for preferring the equation $(M+M')V=(PT)$, with the definitions indicated, to the more commonly used formulations.

The preferred notation may be characterized, in general terms, as the result of a desire to honor the double criterion that the notation for an expression designed to represent the total stream of money against the total of objects offered for

[4] (PT) is here taken to mean, as is commonly the case, the "volume of transactions," as expressed in money. The term is enclosed in parentheses merely by way of avoiding, at this stage of the argument, (1) the range of questions associated with those "transactions" that have been alleged to be incapable of being resolved into a physical volume of transactions times a "specifiable price," and (2) the problems associated with the concept of a "plurality of price-levels." On the former problem, see, in addition to p. 57, above, pp. 572 ff., below; and on the latter problem, see Chapter XVII, below.

sale—we are not yet concerned with the complications introduced when we take account of the "income" variants of the equation, different types of cash-balance, a "plurality" of "price-levels," the peculiarities of "security-transactions," and so on—should be sufficiently elaborate to do justice to the more important variables involved, but not so elaborate as to include details that are either of minor economic significance or are best dealt with separately.

There is no doubt, for example, that the equation $MV=PT$, when M represents the total of cash-balances of all kinds, is adequate for many purposes. It is not sufficiently elaborate, however, to call attention to the difference between the forces affecting the amount of cash-balances in the form of money of ultimate redemption (the M of our equation) and those affecting the amount of cash balances in the form of money-substitutes (M')—the two, in the equation $MV=PT$, being lumped together under M. Yet the problems associated with the magnitude of M', in particular, are of such crucial importance that it must be regarded as a merit of those forms of the "equation of exchange," including the early equation of Lubbock, which have given a specific notation to the term in question, that they should have done so.

It will be recalled that Lubbock included in his equation not only a term (nC) corresponding roughly to the MV of Fisher's equation of exchange, but also terms (lD and mB) representing money-substitutes in the form of bank-deposits (D) and bills of exchange when they are used as money (B), multiplied by their respective "velocities of circulation" (l and m).[5] Similarly, Levasseur included, in addition to a term M, representing "the quantity of precious metals," a term C_R, representing "credit in all its forms."[6] Walras, in both his first (Fisherine) and second ("cash-balance") formulations of the "quantity equation," included a term F, representing the "fiduciary circulation," as well as a term representing the "metallic circulation."[7] Newcomb, likewise, writing F for the "total flow of the currency," which was taken to be equal to the "volume of currency" multiplied by its "rapidity of circulation," proceeded to write also $F = F' + F''$, in which F' represented "the portion of F due to the circulation of material money" and F'' the

[5] Cf. above, p. 11.
[6] Levasseur, *La Question de l'Or*, 150.
[7] Cf. my "Leon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," *loc. cit.*, 579, 583 ff.

"sum total of payments by cheque." [8] The later examples of this type of usage are too well known to warrant citation.

On the other hand, it is questionable whether, for the central problems involved in the Theory of Prices, much is gained by following the example of those who have thought it necessary to give a specific notation for every form of currency in use—to the point, for example, of giving a special term for silver dollars and another for silver certificates.[9] It will be observed, at the same time, that there is nothing in the procedure here proposed that would prevent the subsequent break-up of any one of the terms included in our preferred notation whenever such a break-up appears advisable for a better handling of detailed problems. There is nothing, for example, to prevent our writing $M=M_1+M_2+ \ldots M_n$, in which the subscripts $1, 2 \ldots n$ refer to various types of money of ultimate redemption, when more than one type is used; just as there is nothing to prevent our writing $M'=M'_1+M'_2+ \ldots M'_n$, in which the subscripts refer to various types of money-substitutes, such as (1) bank-deposits, (2) bank-notes, (3) bills of exchange used as money, and so on.[10] All that is here suggested is that, for purposes such as those which will occupy us in the following pages, it will be sufficient to differentiate between M and M'.

[8] Newcomb, *Principles*, 321 ff.

[9] See, for example, Norton, *Statistical Studies*, 4 ff. This, of course, is an extreme example (cf. also p. 148, n. 11, below). It is perfectly possible that, under certain conditions, it may be important that the facts with respect to the "velocities of circulation" attaching to the various *forms* of currency be known by those responsible for monetary *policy*, and therefore anxious to know whether a given expansion of means of payment in various forms represents an "inflation" as compared with the situation previously prevailing. Cf. in this connection, J. Viner, *Studies in the Theory of International Trade*, 130 ff., 248 ff. There are cogent reasons, however, deriving directly from the methodological implications that may be said to underlie the "cash-balance approach," for arguing that the break-up of cash-balances into such segments as those represented by "consumers' balances," "traders' balances," and so on, is, under most conditions—particularly when no one type of currency is singled out for distrust by the community—very much more significant than is a break-up on the basis of the type of *currency* composing each type of cash-balance.

[10] For an example of a break-up of M' in this manner, cf. the reference to Lubbock, above; and for an example of the break-up of M, see the reference to Norton in the preceding note. It goes without saying, obviously, that both M and M' could be still further broken up for the purpose of dealing with special problems. See, for example, what is said with respect to the breakup of $(M + M')$ into consumers' balances, traders' balances, and so forth, on pp. 404 ff. and 412, below. An attempt has even been made,

In this respect, the preferred notation is essentially that of Fisher. There are, however, other differences. It will be observed, in the first place, that instead of writing $MV+M'V'$ we have written $(M+M')V$. This, obviously, is less elaborate than Fisher's notation, as it is less elaborate than that of Lubbock or of any of those writers who have insisted that the notation of the equation be such as to assign a special "velocity of circulation" to each type of currency in use.[11] The chief argument for the preferred notation with respect to "velocity," however, is not that it is simpler; the chief argument is that the preferred notation avoids the implication that, in most situations, any great amount of significance attaches to differing "velocities" of various types of "currency," as compared with the significance attaching to different types of cash-balance, such as "consumers' balances," "traders' balances," and so on. For purposes of preliminary statement, therefore, it is much sounder to avoid such an implication by the use of a single term for "velocity" which is applied to the total of cash-balances, even though the latter may be composed of various forms of currency.

It will be noted, in the second place, that the M, in our preferred notation, is defined as "that part of the stock of money of ultimate redemption which is kept in the form of cash-balances." In Fisher's notation, M was the "amount of money in circulation."[12] If, as seems to have been the case with Fisher, by "in circulation" is meant the same thing as "held as cash-balances outside of banks," the only important claim that may be advanced for the preferred formulation is that it is less likely to lead to the type of error

on occasion, to subdivide cash-balances on the basis of the type of banking operation that gave rise to the media of exchange making up the cash-balances. See, for example, M. Fanno, "Die reine Theorie des Geldmarktes" in *Beiträge zur Geldtheorie,* edited by Hayek (1933), 11, 17.

[11] For a particularly extreme example of this practice, see Norton, *Statistical Studies,* 5, where no less than six specific terms for "velocity" are introduced to correspond to the six varieties of "media of exchange" to which Norton had give specific notation. It is true that Norton also introduced a single term V to represent "the weighted average velocity of the whole stock of money" (p. 6); but it is also true that he felt that "investigations to determine the velocities of different kinds of money . . . could hardly fail to have extremely fruitful results" (*loc. cit.*). Cf. what is said on this matter above, p. 147, n. 9.

[12] *The Purchasing Power of Money,* 24.

which is often involved in the drawing of distinctions between money "in circulation" and money not "in circulation." [13] On the other hand, there is a genuine difference in substance between *M* when it is defined as including only money of ultimate redemption and when it is defined as "money" in Fisher's sense of the term; for the latter includes not only money of ultimate redemption, but also such items as bank-notes.

Fisher's "money," like that of Kemmerer in his *Money and Credit Instruments in Relation to General Prices*, included "fiduciary money" —of which bank-notes were part—as well as "primary money." [14] It should be noted also, in passing, that Fisher's distinction between "primary money" and "fiduciary money" is not the same as our distinction between "money of ultimate redemption" and "money-substitutes." "Money is called 'primary,'" according to Fisher, "if it is a commodity which has just as much value in some use other than money as it has in monetary use." [15] Our "money of ultimate redemption," on the other hand, might be irredeemable paper, and even, within limits, certain forms of token currency, which Fisher would include under "fiduciary money" along with bank-notes.[16] It will be observed also that our "money of ultimate redemption" is not the same thing as Pigou's "legal tender money," or Keynes's virtual equivalent thereof—"Money Proper" or "State Money." [17] It is hardly necessary to state in detail the reasons for preferring the category "money of ultimate redemption" to its various alternatives; it should be sufficient, without raising

[13] On the type of error referred to, see below, pp. 459 ff. There are, to be sure, other differences between Fisher's "money in circulation" and our *M*, apart from the fact, discussed in the text, that Fisher's *M* includes such forms of "fiduciary money" as bank-notes, whereas our *M* does not. Fisher's *M* excluded from "money in circulation," for example, not only money "in banks" but also money in the "United States government's vaults." Insofar, obviously, as the latter included not only currency reserves, but also—as was often true prior to the establishment of the Federal Reserve System—the current cash-balance of the government, this latter part of the money in the "United States government's vaults" would be included in our *M*. In the case, moreover—admittedly a rare one—in which the statistics for money "in the banks" included money earmarked for the use of the depositor and therefore not part of bank-reserves, such money would be included in our *M*, though it would not be included in Fisher's "money in circulation." There is, at any rate, no ambiguity whatever in the conception of *M* and *M'* as representing, when taken together, the "total of cash-balances." It is the equivalent of Hawtrey's "unspent margin." Cf. also E. Lindahl, *Om förhållandet mellan penningmängd och prisnivå*, 4.

[14] Fisher, *Purchasing Power of Money*, 11 ff.; Kemmerer, *Money and Credit Instruments*, 94. Cf. also p. 152, n. 26, below.

[15] Fisher, *Purchasing Power of Money*, 11.

[16] *Ibid.*, 12.

[17] Pigou, *Essays in Applied Economics*, 175 ff.; Keynes, *Treatise*, I, 5 (though see also the last sentence on p. 6 of the same work).

the question whether the importance of the legal tender quality has not been greatly overstressed (particularly by English writers), to point to the fact that what matters, for purposes of dealing with the forces determining the magnitude of M', is the amount of currency capable of serving as ultimate banking and currency reserves (our "money of ultimate redemption") rather than the amount of money capable of discharging debts, although in practice the two may be, even if they have by no means always been, identical.

The reason for confining the M of our formulation to cash-balances held in the form of "money of ultimate redemption" should be obvious as soon as one considers the arguments advanced, say, by Fisher, with respect to the nature of the forces determining the relation between his M and his M', on the one hand, and the relation between his M' and what we have called M_r (that is, the amount of money of ultimate redemption kept as banking and currency reserves) on the other. It was unfortunately characteristic of Fisher's exposition that he should have tended to argue that the type of factor determining the first relation was precisely the same as the type of factor determining the second.[18] At the very least, this involved the assumption that the ratio M/M_r tended to be constant; yet, obviously, this assumption is anything but in the nature of a self-evident proposition.

Curiously enough, at one point in the *Treatise* Mr. Keynes also was prepared to argue that the "actual facts" warranted the assumption of a constancy in the ratio M/M_r, or, as he put it, in the proportions "in which the total stock of State-Money is held by the Public, the Member Banks, and the Central Bank"—despite the circumstance that the "actual facts" which he presented at another point in the *Treatise* do not seem to support this conclusion.[19] Fisher, on the other hand, was prepared, on at least one occasion, to provide a logical argument for supposing that the relationship M/M_r could be expected to be constant, instead of merely assuming that it would be constant, as he did when he adduced, in support of the contention that the ratio M/M_r would be expected to be constant (apart, of course, from "transition periods"),

[18] See, for example, *The Purchasing Power of Money*, p. 50, where, in an attempt to demonstrate that "the quantity of circulating credit, M', tends to hold a definite relation to M, the quantity of *money in circulation*" (italics mine), Fisher pointed not only to the fact that "individuals, firms and corporations preserve more or less definite ratios . . . between their money and deposit balances," but also to the fact that "bank reserves are kept in a more or less definite ratio to bank deposits."

[19] Cf. the *Treatise*, I, 31, and II, 51 f.

the fact that "bank reserves are kept in a more or less definite ratio to bank deposits."[20] His argument, however, was that the "fixity" of the ratio M/M_r followed from a previous attempt to demonstrate that the ratios M'/M and M'/M_r both tended to be constant.[21] This in itself would justify the provision of a separate notation for that part of the "quantity of money" which serves as bank-reserves; and indeed at one point Fisher provided such a notation—namely, μ, which corresponds, *mutatis mutandis*, to our M_r.[22] It is worth noting also that Kemmerer, who, like Fisher, was desirous of simplifying the reasoning by assuming constancy in the ratio M/M_r, had likewise found it necessary to introduce a special notation to refer to M_r. In his case, our M_r appeared as T/X, in which T represented "the amount of money in circulation [*sic!*] inclusive of bank-reserves" (in other words, the $M + M_r$ of our notation) and X was a figure such that T/X represented "the proportion of T [read: the amount of 'money'] represented by bank-reserves." In other words, X would be equal, in our notation, to $(M + M_r)/M_r$.[23]

Moreover, for many of the most difficult of the problems involved in a statement of the forces determining the magnitude of the variables which in turn determine prices, there can be little question that the ratio M'/M_r is of far more nearly fundamental importance than the ratio M/M_r. For, in effect, the former ratio may be said to summarize the whole of received theory with respect to the effect of banking operations upon prices. In view of the fact that the total stock of money of ultimate redemption is equal to $M+M_r$, and in view of the further fact that an adequate account of the forces determining the ratio M'/M_r would deal not only with the forces which work upon the ratio by increasing M', but also with the forces which work upon it by decreasing M_r, such an account would automatically include an account of the forces determining the relationship between M_r and M.

The reader is reminded that throughout this study our primary concern is with the Theory of Prices under a "closed system." Under such a system, it would be literally true that any decrease in M_r would be reflected in an increase in M, and vice versa—as in the case, for example, of an "internal drain" involving the hoarding of money of ultimate redemption, and the reversal of such a drain.[24] The extension of the

[20] Cf. above, p. 150, n. 18.
[21] Fisher, *Purchasing Power of Money*, 52.
[22] *Ibid.*, 162.
[23] Cf. Kemmerer, *Money and Credit Instruments*, 89 n.
[24] Cf., in this connection, p. 159, n. 2, below.

field of investigation beyond the limits of a "closed system" would, of course, make it necessary to take account of the "external drain," as a result of which M_r might decline without a corresponding increase in M resulting. So long, however, as our total stock of money of ultimate redemption, which we may represent by M_q, is defined as the total stock of such money in a given country, then, since $M_q = M + M_r$, it would still be true that a study of the forces determining the magnitude of M_r would automatically include a study of the forces determining the relationship between M and M_r, inasmuch as the only new fact introduced into the situation would be a change in the total M_q.[25]

When, finally, it is pointed out that bank-notes, for example, which Fisher included in his M, are, particularly in countries with an "elastic" note issue, much more reasonably included (along with items such as bank-deposits) in the M' which is built upon the substructure M_r, it will be seen that the proposed change in notation from $MV+M'V'=PT$ to $(M+M')V=PT$, with the changes in definition that are involved, is something more than a minor formalistic detail. For, by virtue of its redefinition of M and M', the proposed reformulation makes it possible to deal with the issues involved in the determination of the ratio M'/M_r at once more inclusively and with more precision than it is possible to deal with the relationship between M' and M, as Fisher defined the terms.[26] It is, indeed, no accident that, from the time of Lubbock, writers have suggested a formulation which, by introducing a term equivalent to our c, would state the problem of the relation between "money" and "money-substitutes" as one concerning primarily the ratio M'/M_r (the equivalent of our c), rather than the ratio M'/M.

Lubbock wrote $C = f + g + D/k$, in which C represented "the money in the country," f "the bank deposits [read: bank notes] *out*,"

[25] For a somewhat more elaborate version of the formulation $M_q = M + M_r$, see Fanno, "Die reine Theorie des Geldmarktes," *loc. cit.*, 10.

[26] One wonders, indeed, whether a usage similar to that suggested would not have been followed by Fisher—and by Kemmerer as well (cf. above, p. 149, n. 14)—if their work had been done after, instead of before, the establishment of the Federal Reserve System, which replaced the "inelastic" note-issue of the National Banking system with a type of currency the relation of which to bank-reserves (in this case the reserves of the Federal Reserve banks) was determined by much the same type of legal regulation as that which controlled the relation of bank-deposits to bank reserves (in this case, of course, the relation of the deposits of the Federal Reserve Banks to their reserves).

g "the coin in circulation," D "the *deposits* in the hands of the Bank of England, and of all other bankers," and D/k "the quantity of coin and bullion held by the Bank of England and by all other bankers."[27] If, for the present purpose, we identify Lubbock's D with our M', it follows that his D/k is equal to our M_r. When the expression $D/k = M_r$ is written in the form $M'/k = M_r$, and when it is remembered that $M' = cM_r$, it becomes obvious that Lubbock's k is the equivalent of our c. Though Lubbock's exposition is not without its faults—as, for example, when he applied his term respresenting "velocity of circulation" not only to the f and g of his formulation, but also to D/k (that is, to our M_r)—it is the more noteworthy because of the fact that a number of the later writers who introduced a term representing the ratio of money-substitutes to "money," identified the latter, not with the amount of money serving as reserves (M_r), but with either the total stock of money ($M + M_r$) or with "money in circulation" (M). This was true, for example, of Walras, whose f represented the ratio of the "fiduciary circulation" to the "quantity of metallic money."[28] It is true also of Roos, whose γ represents the relationship of "bank-credit" to "currency," the latter being identified with "money in circulation."[29]

The superior example of Lubbock was followed, on the other hand, by Norton, whose K, in the expression $D = KR$, becomes the equivalent of our c when his D ("deposits") is identified with our M', since his R represents money in bank reserves (our M_r); it was followed by Fanno, whose π represents the ratio of bank-notes to *that part* of the total stock of money of ultimate redemption which acts as reserves for the bank of issue, while π_1 represents the ratio of bank-deposits to the currency-reserves of the banks of deposit; and it was followed also by Pigou, whose h, defined as "the proportion of actual legal tender . . . that bankers choose to keep against the notes and balances held by their customers" is obviously the virtual equivalent of our c.[30] It is worth noting, finally, that the practice under discussion was followed by Keynes himself in his *Monetary Reform*, where the r in the expression $n = p(k + rk')$ was defined as "the proportion . . . of their potential liabilities (k') to the public" which "the banks keep in cash."[31]

It should hardly be necessary to add, as a final word, that there is nothing in all this discussion which argues for a moment either against the use of equations of the general Fisherine form—of which, after all, the proposed formulation is but a variant—or for the suggestion that any one of the variants of the so-called "Fisherine" equation is the "right"

[27] Lubbock, *On Currency*, 24.
[28] See my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.*, 585.
[29] Cf. Roos, *Dynamic Economics*, 233, 235.
[30] Norton, *Statistical Studies*, 8 ff.; Fanno, "Die reine Theorie des Geldmarktes," *loc. cit.*, 6 f.; Pigou, *Essays in Applied Economics*, 180.
[31] Keynes, *Monetary Reform*, 84 f.

form, and all others are "wrong." As in all shorthand formulations of this type, the test of superiority must be held to lie in the greater or less degree of usefulness that will be found in practice to attach to one formulation as compared with others equally "possible." No further claim is made for the proposed rewriting of the Fisherine equation than that it seems to combine, on the one hand, a maximum of final simplicity and a minimum of rewriting of the most widely known form of the equation, with, on the other hand, the degree of articulation that is desirable if the equation is to be used as a skeleton for the discussion which follows.

III

The Quantity of Money of Ultimate Redemption

The substance of received analysis with respect to the forces determining the magnitude of the stock of money of ultimate redemption ($M+M_r$) is so generally accepted that it may be passed over here with little comment.[32] The notion of cost of production in relation to mint and market price as the element which, under modern conditions, determines the supply of the standard metal; the analysis of the elements determining this cost of production; the effect of the industrial demand for the money metal upon the division of the supply between the monetary and the arts uses; the "compensatory" process set up when there is a divergence between mint and market ratios, under systems in which more than one standard metal is used, as in the case of bimetallism; "Gresham's law," in all its varied applications; the theory of the international distribution of the money metals and the "external drain," in relation to the theory of international prices, international demand, and capital movements: all these were commonplaces in the monetary theory of the nineteenth century, which in turn had in-

[32] The literature on the subject is so vast that little would be gained by attempting to single out any one set of references. Attention may be called, however, to the convenient summaries of the more important elements of received doctrine in Fisher, *Purchasing Power of Money*, Chaps. VI and VII; Pigou, *Essays in Applied Economics*, pp. 189 f.; and Neisser, *Der Tauschwert des Geldes*, Chap. III.

herited a number of elements from the eighteenth century and earlier.[33] Much the same thing could be said of the theory of the forces determining the amount of $M + M_r$ when the latter sum, though it still measures the total stock of money of ultimate redemption, takes some form other than that which Mr. Keynes has called "Commodity Money."[34] Here, for example, one would put the theory of the limits set upon that part of the stock of money of ultimate redemption, under a metallic standard, which is not simultaneously the standard metal (that is, the metal bought and sold by the government freely at a fixed price), as in the case of the so-called "limping standard" and certain forms of subsidiary coinage; the theory of seigniorage; the familiar propositions respecting the relation of the condition of the budget to "currency inflation"; the various possible mechanisms of "currency inflation"; the avowed or tacit transformation of what was originally a form of what Mr. Keynes has called "Bank Money" into the equivalent of what he calls "State Money," and so on.[35] These are matters of such common acceptance that their very mention, which can arouse in trained economists only the feeling of boredom that a trained musician must feel when forced to listen to a ponderous exposition of the first principles of harmony, is to be justified only because an enumeration of them serves to remind us of what we, as well as uninstructed

[33] It goes without saying that the statement that the more important elements in received doctrine on the points enumerated "were commonplaces in the monetary theory of the nineteenth century" is not to be taken as suggesting that they represent a closed body of doctrine incapable of further improvement. On the contrary: it stands to reason, for example, that with every advance in "general" economic theory in the field, say, of the theory of "costs," the statement of the theory of cost of production as affecting the supply of the standard metal would be capable of improvement. See, for example, what is done with the most modern apparatus of cost curves and "revenue" curves by W. J. Busschau in his *The Theory of Gold Supply*, Oxford, 1936.

[34] *Treatise*, I, 7. It will be observed that Keynes's "Commodity Money" is virtually identical with Fisher's "primary money" (cf. above, p. 149, n. 15).

[35] Cf. the *Treatise*, I, 6. It should be noted in passing that a very large part of doctrine thus summarized as lying behind the $M + M_r$ of our quantity equation is "non-statical"—or, if one wishes, "dynamical"—in character, in the sense that it is concerned both with the forces making for change in the total of $M + M_r$, and therefore in prices, and also with the actual mechanism of change and of the process of transition from one equilibrium position toward another.

laymen, tend too often to forget; namely, that the much-emphasized "differences" among economists in general and monetary theorists in particular are, as often as not, the inevitable differences which arise in scientific investigation before newly discovered fields are thoroughly explored and charted, but which leave a very large body of doctrine as completely untouched as advances in certain branches of the natural sciences have left the bulk of material taught in courses dealing with the "first principles" of the subject in question.[36]

There could, indeed, be no better confirmation of this judgment than that which is provided by the fact that, in both the *Treatise* and the *General Theory,* the matters mentioned thus far are treated as enjoying such general acceptance that they are not even discussed at length, to say nothing of their being seriously challenged.[37] It takes only a slight knowledge of the literature of monetary theory to realize that this has not always been so; and while in the history of our subject there are too many examples of premature satisfaction with results obtained in its leading departments to warrant the suggestion that nothing is likely to be added to or changed in the body of doctrine thus far summarized, there is no reason why we may not contemplate "the possibility of economic generalizations which shall supersede some now holding their place in our text-books" with the same calm confidence which Cairnes displayed, in

[36] It should hardly be necessary to add that, in addition to the "inevitable differences which arise in scientific investigation before newly discussed fields are thoroughly explored and charted," the "differences among economists" which seem to outsiders to provide so damning a commentary upon the state of economics as a discipline are differences with respect to *policy,* which, for a number of obvious reasons, are perfectly compatible with a very large measure of agreement with respect to the purely analytical part of our discipline. I may, perhaps, be pardoned for calling attention to some remarks of my own on the significance to be attached to the "differences" evidenced by economists in their discussions of matters of monetary policy, in the *Journal of Farm Economics,* XVII (1935), 234 ff.

[37] For typical illustrations of the *Treatise's* treatment of the problems thus far indicated, in the few cases in which they were discussed in any detail, see the treatment of the function of the "external drain" in the operation of an international standard, Chapter XXXIV (II, 279 ff.), and the discussion of gold shortage (*auri sacra fames*), Chapter XXXV (II, 289 ff.), including the relation of the latter problem to the arts demand for the money-metal (II, 294).

the face of Comte's attacks more than a half-century ago, that a very large number of these generalizations may be regarded as a definitive acquisition for our subject.[38]

[38] Cf. Cairnes, "M. Comte and Political Economy," *loc. cit.*, 302.

CHAPTER SEVEN

The Quantity of Money-Substitutes, M'

I

Bank Reserves and the "Creation" of Deposit Currency

IT WOULD not be true to say that the extent to which general agreement prevails with regard to the forces determining the size of the stock of money-substitutes (the $M'=cM$ of our quantity equation) is as great as the agreement respecting the forces that determine the size of the stock of money of ultimate redemption ($M+M_r$). Yet there can be no question whatever that the area over which such agreement may be said to prevail is very large indeed; and once more an indication of this may be found in the fact that whole chapters in Mr. Keynes's *Treatise* were little more than a re-writing—doubly welcome because of its intelligence and balance—of what is after all received doctrine with respect to the forces lying behind (that is, determining the magnitude of) the M' of our quantity equation.

If, for example, we remember that $M'=cM_r$, it is immediately obvious that the chapters of the *Treatise* devoted to the forces determining the magnitude of our M_r (such as those dealing with the factors affecting the size of member bank reserves, on the one hand, and the size of central bank reserves, on the other, including the effect upon the latter of different methods of note-issue) also represent in reality a continuation of the kind of discussion with respect to the nature of the forces determining M' that we have had in monetary literature ever since money-substitutes in general, and particularly those money-substitutes that arise from banking operations, were recognized as a force affecting prices.[1] Precisely the same thing may be said of such chap-

[1] For references to early examples of a recognition of the influence of M', in its various manifestations, upon prices, see above, pp. 96 f., and especially

ters as that on "The Ratio of Bank Money to Reserve Money," which deals with some of the more obvious forces lying behind the $c=M'/M_r$ of our equation.[2]

It will be noted, moreover, that the received doctrine with respect to the forces determining M_r and c is in large part a simultaneous contribution to an understanding of the mechanism whereby changes in certain of the magnitudes of our equation (say, changes in the quantity of standard money) work out their effects upon the price-level.[3] This point is worth more than passing emphasis, for a number of

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notes 56 and 57, thereto. The earlier discussion of the forces determining the M_r of our formulation of course reached its height only much later—say, at the time of the Currency and Banking School controversy, and the years following. For the parts of the *Treatise* dealing with the factors affecting the size of bank-reserves (M_r), see especially Chapter XXXII ("The Control of the Member Banks") and Chapter XXXIII ("The Regulation of the Central Reserves") of the *Treatise*. There was in these chapters, to be sure, an occasional instance in which Mr. Keynes permitted himself some statements with respect to both the history of banking practice and the history of ideas on such practice to which some commentators have taken exception. This was true, for example, of his statement that "in pre-war days . . . 'open-market policy' in the modern sense was virtually unknown" (*Treatise*, II, 229). Cf. the comment on a comparable passage in the *Treatise* (II, 170), by Viner, *Studies*, 257. It is, however, worth noting that even if anything more than a natural exuberance of expression was involved in Mr. Keynes's case (contrast the qualifications on II, 229 with the absence of such qualifications on II, 170), it is hardly fair to attribute unqualified statements of the kind criticized to "present-day literature on banking" as a whole (cf. Viner, *loc. cit.*). Professor Viner himself cites (*Studies*, 261, n. 22) a passage from Hawtrey's *Art of Central Banking* that provides one example to the contrary; cf. also Hawtrey's *Monetary Reconstruction*, 2d ed., 142 f. For an example of the type of statement respecting the history of theory and practice in the matter of open-market operations which is to be found even among those representatives of "present-day literature on banking" who are anything but sympathetic to what they regard as an unwise extension of the scope of open-market operations in recent years, see H. P. Willis, *The Theory and Practice of Central Banking* (1936), 180 f.

[2] See Chapter XXV (II, 49 ff.) of the *Treatise*. Included in this chapter is a discussion of the internal drain (pp. 51 f.), which, for all its sketchiness and obliqueness, was doubly welcome in view of Mr. Keynes's announced determination, in the first volume of the *Treatise* (I, 31), to "simplify the argument" by assuming that *"all the Current Money in the hands of the public is Member Bank Money, i.e., Bank Deposits"* (italics Keynes's). This latter procedure is not only opposed to that which Mr. Keynes himself followed when, in his *Monetary Reform*, he wrote $n=p(k+rk')$, but also, since it amounted to assuming that the M of our formulation is equal to zero, would, if literally followed, have made impossible a study of the forces determining the internal drain, which is concerned, in part, with the interrelations between M_r and M.

[3] Cf. also, in this connection, the comment, on p. 155, n. 35, above, with respect to the elements of "mechanism" contained in the received theory which may be held to lie behind the $M+M_r$ of our formulation.

reasons. It will be recalled, for example, that one of Mr. Keynes's main criticisms, in the *Treatise,* of the older quantity equations was that they were "ill-adapted" to the purpose of exhibiting "the causal process" by which the price-level is determined, and the "method [that is, the mechanism] of transition from one position of equilibrium to another"; and it will be recalled also that it was supposed to be a "principal object" of the *Treatise* to fill this supposed gap by showing the "mode of operation" (that is, the mechanism) whereby "the factors which tend to bring about changes in the value of money" exert their influence.[4] It should hardly be necessary to point out that we have, in the body of doctrine to which reference is here made, a sufficient answer to the suggestion that the usefulness of the older equations for the purpose under discussion may be tested by a blind manipulation of the set of symbols contained therein, instead of by an examination of the analysis which these symbols are intended to summarize.[5] Precisely the same answer must be given, obviously, to those writers who, in advancing the charge (made doubly paradoxical by the accidents of linguistic usage) that the trouble with the supposedly "mechanistic" quantity equations is that they fail to provide a satisfactory "mechanism" of price-change, have implied that the use of "quantity equations" somehow either involves an attempt to dispose of the problems involved by a peculiarly blind manipulation of the symbols contained in the quantity-equations, or that it gives evidence of a complete lack of interest in the processes by which prices change.[6]

One of these processes, for example, is that by which banks may be said to create that particular form of *M'* which is represented by bank deposits. As it happens, economists began to concern themselves with this particular

[4] Cf. the *Treatise,* I, 133, 153, 243.

[5] Cf. what is said on this matter on pp. 81 ff., above.

[6] For a characterization of Fisher's *"equation of exchange"* as a "resuscitation . . . of the more mechanistic forms of the quantity theory of the value of money," see, for example, Hayek, *Prices and Production,* 3 (italics mine); and for a characterization of certain forms of the "quantity theory" as "mechanical," because they fail to provide an adequate "mechanism" of variations in the value of money, see L. Mises, *The Theory of Money and Credit,* 140. Cf. also Mises's comments on the implications of the Fisherine equation, in this respect, *op. cit.,* 143 f.

"process" at virtually the same time that there came to be widespread recognition of the fact that deposit currency is a form of *M'*—that is, that it is a factor affecting prices.[7] Mr. Keynes's justly praised chapter on the subject of the "creation" of bank-deposits pays implicit tribute to the work of those economists precisely by virtue of the modesty and balance with which its frankly un-"revolutionary" pages were written.[8] If the praise accorded to this chapter has sometimes been tempered by an implication that Mr. Keynes did less than justice to certain of his predecessors among writers of our own day, the obvious retort must be that the facts with respect to the history of doctrine on the subject show that he was right in treating as part of received doctrine what seemed to others a breach with "time-honored theory" on the subject.[9]

The "major error" in the older literature which, it was implied, Keynes was not sufficiently generous in crediting C. A. Phillips with having been "the first to straighten out," has to do, at bottom, with the "distinction between credit extension by an individual bank, and that of banks taken in the aggregate."[10] That there is some warrant for charging the "error" in question to certain textbooks current at the time Phillips wrote, may be granted.[11] It is equally certain, however, that

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[7] James Pennington provides the most striking case in point. See the reference given below, p. 163, n. 16. On Pennington's predecessors so far as a simple recognition of the fact that deposit-currency is a form of *M'* is concerned, cf. above, p. 97 and especially note 57 thereto.

[8] See the *Treatise*, I, 23 ff.; cf. also II, 50 and 279.

[9] For an example of an implication of the kind indicated, see the comment by J. H. Williams on the relationship of Keynes's analysis to that of C. A. Phillips, in Williams's article, "The Monetary Doctrines of J. M. Keynes," 548. The characterization of the particular doctrine involved as the "time-honored theory" is to be found in Phillips's *Bank Credit* (1920), 34.

[10] Cf. Phillips, *Bank Credit*, 32.

[11] It is worth noting that Phillips (*Bank Credit*, 34 ff.) selected for extensive quotation, as "typical of the traditional treatment," a passage from the textbook by Horace White—a textbook, it may be remarked, generally valued for its chapters on the history of money and banking, rather than for its often extremely unsatisfactory incursions into the field of monetary theory. Of the three other works cited by Phillips (p. 33 n.) as representative of the "old theory," moreover, two were textbooks on banking practice, and one of them—Kniffin's *The Practical Work of a Bank*—had avowedly based its exposition on that of White (cf. Kniffin, 14, n. 1). On the third work cited—namely H. G. Moulton's article, "Surplus in Commercial Banking"—see below, p. 167, n. 31, and p. 170, n. 36; and on Phillips's treatment of Alexander Hamilton and H. D. Macleod, see below, p. 164, n. 22. It is worth noting, also, that the three additional references to "erroneous ideas" of the kind attacked by Phillips which are provided by

the error cannot be said to represent the "time honored theory," in the sense of a theory that had "appeared quite uniformly in the earlier books." [12]

It has been shown by Professor Viner, for example, that the point made with such emphasis by Phillips—namely, that the expansion of any one bank in a system is limited by the action of competing banks—had been made with respect to *note-issuing* banks at least as far back as 1773, and appeared with considerable frequency in the early years of the nineteenth century.[13] This in itself is of considerable importance; for it can hardly be denied that it was much easier to observe the operation of the principle in the case of deposit-banking, with its virtually inevitable provision for the clearing of checks, than it was in the case of note-issuing banks, which might or might not have formal clearing arrangements. In fact, however, it is not necessary to base the argument for believing that the "error" attacked by Phillips did not "appear quite uniformly in the earlier books" on the assumption that, since the earlier writers were aware of the operation of the limiting principle in the case of note-issuing banks, they must have been aware of its operation in the case of banks of deposit. The simple fact of the matter is that evidence can be cited to show that the point was perfectly clear in the minds of those writers whose work was most influential, at the beginning of the nineteenth century, in making a commonplace the proposition that banks may "create" check-currency in much the same way that they may "create" bank-notes.

It is surely not without significance, for example, that Henry Thornton's brief development of the notion of the "creation" of check-currency should have appeared in connection with a description of the process of clearing checks; for it is inconceivable that he could have been unaware of the bearing of the clearing process upon the difference between the possibilities of expanding credit open to one bank within a banking system and that open to the banking system as a whole.[14]

R. G. Rodkey, *The Banking Process* (1928), p. 44 n., are likewise to current textbooks. Curiously enough, moreover, it is anything but certain that, in the light of the comment by Rodkey himself on p. 39 n. of the work cited, all the writers thus referred to can be shown, in the passages quoted by Rodkey, to have held the "erroneous ideas" that he attacked. Cf., on this matter, p. 167, n. 31, below.

[12] Cf. Phillips, *Bank Credit*, 34, and Williams, "The Monetary Doctrines of J. M. Keynes," *loc. cit.*, 548. Phillips's description of the ideas that he attacked, as being representative of "the traditional theory," or "the orthodox explanation" of credit expansion, was accepted also by J. S. Lawrence, *Stabilization of Prices* (1928), 329, 332; and, described as "the older theory," it is alleged by G. W. Edwards, *Principles of Banking and Finance* (1932), to have "held undisputed sway until recent years when it was challenged by Professor Phillips" (p. 156).

[13] See Viner, *Studies*, 238 f., and 156, n. 9.

[14] Cf. Thornton, *An Inquiry into the Nature and Effects of the Paper Credit of Great Britain*, 44 n., of the Philadelphia edition of 1807. The same comment applies, obviously, to all those early writers on the "creation" of check-currency who, like Thornton, discussed the phenomenon in connection with the clearing process. See, for example, Thomas Attwood, *Prosperity Restored* (1817), 29.

Much more striking, however, in this respect, is the case of James Pennington, whose title to the claim of "father" of the theory that banks may create check-currency is certainly deserved from the standpoint of the degree of influence exerted by his analysis as compared with that of earlier writers who had made the same point.[15] For Pennington was in fact extremely careful to state that the assumption, in the first part of his exposition, that "instead of seventy London bankers, there is only one," was adopted merely "in order to simplify the consideration of this subject." He then went on, not only to "take the case of two or more bankers, instead of one," but also to meet the objection that "although such a process as is here described may possibly take place when there are only two banking establishments, yet when there are so many as seventy they will operate as a check upon each other; and that, if any one of them should venture unduly to extend its discounts or its purchases, it would be warned of its imprudence by an inconvenient diminution of its cash reserve."[16]

Nor is it to be supposed that the "discovery" of the principle that the ability of one bank in a system to expand its issue of check-currency is limited by the action of competing banks, having once been made, was allowed to be forgotten. The discussion by John Stuart Mill, for example, of the "creation" of check-currency appeared, like that of Thornton, in connection with a discussion of clearing-house arrangements; and again it is impossible to suppose that he was not aware of the bearing of the latter upon the expansion possibilities open to a single bank within a system of competing banks.[17] Again also, however, it is not necessary to rely upon presumptions of innocence in order to establish the fact that the "error" emphasized by Phillips did not appear "quite uniformly in the earlier books." In Adolf Wagner's *Beiträge zur Lehre von den Banken* (1857), for example, there was a perfectly clear account of the operation of the practice of clearing in preventing a single bank, in a system of competing banks, from expanding more rapidly than its competitors; and it is noteworthy that Wagner, instead of regarding this point as a discovery of his own, referred freely to the current literature on the subject for confirmation.[18] Wagner's discussion of the point, in turn, was known to Wicksell, who, in commenting in his *Lectures* upon this part of Wagner's argument, added that of course the principle in question was true "not only of note-issuing banks but also of banks in general."[19] The point itself, moreover, had already been

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[15] On the place of Pennington in the history of the doctrine in question, see again Viner, *Studies*, 243 f.

[16] See the "Paper Communicated by Mr. Pennington," printed as an Appendix to Tooke's *Letter to Lord Grenville on the Effects Ascribed to the Resumption of Cash Payments on the Value of the Currency* (1829), 119 ff.

[17] See Mill's *Principles*, Book III, Chap. XI, sec. 6 (pp. 520 ff. of the Ashley edition).

[18] Cf. Wagner's *Beiträge*, 57 ff.

[19] See Wicksell's *Lectures*, II, 188, and cf. H. S. Ellis, *German Monetary Theory, 1905–1933* (1934), 158.

made by Wicksell himself not once, but many times elsewhere in his writings.[20]

From Thornton to Wicksell is a long span—almost a century; and, in view of the fact that the correct doctrine, instead of having been "discovered" at the beginning of the century only to require resuscitation by Wicksell at its end, had appeared at regular intervals over the hundred years, the burden of proof is surely on those writers who have argued that "the accepted statements of banking theory, with scarcely an exception," had made no "distinction between credit extension by an individual bank and that of banks taken in the aggregate."[21] It would indeed have been of some interest—at least to those who would accept only with reservations Pantaleoni's dictum that the history of economic ideas ought to be a history of the successive discovery of truth, rather than the history of repeated errors—if the writers who have been so critical of "the accepted statements of banking theory" had provided us with an account of precisely the way in which the error they had "discovered" had crept into the literature, and of the extent to which it was actually held by writers of sufficient eminence to warrant their being taking as representatives of the "traditional theory" on the subject.

Actually, we have been told merely that the error that is supposed to represent the "time-honored theory" was "handed down consistently from the days of Alexander Hamilton to the present," and that it had been held by H. D. Macleod.[22] That the error was "handed down consistently" from the early nineteenth century to our own day, we know to be simply not true. Alexander Hamilton, whatever may be thought of his importance as an economic practitioner, was hardly of sufficient importance in the history of economic doctrine to warrant his being accepted as a representative exponent of the "traditional theory" on the subject, even if it were clear beyond question that he held the "error" attributed to him.[23] Macleod is in a different category; and

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[20] Cf. Wicksell, "Der Bankzins als Regulator der Warenpreise," *Jahrbücher für Nationalökonomie und Statistik*, LXVIII (1897), 237 f.; *Interest and Prices*, 85, 111, 119; *Lectures*, II, 86, 188; "The Influence of the Rate of Interest on Prices," *Economic Journal*, XVII (1907), 217.

[21] So Phillips, *Bank Credit*, 32. The only citation given by Phillips to authors who had in some degree approximated his own position was to Davenport's *Economics of Enterprise* (Phillips, *loc. cit.*).

[22] So Phillips, *Bank Credit*, 34, 37. The choice of authors thus cited leaves virtually no doubt that White's *Money and Banking* was taken as an authority with respect to the history of doctrine on the subject. See the quotation from Hamilton and the citation of Macleod on p. 196 of the 5th (1914) edition of White's textbook. The reference by Lawrence (*Stabilization of Prices*, 329) to Macleod, seems, in turn, to have been derived from Phillips.

[23] The passage on the basis of which Phillips attributes the error in question to Hamilton is presumably that quoted by White (cf. the preceding note) and which appears on pp. 55 f. of Hamilton's *Papers on Public Credit, Commerce and Finance* as edited by S. McKee, Jr. (1934). Hamilton's principal "error," if such it can be called, is the first of those which may properly be charged against the exposition of Macleod; namely, the

it is of some interest to establish the degree of error which he can properly be charged with holding in the matter under discussion.[24]

It is beyond question that Macleod was guilty of at least bad exposition on two points in which he served as an unfortunate model for certain later writers of textbooks.[25] The first of these was his habit of speaking of "the banker" or "a banker" when the latter was intended to be merely a figure of speech for "the banking system."[26] In the light

habit of speaking of "a bank" when "the banking system" is meant (cf. also p. 57 of Hamilton's *Papers*). It is noteworthy, however, that a sentence or two before the one beginning the passage quoted by White, Hamilton spoke, not of "a bank," but of "banks." With respect, moreover, to the second and more serious fault of Macleod's exposition, to which attention is called in the text (namely, the suggestion that banks, either individually or in the aggregate, can create credit to a multiple of the amount of *deposits* left with them regardless of the type of "currency" represented by the "deposit"), it may be pointed out that Hamilton's assertion was merely that "banks in good credit" could expand to a multiple at their holdings of *"gold and silver"* or *"coin"* (*Papers*, 55–57; italics mine).

[24] It is striking that neither Phillips, nor, so far as I am aware, any of those who have accepted his attack on "the traditional theory" should have attempted to deal in detail with Macleod's position. The single page reference to Macleod given by Phillips, for example (*Bank Credit*, p. 37, n. 2), and by Lawrence (*Stabilization of Prices*, 329), is that which was given by White (*Money and Banking*, 195). On the other hand, my efforts to track down the origin of the supposed "error" have profited greatly from conversations with Mr. Clarence A. Nelson, of the University of Minnesota, whose interest in the historical development of the idea of the "creation" of bank-deposits has led him to pursue the matter further, on the historical side, than I have been able to.

[25] The two points in Macleod's exposition emphasized in the text are, of course, not the only ones that may be regarded as regrettable, from the standpoint of the amount of controversy to which they have given rise. Macleod's emphatic insistence, for example, upon contrasting his own analysis with that of writers who have regarded a "banker" as "an intermediary between those who want to lend and those who want to borrow" (see, for example, *The Theory and Practice of Banking*, Vol. I, Chap. III, sec. iv, par. 15; *The Theory of Credit*, Vol. II, Part I, 373 f.; *Elements of Economics*, I, 378; *Elements of Political Economy*, 292; *Elements of Banking*, 153; *Dictionary of Political Economy*, I, 75; *History of Economics*, 205), while it was not by any means entirely mistaken, nevertheless alienated unnecessarily those who would insist that the individual banker does in fact base the amount of his loans upon the amount of funds deposited with him, and is thus in effect an "intermediary" between lenders (that is, depositors) and borrowers, even if it is true that a large part of the funds which are "deposited" with the banker and which affect the scope of his own lending operations, are in the form of check-currency previously "created" by other banks within the banking system. There can be little doubt, however, that the chief stumbling blocks in Macleod's exposition have been the two indicated in the text.

[26] This was particularly characteristic of the crucial passage, "On the Mechanism of Banking," which is regarded as containing the egregious "error" in Macleod's analysis. See *The Theory and Practice of Banking*, I, Chap. III, sec. iv, par. 18; *The Theory of Credit*, II, Part I, 361 ff.; *Elements of Economics*, I, 383 ff.; *Elements of Political Economy*, 290 ff.; *Elements of Banking*, 150 ff.; *Dictionary of Political Economy*, I, 71 ff.; *History of Economics*, 208 ff. On the grounds for suggesting that by "a banker" or "the banker" Macleod meant "the banking system," see below, p. 166, and especially n. 29, thereto.

of the fact that the positive "error" with which he has been charged is alleged to have sprung directly from a failure to "draw a sharp line of distinction between credit extension by an individual bank and that of banks taken in the aggregate," his failure to make clear that he was really speaking of the banking system must be regarded, at least in the light of the later discussion by writers such as Phillips, as a serious error of omission. Whether, however, he is to be charged with a positive error of commission, of the kind that has been charged against him, as well as against certain of his popularizers, is another matter.

The essence of the "error" attributed to Macleod was an alleged failure to see that "a bank" would be prevented from multiple expansion in a degree greater than that adopted by its competitors by "a loss of cash through unfavorable clearing house balances." [27] In fact, however, Macleod repeatedly gave evidence that he was aware of the fact that his whole argument was built on the conscious assumption that "the chances are that about an equal number of the customers of bank A will have about equal claims against bank B; and so on among any number of banks." [28] Macleod himself felt that "the ingenious arrangements of the Clearing House" had brought it about that "all the banks which join in the clearing are really and practically formed into one huge banking institution, for the purpose of transferring Credits among each other, just as Credits are transferred from one account to another in the same bank," and it was because of this that he felt warranted in presenting his theory with respect to the "creation" of check-currency in terms of "a bank," on the assumption that the reader would understand that by "a bank" was meant "the banking system." [29] The error of omission represented by his failure to emphasize the distinction is therefore clear; what is anything but clear is that the distinction itself

[27] Cf. Phillips, *Bank Credit,* 36 ff. The point was made, of course, against White; but the latter was taken as "following" Macleod (p. 34).

[28] Macleod, *History of Economics,* 212 f.; also *The Theory and Practice of Banking,* I, 124 (of the 2d [1866] edition); *The Theory of Credit,* II, Part I, 373; *Elements of Banking,* 157; *Dictionary,* I, 73. See especially the passage on pp. 297 ff. of Macleod's *Elements of Political Economy:* "In order to save repetition, let us consider the case not of a single bank, but of several banks transacting business on the same principles, in the same locality . . . ," etc. The following (p. 298), with respect to "the considerations which limit his [the individual banker's] power of buying debts with 'promises to pay' " is particularly noteworthy: ". . . the great art in banking is taking all these chances into consideration, observing, in the first place, how many payments are actually demanded; and, secondly, if they be demanded, *what proportion of them are settled and cancelled by cross claims and obligations on other banks,* so as to save the actual coin." (Italics mine.)

[29] It is worth comparing Macleod's likening of "all the banks which join in the clearing" to "one huge banking institution" with the comment of Phillips (*Bank Credit,* 73) that the "banking system . . . may be likened to a single great bank doing the entire banking business of the country." Phillips's simile occurs, strangely enough, in a section entitled (p. 72) "The Old Theory and the New Contrasted." In view of the fact that the outstanding representative of the "Old Theory" is supposed to have been Macleod, it is not easy to see in just what the "contrast" is held to lie.

invalidates in any serious respect his conclusions regarding the power of "banks" to expand on the basis of a net accretion of reserves, on the conditions with respect to the generality of banking expansion which he specifically assumed for the purpose of his argument.[30] The same thing must obviously be said on behalf of those writers who, though they have been charged with a failure to see that the process of multiple expansion on the basis of a given net accretion of reserves would be limited to the extent that individual banks are open to the threat of "unfavorable clearing balances," nevertheless made it clear that their whole argument was constructed on the assumption that such adverse clearing balances are avoided by the receipt, in the form of deposits, of checks on other banks which are themselves the result of other banks' lending operations.[31]

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[30] Macleod's position, as thus stated, should be borne in mind in judging the argument presented by Phillips (pp. 74 ff.) under the heading "Anticipated Criticism Answered." Phillips admits explicitly (p. 74) that "if all banks were expanding their loans at the same rate . . . , the contention [that checks drawn upon the lending bank by its depositor-borrowers against the deposited proceeds of the new loans would be offset by the deposit in the lending bank of a corresponding amount of checks . . . drawn upon other banks in consequence of loans made by those other banks to *their* depositor-borrowers] would be valid." His only objection is that "additions to the reserves of a banking system, except in the most extraordinary cases, are made, at any given time, not by the deposit of cash simultaneously in all the banks of a system but by the deposit of funds in only a small proportion of the banks, whence they are scattered throughout the system" (*ibid.*). This is true; but it is quite irrelevant to the question as to how, given a net addition of reserves to the banking system—whether this addition is or is not *in the first instance* distributed evenly among all individual banks—it is possible to have within the system an expansion of bank-currency to a multiple of the net addition to reserves. The answer must necessarily be that which Macleod gave: namely, *some* degree of simultaneous expansion, which would make possible the offsetting of checks through the clearing house.

[31] Agger, for example, who is cited by Phillips (*Bank Credit,* 33 n.; cf. also Rodkey, *The Banking Process,* 41 n.) as typical of those holding to "the old theory" that "the receipt of another $100,000 in cash . . . would enable the bank to add another $1,000,000 to its loan item," specifically assumed that an individual banker's "cash reserve of 10% is adequate to meet direct demands for cash *as well as possible debit balances at the clearing house,*" these "clearing house requirements arising from the *checks drawn by the depositors*" (*Organized Banking,* p. 32; italics mine). Even Moulton, in that part of his article cited by Phillips (*loc. cit.*) in this connection, was careful to take account of the case in which some of the checks "drawn on local banks are sent outside and are deposited in the banks of other communities," with the obvious effect upon clearings (p. 1009 of the article cited by Phillips). Moulton's error indeed (on which see below, p. 170, n. 36) crept in only when he passed to his avowedly heretical argument with respect to the effect of additions to surplus (pp. 1011 ff.). Even White, the selection of whom as a representative of "the traditional theory" is justified more from the standpoint of the openings that his exposition gave for attack than for its faithful reproduction of "the traditional theory," pointed out, in a passage not reproduced by Phillips (White, *Money and Banking,* 197) that if the depositors draw checks "to persons who are not depositors in the same bank . . . , they could not all have been paid." Of

The second element in Macleod's exposition that opened the way to misunderstanding was in fact much more serious than the first. This was his loose habit of using, for purposes of illustration, the case of "a bank" that was thought of as creating deposits to a multiple of the amount of "money" or "cash" *deposited* with it.[32] The reason why this method of exposition was unwise in the extreme is that it was likely to lead the unwary reader to suppose that it lay in the power of "a bank" to "create" deposits to a multiple of the amount of *deposits* left with it, *regardless of the form of currency in which the deposit was made.* Actually, of course, the banks composing a banking system can, under the most favorable conditions (that is, on the assumption that the individual banks will each create check-currency in approximately equal amounts) "create" credit to a multiple only of the *net additions to their total reserves.* If they can create credit to a multiple of the funds "deposited" with them, this will be possible only because the funds thus deposited happen to be of such a form that they can serve as reserves *without causing a contraction in "reserves" elsewhere.*

One can hardly suppose, it is true, that Macleod would ever have argued that "a bank" can expand to a multiple of the "deposits" left with it if these deposits are made entirely in the form of *checks on other banks.* To have so argued, indeed, would have been to miss the whole point of the theory respecting the "creation" of "credit" by commercial banks—namely, that such banks "create" a form of currency (namely, *check-currency*) which, when deposited in other banks, adds to the "deposits" of these other banks in exactly the same way that a deposit of, say, specie would add to their deposits.[33] The difference

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the discussion in Moulton's *Financial Organization of Society,* cited by Rodkey (loc. cit.), the same thing is to be said that was said of the article cited by Phillips, with the addition that, fortunately, the application of the "error" to the case of surplus is omitted, being referred to only elsewhere in a footnote (p. 531). In the passage cited by Rodkey from Dunbar, finally, the phrase "the enforcement of the liability for deposits" (Dunbar, *Theory and History of Banking,* 32) should be read in the light of the discussion on pp. 45 ff. of Dunbar, with respect to the mechanism that may be expected to operate "if . . . we suppose the parties concerned to keep their accounts with different banks." See especially p. 52: ". . . the chief assurance against excessive expansion *on the part of any single bank or banker* is given by the certain demand for prompt and frequent settlement, occasioned by the *establishment of the clearing house.* . . ." (Italics mine.)

[32] See the references to Macleod given on p. 165, n. 26, above.

[33] It may be remarked, in passing, that it is extremely doubtful whether a large part of the confusion that has surrounded the question of the power of banks to "create *deposits*" would have arisen if the mechanism of "creation" had always been described in terms of the creation of "check-currency," and of the conditions under which such "currency" will represent a net addition to the circulating medium, with the subsequent effects upon "deposits" of such an addition to the "depositable" circulating medium. When put in these terms, it becomes possible to describe with much more precision than is often found the true nature of the distinction between commercial banks and savings banks, as well as the true nature of the implications surrounding the proposition that both types of bank are

between the deposit, on the one hand, of specie or of other forms of hand-to-hand currency capable of acting as a net addition to total reserves, and the deposit, on the other hand, of "currency" in the form of bank-checks, is that the former do not represent claims on other banks which, upon presentation, limit the power of these banks to issue check-currency in greater proportion than the presenting bank has expanded its own check-currency, whereas bank-checks do represent such claims. It follows, therefore, that propositions with respect to the power of a commercial bank to bring about multiple expansion should always have been put in terms of the bank's power—on the assumption that other banks in the system expanded simultaneously—to create check-currency on the basis of a net addition to total reserves up to the point permitted by the requirement that it hold a given amount of reserves against its redemption obligations. They should never have been put in the misleading form of a power to "create credit" to a multiple of the "money" *deposited* with banks, without careful specification of the source and character of the "money" so deposited.

There can be no doubt, therefore, that this second element in Macleod's exposition opens him to the charge of having committed a second sin of omission: namely, that of having failed to make clear that the "money" which, when deposited with the banking system, provides the basis for credit expansion to a multiple of the "money" so deposited, is "money" which represents a *net* addition to the reserves of the banking system, and not, as in the case of the deposit in one bank of checks on other banks, "money" which, while it adds to the "reserves" of the bank in which it was deposited, simultaneously cuts down the reserves of the banks on which the checks were drawn. Nor can an adequate apology for Macleod's exposition in this respect be found in the fact that he meant by "money" something quite different from the forms of "credit" represented, say, by bank-checks.[34] For it was part of Mac-

"intermediaries" between their borrowers and their depositors. It must be said, in justice to Macleod—though he cannot be exonerated of the vice of exposition pointed out in the text—that he himself regarded the essence of commercial banking as consisting of "the creation and issuing of 'currency,'" and the difference between modern commercial banking and banking by banks of issue as consisting merely of a change in the form of "currency" issued by the banks. See, for example, *The Theory and Practice of Banking*, I, 120, 124 (of the 2d [1866] edition); *Dictionary*, I, 71; and cf. Macleod's reiteration of the proposition that "Notes and cheques are . . . equally *Circulating Medium*, or Currency" (*Theory of Credit*, II, Part I, 371). Cf. also the *Dictionary*, I, 74, and *Elements of Political Economy*, 304.

[34] That Macleod did mean precisely this by "money" in contexts other than the one under discussion is illustrated, for example, by the fact that he repeatedly attached considerable "interest" and "practical importance" to the problem of "the proportions which credit and money bear to each other in modern commerce." See, for example, *The Theory and Practice of Banking*, I, 156 ff. (of the 2d edition); and cf. *The Theory of Credit*, I, 293 ff., II, Part I, 504 ff., and *Elements of Economics*, I, 324 ff. Macleod was, to be sure, not always consistent on the nature of the criterion which was held to distinguish "money" from credit. Sometimes, for example,

leod's system of "monetary metaphysics" that he should have been particularly anxious to stress, not the *differences* between "money" and "credit," but their similarities—to the point, indeed, of arguing that "money" was merely "the highest and most general form of Credit." [35] Again, however, while there can be no question of attempting to exonerate Macleod of the charge of misleading exposition, fairness to him demands that he be charged with no greater sin than this. At any rate, I have not been able to find a single instance in Macleod's writings in which it is clear that he would actually have argued that the acquisition of "cash" by "a bank," if this "cash" did not represent an addition to the net reserves of the banking system, would provide the basis for multiple expansion within the banking system. The only instance, indeed, that I have found in which this type of conclusion was clearly implied by a writer of standing is that which is represented by H. G. Moulton's extraordinary argument respecting the effect of the accumulation of surplus—an argument, it should be observed, which was presented in an article avowedly written in opposition to the views generally expressed by "writers in banking theory." [36]

"money" was regarded as "an intermediate and equivalent merchandize"—that is, as what Keynes (cf. the *Treatise*, I, 7) called "commodity money" (see *The Theory and Practice of Banking*, I, 24; *Elements of Political Economy*, 35 ff.; *Dictionary*, I, 655). In other cases, nothing was held to be money unless it possessed the quality of legal tender (cf. *The Theory of Credit*, I, 82; *Elements of Economics*, I, 181; *History of Economics*, 498). The point made here, however, is merely that Macleod always made a distinction of some kind between "money" and "credit." It must be said also, in partial defense of White, who copied faithfully Macleod's habit of using, for purposes of illustration, the case of a "deposit" of "money" in a bank, that his textbook began with a very emphatic distinction between "money" and "promises to pay money" (White, *Money and Banking*, 1; and cf. p. 195 of the same work).

[35] *Theory and Practice of Banking*, I, 19; *Elements of Banking*, 21; *Theory of Credit*, I, 82. Cf. also Macleod's statement to the effect that "*Gold* and *Silver* may . . . be justly termed *Metallic Credit*" (*Elements of Economics*, I, 176; *History of Economics*, 493). It may be emphasized that what is here criticized is Macleod's *exposition;* in the light of the passages cited in the preceding note, it is perfectly obvious that Macleod himself did not deny that there are significant differences between "money" and "credit." Even such a statement, for example, as that "*Money* is therefore what is termed *Credit*" (*History of Economics*, 489) is to be interpreted in the light of the further statement that "In Economics all Money is Credit, but all Credit is not Money." (*Ibid.*, 497.)

[36] See Moulton, "The Surplus in Commercial Banking," *Journal of Political Economy*, XXV (1917), 1011 ff. Moulton's argument was that the accumulation of surplus is not "an added protection to the depositors" because, being represented in the first instance by assets in the form of "cash," it makes possible the expansion of deposits to a multiple of such cash, with the result that the ratio of deposits to double-liability capital stock is impaired. The fallacy in this argument, of course, as Phillips (*Bank Credit*, 89 ff.) points out, is that there is no reason whatever for assuming that the "cash" obtained through earnings available for "surplus" represents a net addition to the reserves of the banking system. It may be noted here merely (1) that Phillips himself recognized that Moulton's argument was a "new" one (*Bank Credit*, p. 84) and (2) that the argument, unlike that

Such, then, is the history of the egregious "error" that had supposedly been "handed down consistently from the days of Alexander Hamilton to the present," and had "appeared quite uniformly in the earlier books." A perfectly correct statement of the point in question had been provided by the writers who were directly responsible for the introduction of the discussion of the "creation of bank-deposits" into the main stream of nineteenth century banking literature. This correct statement was carried on by writers of the highest standing, from John Stuart Mill to Wicksell. A form of exposition was adopted by Macleod, alone among the figures of first importance in nineteenth century economics, and the one writer of influence most given to extreme and paradoxical statement, which contained the seeds of misunderstanding, even though it is by no means clear that Macleod himself was guilty of the specific logical error with which he has since been charged. The least satisfactory aspects of his exposition were copied by a number of textbook writers, and in one case the writer of an avowedly heretical article drew a conclusion from these details of exposition that was definitely erroneous. These textbook writers and the writer of the heretical article were then regarded as authoritative sources for an understanding of the "traditional theory" on the subject, in the form of an "error" which was assumed, without proof, to have "appeared quite uniformly in the earlier books." The episode—in which, it should be noted, Mr. Keynes played a role of complete innocence—is surely not without its moral for the purpose underlying a study such as the present one.

II

The Rôle of the Rate of Interest

The very emphasis, moreover, in the *Treatise,* upon the ratio $c(=M'/M_r)$ as the crucial step in the "route" by which "the injection of an increased quantity of money into the monetary system" will "bring about a new equilibrium at a changed price-level" is likewise an indication that certain of Mr. Keynes's utterances, in his *General Theory,* with respect to "the way in which changes in the quantity of money work their way into the monetary system," are not to be interpreted as asserting a claim to priority in the posing of the problem.[37] It is certain, at any rate, that no such claim

attributed to Macleod, leads to conclusions which are fallacious not only when applied to the "individual bank," but also when applied to the "banking system as a whole."

[37] Cf. the *Treatise,* I, 262 f., and the *General Theory,* 173. A discussion of the treatment, in the *General Theory,* of the problem as to "where, and how, the quantity of money enters into the economic scheme" (*General Theory,* 168), and the relationship of this treatment both to the traditional analysis in the subject and the argument of the *Treatise,* must be deferred to Volume II of this study.

could be substantiated: for the emphasis in the *Treatise,* in particular, is a tribute not only to those of whose work the contribution of Marshall was anything but a unique example, but also to those writers, from Cantillon through Cairnes to writers of our own day, who, in dealing with other conditions or with an earlier stage in the process of monetary expansion following upon gold-discoveries, provided contributions to an understanding of the mechanism of price-change on a different, but equally important terrain.[38]

It would not be true to say that all those who have worked with the familiar quantity equations have on all occasions shown themselves aware of the necessity for dealing with such issues. It is, however, worth recalling the reaction of one of the most eminent sponsors of the familiar quantity equations, Irving Fisher, upon one famous occasion when Mr. Keynes, with more than a little justice, took him to task for having seemed to be content to show "*that* the changes in the quantity of money do affect the price-level," instead of going on to "show *how* they do so."[39] What is to be noted is that instead of attempting to argue that the issues were of no importance, Fisher not only accepted the criticism as sound, but also indicated the nature of the type of analysis that must be provided in order to fill what was otherwise a notable gap, by referring to the writings of "other writers on this subject, particularly Cairnes," just as, on previous occa-

[38] See Hayek, *Prices and Production,* 8 ff., for a summary of the contributions of writers such as Cantillon, Cairnes, and others to our understanding of the process whereby changes in the quantity of money "work their way" into the economic system—contributions which some historians of doctrine have characterized as contributions to "monetary dynamics" (cf., for example, the references to F. Hoffmann on p. 84, n. 30, above). See also the comments by T. E. Gregory in his Introduction to Tooke and Newmarch's *History of Prices,* 24 f. (cf. Tooke's *Considerations on the State of the Currency,* 23 n.), and p. 116 (cf. the *History of Prices,* VI, 136 f., 170, 188 ff., 230 ff., 810 ff.). What must strike one, in reading these early writers, is their full consciousness of the fact that the problem of mechanism was of the greatest importance on its own account, and had by no means always been given the attention it deserved. See, for example, Cantillon's famous comment on Locke, p. 161 of Higgs's edition of the former's *Essai;* Tooke's comment on a statement of G. R. Porter, p. 69 of the former's *Inquiry into the Currency Principle;* and Cairnes, *Essays in Political Economy,* p. 55. Cf. also Wicksell, *Lectures,* II, 160.

[39] See Keynes's review of Fisher's *Purchasing Power of Money* in the *Economic Journal,* XXI (1911), 394 f. The summary in the text of Keynes's criticism is that given by Fisher in his reply to Keynes in the second (1920) edition of *The Purchasing Power of Money,* xiii.

sions, he had referred to the writings of Marshall and Wicksell.[40] If no other instance were available for citation, this episode alone would show how essentially absurd is the position of those who, upon sighting a "quantity equation," have assumed that the writer using it must have meant to indicate his contentment with a "purely mechanical" account of the way in which money affects prices.

In the present instance, the most striking example of the contrary procedure is the emphasis placed on the *rate of interest* as the link by which changes in $M'(=cM_r)$, and therefore in prices, are brought about. No one familiar with the discussion of the effect of variations in bank-rate upon the amount of bank borrowing, from Thornton and Ricardo through the Currency and Banking School controversy down to Marshall, could have argued that emphasis upon these variations as the crucial step in the mechanism of price-change should be regarded as a novelty; and indeed, whatever may be said otherwise of Mr. Keynes's treatment of the history of doctrine in the *Treatise*, he cannot be charged with having claimed complete novelty for his analysis in this respect.[41]

The general outlines, if not the complete details, of the history of doctrine upon this point are by this time fairly clear.[42] The relation, for example, of certain parts of Wicksell's analysis to the earlier suggestions of Thornton and Ricardo has been pointed out by a number of writers. Wicksell himself, indeed, had from the very start characterized his doctrine as being merely a "synthesis of the most authoritative views on money."[43] In the preface to *Interest and Prices*, he went out of his way to show how what he had to say tied up with the utterances of Ricardo regarding the place of the rate of interest in the mechanism of

[40] See Fisher, *The Purchasing Power of Money*, xiii. For his references to Marshall and Wicksell—references, to be sure, that do not call attention to the important differences between the analysis of the two writers—see Fisher's "Rôle of Capital in Economic Theory" (1897), *loc. cit.*, 518 f. and 519 n.; and *The Purchasing Power of Money* itself, 59 ff.

[41] See, for example, the references to Giffen and Marshall in the *Treatise*, I, 188; and cf., in this connection, the argument on pp. 262 f. of the same volume. For a discussion of the differences which Keynes believed to exist between his own analysis and the "traditional doctrine" on the subject, cf. below, pp. 178 ff.

[42] See, in this connection, the summary given by Hayek, *Monetary Theory and the Trade Cycle*, 109 f., and the same author's *Prices and Production*, 13 ff.; also Viner, *Studies*, 149 ff., 211, 213 f., 256 ff., 277 f., 284, 286, and 288.

[43] Cf. Wicksell, "Der Bankzins als Regulator der Warenpreise," *loc. cit.*, 241.

price change.[44] It is hardly surprising, therefore, that when, in 1906, and again in 1909, Professor D. Davidson called attention to other resemblances between the argument of Wicksell and that of Ricardo which the former, in his own words, had "overlooked," Wicksell should have hastened to agree that in fact Ricardo's theory was "very much on the same lines" as the theory that Wicksell himself had "developed."[45] In 1916, again, Professor Davidson called attention to the contribution of Thornton in this connection; and once more Wicksell hastened to acknowledge the similarity.[46] It may be remarked in passing that both citations take on particular interest in view of the fact that, according to Keynes's *Treatise*, before 1837 ideas with respect to " 'Bank-rate policy,' in the modern sense . . . did not exist," and that "in the works of Ricardo, for example, nothing of the sort is to be found."[47] The citations show at least that the most important single strand in the theoretical analysis underlying "Bank-rate policy" was to found in these earlier writers.

Some support, to be sure, would seem to be given to Mr. Keynes's statement regarding Ricardo by Professor Viner's contention that Ricardo "ordinarily denied any relationship between the rate of interest and the quantity of money, and presumably also between the rate of interest and the demand for loans."[48] Viner himself, however, gives, in support of his use of the word "ordinarily," only two citations to Ricardo, neither of which, as it happens, is really convincing. The first passage, for example, from the first of Ricardo's three "letters on the price of gold," to the effect that "whilst the Bank is willing to lend, borrowers will always exist," certainly does not represent an explicit "denial" of "any relationship between the rate of interest and the de-

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[44] Cf. *Interest and Prices*, xxiv.

[45] See Wicksell, *Lectures on Political Economy*, II, 200; and cf. Davidson, "On the Concept of the Value of Money" ("Något om begreppet penningens värde"), *Ekonomisk Tidskrift*, VIII (1906), 462, and "On the Stabilization of the Value of Money" ("Om stabiliseringen af penningens värde"), *loc. cit.*, XI (1909), 2 n. The passage from the *Lectures* cited at the beginning of this note is so unequivocal in its acknowledgment of Ricardo's contribution that other passages in Wicksell's writings in which something less than justice seems to be done to Ricardo must be interpreted as having to do only with minor details, and not with the central proposition itself. For examples of such passages, see the *Lectures*, II, 178 ff., and "Hinauf mit den Bankraten!" *Archiv für Sozialwissenschaft und Sozialpolitik*, XLI (1916), 753; and for the interpretation of the first of these, in particular, cf. below, pp. 256 f. Cf. also what is said concerning Ricardo's ideas on the subject of the "relationship between the rate of interest and the quantity of money, and . . . between the rate of interest and the demand for loans" on p. 175, n. 51, below.

[46] See p. xii n. of Wicksell's Preface to the second volume of the German translation of his *Lectures* (*Vorlesungen*); and cf. Davidson, "H. Thornton on the Money-Rate and Commodity Prices" ("H. Thornton, om penningränta och varupriser"), *Ekonomisk Tidskrift*, XVIII (1916), 391 ff.; cf. also the same author, *ibid.*, XXXII (1930), 208 ff.

[47] *Treatise*, I, 186.

[48] Viner, *Studies*, 150.

mand for loans."[49] One has only to remember, indeed, that the Bank's degree of "willingness to lend" would inevitably be reflected in the *terms* on which it was "willing to lend," to observe that the passage becomes perfectly consistent with the passage from Ricardo's *Principles*, cited by Viner, which is quite explicit in accepting the fact of a "relationship between the rate of interest and the demand for loans."[50]

The second passage cited by Viner—from a speech of Ricardo on May 24, 1819—is hardly more convincing. Ricardo was discussing the argument that the resumption of specie payments might result in the issue of less than the "amount of currency which was required" by business, and, more specifically, that it would result in a rate of interest that would discourage borrowing. His answer, as interpreted by Viner —and I concur in the interpretation—was that, on the grounds advanced by "Adam Smith, Mr. Hume, and others," there was no reason to suppose that a contraction in the quantity of money would adversely affect the rate of interest. This may, indeed, be regarded as too sweeping a denial of "any relationship between the rate of interest and the quantity of money." It is, however, anything but clear that this fact is directly relevant to the question whether Ricardo would have formally denied the existence of "any relationship between the rate of interest and the demand for loans." There are grounds, both logical and empirical, for agreeing with Ricardo's implication that monetary contraction would not necessarily raise the absolute level of the rate of interest, even if one is not prepared to accept his supporting argument, which rested on an implied denial—in one sense, at any rate—of "any relationship between the rate of interest and the quantity of money." Indeed, there are grounds, both logical and empirical, for expecting that such monetary contraction might result in a *lowering* of the absolute level of the rate of interest. To be sure, Ricardo should have gone on to demonstrate that, even with an unchanged or even a lowered rate of interest, the demand for loans might be adversely affected by the monetary contraction because of the changed *relationship* between the rate of interest and *the anticipated profit to be made by the use of a bank-loan.* Surely, however, the test as to what Ricardo would "ordinarily" have said on this latter question, which is after all the crucial one, is to be sought not in Ricardo's letters to newspapers or in his parliamentary utterances, which were subject to the restrictions proper to such media, but rather in his formal publications, in which he would be expected to weigh his words with more than "ordinary" care.[51] In these publications, his

[49] The passage is to be found on p. 11 of Hollander's edition of the *Three Letters.*

[50] See Viner, *Studies,* 150.

[51] Viner (*Studies,* 182, n. 19) cites a passage from Ricardo's *On Protection to Agriculture* (which was, of course, a "formal publication") with respect to "the complaints made against the Bank for refusing to lend money on discount at 4 per cent" (pp. 279 ff. of Gonner's edition of the *Economic Essays*), in a context which would imply that Ricardo was there arguing that the maintenance of the discount rate at the same absolute level could not have deflationary consequences. Actually, however, what Ricardo was

position was unequivocally such as to belie the suggestion that he would have denied that there was "any relationship between the rate of interest and the demand for loans." [52] Taken in conjunction with the fact that the two passages cited by Viner, when interpreted in the manner suggested above, are not inconsistent with Ricardo's formal position as expressed in the *Principles,* this circumstance would seem to make at least very doubtful Viner's statement as to what Ricardo "ordinarily" held on the matters under discussion, and thus to remove any possibility of using this statement in support of Keynes's generalization with respect to Ricardo's ideas, or lack of ideas, concerning " 'Bank-rate policy,' in the modern sense."

Nor could anyone aware of the deeper implications of the argument of Wicksell's *Interest and Prices,* for all its inadequacies, suggest that no serious attempt had been made to tie up the theory of money and credit with "general" economic theory—in this case, with the theory of interest, and particularly with the "real capital" implications of some of the most widely held variants of the theory of the forces determining the rate of interest. It is of some importance to stress this fact, if we are to obtain a proper perspective for judging a question that Mr. Keynes has raised anew in his *General Theory*—the question, namely, whether, and to what extent, the theory of money has been tied up with the general "Theory of Value." [53]

The nature of the tie-up between monetary theory and that particular branch of "value theory" that is represented by "utility analysis" is touched upon in later chapters.[54] Here it is necessary only to observe that if by "value theory" we mean the theory of the forces determining all "values," the simple fact that the rate of interest is itself a "value" must mean that all attempts to tie up the theory of money with the general theory of interest represent *ipso facto* attempts to tie up the theory of money with "value theory." It is not necessary, therefore, in order to demonstrate that such attempts have been made, to restate

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arguing against in the passage in question was the contention that the Bank could fix the "permanent rate of interest" paid by the "landed interest," or that it could fix "permanent" interest rates generally. This was a proposition that would certainly have been assented to by Wicksell, for example, who was nevertheless perfectly well aware that the maintenance of the rate of discount at the same absolute level might, under certain circumstances, have deflationary consequences.

[52] Cf. below, p. 191, and especially n. 93, thereto.

[53] Cf. the *General Theory,* 292. Keynes's utterances in this connection will be discussed in more detail in Volume II of this study.

[54] See below, pp. 309, 440 ff., 491 ff.

interest theory in terms of "utility analysis."[55] There are, consequently, grounds for objecting to the implication that a concern with a "Wicksellian natural rate theory" is something that cannot appear "sensible and interesting" to a "value theorist."[56] Rather, as has recently been suggested, the truth is that we are dealing with at least "two streams of thought"—the one represented by "the cash-balance analysis" and the other by those aspects of "capital analysis" that are summarized by the concept of a "natural rate of interest"—each of which, in its own way, represents an attempt to tie up the theory of money with "the general theory of prices," or "value theory."[57]

It should hardly be necessary to add that to characterize Wicksell's attempt to tie up monetary theory with the "theory of interest" as an attempt to bridge whatever gap may be held to exist between monetary theory and the "general theory of value," is not to imply that the attempt was in all respects, or even in major respects, successful.[58] One might say the same thing, however—and, in my opinion, with much greater reason—of Keynes's attempt to tie up the two in the *General Theory*. Yet there would be as little reason, merely because of what many would regard as the shortcomings of his positive analysis, to refuse to accord to Mr. Keynes the merit of having wrestled with the problem of the relation between the "Theory of Money and Prices" and the "Theory of Value," as there was for him to charge "economists" in general with having failed altogether to do so.[59]

The argument, in short, is that precisely the type of analytical contribution that has sometimes been held up as representing an approach to the problem of price-determination

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[55] Cf., in this connection, F. A. Hayek, "Utility Analysis and Interest," in *Economic Journal*, XLVI (1936), 44 ff. It should be added that Hayek himself did not advance, as an argument for relating interest theory to "utility analysis," the possibility of thereby establishing a better *modus vivendi* between monetary theory and the general theory of value. Nor is it intended here to suggest that there is any reason why, even if a restatement of the type indicated is not *necessary* for a demonstration of the point indicated in the text, such a restatement should not be undertaken on its own account.

[56] Cf. J. R. Hicks, "A Suggestion for Simplifying the Theory of Money," *loc. cit.*, p. 3.

[57] So P. N. Rosenstein-Rodan, "The Co-ordination of the General Theories of Money and Price," *Economica*, III, N.S. (1936), 269 ff.; and for a similar suggestion that Wicksell's argument concerning the "natural rate" is an attempt "to combine price theory and monetary theory," see B. Ohlin's Introduction to *Interest and Prices*, xiv. It is not to be supposed, on the other hand, that *only* the "two streams of thought" indicated are relevant to the issue under discussion. Cf. what is said on this matter on pp. 491 f., below.

[58] The issues involved, which revolve about the concept of a "natural rate of interest," are much too complicated to be dealt with adequately in the present study. The writer hopes, however, to publish in the not-too-far-distant future a very extended treatment of the subject.

[59] Cf. above, p. 176, n. 53.

entirely different from that which is represented by the use of supposedly "mechanical" quantity-equations, is part of the body of doctrine lying behind these equations. The relation, again, is not that of two mutually exclusive sets of analytical devices, but rather of successive accretions to a single body of doctrine, in the way in which flesh and clothing are accretions to the underlying skeleton. Without the skeleton, although the flesh and clothing may be built into a satisfactory structure by an artist who knows his anatomy well enough to dispense with sketching in the skeleton each time as he paints, they threaten, in the hands of those less well-grounded in fundamentals, to collapse into a shapeless mass at the first contact with the refractory forms that the problem of price-determination takes on in the real world.

There is therefore something to be said for establishing with some clarity the precise nature of the relationship between the theory of the rate of interest as a factor affecting general prices and the variables included in the "quantity equations," which themselves claim to summarize the forces determining general prices. The performance of this task, indeed, is made unavoidable by virtue of the two-fold circumstance that Mr. Keynes, in the *Treatise,* charged Wicksell, in particular, with having failed to accomplish it, and that he developed his own argument in terms which suggested that the older "quantity equations" were either not capable of indicating the steps by which changes in the rate of interest could be shown to affect general prices, or would lead to wrong conclusions with respect to the nature of the process involved, whereas the equations of the *Treatise* were held to be superior in both respects.[60]

III

Bank-Rate and the Quantity Equations

At more than one point in the *Treatise,* Mr. Keynes argued that "the ordinary Quantity Equation" does not furnish us with "a simple and direct explanation why a rise in

[60] For Keynes's comment on Wicksell, in this connection, see the *Treatise,* I, 186, and cf. below, pp. 181 f. References to the other parts of Keynes's argument which are here indicated are given throughout the pages which follow.

the Bank-rate tends, in so far as it modifies the effective rates of interest, to depress price-levels"; and he alleged further that in dealing with "the *modus operandi* of Bank-rate," we are "compelled to discard" equations of the older type "when we advance to the later stages of the argument and attempt to analyze the actual monetary problems of the day—the problem of the Credit Cycle, for example—because we discover that they are quite ineffective for handling the elements which most matter." [61] This would be a serious accusation, if it could be established; and it was more serious, at the time it was advanced, because Keynes claimed that the superiority of his own Fundamental Equations was shown by their effectiveness precisely at the points at which the older equations showed themselves to be ineffective.[62]

It is well, however, to establish precisely what could have been meant by the statement that *"the ordinary Quantity Equation"* does not furnish us with the desired "explanation." If by this is meant simply that there is no explicit term for "Bank-rate" in the "ordinary Quantity Equation," of course the statement is literally true. It is also, however, quite irrelevant; for, as we have insisted, the usefulness of the "quantity equations" is to be tested, not by a blind manipulation of the terms included in the equations, but by the usefulness of the body of analysis which lies behind each of these terms.

This is not to say, of course, that an explicit term for "Bank-rate" *could* not be introduced into the "ordinary Quantity Equation" in the form of an expression, say, designed, to convey the notion that the magnitude of the M' of the Fisherine equation is a function of the difference between Bank-rate and the anticipated rate of profit to be made by the use of a bank loan. As early as 1913, in fact (that is, only two years after the publication of Fisher's *Purchasing Power of Money*), Professor Fanno, in presenting an expression designed to represent the demand for bank-loans, and based on the theory that this demand derived from the pecuniary volume of transactions (the PT of the Fisherine equation), proceeded to insert in this expression, along with a term corresponding to the term for "prices" in the Fisherine equation, the expression $\phi\ (s, i, t)$, by way of indicating that the demand for bank-

[61] *Treatise*, I, 155, 222.

[62] See, for example, what is said in the *Treatise*, I, 221 f., on the "main advantage" of the Fundamental Equations of the *Treatise* over the older quantity equations.

loans, after allowance for the quantity of media of payment (represented by the equivalent of Fisher's $MV + M'V'$) already available, would be a function of the relationship between the rate of profit to be made by the use of a bank-loan (i) and the rate of discount, or "Bank-rate" (s), over the period of time (t) during which a discrepancy between i and s persisted.[63] It would obviously be easy, following this suggestion—though it was not the method of Professor Fanno himself—to write the M' of the Fisherine equation in the form $M' \cdot \phi\ (s, i, t)$, by way of indicating that the quantity of "bank-money" created would be a function of the discrepancy between i and s, over the period indicated.[64] The point to be made here, however, is that the argument summarized by Professor Fanno's expression $\phi\ (s, i, t)$ is in all cases to be regarded as part of the analysis "lying behind" the term M', and that it was explicitly recognized as such by Fisher, even though the latter, in presenting his own Quantity Equation, did not provide an explicit notation to cover the point in question.[65]

It is, in any case, difficult to see how Mr. Keynes could have meant that it was a fault of the older Quantity Equations that they did not include a specific term for "Bank-rate"; for, as he was himself fair enough to admit—though in a different context—"Bank-rate does not appear explicitly as a factor" in his own Fundamental Equations either.[66] When he went on to say that Bank-rate "cannot, therefore, affect price-levels directly but only indirectly through its

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[63] See Fanno, *Le banche e il mercato monetario,* 220; and cf. the same author's "Die reine Theorie des Geldmarktes," *loc. cit.,* 33.

[64] Fanno's own method is to be explained by the fact that his immediate problem was the statement of the forces determining the demand for and supply of bank loans, rather than of the forces determining the magnitude of the variables included in equations of the Fisherine type. It was therefore designed to call attention to the fact that the "supply" side of his general equation for the "money market," while, like the "demand" side, it was stated in terms of a given amount of "bank money," would reach that amount as a result of the conditions of demand for bank loans, for which the factors summarized by the expression $\phi(s, i, t)$ were held to be directly relevant. See, for example, "Die reine Theorie des Geldmarktes," p. 59. In the earlier formulation given in *Le banche, etc.* (p. 282), a term for the "rate of discount" (s) appeared also in the formula representing the "supply" side (cf. also *ibid.,* pp. 263, 271, 274, 280, 282, 311), but in a setting different from that suggested by the expression $\phi(s, i, t)$.

[65] For examples of Fisher's recognition of the influence of the "rate of interest" on the magnitude of $M'(= c \cdot Mr)$, and of his recognition, in general, of the relation between Bank rate and "the business man's profits" as a factor affecting M', see "The Role of Capital," *loc. cit.,* 519, and *The Purchasing Power of Money,* 59. It is worthy of note that in both cases Fisher referred to the argument of Wicksell (cf. above, p. 173, n. 40) in terms which clearly suggest that he regarded the argument of the latter as part of the analysis "lying behind" M'.

[66] Cf. the *Treatise,* I, 185.

influence on one or more of the factors which do appear in the Fundamental Equation," he was stating the relation between "Bank-rate" and a formulation of the forces determining general prices in terms applicable, without the slightest degree of modification, to "the ordinary Quantity Equation." If, therefore, there are grounds for charging the older "quantity equations" with a failure to provide the desired explanation of the relationship between changes in Bank-rate and prices, and for attributing a superiority in this respect to an apparatus such as that represented by the Fundamental Equations of the *Treatise*, these grounds cannot be found in the mere failure of the "quantity equations" to include a specific term for Bank-rate; in this respect, the two sets of equations are exactly on a par. The only reasonable interpretation of Mr. Keynes's accusation, therefore, would be that the "quantity equations" do not include terms for the variables which are affected by changes in Bank-rate, and which in turn affect general prices; and that therefore any attempt to "link up" changes in Bank-rate with the "quantity equations" must inevitably be as far from successful as Keynes himself believed Wicksell's efforts in this direction to have been.

Keynes's statement that Wicksell "was not successful . . . in linking up his Theory of Bank-rate to the Quantity Equation" has sometimes been discussed as if it meant that Wicksell's lack of success in this respect lay in a failure to indicate specifically the way in which the variables in "the Quantity Equation" would be affected by "Bank-rate" and the way in which these variables, in turn, would affect prices. Indeed, an attempt has been made to answer Keynes on this point by calling attention to those parts of the commentator's own analysis that might be regarded as filling the supposed gap.[67] It must be clear, however, that this cannot be what Mr. Keynes meant by his reference to Wicksell's lack of success in "linking up his Theory of Bank-rate to the Quantity Equation." For, as we have seen, Keynes had argued explicitly that "quantity equations" of the older type were ill suited to the purpose of establishing the relationship between changes in Bank-rate and changes in price-levels; and he cannot have meant to blame Wicksell for having failed to accomplish something that was either not worth

[67] See especially, in this connection, Fanno, "Cicli di produzione, cicli del credito e fluttuazioni industriali," in *Giornale degli economisti*, LXXI (1931), 364, n. 3. For references that would support Fanno's claim (*loc. cit.*) to have "coordinated the theory of Wicksell . . . with the equation of exchange [of Fisher]," see above, p. 180, nn. 63 and 64.

accomplishing or was incapable of accomplishment. He must have meant that the attempt by Wicksell to "link up" his "Theory of Bank-rate to the Quantity Equation" was not "successful" precisely in the sense that it demonstrated the unfitness of equations of the "quantity" type for the purposes of tracing the relationship between Bank-rate and the price-level; it showed, in other words, that the task was impossible of accomplishment.

Upon this interpretation—surely the only fair, as well as the only sensible one—of Keynes's criticism of Wicksell, the apparently contradictory statements of the former with respect to the latter are easily resolved. As is well known, Keynes regarded Wicksell—on grounds the validity of which will be examined below—as having, in some respects, come "closer" to "the fundamental conception" with respect to the *modus operandi* of Bank-rate which was supposed to be one of the features of the argument of the *Treatise* that differentiated it from "the traditional doctrine" on the subject, than did such writers as Marshall and Hawtrey.[68] At the same time, he regarded it as a shortcoming of Wicksell's treatment that, in other respects, it seems "to be reduced to practically the same thing" as the "traditional" proposition alleging that "the level of Bank-rate determines the volume of bank-money and hence the price-level"—in other words, to precisely the "link-up" with the "Quantity Equation" which Keynes himself regarded as misleading and inadequate.[69] Obviously, the last accusation cannot be easily reconciled with the charge that Wicksell was unsuccessful in "linking up the Theory of Bank-rate to the Quantity Equation" if the charge is interpreted as meaning that Wicksell had failed to show *how* changes in Bank-rate are related to changes in prices through changes in the Quantity Equation. It becomes perfectly reconcilable with that charge, on the other hand, when the latter is interpreted as meaning that Wicksell's error lay in the very *attempt* to link up the theory of Bank-rate with the Quantity Equation.

Fortunately for our present purpose, there is no difference between Mr. Keynes and supporters of "traditional doctrine" with respect to our understanding of the content of that particular part of "traditional doctrine" having to do with the nature of the tie-up between "the Theory of Bank-rate" and "the Quantity Equation." The tie-up is, in fact, represented by what Mr. Keynes himself characterized as the first of the "three distinct strands of thought" that he found in "the traditional doctrine"—namely, the proposition that such changes in general prices as could be attrib-

[68] Cf. the *Treatise*, I, 196.

[69] *Treatise*, I, 196 f. On Keynes's understanding of the "traditional doctrine," as represented by the positions of Marshall, Hawtrey, Cassel, and others, cf. the *Treatise*, I, 188, and also what is said on pp. 189 ff., below.

uted to changes in Bank-rate would come about as a result chiefly of the changes in "the quantity of Bank-money" (the *M'* of our Quantity Equation) which could in turn be attributed to changes in Bank-rate.[70]

The fact that we are concerned here with the defense of something that may properly be called "the traditional doctrine" on the subject, rather than with minor variants of, and departures from, this "traditional doctrine," and the further fact that Keynes himself held that the "traditional doctrine" envisaged the tie-up between the "Theory of Bank-rate" and the Quantity Equation as being concerned primarily with changes in *M'*, should make it unnecessary to deal in detail with those commentators on Wicksell, usually regarded as the most outstanding among all those associated with the development of the "traditional doctrine," who have seen in Wicksell's exposition either some fundamental ambiguity as to which of the terms of the Quantity Equation was to be thought of as being directly associated with changes in Bank-rate, or an implicit denial of the proposition that, of these variables, the most important, for the purpose in hand, is *M'*. The most that need be done at this juncture is to indicate the nature of the reasons for believing that the differences between Wicksell's position, in this respect, and that which was regarded by Keynes as representing the "traditional doctrine" are largely nonexistent.[71]

It has, for example, been suggested that Wicksell erred in trying "to find a *direct* connection between too cheap bank-credit and rise of the prices of goods," instead of inserting "plentiful creation of money as a link between them.[72] In fact, however, there are abundant passages in Wicksell's writings which show that he did think of the "plentiful creation of money" (that is, bank-credit, or the *M'* of our equation) as being the crucial link in the process.[73] This can easily be demonstrated

[70] *Treatise*, I, 187 ff. In the passage on p. 187, the word "quantity" in the phrase "quantity of bank money" is italicized: a matter the possible significance of which is discussed below (see p. 186, and especially n. 78 thereto). On the other "strands" which Keynes thought he had discovered in the "traditional doctrine," see below, pp. 216 ff., 285 ff.

[71] In what follows, no attempt is made to discuss the issues raised, in this connection, by the suggestion that acceptance of the concept of interest as a "capitalization factor" implies a conflict with the "first strand" in the "traditional doctrine," as summarized by Keynes. See, on this matter pp. 232 ff., below.

[72] So, T. Greidanus, *The Value of Money* (1932), 83.

[73] Cf., in this connection, the remarks of Ellis, *German Monetary Theory*, 156 f., 304. The references to *Interest and Prices* given in the text above (for page references see below, p. 185, n. 75), may be regarded both as providing support for Ellis's statement that "the central theorem of both *Geldzins und Güterpreise* and the *Vorlesungen* is certainly that bank rate controls the price-level through [its effect upon] the amount of available purchasing power" (Ellis, *op. cit.*, 304), and as providing material for the interpretation of the same author's earlier statement that "Wicksell frequently omits explicit mention of *quantity* of credit in discussing cyclical variations" (*op. cit.*, 157; italics Ellis's). The references may be regarded

even if we confine ourselves to the most famous of Wicksell's works on the subject—his *Interest and Prices* (*Geldzins und Güterpreise*). It is possible, for example, to cite passages from that work in which:

1. The relation between the rate of interest and a given rise in general prices was discussed in terms of an increased "quantity of money," or—under a "developed credit economy"—of "money and credit."

2. The relation between interest and prices was held to reside in the fact that when "the money market is in a fluid condition producers are provided with ample funds."

3. Wicksell took Marshall to task—on grounds, to be sure, that are anything but clear—for having laid "too much emphasis on the *direct* influence that he alleges is exerted by the magnitude of banking reserves on the rate of interest and consequently on prices"—instead, presumably, of stressing the link which is represented by the expansion of *M'* on the basis of these reserves.[74]

4. He defended Ricardo's "habit of referring to a restriction or expansion of the banks' note issue rather than to a rise or fall in their rate of interest" by calling attention to the fact that what really matters is the restriction of credit (that is, of the "banks' note issue," or of their deposits) rather than the movements in the rate of interest, which are merely a means to this end.

5. In discussing the central issue involved in the Currency and Banking School controversy, Wicksell put all emphasis upon the power of the banks to increase or diminish "the quantity of means of payment" as

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as a commentary also upon the implication by Keynes (*Treatise,* I, 196 f.) that one must go to the "form in which . . . [Wicksell's theory] has been taken over from him by Professor Cassel" instead of to Wicksell's writings themselves, in order to find evidence of a belief "that the level of Bank-rate determines the volume of bank-money and hence the price-level," as well as upon the statement of Professor Ohlin that Wicksell's analysis represented an attempt to escape "from the tyranny which the concept 'quantity of money' has until recently exercised on monetary theory" (*Introduction to Interest and Prices,* xiv; cf. also p. 221, n. 43, below).

[74] An alternative interpretation of Wicksell's criticism of Marshall would regard him as charging the latter with having supposed that *all* reductions in the money rate would set in motion a rise of prices, regardless of whether these were absolute reductions or were reductions relative to the anticipated profit to be made by the use of a money loan, that is, the "natural rate," in one of its more satisfactory senses. (See, in this connection, Wicksell's objection to the assumption of any "direct relation," in this sense, between the discount rate and price movements, in *Interest and Prices,* 107.) In that case, however, it would be still more difficult to justify Wicksell's criticism, in view of Marshall's emphasis on the importance of the relation between the rate of discount and what he called "the profitableness of business." See, for example, Marshall's answers to questions 9651, 9675, 9678–9686 (Marshall, *Official Papers,* 41, 48 ff.). For evidence, on the other hand, of Marshall's recognition of the fact that it was the credit *created* on the basis of bank reserves, as the result of a given position of the rate of discount, that affected prices, rather than by the state of bank reserves or the rate of discount *per se*, cf. the answers to questions 9639, 9641, 9650, 9676, 9677.

the method by which they could be expected to influence general prices.

6. He identified the question of the "influence of 'the banks' . . . on prices" with the question, in the first instance, of their influence "on the circulation of money," as represented by their power to "increase the extent of . . . lending . . . by means of notes or by means of cheques."

7. He argued that the extent of an upward movement in general prices which was inaugurated by too low a rate of interest would depend upon the extent to which "a precious metal or some other material substance serves as a monetary basis"—in other words, obviously, upon the extent to which the banks are able to create money-substitutes.

8. He made it clear that whether or not there would be "any alteration in the general level of prices" would depend upon whether "money is obtainable [from banks] in any desired quantity."

9. He argued that the reason why the rate of interest could be expected to affect general prices is precisely that, under "an elastic monetary system," the "supply of money" might be said, as a result of the banks' power to "create" deposits (our M'), to accommodate itself to the increase in the "demand for money loans" that would be expected to result from too low a rate of interest.

10. His "systematic exposition of the theory" presented in *Interest and Prices* made it clear that a difference between Bank-rate and the anticipated profit to be made by the use of bank loans becomes a factor affecting general prices only when, in the face of an increased "demand for loans" from banks, the banks provide "dealers" with an "increased sum in the form of money (bank drafts)."

11. He made the movement of prices, in the face of an increase in total output—as a result, say, of an increase in the "efficiency of production"—depend upon what happens to the "quantity of money lent by the banks," the latter in turn depending upon the relationship between the "money rate of interest" and the "natural rate."

12. He made it clear that discount policy gives promise of securing his desired "stability of prices" because it is through discount policy that the banks are able to perform what he regarded as their "prime duty"—namely, the duty "to provide the public with a medium of exchange." [75]

In the second place, it is to be noted that the mere fact that passages may be cited in which Wicksell speaks of changes in bank rate as affecting "velocity" does not contradict the interpretation of him as having held that Bank-rate would affect prices primarily through its effect upon M'. For one thing, the citation of passages in which Wicksell spoke of changes in "velocity" resulting from changes in Bank-rate loses all its force, for our present purpose, whenever it can be shown that the "velocity" that Wicksell had in mind was what he called "virtual

[75] The passages indicated are to be found on the following pages of *Interest and Prices:* (1) xxiv; (2) 27; (3) 76; (4) 82 n.; (5) 83 f.; (6) 85; (7) 101 (cf. also pp. 136, 139); (8) 105; (9) 110, 135 (cf. Wicksell's *Lectures*, II, 197); (10) 144; (11) 152; (12) 190.

velocity."[76] For Wicksell's "virtual velocity," when translated into the terms of our Quantity Equation, includes not only V, but also $c(= M'/M_r)$. It would be nonsense, therefore, to insist that Wicksell thought of Bank-rate as affecting "velocity" *rather than* M' whenever by "velocity" is meant "virtual velocity"; for an effect of Bank-rate upon M' is, in Wicksell's terminology, necessarily the same thing as an effect upon "virtual velocity."[77]

In the third place, to show that Wicksell thought of Bank-rate as affecting "velocity," even when the latter term is used in its narrower and more commonly accepted sense, is not to show that Wicksell would have denied that Bank-rate would also—and more significantly—affect M'.[78] We shall deal with the relationship between the rate of interest and "velocity," in the more conventional sense of the term, in later chapters of this study.[79] At this point, when our only concern is to deal with that part of the analysis lying behind M' which is concerned with the relationship between Bank-rate and M', it is not necessary to do more than to emphasize the fact that an interest in the possible effects of Bank-rate upon other variables in the Quantity Equation than M' does not imply a denial of, or lack of interest in, its effect upon M'.[80]

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[76] For examples of such a usage, see Wicksell, *Interest and Prices,* p. xxiv, xxx, 42, 62, 65. The usage, which has a history antedating Wicksell by many years, appeared also in Wicksell's earlier "Der Bankzins als Regulator der Warenpreise," *loc. cit.,* 232 f., 237, as well as in his later "The Influence of the Rate of Interest on Prices," *loc. cit.,* 214. Cf. also Wicksell's *Lectures,* II, 27, 67 ff., 87, 145, 149, 159, 169, 172, 215.

[77] Cf., in this connection, Ellis, *German Monetary Theory,* 157, 304. It should hardly be necessary to emphasize the fact that to point to Wicksell's usage with respect to the concept of "virtual velocity" is by no means to imply acceptance of that concept as a helpful analytical device. Reasons for rejecting the concept have already been advanced by several writers, of whom Ellis (*ibid.,* pp. 157, 188) is perhaps the most recent. A detailed discussion of these reasons, with most of which I am in sympathy, must be left, however, to a subsequent publication of mine on the *Velocity of Circulation of Money* (see below, p. 290, n. 1). Cf., in the meantime, what is said on the matter on pp. 366 ff., below.

[78] For an example of Wicksell's recognition of the fact that the rate of interest may affect "velocity" in the narrower sense of the term, see "Der Bankzins, etc.," *loc. cit.,* p. 241 n. (The somewhat cryptic reference to the effect of a high rate of interest in also "furthering the creation of new credit instruments" is made clearer by the corresponding passage in *Interest and Prices,* p. 119.) Cf. also *Interest and Prices,* xxiv, 119; and the *Lectures,* II, 180, 197. It is worth noting, in this connection, that Fisher, to whose recognition of the relationship between the rate of interest and M' we have already called attention (cf. p. 173, n. 40 above), pointed with equal emphasis to the possible connections between the rate of interest and V. See, for example, "The Role of Capital, etc.," *loc. cit.,* 518, n. 1.

[79] The circumstance that the aspect of "velocity" referred to is in fact that which is associated with the concept of "liquidity preference" makes it necessary to defer its discussion to Volume II. See, however, p. 483, below.

[80] In fact, of course, Wicksell's readiness to recognize the possibility that changes in the rate of interest might affect variables other than M' went beyond his recognition of the possibility of effects upon V. See, for example, his recognition of the possible effects of these changes upon that

There is just as little reason, finally, for laying any great amount of stress upon a type of passage in Wicksell's writings dealing with issues that are alleged to have "puzzled" not only the readers who have commented upon it, but also "Wicksell himself": namely, the passages in which it was alleged that even "without the intervention of banks"—and therefore without any change in the quantity of "bank money" (M'), since the latter would be nonexistent—general prices might rise as the result of a discrepancy between the market rate of interest and the anticipated profit to be made by the use of a money loan.[81] All that these passages prove is that, in a setting in which no banks exist, an increase in the total amount of money spent may come about through the effect of a discrepancy between market rate and "natural rate" (in the sense indicated) upon some variable other than M'—say, upon V.[82] They would certainly not prove that Wicksell was for a moment prepared to deny that in a setting in which banks did exist—or, as he put it, in which "organized credit," including especially the "activity of banks," was an important factor—the tie-up between Bank-rate and prices was to be found chiefly in changes in what would correspond to the M' of our quantity equation. This is made particularly clear by the context of the passage in which the type of case under discussion appeared: for this context shows that Wicksell was merely tracing the possibilities of a rise in general prices under the successive conditions of (1) "private credit"—that is, "credit between man and man" rather than bank-credit—without the possibility of bringing into use funds that would otherwise have been left unspent; (2) "private credit" with that possibility—that is, the case under discussion, involving an increase in V; (3) "organized credit . . . under the existing monetary system"—that is, with banks, whose powers to create M' (or, in Wicksell's terminology, to increase the "virtual" velocity of circulation of money) are limited by the need for holding metallic reserves; and (4) "organized credit" under a "pure credit system"—that is, one in which such reserves need not be kept, "at least as far as the internal market is concerned." The whole matter, indeed, is summed up in a passage in *Interest and Prices*, in which instances of the type under discussion were regarded as representing merely a "special case of our general proposition" with respect to the effect of a discrepancy between market-rate and "natural rate" upon the dimensions of the stream of money-expenditure.[83]

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component of T which we shall call the "rate of sale" of goods (cf. below, pp. 563 ff.), in *Interest and Prices*, p. 88; cf. also his *Lectures*, II, 113, and his "Hinauf mit den Bankraten!" *loc. cit.*, 756.

[81] Thus, B. P. Adarkar (*The Theory of Monetary Policy*, 27) adduces, in this connection, a passage found on II, 193 of the English version of Wicksell's *Lectures*.

[82] Wicksell himself was explicit in insisting that what was involved, in cases of the type under discussion, was an increase in V and not an increase in the amount of "substitutes for money" (that is, M'). Cf. *Interest and Prices*, 59.

[83] Cf. *Interest and Prices*, 119.

Acceptance of the proposition that changes in Bank-rate may be expected to operate upon prices through changes in the *M'* of our Quantity Equations would, one would have thought, *ipso facto* imply acceptance of the usefulness of these Quantity Equations for the purpose of tracing the *modus operandi* of Bank-rate upon prices. As we have seen, however, it was precisely the usefulness of the Quantity Equations for the purpose in hand that Mr. Keynes, at the time he wrote the *Treatise,* called into question. Clearly, any attempt to justify such a position would necessarily involve a demonstration that changes in the "quantity of bank money" do not in fact represent a principal link in the chain of events whereby changes in Bank-rate may be expected to affect general prices; and indeed the argument of the *Treatise* with respect to the point under discussion was couched in such terms as to suggest that it was precisely such a demonstration that Mr. Keynes undertook to provide.

Mr. Keynes did not argue in so many words, to be sure, that Bank-rate could not affect prices by affecting the "quantity of bank-money." On the contrary, we were told not only that "the association of changes in Bank-rate with changes in the supply of bank-money is often or generally a factor in the situation," but also that "every effective alteration of Bank-rate must be associated, except in so far as it is balanced by simultaneous alterations in other factors, by *some* alteration in the quantity of bank-money."[84] We were also told, however, that it is not "useful" to say that "a change in Bank-rate changes price-levels *because* it is associated with changes in the quantity of bank money"; that, indeed, "we shall be misled if we lay much stress on the changes in the total quantity of money when we are trying to trace the causation and the stages of a transition"; and that by the time we have added "the numerous qualifications and complications" which have to be introduced, "the theory will really have become a different one."[85]

These charges are sufficiently serious to warrant close examination. When such an examination is made, however, it becomes clear that Mr. Keynes's quarrel was not

[84] *Treatise,* I, 189, 216.
[85] *Treatise,* I, 189, 217, 219.

with the suggestion that changes in Bank-rate, insofar as they can be shown to affect general prices, operate chiefly through changes in the quantity of bank-money—which is the central question involved in a dispute as to whether the Quantity Equations do or do not include terms for the variables affected by changes in Bank-rate, and which in turn affect the price-level—but was rather with arguments that not only are not concerned with this segment of "the traditional theory" with respect to the mechanism whereby changes in Bank-rate affect prices, but are not, in fact, part of generally accepted theory at all.

IV

The "Traditional" Theory of Bank-Rate

It is certainly not part of generally accepted theory, for example, despite Mr. Keynes's clear implication to the contrary, that "the association of changes in Bank-rate with changes in the supply of bank-money" is to be regarded as "invariable," in the sense, for example, that every change in Bank-rate may be expected to be inversely correlated with changes in the "supply of bank-money." [86] It is true that, from the time of Tooke to our own day, there have been those who have thought that they were exploding the "classical" doctrine with respect to the effect of Bank-rate upon both the quantity of money and general prices—or, in Tooke's own words, the "commonly received opinion" that "a low rate of interest is calculated to raise prices and a high rate to depress them"—by presenting empirical evidence to show, for example, that rises in Bank-rate are not necessarily accompanied by declines in the amount of borrowing from banks.[87] It is equally true, however, that it should

[86] *Treatise,* I, 189.

[87] See especially Chapter XIII of Tooke's *Inquiry into the Currency Principle,* on p. 77 of which appears the statement quoted in the text. On Tooke's earlier views, see T. E. Gregory's Introduction to the 1928 reprint of Tooke and Newmarch's *History of Prices,* 24 ff. Typical of his later position, however, is the passage on pp. 83 f. of the *Inquiry* in which empirical "evidence" is adduced in refutation of the "commonly received opinion." Cf. also the *History of Prices,* III, 155, 159. Any discussion, or even any extensive citation, of writers subsequent to Tooke who held essentially his position on the matter must be left to another occasion. Attention may be called, however, to the particularly extreme statement in J. S. Lawrence's

have been obvious from the very beginning that this "refutation" was, as so often in such cases, not a refutation of "classical" doctrine, but of an absurd caricature thereof.

It is, indeed, characteristic of most of the authors, subsequent to Tooke, who have attacked what they regarded as the "classical doctrine" on grounds virtually identical with those advanced by Tooke, that they should have given no indication of an awareness that this type of argument had been advanced almost a century ago, to say nothing of an awareness of the fact that the argument had been answered. It is equally characteristic, on the other hand, that the defenders of the "classical doctrine" should have been sufficiently conversant with the literature to be aware that attempts to refute the "classical doctrine" did not begin in their own day but went back at least as far as Tooke.[88] It is likewise typical that, in stating their rebuttal of the supposed refutation, they should not have asserted that this rebuttal called for a drastic break with tradition, but were perfectly ready to grant, if not to insist, that it was inherent in a correct statement of the "classical" tradition.[89]

It will be observed that no pretense is here made that the "classical doctrine" has *always* been stated in such a way as to make clear the irrelevance, for the purpose in hand, of empirical evidence of the type advanced by Tooke and his followers.[90] It must be remembered, however, that what defenders of the "classical tradition" on any subject in eco-

The Stabilization of Prices ([1928] 387 ff., 420 ff.) of a position which, in its use of empirical evidence as "proof," is essentially identical with that of Tooke. It is characteristic that Lawrence's presentation should have been regarded by others who are out of sympathy with "the bank rate theory of price control" as showing conclusively "how little" this "theory" is "corroborated by facts," and as providing "partial confirmation" of Tooke's argument that a lowering of the rate of discount should be expected to lower prices, and not to raise them. See, for example, J. S. Lewinski, *Money, Credit and Prices* (1929), 54 n., 68 n.

[88] See, for example, the references to Tooke in Wicksell, "Der Bankzins, etc.," *loc. cit.*, 235 n.; *Interest and Prices*, 88; and *Lectures*, II, 182, 202.

[89] It is sufficient, in this connection, to call attention to Wicksell's acknowledgment of the similarity between his own doctrine and that of Thornton and Ricardo. Cf. above, p. 174, and notes 44, 45, and 46 thereto.

[90] The "followers" of Tooke to whom reference is here made are those who adopted his supposed empirical disproof of the "classical doctrine" with respect to the effect of the rate of discount. Actually, not all of those who accepted Tooke's general "Banking School" position accepted his argument on the point under discussion. See, for example, the references to Adolf Wagner on p. 192, below.

nomics have regarded as worth defending are not the chance utterances of the "crowd of unqualified persons," as Cairnes called them, who have contributed to "the miscellaneous literature of economic discussion" in the past, but "the doctrines of the science as expounded in the works of acknowledged masters." [91]

The two "acknowledged masters" whose utterances are important in the present connection are Thornton and Ricardo. The former made it perfectly clear that it was not movements in the rate of interest alone that would determine whether or not there would be an increase in the demand for "loans at the Bank," and therefore an expansion of credit: the question, he insisted, turned "principally on a *comparison* of the rate of interest taken at the bank" with "the mercantile or other gain to be obtained by the employment of the borrowed capital." [92] Ricardo made it equally clear that "the applications to the Bank for money" depend, not upon the movements in the rate of interest alone, but upon "the comparison" between the rate of interest and "the rate of profits that may be made by the employment" of the money borrowed.[93]

It goes without saying that Thornton and Ricardo are not the *only* "acknowledged masters" who might be cited in this connection. One thinks here not only of Wicksell, to whose utterances in this connection references are given below, but also of Marshall, who, as we have seen, regarded, as the factor which may be expected to affect the amount of borrowing, not the absolute height of the rate of discount, but its height

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[91] Cf. Cairnes, "M. Comte and Political Economy," *loc. cit.*, 286.

[92] See, for example, the quotation from Thornton's *Inquiry into the Nature and Effects of the Paper Credit of Great Britain* given by Hayek in the latter's *Prices and Production*, p. 13; cf. also Gregory's Introduction to Tooke and Newmarch's *History of Prices*, p. 44, where a reference is given also to a related proposition advanced in the Bullion Report.

[93] Ricardo, *Principles of Political Economy and Taxation*, Chapter XXVII (p. 352 of the Gonner edition). Cf. also *Minor Papers on the Currency Question*, edited by Hollander, pp. 57, 85. It may be remarked in passing that it is passages of this type that must be regarded as the starting point for the construction of a satisfactory theory of Bank rate, rather than those in which, by referring to the "natural level" of the rate of interest, Ricardo has seemed to some commentators to have anticipated the modern concept of a "natural rate of interest," with its various, and by no means equally acceptable, connotations. See, in this connection, Hayek, *Prices and Production*, 14, and G. Kepper, *Die Konjunkturlehren der Banking- und der Currencyschule* (1933), 27.

relative to "the profitableness of industry."[94] Nor must it be thought that there was a complete blank between Thornton and Ricardo, at one end of the century, and Wicksell and Marshall at the other. J. R. McCulloch, for example, in successive publications extending over the period from 1818 to 1859, continued to repeat the doctrine virtually in the form in which it was stated by Ricardo.[95] Continuity with the next generation of economists was immediately established, moreover, when what was virtually the "classical" doctrine appeared in Adolf Wagner's *Beiträge zur Lehre von den Banken,* published in 1857—an episode that is the more notable because of Wagner's acceptance otherwise of Tooke's general "Banking School" position.[96] Wagner was, indeed, quite explicit in insisting that there was a "gap" in the Banking School argument which alleged that since "bank notes get into trade only through demand," one may conclude that "the issue of notes always presupposes a need for them," in the sense that the banks could do nothing to stimulate this demand.[97] After all, Wagner argued, it is possible for banks, within limits, "themselves to call forth or give rise to 'needs of trade.' " "For as soon as the rate of interest at which it is possible to obtain purchasing power from the banks is so low *in relation to the profit to be made thereby* that, after deduction of interest cost from the total profit and even after allowing for the risk-premium, an unusually high remainder is left, the low bank-rate will naturally induce many people to take loans from the bank."[98] It would be idle to deny that Wagner's exposition was hurt by his continued adherence to the less satisfactory parts of the banking school position.[99] It remains true, nevertheless,

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[94] Cf. the references to Marshall on p. 184, n. 74, above. Particularly striking, in this connection, is the footnote that Marshall felt impelled to add to his answer to question 9676 (*Official Papers,* 48). It is equally striking that Mr. Keynes, who quoted (*Treatise,* I, 188, n. 1) the passage to which the footnote was added, in support of his interpretation of Marshall, should not have quoted the footnote or have suggested that any particular significance attached to it.

[95] See, for example, the *Edinburgh Review,* XXXI (1818), 62; *A Note on Money* (reprinted from McCulloch's edition of the *Wealth of Nations*) (1827), 35; "A Treatise on Money" (reprinted, "with corrections," from the eighth edition of the *Encyclopedia Britannica,* in McCulloch's *Treatises and Essays* [2d ed., 1859]), 37.

[96] Wagner's support of Tooke's position on the relation between note-issue and the "demand" of the community for funds is too well known to require an extensive list of citations. Since, however, the citations which follow are from Wagner's *Beiträge,* attention may be called to the explicit acceptance of Tooke's general position which appears on p. 122 of that book.

[97] See Wagner, *Beiträge,* 237.

[98] Wagner, *Beiträge,* 237. Cf. also *ibid.,* p. 277: "Let us assume that because of the increased need for capital the rate of discount would ordinarily have had to rise, but that it is kept artificially low. . . . In that case the inclination to ask for credit would be greater than if the rate of discount were at its higher, and natural level; for the low rate would not then be in the right *relation to the putative* (*muthmasslichen*) *profit which could be made by the use of the loan.*" Italics mine in both cases.

[99] See, for example, his insistence, *Beiträge,* 238, that despite his general

that he recognized not only that "too low" a rate of discount would result in the creation of deposit-credit (included in the M' of our Quantity Equation), but also that the criterion as to whether a given bank-rate was too low was its "relation to the profit to be made" by the use of a bank-loan. This would in itself justify the practice of those who have regarded Wagner, so far as the concept of a "natural rate of interest" is concerned, as a precursor of Wicksell, who was of course essentially of the generation that followed that of Wagner.[100] Finally, since Mr. Keynes included Professor Pigou among the representatives of the "first strand" in the "traditional doctrine" with respect to the effect of Bank-rate upon prices—a "strand" which Keynes charged with having implied that the "association of changes in Bank-rate with changes in the supply of bank-money" is "invariable"—it may be pointed out that on the very page which Keynes cited from Pigou's *Industrial Fluctuations* in support of his interpretation, equal place is given to the "conditions of *demand*" for bank funds along with the conditions of supply, and also that the very part of Pigou's argument elsewhere which Keynes regarded as a "fourth factor" in the "traditional doctrine"—namely, what Pigou called the "expectations of profit" of business men—is proof that Pigou could not have regarded the "association of changes in Bank-rate with changes in the supply of bank money" as "invariable."[101] It

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argument with regard to the effect upon borrowing from banks of a rate of discount which is low relative to the profit to be made thereby, it was still not true that note-issue would become "excessive" or that a "*general* rise in the price of commodities" would result. Actually, of course, Wagner's position, while it is certainly difficult to defend as he stated it, was not quite so bad as it seems; its weaknesses are associated largely with Wagner's unwillingness to attribute to an increased note-issue a major responsibility for a rise in general prices, or to attribute to the latter a major role in the "exaggerated speculation" which he regarded as the inevitable result of "too low" a rate of discount.

[100] See, for example, E. Sjöstrand, *Centralbankens väsentlige Funktioner* ("The Essential Functions of Central Banks," Stockholm, 1910), 57, 129. The seeker after parallels between the discussion of Wagner, in his *Beiträge*, and the discussion that has grown up about the concept of a "natural rate" since Wicksell, may note—with all due warning in view of the looseness of much of Wagner's exposition—(1) his use of the concept of a "correct" rate of discount (*op. cit.*, 238; cf. Pigou's "proper rate," in the latter's *Theory of Unemployment*, 212 ff.); (2) his suggestion that this rate would be "best fixed" by "competition" among banks (*op. cit.*, 238, 277; cf. Mises, *Theory of Money and Credit*, 398 f.); and (3) his insistence that Bank-rate could not remain for a considerable period out of adjustment with "the disposable capital of the country," since this "disposable capital" would then be "absorbed" to too great an extent in fixed investment, with the result that there would be a "shortage of available capital," an inevitable stoppage of enterprise, and a complete "loss" of the capital thus misdirected (*op. cit.*, 238 f.).

[101] For Keynes's treatment of Pigou in connection with the "first strand" of the "traditional doctrine," see the *Treatise*, I, 188; and for his treatment of Pigou as representative of those stressing the "fourth factor," see the *Treatise*, I, 199 f. For Pigou's treatment of the "expectations of profit" on the part of businessmen, see *Industrial Fluctuations*, 122; also 120 f. and Chapter XVII of the same work.

was hardly necessary to make the paradox more striking by *disapproving*, as Mr. Keynes did, of Pigou's emphasis on "expectations," despite Mr. Keynes's own emphasis, elsewhere in the *Treatise*, on the necessity of taking account of precisely these "expectations."[102]

In the light, indeed, of passages such as those quoted above from Thornton and Ricardo, one can only wonder why Tooke, for example, should have thought that he was refuting traditional doctrine—it is significant that he himself quoted neither of the two great writers in this connection—when he insisted that "the mere facility of borrowing" would not result in credit expansion if there were not simultaneously a "prospect of gain" from the use of money loans.[103] One can only wonder, also, why he should have thought that emphasis on the importance, for the amount that will be borrowed, of "the opinion of dealers or speculators, more or less exaggerated, of the prospect of markets" demanded that we deny that the "rate of interest or discount" has any significant effect upon this amount of borrowing.[104] All that Tooke was doing, in this part of his argument—we are not dealing here with his attempt to show that the "traditional" theory "is not only not true, but the reverse of the truth"—was to set an example of the treatment of "classical doctrine" which is only too relevant to the present discussion, as it is only too characteristic of the prac-

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[102] For examples of an emphasis, in the *Treatise*, upon the necessity for taking account of "expectations," see I, 159 f., 212, 264. Cf. also Keynes's "Reply to Dr. Hayek," *loc. cit.*, 11, where he attempted to reconcile his statements with respect to the relationship of the "natural rate" to the "existing psychology of the market" with other statements in the *Treatise*, according to which changes in the "natural rate" would be expected to occur only over long periods (see, *e.g.*, II, 204). The more general aspects of Keynes's handling, in the *Treatise*, of the element of "expectations," or "anticipations," as compared with the treatment accorded to that element in the *General Theory*, will be discussed in Volume II of this study.

[103] See Tooke, *Inquiry*, 78 f. Cf. the *History of Prices*, III, 159.

[104] *Inquiry*, 86 f. A similar set of passages—quoted, interestingly enough, by Keynes (*Treatise*, I, 195 f.)—had already appeared in the third (1840) volume of Tooke's *History of Prices*, 153 ff. Significant, in these earlier passages, are the admissions—extremely grudging, it is true—to the effect that "given the force of the motive [to borrow in the hope of making a profit], the extent to which it is acted upon is doubtless affected as regards persons who can buy only on credit, or who must borrow in order to be able to pay, by the greater or less facility of borrowing" (p. 154). Even such grudging admissions had disappeared, however, by the time of the publication of the *Inquiry* in 1844. For Keynes's own use of the *History of Prices* passage, see below, pp. 196 f.

tice of heretics generally: the practice, namely—in the words of a contemporary defender of classical doctrine in other fields—of plundering the classical writers and abusing them at the same time.[105]

One could not wish, indeed, for a better statement of the "classical" doctrine—in the form, as it happens, in which Wicksell stated it—than that presented by Mr. Keynes as one of the chief among those "numerous qualifications and complications" that were supposed to make the correct theory of the *modus operandi* of Bank-rate a "different one" from the traditional theory. "It is not," said Mr. Keynes, "strictly speaking, a change in Bank-rate as such which needs to be associated with a changed quantity of money, but a change in the market-rate of interest relatively to the natural rate."[106] Yet if this was the "essential element" that Mr. Keynes accused the "traditional doctrine" of overlooking, he was guilty of complete misrepresentation of the substance of that doctrine. It is, at any rate, difficult to see why he should have regarded it as one of the "numerous qualifications and complications" which, when introduced into the "traditional" account of the relationship between changes in Bank-rate and changes in the "quantity of bank-money," would result in a theory that is "different" from the traditional one on the subject.[107]

For those who are interested, as we are, in establishing the reasons for whatever "haze" may surround the central issues involved in the Theory of Prices, it is of some importance to point with emphasis to the example provided by Wicksell in the present instance. No one would deny—indeed, the contrary has been affirmed even by writers sympathetic to the argument of Keynes's *Treatise*—that, in stressing "over and over again, that it is not the absolute height of the market rate but its height relative to the 'normal' rate, that causes price fluctuations," Wicksell must be regarded as having made a "distinctive contribution" to the theory of

[105] Cf. L. Einaudi's Introduction to *Che cosa vuole America?* (the Italian [1934] translation of H. A. Wallace's *America Must Choose*), 13. On that part of Tooke's doctrine which represented an attempt to prove that "the commonly received opinion" was actually "the reverse of the truth," cf., below, pp. 249 ff.

[106] *Treatise,* I, 217 f.

[107] Cf. above, p. 188, n. 85, and the references to the *Treatise* there given.

the relationship between changes in Bank-rate and changes in general prices.[108] It is certainly no minimization of Wicksell's achievement, however—least of all in the eyes of those who are aware of the way in which the existing apparatus of economic theory has actually been built up—to point to the relevant passages in such writers as Thornton and Ricardo in proof of the contention that this particular "contribution" of Wicksell was not only not in conflict with what is properly to be regarded as "the traditional doctrine," but amounted, in fact, merely to an emphatic reiteration of what these writers had themselves explicitly stated. What is of particular interest for our present purpose is that Wicksell himself did not, even in his very earliest presentation of the doctrine under discussion, suggest that he was supplying an "essential element" which had been missing from the classical doctrine, and the addition of which would have the effect of transforming the classical doctrine into quite a "different" one. In his earliest, as well as his later writings, as we have seen, Wicksell explicitly called attention to the fact that his own doctrine was merely a "synthesis" of the "more authoritative views"—or, as we should say, of received doctrine—on the subject.[109]

Surely it is not unfair to call attention, in this connection, to the contrasting practice of Mr. Keynes. He, like Wicksell, found it necessary to deal with Tooke's reference to "actual experience" as demonstrating that "falling commodity prices are often associated . . . not with a rising rate of interest, but with a falling rate"; indeed, he devoted an entire section, in the second volume of the *Treatise,* to what he called the "Gibson Paradox," which was, in effect, precisely the type of positive correlation between prices and the absolute level of interest rates to which attention had been called by Tooke.[110] Mr. Keynes himself did not hesitate to say that "theoretical economists have mostly ig-

[108] So Adarkar, *The Theory of Monetary Policy,* 27. For examples of Wicksell's repeated emphasis on this point in his *Interest and Prices,* see pp. xxv, xxviii, 89, 107, 114 f., 167, 171, 190 of that work.

[109] Cf. above, p. 173, and especially note 43 thereto.

[110] For Keynes's reference to Tooke, see the *Treatise,* I, 196; and for his treatment of the "Gibson Paradox," see the *Treatise,* II, 198 ff., 386. In the latter passages, Keynes makes no reference to his earlier comment on Tooke; and in neither passage does he refer to the fact that Wicksell, in

nored" this type of evidence.[111] Yet he himself gave proof that they had not done so, even if he did not cite Wicksell in this connection, when, in attempting to "explain" the paradox, he advanced a "theoretical hypothesis" which, for purposes of the problem in hand, is virtually identical with the "theoretical hypothesis" that Wicksell had advanced over thirty years before; namely, that the "paradox" was due to a lag of movements of the market-rate behind the "natural rate" of interest.[112]

Within the limits of the present study, it is obviously impossible to discuss in detail the implications, for the theory of the "natural rate," which are inherent in its use for the purpose in hand.[113] It may, how-

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Chapter XI of his *Interest and Prices,* which was entitled "Actual Price Movements in the Light of the Preceding Theory," dealt precisely with what Mr. Keynes characterized as "one of the most completely established empirical facts within the whole field of economics," and which Wicksell himself summarized by the statement that "it has always been observed that, broadly speaking, a low discount rate accompanies *low* and not high prices, while an abnormally high discount rate is scarcely ever found to prevail except when commodity prices are *high*" (*Interest and Prices,* 165; italics Wicksell's).

[111] *Treatise,* II, 198. It should hardly be necessary here to point out that the case of Wicksell is not the only one which could be cited by way of demonstrating that "theoretical economists" did *not* "mostly ignore" the facts to which Keynes refers, whatever one may think of their attempts to explain those facts. One would have to include—to go no further—not only Irving Fisher, whom Keynes himself mentions in this connection (*Treatise,* II, 202), but also those whom Fisher himself listed as having anticipated his discussion of the issues involved in what he called "Appreciation and Interest." See Fisher's *Appreciation and Interest* (1896), 4 f.; also the same author's *The Rate of Interest,* 356 f. The lists of writers there given could be greatly extended.

[112] See Keynes, *Treatise,* I, 196 n., II, 203 ff., 386; and cf. Wicksell, *Interest and Prices,* p. 167; also his "Der Bankzins, etc.," *loc. cit.,* 239 f., and his *Lectures,* II, 205. On the relation of Keynes's explanation of the "Gibson Paradox" to Wicksell's, see Ellis, *op. cit.,* pp. 306 ff. It may be noted that the similarity extends even to a consideration—and a rejection, although on different grounds—of Fisher's explanation of the "paradox." Cf. *Interest and Prices,* p. 166, with the *Treatise,* II, 202. It need hardly be added that the case for a combination of the elements of Fisher's doctrine with a "natural rate" doctrine, for purposes of explaining the phenomenon under discussion, is anything but as hopeless as it appears from Keynes's discussion of Fisher. See, in this connection, C. O. Hardy, "Savings, Investment, and the Control of Business Cycles," *loc. cit.,* 394 n., and Hayek, "Reflections, etc.," II, *loc. cit.,* 38 f.

[113] Still less, obviously, is it possible to present here the reasons which would argue for an abandonment of the very *expression* "the natural rate," despite the argument which is implied in Mr. Robertson's delightful appeal to the authority of *Through the Looking-Glass* (cf. Robertson, "Industrial Fluctuation and the Natural Rate of Interest," *Economic Journal,* XLIV [1934], 650). It need only be stated here that the expression is used at this point and in the pages which follow in the sense of "the anticipated profit to be made by the use of a money loan," unless otherwise indicated.

ever, be pointed out that, according to one of the meanings that have been assigned to it, "the natural rate" is to be identified with "the anticipated profit to be made by the use of a money-loan."[114] If this meaning, rather than any of the others that have been attached to the concept, is used in explanation of the "Gibson paradox," it is certainly not true, despite the argument of some writers to the contrary, that to say that prices are rising or falling *because* the market rate is below or above the "natural rate" is merely "a recondite way of saying that prices are rising or falling."[115] This would be true only if the "natural rate" were *defined* as the rate, for example, that will keep the price-level from rising or falling.[116] It is, therefore, of some importance to note that the truly "classical" part of the theory with respect to the relationship between the interest rate and general prices did not use a concept of the "natural rate" in the sense of a rate designed to keep the price-level stable; instead, it contented itself with an insistence on the proposition that the extent to which a given rate of interest would result in an ex-

[114] For a partial list of other meanings that have been assigned to "the natural rate," cf. below, pp. 201 ff. For examples, however, of the use of the concept in the sense indicated in the text, it is not necessary to go outside the writings of Wicksell himself. See, for example, "Der Bankzins, etc.," pp. 236, 241; *Interest and Prices*, pp. xxv, 89, 120; "The Influence of the Rate of Interest on Prices," *loc. cit.*, 217; *Lectures*, II, 186, 207 (cf. also p. xiii n. of the German version of the *Lectures* [*Vorlesungen*]; and "Professor Cassel's System of Economics" (p. 250 of the translation included in Vol. I of the English translation of the *Lectures*). It will be noted, in passing, that when the "natural rate" is defined as in the text, the factor of anticipations—or, as Mr. Keynes called it, the "psychological" element in anticipated profit, which, at one point in the *Treatise* (cf. above, p. 193, n. 101), he regarded as a "fourth factor" in the "traditional doctrine"—is automatically included in the concept of a "natural rate" (and therefore in the theory of the relationship between Bank rate and general prices through the effect of the former upon the amount of *M'*); as, indeed, despite Mr. Keynes's expressed dissatisfaction with "expectations" as an "explanation" of the relationship between Bank rate and prices, it was automatically included in his own "prospective income which the entrepreneur anticipates from current investment." Cf. below, p. 200, and especially n. 118 thereto.)

[115] Similar statements have been made by many writers. The passage quoted, however, is from Ellis, *German Monetary Theory*, 307. Some ambiguity attaches to Ellis's position, to be sure, because he "approves" the "resolution of the Gibson paradox offered by Wicksell and Keynes" from "the purely ideological viewpoint" (*loc. cit.*; cf. also *ibid.*, 305). If by an "ideological viewpoint" is meant one that is logically satisfactory, there can be no quarrel; for then the dispute would turn entirely upon the functions of economic theory generally. It remains true, however, that the statement quoted is an unfortunate one.

[116] The difference between the two types of usage is brought out by a distinction that Wicksell himself sometimes drew (see, for example, "Der Bankzins, etc.," *loc. cit.*, 238 f.), although, unfortunately, not consistently, between the "natural rate" and the "normal" rate. The *"natural"* rate, the anticipated profit to be made by the use of a money loan, would explain why a given Bank rate would be expected to cause changes in the amount of borrowing from banks and therefore movements in the price level; the *"normal"* rate, the Bank rate which keeps prices stable, would accomplish this result *because* it was equal to the "natural rate."

pansion of credit—and therefore, under certain conditions, in a change in prices—would depend on the relationship between the market rate of interest and the anticipated profit to be made by the use of a money loan.

The important thing, for our present purpose, is that, whereas Wicksell went out of his way to establish the fact of continuity in doctrine as between his own argument and that of the classical economists whom Tooke believed he was "refuting," Mr. Keynes not only referred neither to the classical economists nor to Wicksell as having preceded him in advancing this "explanation," but regarded the point in question as one of those that would have the effect of transforming his own theory into one quite "different" from the traditional theory on the subject.[117]

Surely the tale provides its own moral. Nor is the moral in any wise obscured by the fact that there was much more in Keynes's concept of a "natural rate"—defined principally as the rate that would equalize "savings and investment"—than a reiteration of the classical proposition that "it is not a change in Bank-rate as such which needs to be associated with a changed quantity of money," but a change in the relation of the market-rate of interest to the anticipated profit to be made by the use of a money loan. For that matter, there was very much more than this in Wicksell's own concept of a "natural rate," either according to any one of the formal definitions Wicksell gave to it, or according to any one of the further definitions that he assigned to it by the terms of his argument. It can be shown, however, that it is precisely with respect to the details of the superstructure built upon the classical foundation by both Wicksell and Keynes, rather than with respect to the classical foundation itself, that there is most room for difference of opinion.

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[117] It should be noted that Mr. Keynes nowhere states *which* of the several "points" that he raises in his discussion of "The Relation of Bank-rate to the Quantity of Money" (*Treatise*, I, 216 ff.) he regarded as the "essential element" that he believed the "Traditional Doctrine" on the subject "overlooked," nor did he indicate which one of them would have most effect in transforming the traditional theory into a "different one." The point now under discussion, however—namely, the suggestion that "it is not a change in bank-rate as such which needs to be associated with a changed quantity of money," but a change in the market rate of interest relative to the anticipated profit to be made by the use of money loan—seems to have bulked as large in his analysis as any.

The "classical" element in Keynes's concept of a "natural rate" was represented by his virtual definition of that rate as "the prospective income which the entrepreneur anticipates from current investment," this "natural rate" then being compared with the "market rate"—that is, "the rate of interest which he [the entrepreneur] has to pay in order to be able to finance" the investment in question.[118] This was, of course, not Keynes's *formal* definition, which was "the rate at which saving and the value of investment are exactly balanced."[119] Nor is it necessarily the same thing as the "natural rate" when the latter is defined as the rate of interest which will bring it about that "the price-level of output as a whole exactly corresponds to the money-rate of the efficiency-earnings of the Factors of Production."[120] It is certainly not

[118] *Treatise*, I, 154. There are similar implied definitions of the "natural rate" elsewhere in the *Treatise*, though they involve different degrees of emphasis both upon the element of "prospectiveness" and the role of "fixed capital." Cf., for example, what is said on II, 380, with respect to "the return on actual current investment," with what is said on I, 204, with respect to the "estimated prospective yield of fixed capital"; on I, 207, with respect to the effect of a "prospect of losses" upon the "natural rate"; and on I, 212, with respect to the effect upon the "natural-rate" of an "altering" of "expectations as to the future course of prices." These passages may have provided some justification for the statement that it is "not . . . clear," from the *Treatise*, whether Keynes intended to "identify" his "natural" rate with "the prospective yield" from investment (so, for example, Adarkar, *The Theory of Monetary Policy*, 47 f.). They also, however, make it difficult to see what basis there is for the flat statement that "Keynes's 'natural' rate is not the same . . . as the prospective yield of new investment" (*ibid.*, p. 50). For the purposes of the present argument, it is not necessary to go beyond the two facts that (1) even the sponsors of the latter interpretation were forced to admit that Keynes's argument amounted to the contention that "the position of the market rate relative to that of the prospective yield is the main factor in the determination of investment policies" (Adarkar, *op. cit.*, p. 48), and that (2) it was certainly Keynes's contention that it was the position of the market rate relative to that of the "natural rate" which would determine the extent of "investment" (see, for example, the *Treatise*, I, 158, 206, 208, 211 f.). Cf. also the following note.

[119] *Treatise*, I, 155, 197. It is obvious that this proposition adds a further element of definition to the implied definition of the "natural rate" as the "prospective income which the entrepreneur anticipates from current investment," by providing a criterion for the setting of the market rate, and therefore for the location of the "marginal" yield on "prospective" investment. Cf., on this matter, p. 252, n. 50, below. It is obviously incorrect, therefore, to suggest that because Keynes defined the "natural rate" formally as the "rate which equalizes the rate of saving, S, with the rate of investment, I," he meant to *deny* that the "natural rate" is "the same as . . . the prospective yield of new investment." (Cf. Adarkar, *The Theory of Monetary Policy*, 49 f.)

[120] *Treatise*, I, 155. The identification of this definition of the "natural rate" with the one immediately preceding of course follows from the second Fundamental Equation of the *Treatise*. In the light of the discussion of the Fundamental Equations presented in Chapter V, above, however, it should be obvious that any satisfactory proof of the identity of the two definitions would have to be based upon an argument quite independent of those equations.

necessarily the same thing as the rate that will bring about "stability . . . of the price-level of output as a whole."[121] Nor, again, is it necessarily the same thing as the rate which will provide either "industrial stability" (a highly ambiguous term), on the one hand, or constancy of employment or output, or "full" employment, on the other—the latter criteria, as has already been pointed out, themselves being anything but necessarily identical.[122] A further series of definitions of the "natural rate," moreover, was implied in occasional *obiter dicta* with respect to the way in which that rate was held to be affected by the amount of "real capital" in existence.[123] The point to be made here, however, is that it is precisely the reconciliation of the various parts of the superstructure that Keynes built on the classical foundation—to say nothing of their reconciliation with the various definitions of the "natural rate" which have been given by other writers—that has given rise to most difficulty. Our only concern here is to establish the fact that, in the part of his actual account of the relationship between the rate of interest and the quantity of bank-money that is here under discussion, Mr. Keynes really used the definition first cited, which happens to be the heart of that "traditional" doctrine whose differences from his own doctrine he was so anxious to establish.

Much the same thing may be said of Wicksell. Attention has already been called to the fact that one of Wicksell's informal definitions of the "natural rate" was "the anticipated profit to be made by the use of a money-loan."[124] Neither of his two formal definitions, however, was couched in these terms. In *Interest and Prices,* for example, the "natural rate" was formally defined as the rate that would be established if "real capital" were lent *in natura.*[125] In his *Lectures,* on the

[121] See, for example, the *Treatise,* II, 220, also II, 222, 350, and I, 183; and cf. what is said, in this connection, on p. 114 above.

[122] For an interpretation of Keynes's argument as implying that the maintenance of II at a level equal to E/O—or, according to the argument of the *Treatise,* the maintenance of an equality between "Saving" and "Investment"—would secure "industrial stability," see Adarkar, *The Theory of Monetary Policy,* p. 58 n. (Cf., however, C. O. Hardy, in the *American Economic Review,* XXI [1931], 154). On "full employment" and "constant employment" as criteria of "equilibrium" along with an equality of Savings and Investment, and the relation of both "full employment" and "constant employment" to constancy of output, see p. 76, above, and especially notes 12 and 13, thereto.

[123] See, for example, the *Treatise,* II, 204: "Since the annual increment in any year to the aggregate of capital is small relatively to this aggregate, movements in the natural-rate of interest are—subject to interruption by such things as wars—long period movements extending over decades." Also II, 207: "It is *investment,* i.e., the increased production of material wealth in the shape of capital-goods, which alone increases national wealth, and can alone in the long run bring down the natural-rate of interest."

[124] Cf. above, p. 198, n. 114.

[125] See, for example, *Interest and Prices,* pp. xxv f., 102. The English translator renders the phrase *in natura* by the English "in kind." *In natura,* however, is not German, but Latin; and to render it by a phrase which fails to show its connection with the phrase "the natural rate" is to miss the significance of a linguistic accident the consequences of which have

other hand, the "natural rate" was formally defined as "the rate of interest at which the demand for loan-capital and the supply of saved media correspond exactly to each other." [126] These two formal definitions, moreover, are by no means the only ones that appear in Wicksell's writings. Indeed, it is not necessary to go outside of *Interest and Prices* to find the following definitions of the "natural rate" either expressly stated or implied by the terms of the argument:

1. The "natural rate" is the rate that is set by the supply of, and the demand for, "real capital." [127]

2. The "natural rate of interest" is approximately equal to the "real profit of business enterprises." [128]

3. The "natural rate of interest" is the rate which discourages any borrowing of money or credit that might lead to a rise in prices, and which encourages precisely the amount of borrowing necessary to pre-

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been as amazing as, in my opinion, they have been deplorable. The most far-reaching of these consequences has been the identification of the "natural rate" as the rate which would be set if "real capital" were lent *in natura*, with the "natural rate" in a sense in which the word "natural" has the connotations attaching to the word in phrases such as the "natural price"—that is, the *equilibrium* price—of the classical economists. A detailed examination of the validity of this identification, however—and, indeed, of the identifications involved in all the other definitions cited in the text—must be left to the extensive publication on *The Natural Rate of Interest* mentioned on p. 40, n. 2, above.

[126] Cf. the *Lectures*, II, 193. I have ventured to substitute the phrase "saved media" for the English translator's "savings," on the ground that it is not only a more literal rendering of the Swedish *sparmedel* and the German *ersparte Mitteln*, but is much more likely to call attention to what, in my opinion, is an indisputable fact—namely, that Wicksell himself, at the time he published his *Lectures*, thought of his "normal" or "natural" rate as a *money* rate, and of the "savings" which constituted the supply as savings *in the form of money*. It is to be admitted that in the paraphrase of the *Lectures* definition which appeared in the preface to the first (1906) Swedish edition (cf. p. xxii of the 3d [1929] Swedish edition) of those *Lectures*, there appears, in place of the expression "saved media" (*sparmedel*), a word which might well be translated "saving" (*sparverksamheten*); though it may also be not without significance that this paraphrase was not included in the parts of the preface to the first Swedish edition which Wicksell included in the German edition of 1922 (p. x).

[127] For references to pages in *Interest and Prices* in which this definition, as well as those that follow, appear, see below, p. 203, n. 129. With respect to this first usage, moreover, it may be pointed out that a special variant thereof is implied whenever by "real capital" is understood "real capital" in its "liquid," "free," or "uninvested" form—that is, in the form of consumers' goods. For examples of such a usage, see *Interest and Prices*, 102 f., 122 f., 126 ff., 137 f., 146, 148 f., 163, 171, 174.

[128] The German original, "der reale Zins der Unternehmungen," is rendered by the English translator as "the real interest of actual business" (*Interest and Prices*, xxv). It can be shown, however, that much turns upon the precise meaning to be attached to the term "business *enterprises*" (*Unternehmungen*). On the other hand, it is only fair to note that Wicksell himself sometimes used the term "business gains" (*Geschäftsgewinne*) as the loose equivalent of the "natural rate." See, for example, "Der Bankzins," *loc. cit.*, 236.

vent a fall in prices; it is, in other words, the rate that keeps the price-level stable.

4. The "natural rate of interest" is a covering term for all factors affecting the borrowing of money, other than changes in the money-supply and consequent changes in the money-rate of interest.

5. The "natural rate of interest" is the rate which will determine such a supply of, and demand for, credit that there will be no pecuniary factor (such as changes in the general price-level) affecting the amount of business profits.

6. The "natural rate of interest" (that is, the rate at which the money rate must ultimately stand) is the rate that, under present conditions of banking administration, the banking authorities are forced to adopt as a result of the fact that bank-reserves are exhausted—this exhaustion being due chiefly to the additional demand for credit brought about by the rise of prices which is made inevitable by the fact of a limitation of the amount of "real capital" in existence.[129]

When, moreover, account is taken of the translations of the "natural rate" that have been given by writers avowedly in the Wicksellian tradition, the list of definitions becomes further extended. The formal definition in the *Lectures*, for example, has been translated by some of these writers—though it is extremely important to notice that it was never so translated by Wicksell himself—as "the rate at which the amount of new investment corresponds to the amount of current savings."[130] If, moreover, one chose to go outside the immediate Wick-

[129] For examples of each of these usages—the list of citations is not intended to be exhaustive—see the following pages of *Interest and Prices*: (1) xxvi, 150 ff., 171; (2) xxv; (3) 100, 102, 120, 189 (cf. also "Der Bankzins," *loc. cit.*, 233, 241); (4) 89, 106; (5) 166; (6) xxvii, 108-110, 117, 136. A still further definition has been implied by the English translator, who renders one of the synonyms for "the natural rate"—namely, *der Verkehrszins*—as the "uncontrolled" rate of interest (115, 134). It is unfortunately true that others than Wicksell, influenced, perhaps, by the connotations of the word "natural" as it appears, for example, in Böhm-Bawerk's essay *Macht oder ökonomisches Gesetz?* (see the latter's *Gessammelte Schriften*, p. 232, and especially p. 278) have imported this connotation into the concept of the "natural rate" when the latter was used in what was supposed to be the Wicksellian sense. The consequences of this usage, however, have been anything but happy; and fairness to Wicksell demands that it be pointed out that he himself did not seem to mean more by "der Verkehrszins" than "the rate obtainable from trade" (*Verkehr*), which he believed—on grounds that are by no means self-evident—to be equivalent to "the interest return on the capital invested in the various enterprises, after deduction of everything that is to be attributed to monopoly gains, business secrets, advantageous positions, etc." ("Der Bankzins," *loc. cit.*, 236.)

[130] The list of writers who have made this translation is very extensive. It is, however, significant not only that, so far as I am aware, the usage cannot be found in any of Wicksell's writings, but also that—again, so far as I am aware—virtually all the instances of such a translation have appeared subsequent to Keynes's *Treatise on Money*. For particularly clear examples of the translation in question, see Hayek, *Reflections*, II, *loc. cit.* (1932), 22; G. Myrdal, "Der Gleichgewichtsbegriff als Instrument

sellian tradition, still other definitions could be added which could have the effect only of emphasizing the difficulties of reconciliation already inherent in Wicksell's own writings. The important points to be made, however, are that, whatever else there is in Wicksell, there is the definition representing the heart of the classical tradition—namely, the "anticipated profit to be made by the use of a money loan"; and that, as in the case of Keynes, it is this definition which was used, in the "traditional doctrine," for the immediate handling of the problem with which we are here concerned—namely, the relationship between the quantity of bank money and the rate of interest.

It should hardly be necessary to add that such differences of opinion as exist with respect to the relationship between the various parts of the body of doctrine constructed on the classical foundation constitute no ground for condemning the whole of that segment of received doctrine which is summed up by the concept of a "natural rate of interest." [131] Such differences of opinion are, after all, a normal feature of scientific advance: pioneers wandering in territories as yet unexplored are almost certain to be drawn into by-paths which later investigators, warned by the mistakes of their predecessors, are able to avoid, if only they are prepared to study with care and to learn from the experiences of these predecessors, thus consolidating the definitive achievements of these pioneers against the over-hasty rejection of the good along with the bad parts of their argument.[132] The critical

der geldtheoretischen Analyse," *loc. cit.* (1933), 409; B. Ohlin, Introduction to *Interest and Prices,* xix ff. The usage of Myrdal is particularly interesting, in this connection, in view of the fact that, in the earlier Swedish version of his study (*Ekonomisk Tidskrift,* XXXIII [1931], 211), the corresponding section had been entitled, not "Equilibrium between Savings and Investment"—an expression which was characterized as "applying the terms which are *now* more usual"—but "Equilibrium between the Supply of and Demand for Saving," which was the usage that had been adopted by E. Lindahl, in his study *Penningpolitikens Medel* ("The Methods of Monetary Policy" [1930]; see, for example, p. 122), on which Myrdal's own work is so largely based.

[131] This is not to say, of course, that these differences of opinion do not argue for an abandonment of the *expression* "the natural rate of interest," which has come to mean all things to all men. If, in this study, I have continued to speak of "the natural rate," I have done so (1) with the express warning that, unless there is a specific indication to the contrary, the "natural rate" is to be understood in the sense of "the anticipated profit to be made by the use of a money loan"; and (2) in the desire to avoid, so far as possible, the raising of issues an adequate discussion of which would require the kind of extended monographic treatment I hope to provide in a later publication.

[132] A detailed examination of those arguments with respect to the "natural rate of interest" which seem to the present writer to amount to throwing

discussion of the concept of a "natural rate" which we owe particularly to those Swedish writers who were themselves bred in the Wicksellian tradition is proof of the possibilities in this direction; and it is no minimization of their contributions to suggest that there are grounds for believing that these possibilities are by no means exhausted as yet.[133] The point in citing these writers in the present instance is merely to show as clearly as one could wish that it is one thing to begin critical and constructive work at the point at which the received argument can be demonstrated to be inadequate or unsatisfactory; it is quite another to attack received doctrine on grounds that can be shown, by a simple comparison of the substitute doctrine and the details of something that may properly be called the "traditional doctrine"—rather than the details of a caricature thereof—to have been unfounded. In the present instance, the question involved is whether it was fair to criticize the "traditional" theory with respect to the relation between Bank-rate, general prices, and the quantity of "bank-money" (M') on the ground that this "traditional doctrine" failed to recognize that "it is not, strictly speaking, a change in Bank-rate as such which needs to be associated with a changed quantity of money," but a change relative to the anticipated profit to be made by the use of a money-loan. It is in the light of the answer that will be given to this question that the moral of the tale recounted above must be drawn.

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the baby out with the bath water must be left for the publication referred to in the preceding note. Since, however, the attitude toward the concept adopted by Mr. Keynes in his *General Theory* may be regarded as providing something of a case in point, it will be discussed in Volume II of this study.

[133] The most important names among the Swedish economists referred to—apart, of course, from Professor Davidson—are those of E. Lindahl and G. Myrdal, whose principal works, for this purpose, are referred to above, p. 203, n. 130. For references to the more important papers published in the Swedish *Ekonomisk Tidskrift* on the subject, see the references given by Ohlin in his Introduction to *Interest and Prices*, x, xvi f., xix. For an account in English of "The Monetary Doctrines of Professor Davidson," see the article under that title by B. Thomas, in the *Economic Journal*, XLV (1935), 36 ff.; and for a general, though admittedly brief and fragmentary, account of "Swedish Monetary Theory since Wicksell," see Chapter III of the same author's *Monetary Policy and Crises* (1936).

CHAPTER EIGHT

The Quantity of Money-Substitutes, *M'* (*Continued*)

I

Bank-Rate, *M'*, and "Proportional" Price Change

IT WAS not unreasonable to hope that Mr. Keynes's further reasons for dissatisfaction with the "traditional" account of the relationship between changes in bank rate and changes in general prices would turn out to have been better founded than was his implication that the "traditional doctrine" assumed that the "association of changes in Bank-rate with changes in the supply of bank-money" was "invariable," in the sense that it assumed that every change in bank rate involved a corresponding change in the quantity of bank money. Unfortunately, however, these further objections to the "traditional" formulation are, if anything, less well-founded than the objection already discussed. Mr. Keynes's second objection, for example—incredible as it seems—was summed up in the clear implication that the "traditional doctrine" assumed that, in those cases in which changes in bank rate were associated with changes in the "quantity of bank-money," the effect on prices would be "proportionate to the changes in the supply of [bank-] money."[1] In the light of the discussion presented in our earlier chapters—particularly in Chapter Two—it should hardly be necessary to point out that we are obviously dealing here with another instance of Mr. Keynes's strange obsession with the notion that "the Quantity Theory," in its crudest forms, is an integral and inseparable part of the generally accepted apparatus summed up under the head of "The Theory of Prices."

[1] See, for example, the *Treatise*, I, 189.

What, after all, is there in the familiar "Quantity *Equations*," for example, that would demand acceptance of the proposition that the relation between an "alteration in the quantity of bank-money" which is "associated" with "an alteration of bank-rate," on the one hand, and the "price-level," on the other, is "simple or invariable," in the sense that price levels may be expected to change "more or less in the same proportion as the change in the quantity of bank-money"? [2] As we have seen, it is precisely the function of the received Quantity Equations—as contrasted with an equation, say, of the form $M=P\cdot k$, in which k is an arbitrary constant—to point to the factors other than the quantity of money which affect prices: to show, in other words, precisely why the price change associated with a given change in the quantity of money is sometimes "more" and sometimes "less" than proportional. What ground, then, was there for suggesting that a decision as to whether it is "useful to say that a change in bank-rate changes price-levels *because* it is associated with changes in the quantity of bank-money" should turn upon acceptance or rejection of the proposition that factors other than the "quantity of bank-money" also affect the price level? [3] To say that changes in the "quantity of bank money" represent an extremely important factor affecting prices is not equivalent to saying that prices are not affected by other factors, such as—to take examples from Mr. Keynes's list of "qualifications and complications"—changes in velocities of circulation, the volume of output, or the volume of transactions other than those involving the final sale of output, which require an addition to the total supply of cash balances if price fall is not to result.[4] Changes in any one of these items will be

[2] Cf. the *Treatise*, I, 216 f.

[3] Cf. the *Treatise*, I, 216 f.

[4] The list of factors which, according to Keynes (*Treatise*, I, 216) must be in "due relation" to the volume of bank money, before a "simple or invariable relation" can be established "between the effect of an alteration of bank-rate on the price-level . . . and the associated alteration in the quantity of bank-money" includes, in addition to the factors mentioned in the text, "the rate of earnings" and the "rate of profit." The latter, obviously, has to do primarily with the "natural rate," discussed in the preceding chapter. The former, insofar as it represents *costs*, would in turn be related to the "rate of profit": though, from the context, one assumes that at this point Keynes was thinking more of the type of consideration listed on I, 217 of the *Treatise*, under (*a*)—on which see below, pp. 208 ff.

reflected in changes in the variables of the Fisherine equation—specifically, in V and T. To say, merely because changes may be expected to occur in V and T, that "we shall be misled if we lay much stress on changes in the total of money when we are trying to trace the causation and the stages of a transition" in the process by which changes in bank rate affect prices is like saying that we should abandon the use of "Quantity Equations" because "the Quantity Theory," in its crudest forms, is untenable.[5] Yet this is precisely what Mr. Keynes was saying in four out of the six "points" which he adduced by way of showing that "it is not useful to say that a change in bank-rate changes price-levels *because* it is associated with changes in the quantity of bank-money." [6]

The two remaining "points"—namely, those made in paragraphs (*b*) and (*f*) of the passage cited—have to do with the proposition, discussed in the preceding chapter, that "it is not, strictly speaking, a change in bank-rate as such which needs to be associated with a changed quantity of money, but a change in the market-rate of interest relatively to the natural rate." Point (*b*) states the general proposition, and point (*f*) applies it to the special case of gold imports due to capital movements resulting from a differential between the market rates in two countries. With respect to the latter point, it is sufficient here to call attention to the fact that the international complications involved in the problem of discount policy—complications which have seemed to some critics of Wicksell to be of such great practical importance as to constitute a sufficient reason for refusing to consider the logical foundations of his general argument—were recognized frankly by Wicksell as early as his *Interest and Prices*, in which he advanced a proposal for international coöperation between central banks as a means of dealing with the difficulty.[7]

It is easy to show, on the other hand, that the other four "points" have to do with elements in the Quantity Equation other than the "quantity of money." Under point (*a*), for example, we were told that

[5] The statement quoted from Keynes is to be found on I, 219 of the *Treatise*.

[6] For the "six points" referred to, see the *Treatise*, I, 217 ff. For a discussion of them which differs in some respects from the one presented in the text—though the differences are largely differences of emphasis—see Ellis, *German Monetary Theory*, 422 ff.

[7] See, for example, *Interest and Prices*, 111 ff., 119, 157 ff., 190 ff. A discussion of the criticisms of Wicksell based on these international complications must be left for another occasion. See, however, what is said concerning what Keynes characterized as the "second strand" of the "traditional doctrine" with respect to the relationship between bank rate and prices, on pp. 216 f., below.

a "rise in prices, which is due to an increase in the rate of earnings, will require a larger quantity of bank-money to support it than an equal rise which is due to the emergence of profits."[8] The reasons for this turn upon the different characteristics of the cash balances that are held by those in receipt of "profits"—which, in the terminology of the *Treatise,* do not make part of "income"—and the cash balances which are held by those in receipt of "income" (or "earnings").[9] The *absolute* height of the two types of balance demanded, for example, may be different because the total of expenditure against which each group of balances is being held may differ by virtue of the fact that both (1) the objects on which money is to be spent (T), and therefore (2) their prices (P) may differ.[10] Let us write the M' of our Quantity Equation as equal to $M'_1 + M'_2$, the subscripts referring to the two types of cash balance.[11] In the case under discussion, a change in T has come about as a result of the change in the objects of expenditure against which cash balances are being held. If relative prices have not changed in the meantime, this must mean also that the prices (P) involved in the total (PT), when the latter is regarded as measuring the absolute demand for cash balances at a given price level, "velocities" remaining the same, will differ from the earlier P, since the new T will, in the first instance, bring its own set of prices with it. Whether these new prices can be actually realized, however—or, what comes to the same thing, whether the demand for cash balances at a given absolute level will be satisfied—will depend upon whether the total quantity of bank money ($M'_1 + M'_2$) is so adjusted as to maintain prices at their old level despite the change in T. If it is not so adjusted, prices

[8] *Treatise,* I, 217.

[9] Keynes did not have in mind such differences as derive from the fact that the cash balances held by certain groups of "income" recipients are likely to be held in some form of hand-to-hand currency, rather than in the form of bank deposits. A discussion of the consequences of this fact, which has to do directly with the "internal drain," was excluded from Keynes's analysis by the terms of his general theoretical setup. Cf. what is said on this matter on p. 159, n. 2, above.

[10] Cf., in this connection, the *Treatise,* I, 245. On the importance of distinguishing between the demand for a cash balance of a given *absolute* amount (in terms of a number of monetary units) and the demand for a cash balance of a given size *relative to outlay,* see below, pp. 444 ff. It will be noted that the argument which follows, the various steps in which are translated in every case into the terms of the older Quantity Equations, is part of what Keynes included in Book IV of the *Treatise* under the heading "The Dynamics of the Price-Level." It therefore provides an additional commentary upon Keynes's contention that the older Quantity Equations were not "useful" for purposes of "dynamic" analysis.

[11] On the justification of this step, cf. what is said on p. 147, above. For purposes of the present discussion, it is obvious that M'_1 and M'_2 correspond essentially to Keynes's "Income Deposits" and "Business Deposits A," respectively—in other words, to his "Industrial Circulation" (cf. *Treatise,* I, 244). Something is to be said, however, for retaining a notation which will show the relationship of the argument under discussion to the "quantity of bank-money" (M'), and to the older types of "Quantity Equations" generally.

(P) will change as a result of the change in T.[12] We are dealing, in other words, simply with a case in which the price change which is associated with a given change in the quantity of bank money will not be strictly proportional to the change in the quantity of bank money, because of the change that has come about in the magnitude of T.[13]

Point (c) is merely an elaboration of point (a), since it considers two cases, in one of which the extent and the character of the absolute demand for cash balances, as affected primarily by the magnitude and composition of T, will not change appreciably, and in the other of which it will. Concretely, it was argued—the validity of the argument is not here in question—that a discrepancy between market rate and natural rate which comes about through a change of the "natural rate" (say, as a result of a change in the volume of savings) is less likely to affect T than one which comes about through a change in the market rate. In either case, all that the argument amounts to is that any estimate as to the effect upon the price level of a given change in the quantity of bank money (M') which may be expected to result from a discrepancy between market rate and natural rate, must take account of the probable effect of such a discrepancy upon the volume and composition of T. The *composition* of T, it will be noted, is particularly relevant for the rôle of *prices* in determining the absolute demand for cash balances; although, as in the case considered under point (a), whether or not the prices which it is desired to obtain will actually be realized will depend upon what happens to the quantity of bank money (M').

Point (d) merely states that bank rate may affect velocities of circulation as well as the volume of bank money (M'), with the result that whatever change in bank money comes about as a result of a change

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[12] This, of course, is nothing more nor less than a translation of Keynes's proposition that "the quantity of money which is adequate to support a rise of prices due to the second term of the Fundamental Equation [that is, $(I - S)/O$, or Q/O] is inadequate to support an equal rise due to the first term [that is, E/O], with the result that, as the first term increases, a *reaction in prices becomes inevitable through lack of enough money to 'finance' them*" (*Treatise*, I, 217; italics mine). I have italicized the last part of this proposition by way of showing that if we mean by "cause" simply a *necessary condition*, it would still be true that, on Keynes's own showing, the quantity of money would be a "cause" of the price change, so that it is not necessary to raise the ghosts of ancient controversy which are suggested by the statement that, according to Keynes's argument on this point, "causation runs from right to left in the Fisher type of equation." Cf. Ellis, *German Monetary Theory*, 423.

[13] Keynes's analysis elsewhere (see, for example, *Treatise*, I, 245) would indicate that a further reason for disproportionality would reside in the fact that since there are reasons for supposing that differing velocities would attach to M'_1 and M'_2—in his terminology, "Income Deposits" and "Business Deposits A" (cf. p. 209, n. 11, above)—a change in the proportion of total balances represented by M'_1 and M'_2, respectively, would result in a change in the figure for global velocity—that is, the V of our quantity equation. Since, however, Keynes chose to discuss the consequences of this possibility under his point (d), what is said below in our discussion of that point may be applied, *mutatis mutandis*, to the present argument.

in bank rate will have a more than proportional or less than proportional effect upon prices, in accordance with the movements in V. Further discussion of the effect of changes in the rate of interest upon the size of cash balances *relative to outlay* (which is merely another way of characterizing its effect upon V) must be deferred to a later point in this study.[14] For our present purpose, it is sufficient to call attention to the fact that such a possibility was explicitly recognized by both Wicksell and Fisher—to go no further.[15] There is certainly no reason whatever for seeing in this simple fact, which is part of the analysis "lying behind" the V of our Quantity Equation, an argument for minimizing the importance of another element in that Quantity Equation—namely, M'—or for suggesting that an analysis which does not distinguish between the effects of bank rate upon M' and its effects upon V is better than one which does.[16]

Point (*e*) has to do with possible effects of bank rate upon both T and V, through its effect upon the amount and character of financial transactions.[17] An increase in the amount of such transactions, insofar as these transactions would involve the use of bank money at all, would require the maintenance of cash balances for carrying them on. We have, then, an increase in T exactly comparable to that involved in the case considered under point (*a*), the only necessary addition being the writing of M' as equal, not merely to $M'_1 + M'_2$, but to $M'_1 + M'_2 + M'_3$, in which M'_3 represents the volume of cash balances devoted to financial transactions.[18] There are, however, reasons for believing that the velocity of circulation attaching to these new balances (M'_3) will not be the same as that attaching to either M'_1 or M'_2; and there are reasons, also—for the indication of the nature of which everyone must feel in debt to Mr. Keynes—for believing that if M'_3 is

[14] Cf. above, p. 186, n. 79.

[15] Cf. above, p. 186, and especially note 78 thereto.

[16] The issues involved are identical with those that are involved in the claims for certain variants of the "income-approach" which would imply that emphasis upon the importance of money-*income* implies a disapproval of emphasis upon the quantity of money as a factor affecting prices. See, on this matter, pp. 346 ff., below. Here it is necessary merely to point out that it is only in the light of the argument there presented that it is possible to judge Mr. Hawtrey's contention, in his discussion of "the need for a revised theory of Bank-rate" (*The Art of Central Banking*, pp. 144 f.), that "it is not really necessary to introduce the quantity of currency into the analysis at all" (p. 145). The same thing may be said of Hawtrey's attempt to answer Keynes's charge that he (Hawtrey) had accepted "the view of 'Bank rate as acting directly on the quantity of bank credit and so on prices in accordance with the Quantity Equation' " by insisting that it was *not* "through changes in the [absolute size of the] unspent margin" (that is, M'), but rather "through changes in consumers' income and outlay" that he thought of the price-level as being affected by "an acceleration or retardation of the creation of credit" (Hawtrey, *op. cit.*, p. 363).

[17] On the general place of financial transactions in the "Theory of Prices," see below, pp. 576 ff.

[18] M'_3 thus corresponds essentially to Mr. Keynes's "Financial Circulation." Cf. *Treatise*, I, 244, and also p. 209, n. 11, above.

further subdivided into M'_{3a} and M'_{3b}, greatly differing velocities of circulation may attach to these two parts of M'_3, the two parts of M'_3 being themselves likely to change in relative magnitude at different phases of a speculative boom.[19] If, therefore, changes in bank rate result in a change either in the magnitude of M'_3 as a whole, or in the relative magnitudes of M'_{3a} and M'_{3b}, such changes in bank rate may be expected to be accompanied by changes in the value of the global V of our simple Quantity Equation.[20] Again, however, there is not the slightest reason whatever for arguing that recognition of the possibility that changes in bank rate may affect other variables in our Quantity Equation than the "quantity of bank-money" (M') constitutes a reason for insisting that "it is not useful to say that a change in bank-rate changes price-levels *because* it is associated with changes in the quantity of bank-money." Whatever Mr. Keynes was quarrelling with here, it was certainly not the substance of received tradition with respect to the relationship between changes in bank rate and the price level through the intermediacy of changes in the "quantity of bank-money."

II

The "Order of Events"

From the argument of the preceding chapter, it must be obvious that, so far from its being true that emphasis upon "changes in the quantity of money" involves indifference to, or a denial of, the importance of changes in the relationship between bank rate and "natural rate," one emphasis is merely complementary to the other.[21] Yet Mr. Keynes

[19] It should be obvious that M'_{3a} and M'_{3b} correspond essentially to Keynes's "Business Deposits B" and "Savings Deposits." See the *Treatise*, *loc. cit.* On Keynes's treatment of the "velocity of circulation of Savings Deposits," see below, pp. 468 ff.

[20] The reader is again reminded that the Quantity Equation $(M + M')V = (PT)$ is to be regarded only as the starting point for further analysis, which would in turn be expected to result in more elaborate formulations of the "Quantity Equation." In all cases, however, there would be no ground whatever for suggesting that the use of these more elaborate formulations involves an "abandonment" of the simple formulation, in the sense in which it was suggested that the equations of the *Treatise* involved an abandonment of the simpler formulation.

[21] Another way of putting the point would be to insist that to ask whether it is the rate of interest *or* the "quantity of money" which is important is to raise an antithesis which, in the light of the actual substance of received doctrine with respect to both, is entirely false. It is, perhaps, not without significance for a judgment as to the nature of the impact of the *Treatise* upon contemporary monetary theory, that this type of question, which ought really never to have been raised at all, was raised with increasing frequency *after* the publication of the *Treatise*. See, for example, L. Currie, *The Supply and Control of Money in the United States* (1934), 5 ff., and J. W. Angell, *The Behavior of Money* (1936), 164 f.

found certain "fundamental" reasons, which we must consider now, for arguing that all emphasis must be put upon "changes in bank-rate relatively to the natural-rate, *rather than* on changes in the quantity of money." [22]

The most explicit of these reasons had to do with the "order of events" in the process by which changes in bank rate might be expected to affect prices.[23] According to Mr. Keynes, the correct "order of events" was that a change in the quantity of money would, "other things being equal," bring about a bank rate which "will change the market-rate of interest relatively to the natural rate." [24] According to Mr. Keynes, also, it was definitely wrong to suggest that, as a matter of the "order of events," "a change of bank-rate affects the price-level because, in order to make the new bank-rate effective, the quantity of money has to be altered." [25] It is, however, easy to demonstrate that, *so far as the "order of events" is concerned,* the two statements which were thus presented as alternatives are not true alternatives in the sense that one must be accepted and the other rejected, but are, on the contrary, mutually complementary descriptions of the "order of events" which may be expected under different sets of circumstances.

It is, for example, nothing less than absurd to imply that supporters of "the traditional doctrine" would for a moment have been prepared to insist that Mr. Keynes's own "order of events" is either impossible or uncommon. On the contrary, as we have seen, it was precisely this order of events

[22] *Treatise,* I, 219 f. Italics mine.

[23] *Treatise,* I, 220.

[24] *Treatise,* I, 220. From the context, it seems certain that Mr. Keynes must have meant to say that the change in bank rate will change the *position of* the market rate of interest (that is, bank rate) relative to the "natural rate." He was not, that is to say, interested, at this stage in his argument, in the complex of problems which are associated with the "control" of "market-rate" (in the sense of a rate charged by member banks) by "bank rate" (in the sense of the rate charged by the central banks); so that, for purposes of the argument, "bank rate" and "market rate" may be taken as identical. See especially, in this connection, I, 200 f. of the *Treatise,* where it was expressly stated that "we shall here assume that changes in bank-rate affect the market-rate of interest in the same direction," and that "the relationship between the official bank-rate, the 'effective' rate of discount and the market-rate of interest" was to be left for discussion in Chapter XXXVII. On the confusion to which Keynes's exposition on this head has given rise, cf. below, p. 219, n. 42.

[25] Keynes, *Treatise,* I, 220.

which was suggested by writers like Marshall, who were anxious to trace the actual mechanism whereby new gold would be expected, under modern conditions, to get in its effect upon prices.[26] It was of the essence of this argument that the addition of the new gold to bank reserves would be expected, "other things being equal"—in Mr. Keynes's own words—to "change the market rate of interest relatively to the natural rate"; and they would certainly have agreed with Mr. Keynes that "it is only through the complex movements thus set up that a new equilibrium position is eventually reached, with a price-level corresponding to the new quantity of money."[27] Concretely, they would have argued that, as a result of the change in market rate relative to the "natural rate," there would be, on the basis provided by the increase in the "quantity of money" serving as bank-reserves, an expansion in the quantity of *bank* money; and they would have argued further that it was this change in the quantity of "bank-money" which would bring about a "new equilibrium position" of the price level, "corresponding to the new quantity of money." In terms of our Quantity Equation, the first increase in the "quantity of money" would be represented by an increase in M_r, and therefore—until an expansion of credit upon the basis of M_r is brought about—a decline in $c(=M'/M_r)$; the second increase in the "quantity of money" would be represented by an *increase* in c, by way of an expansion of the "quantity of bank-money" (M').

If there are possible grounds for detecting a difference between the account presented by Mr. Keynes and the "traditional" account, it would turn upon the connotations of exclusiveness which may be held to lie in Mr. Keynes's use of the word "only" in his statement that it is *only* by changing the market rate relatively to the natural rate that a change in general prices can be brought about.[28] If we include, in the "traditional" account of the way in which new money affects prices, as we should, the contributions of writers like Cairnes, as well as those of writers like Marshall, it is obvious that conditions may be imagined

[26] Cf. p. 172, above.
[27] *Treatise*, I, 220.
[28] Here, again, Mr. Keynes's exposition managed to stir up trouble which need never have arisen, in the light of the statements in the text concerning the content of "traditional" theory with respect to the point under discussion. See, for example, Currie, *The Supply and Control of Money*, 7 ff.

under which new money may get in its effect upon prices without working through the mechanism of increased bank reserves and a lowering of the market rate of interest below the "natural rate."[29] This was ultimately not only admitted by Wicksell, but insisted upon by him against the arguments of one of his disciples who had undertaken to defend Wicksell's earlier, and more exclusive, position, as represented in *Interest and Prices*, against the broader presentation of the *Lectures*.[30] One could go on to show that, in any complete description of the forces determining the amount of "bank-money" (*M'*), account would have to be taken of the various forms of governmental inflation, including those in which the degree of importance to be assigned to a discrepancy between the "market-rate" and the "natural rate" would be changed greatly by the fact that the principal borrower from banks —namely, the government—is not concerned with calculations involving the "anticipated profit to be made by the use of a money loan."[31] All this, however, does not alter the fact that, under other conditions—and indeed, under most of the conditions which, in modern times, are associated with changes in the "quantity of money" in the sense of money of ultimate redemption—the "order of events" described by Mr. Keynes would not only be acceptable to, but would be insisted upon by defenders of "the traditional doctrine" with respect to the relation between changes in the "quantity of money," changes in the relation of the market rate to "natural rate," and changes in general prices.

It is, indeed, difficult to avoid the feeling that if Mr. Keynes had been more acutely aware of the rôle played in

[29] Cf. above, p. 172, and especially n. 38 thereto.

[30] For Wicksell's own statement of the differences between the formulation of *Interest and Prices* and that of the *Lectures*, in this respect, see the section from the Preface to the first Swedish edition of the *Lectures* which Wicksell reprinted in the Preface to the German version (xii ff.). The difference between the two formulations may be tested by comparing the general presentation in *Interest and Prices* with the *Lectures*, II, 177 f., 197 f., and especially 215 ff. The subsequent discussion to which reference is made in the text is contained in the articles by G. Åkerman, "Inflation, penningmängd, och ränta" ("Inflation, the Quantity of Money, and Interest"), *Ekonomisk Tidskrift*, XXIII (1921), 143 ff.; Wicksell's rejoinder thereto, in the form of an article under the same title, *loc. cit.*, 167 ff.; and a further article entitled "Reply to Cand. Åkerman" (*loc. cit.*, XXIV [1922], 10 ff.); D. Davidson, "Om begreppet normal penningränta" ("On the concept of a normal money rate"), *loc. cit.*, XXIV (1922), 13 ff.; and S. Brisman, "Räntan vid direkt inflation" ("Interest under Direct Inflation"), *loc. cit.*, pp. 1 ff. A discussion of the implications of this discussion for the concept of a "natural rate" must of course be left for further occasion.

[31] This is not to say, of course, that, under such conditions, no significance would attach to a discrepancy between the "market-rate" and the "natural rate." So long, for example, as there was any commercial borrowing from banks, it would still be of importance. A detailed discussion of this problem, however, as well as of the consequences of the whole argument for the concept of a "natural rate," must again be left to another occasion.

the "traditional doctrine" by the "order of events" which he regarded as the correct one, but with which he believed the "traditional doctrine" to be in conflict, he would not have treated as he did what he regarded as the "second strand" in the "traditional doctrine" with respect to the *modus operandi* of bank rate—namely, the tendency to "regard Bank-rate policy primarily, not as a means of regulating the price-level, but as a means of protecting a country's gold reserves by regulating the rate of foreign lending."[32] He was strangely troubled by this element in the "traditional doctrine." "It is by no means obvious," he wrote, "how it is connected with our first strand, and I know of no author who has attempted the synthesis."[33] Yet, surely, if the "traditional doctrine" with which we are here concerned is the doctrine with respect to the way in which changes in bank rate may be expected to affect prices, the connection of this "second strand" with the first is perfectly obvious.

The heart of the traditional doctrine was, indeed, the "first strand," which alleged that bank rate affects general prices primarily *insofar* as it affects the quantity of bank money: if bank rate fails to affect the quantity of bank money, it will not, according to the "first strand," affect price levels. The "quantity of bank-money" (M'), in turn, is, according to our Quantity Equation, equal to $c \cdot M_r$. If M_r increases as a result of the effect of a rise in bank rate upon gold imports—although, obviously, M_r may increase as the result of a change in any number of other factors affecting the balance of international payments—the effect would undoubtedly be, in Mr. Keynes's words, to "*increase* the basis of credit above what it would have been otherwise."[34]

That there is no conflict between this and the "first strand," however, becomes obvious as soon as it is recognized that Mr. Keynes might just as well have italicized the word *basis* in the phrase "the basis of credit."[35] Whether an in-

[32] *Treatise,* I, 189.

[33] *Treatise,* I, 190. It may be remarked, in passing, that Wicksell was certainly one "author" who was aware of the nature of the desired "synthesis." Cf. below, p. 217, n. 36.

[34] See the *Treatise,* I, 190.

[35] It is obvious, therefore, that there is no need to adopt what Mr. Keynes characterized as "probably the underlying assumption of the traditional doctrine, *i.e.,* that high bank-rate simultaneously reduces the aggregate

crease in the "quantity of bank-money" will actually result from the enlarged *basis* for expansion will depend upon the relation of bank rate to the "natural rate." [36] All that Mr. Keynes uncovered, in his "second strand," was one of the factors affecting M_r, which, in the case under discussion, will be affected first in the "order of events." It remains true that bank rate will affect prices by affecting the "quantity of bank-money" (M'); that M', in turn, will be affected by whatever changes take place in its two components, M_r and c; that under most conditions the principal factor affecting c will be the relation of market rate to the anticipated profit to be made by the use of a money loan; and that the "traditional doctrine's" description of the order of events as between changes in the "quantity of money" and changes in the relation of bank rate to "natural rate" is, in this particular case, precisely the same as the description which Mr. Keynes regarded as the correct one.

It would be easy to give additional examples of situations in which the "order of events" would be exactly that described by Mr. Keynes, including those in which the increase in the "quantity of money" that would bring market rate below the "natural rate" would be an increase in the "quantity of money" other than money of ultimate redemption. It has, for example, become an accepted part of central bank practice —particularly in recent years—to attempt to bring about a credit expansion by central bank purchases of securities in the open market. The increase in the "quantity of money" which is thereby involved, in

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superstructure of credit whilst increasing that part of its basis which consists of gold" (*Treatise,* I, 190 n.). The "traditional doctrine," instead of involving any such "underlying assumption," would have argued precisely as Mr. Keynes argued—namely, that a contraction of the credit superstructure upon the enlarged *basis* for expansion would not result "invariably" from the rise in bank rate, "but would require various special conditions for its fulfilment." Cf. the references to Wicksell in the following note.

[36] This, as a matter of fact, was precisely the solution presented in point (*f*) (*Treatise,* I, 219) in the list of "points," discussed on pp. 208 ff., above, which Mr. Keynes regarded as summarizing the differences between his own account of the "*modus operandi* of bank rate" and the traditional one. In fact, of course, the influence of the rate of discount upon capital movements, which had been stressed by writers on monetary policy at least as early as Tooke (see, in addition to the reference to Tooke's *History of Prices* given by Keynes [*Treatise,* I, 189], the note added by Tooke to the fourteenth of the celebrated seventeen theses which he advanced in his *Inquiry into the Currency Principle* [reprinted in the *History of Prices,* VI, 637]), was explicitly taken into account by Wicksell, who based his discussion on that of Tooke. See, for example, *Interest and Prices,* 92, and especially 112 f.

the first instance, would be an increase in Central Bank Money—that is, in the deposits maintained by "member banks" at the Central Bank. The effect of the excess member-bank reserves thus created, so far as the member banks themselves are concerned, would be in all essentials identical with the effect of excess member-bank reserves created by an increase in the amount of money of ultimate redemption—say, in the amount of gold imports.[37] There would be a tendency for market rate to fall below the "natural rate," with the usual effects upon the "order of events" which Mr. Keynes, at the time of writing the *Treatise*, regarded as the correct one.

It is, however, one thing to recognize that a given "order of events" as between changes in the "quantity of money," on the one hand, and changes in bank rate relative to market rate, on the other, is a possibility that is virtually certain to be realized under a given set of conditions; it is quite another to suggest that acceptance of a different "order of events" as the one which may be expected under other circumstances somehow involves error. In the present instance, as we have seen, we were asked by Mr. Keynes to accept the conclusion that, from the standpoint of "the order of events," it was *not* true to say that "a change of bank-rate affects the price-level because, in order to make the new bank rate effective, the quantity of money has to be altered."[38] If there is a difference between this proposition and the proposition describing the "order of events" which Mr. Keynes himself regarded as the correct one, it would have to turn upon the question whether a departure of bank rate from the "natural rate" is possible *before* there is any change in the quantity of money, regardless of whether the "money" involved is money of ultimate redemption or "bank-money."

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[37] The same thing would be true, of course, with respect to certain methods of issuing governmental paper money.

[38] *Treatise*, I, 220. The reader is reminded that in what follows, I am assuming that, by "effective," Mr. Keynes did not mean "effective in controlling the member-bank rate." If that were his meaning, the answer would be that the whole argument would simply be pushed one stage further back, since we should be dealing with the case of an increase in "Central Bank money" discussed above. In fact, however, as is pointed out on p. 213, n. 24, above, Mr. Keynes explicitly assumed, in his discussion of "The General Theory of Bank-rate," that "changes in bank-rate affect the market-rate of interest in the same direction," leaving the discussion of the relation between the central bank rate and member bank rate for the second volume of the *Treatise*. See the *Treatise*, I, 201; and cf. also p. 219, n. 42, below.

It may be stated at the outset that, so far as the "traditional doctrine" is concerned, there is no doubt whatever that it envisaged such a possibility. Wicksell, for example, certainly had in mind such instances whenever he discussed the case of a discrepancy between market rate and "natural rate" which was due to a change in the "natural rate" rather than to a change in bank rate.[39] Indeed, as we have seen, Mr. Keynes himself envisaged such a possibility in his discussion of the "Gibson Paradox."[40] A discrepancy between bank rate and "natural rate" might result, moreover, from any number of causes which would lower the market rate before there is any increase in the "quantity of money." One thinks, in this connection, of the discussion as to the relative importance, among the factors which may induce bankers to lower the market rate below the "natural rate," of the working of a competitive banking system, on the one hand, and, on the other, of the influence of pressure for cheap money by influential groups in the community or by political or banking authorities.[41] In the light of such possibilities, it must be obvious that the judgment of supporters of the "traditional doctrine" would be that expressed by Mr. D. H. Robertson: namely, that "this whole controversy is of the hen-and-egg order."[42] If, that is to say, the "hen" is a

[39] For examples of Wicksell's discussion of this type of situation, see *Interest and Prices,* 89, 167; and cf. also "Der Bankzins," *loc. cit.,* 236, 239, and Wicksell's *Lectures,* II, 186, 206.

[40] Cf. the references to the *Treatise* given on p. 197, n. 112, above.

[41] Cf., in this connection, Hayek, *Monetary Theory and the Trade Cycle,* 144 ff., 173 ff., and L. von Mises, *Geldwertstabilisierung und Konjunkturpolitik* (1928), 58 ff. It is true that the latter has sometimes (for example, on p. 59 of the work cited) presented his argument with reference to the influence of a cheap-money "ideology" upon bank rate in terms which would suggest that the issue of bank money is undertaken *first,* in order to bring down the rate of interest subsequently; but it is obvious that the actual "order of events" could perfectly well be reversed.

[42] See Robertson, "Mr. Keynes's Theory of Money," *loc. cit.,* 405. It is obvious, therefore, that it is not correct to interpret Mr. Robertson as having argued that the "order of events" *must* be "a change in the quantity of money which in turn affects the market rate of interest." Contrast Currie, *The Supply and Control of Money,* 5. One is inclined, indeed, to suspect, from Currie's use of the proposition that "the *central* bank can affect the rate of interest only by its control of the supply of money," that he was thinking of the power of the *central* bank to affect *member* bank rates, and of the weapons—such as open market operations—available to the central bank for the accomplishment of this purpose in countries like England and America. For the reasons for believing that this type of problem was not involved in Keynes's discussion of the "order of events,"

change in the quantity of money and the "egg" is a discrepancy between market rate and natural rate, then, according to the "traditional doctrine," whether the "order of events" is one in which the egg produces the hen or one in which the hen produces the egg depends entirely upon the particular set of conditions involved in a given situation.

So clear is this conclusion that one is again driven to ask why Mr. Keynes should have found in his understanding of "the order of events" involved in the *modus operandi* of bank rate a "fundamental" reason for disagreeing with the "traditional doctrine" with respect to the relative stress to be laid upon "the level of market-rate of interest relatively to the natural rate" and upon "changes in the quantity of money." Since it has been shown that the special "order of events" which Mr. Keynes regarded as the correct one was perfectly acceptable to supporters of "the traditional doctrine," one would be forced, in order to establish a difference between Mr. Keynes and the "traditional doctrine," to assume that Mr. Keynes would literally have been prepared to deny the possibility of an "order of events" which assumed that the process might start with a movement of bank rate away from "natural rate," even before there was any change in the "quantity of money." Yet such an interpretation is extremely difficult to adopt, for at least two good reasons.

The first of these reasons is that, as was pointed out above, certain specific parts of Mr. Keynes's analysis—as in the case of the "Gibson Paradox," for example—were based upon the assumption that a discrepancy between bank rate and natural rate may come about from the "natural rate" side—that is, without any necessary prior intervention of a changed "quantity of money" of such a character as to lower the market rate.

The second reason is that if we were to interpret Mr. Keynes as having argued that the "order of events" requires

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see above, p. 213, n. 24, and p. 218, n. 38. There can be no doubt, on the other hand, that Keynes's exposition on the subject has led to general misunderstanding. See, for example, his discussion of the relation of changes in the "quantity of money" to the "effectiveness" of bank rate in the *Treatise,* II, 366; and cf., in this connection, Hayek, "Reflections," Part II, *loc. cit.* (1932), 24.

that the "quantity of money" must be changed *before* bank rate could be reduced below market rate, we should be interpreting him as having demonstrated the opposite of what he set out to prove—namely, that all stress should be laid upon the "level of market-rate relatively to natural rate, rather than upon changes in the quantity of money." Indeed, in insisting that there must be a *prior* change in the quantity of money, he would be exalting changes in the quantity of money to a position which had been accorded to it by none of the outstanding figures in the development of "the traditional doctrine" with respect to the relationship between changes in bank rate and changes in the "quantity of money." Passages can be cited from Wicksell, for example, in which, from the fact that a discrepancy between bank rate and natural rate may come about before there is any increase in the "quantity of money," he deduced the conclusion that the "abundance or scarcity of money, and in particular the quantity of cash held by the banks, is now [that is, in such cases] imbued with a merely secondary importance."[43] Surely Mr. Keynes could have meant to say no less.

[43] So, for example, *Interest and Prices,* 167. That there is nothing, however, to warrant the suggestion that Wicksell felt that the "abundance or scarcity of money" had a "merely secondary importance" under all conditions and for the general purposes of the "Theory of Prices," is obvious from the fact that he went on immediately to point to the case in which such "abundance or scarcity" *would* have "an independent significance in regard to movements of prices"; namely, the case in which such "abundance or scarcity" would itself affect "market rate." Still less basis would there be, obviously, for using such passages as that quoted in the text to support an interpretation of Professor Ohlin's comment (Introduction to *Interest and Prices,* p. xiv) that "Wicksell successfully escaped from the tyranny which the concept 'quantity of money' has until recently exercised on monetary theory" as meaning that, according to Wicksell, we could dispense with the substance of the analysis embodied in our "Quantity Equations." In fact, of course, Professor Ohlin's reference to Wicksell's "brilliant assumption of a pure credit economy" (cf., for example, *Interest and Prices,* 70, 104) would indicate that the former was thinking of the quantity of *money of ultimate redemption;* so that the only dispute would concern the question as to how great the degree of "tyranny" of which he complains has actually been in the historical development of monetary theory. It is worth noting, at any rate, that Wicksell himself, so far from arguing that changes in the quantity of money of ultimate redemption are of no appreciable importance under modern conditions, actually insisted that "the influence of the supply of precious metals" was one of the "cornerstones" of the "mechanics of prices," the other "cornerstone" being his argument with respect to the "influence of credit or the rate of interest." (Cf. Wicksell, "The Influence of the Rate of Interest on Prices," *loc. cit.,* 213, 218.)

We are, therefore, driven to the conclusion that Mr. Keynes's insistence upon emphasizing the importance of a discrepancy between market rate and natural rate "rather than" the importance of changes in the "quantity of money" cannot, despite Mr. Keynes's explicit assurance to the contrary, have been meant to turn upon the issues involved in the tracing of the "order of events": the difficulty must have lain elsewhere. To have established this fact is to have removed the basis for another of the *Treatise's* attacks upon "traditional doctrine."

III

M' and the Maintenance of a Low Bank Rate

Faced with the difficulty of finding any real difference between Mr. Keynes's doctrine with respect to the *modus operandi* of bank rate and the traditional doctrine on the subject so far as the *order of events* is concerned, the commentators on the *Treatise* turned elsewhere in their attempt to find a basis for Mr. Keynes's implication that a true alternative was represented by emphasis, on the one hand, upon "changes in Bank-rate relatively to the natural rate," and "changes in the quantity of money," on the other. Concretely, the question then became whether, abandoning all emphasis upon the "order of events," Mr. Keynes's suggestion that it is wrong to say that "a change in bank-rate affects the price-level because, in order to make the new bank-rate effective, the quantity of money has to be altered," could be interpreted as alleging that "a change in bank-rate" could be made "effective" even if the quantity of money were *not* altered.[44] This, in turn, involved two possible interpretations, depending upon the meaning that was assigned to the word "effective." [45]

The first meaning of "effective," in this context, had to do with the question whether the banks could *continue to*

[44] This was clearly the interpretation put upon Keynes's argument by Hayek, "Reflections," Part II, *loc. cit.*, 23.

[45] The reader is reminded again that, by the terms of his argument, Mr. Keynes could not have intended to discuss the "effectiveness" of the rate of the central bank in controlling member-bank rates. Cf. above, p. 213, n. 24, and p. 219, n. 42.

maintain bank rate at a level lower than that of "natural rate" if the "quantity of [bank] money" were not increased.[46] On this point, undoubtedly, the "traditional doctrine" was perfectly explicit; for it argued emphatically that *in a free market without the rationing of credit,* an attempt to keep bank rate below the "natural rate"—the latter being interpreted as meaning the anticipated profit to be made by the use of a bank loan—would result in such an increase in the demand for bank funds at the low bank rate that only one of two things could happen. The banks might, on the one hand, give up entirely the attempt to keep bank rate at the low level: in that case, the bank rate would cease to be "effective." On the other hand, they might persist in the attempt; in that case, however, the low rate would be kept "effective" only so long as the banks were able to satisfy the increased demand for bank funds by creating a supply of "bank-money" *ad hoc.*[47]

The substance of the "traditional doctrine" has been so clear on this point that the only part of it calling for comment here is that which was italicized above—namely, that the doctrine could be expected to hold only in a free market without the rationing of credit.

It is obvious, in the first place, that the inclusion of this condition, instead of representing an easy method of escape, amounts merely to a translation into more concrete terms of the condition that the "bank-rate" must continue to be "effective" in the sense under discussion: that is, it must represent a price at which bank credit is actually obtainable by all borrowers of unquestioned soundness who are prepared to pay the price. No important economist has ever denied that it would be possible, by the device of rationing, to maintain a low price in the face of an increased demand, whatever he might think of the wisdom of such a device as compared with that of allowing the distribution of resources to be controlled by the working of the pricing system. On the contrary, Wicksell himself implied as much, as early as his *Interest and Prices,* in his discussion of Ricardo.[48] It is worth noting,

[46] Cf. Hayek, "Reflections," II, *loc. cit.,* 23.

[47] For references to Wicksell, which may be taken as typical of the "traditional" doctrine on this head, see Hayek, "Reflections," II, 24. (The corresponding references to the English translation of the *Geldzins und Guterpreise* and the *Vorlesungen* are pp. 110 and 194, respectively; there is an even more explicit passage, however, on II, 198 of the *Lectures.*) That in this respect Wicksell was a faithful custodian of the "traditional" position should be clear from a comparison of the passages quoted above with the celebrated passage in Ricardo's *High Price of Bullion* (*Essays,* edited by Gonner, p. 35; and cf. Hayek, *Prices and Production,* 14).

[48] Cf. Wicksell, *Interest and Prices,* 82 n.

also, that those Swedish economists, all of them avowed adherents to the segment of the "traditional doctrine" which is here under discussion, who made part of the special committee appointed by the Swedish Minister of Finance in 1918 to report on the monetary situation, were prepared to accept rationing as an alternative to the raising of bank rate, such "rationing" being regarded as an experimental method of restricting the issuance of bank credit in the face of an increased demand.[49]

It should be obvious, in the second place, that no serious limitation is placed upon the usefulness of the traditional theorem, as stated above, by the mere fact that, from one point of view, banks may always be said to be "rationing credit," in the sense that they are always selecting, from a "fringe of unsatisfied borrowers"—to use Mr. Keynes's phrase—those to whom they are prepared to lend at any given rate of interest.[50] In this sense, obviously, manufacturers and dealers in ordinary commodities might also be said to be "rationing" their goods at all times, since ordinary counsels of prudence will force them to refuse goods to those whose credit standing is not satisfactory, and to favor those customers whose credit standing is of the highest.

Nor is it difficult to translate this simple fact of experience into the terms of conditions of supply of, and demand for, a given commodity. On the one hand, the satisfaction of a large number of "unsatisfied" purchasers (or "borrowers") at a given price (or rate of interest) may be accompanied by a simultaneous improvement in the terms upon which those with higher credit standing are able to borrow. In this case, obviously, the whole supply curve may be said to have shifted downward. This was, in fact, what Wicksell obviously had in mind when he suggested that to speak of the effects of "an easing of credit" was a "more general" way of putting the proposition than to speak only of the effects of a reduction in the rate of interest.[51]

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[49] For a brief account of the episode referred to, see B. Thomas, *Monetary Policy and Crises,* 44 ff., where footnote references are given to the more important Swedish writings on the subject. A discussion of the contributions contained in these writings, from the standpoint of the light they throw upon the concept of a "natural rate," must be left for another occasion.

[50] Cf. Keynes, *Treatise,* I, 212 f., II, 364 ff. The concept was implied at other points in the *Treatise,* even if it was not given the specific designation indicated above. See, for example, the *Treatise,* I, 204, on why "market quotations are not at all times an equally good index of the ease with which new borrowers for investment purposes can be accomodated."

[51] See, for example, Wicksell, *Interest and Prices,* 87 (cf. also p. 96). See, in addition, the reference to "the terms of credit"—a phrase which appears also in Keynes's *Treatise* (for example, I, 184)—rather than to the "market rate of interest," on pp. 95, 105, 146, 149 of Wicksell's book. Substantially the same emphasis was given by Marshall in his *Evidence* before the Gold and Silver Commission; cf. the quotation given by Mr. Keynes himself, *Treatise,* I, 188, n. 1. It is obvious that to put the matter as concerning the "terms of credit" rather than "the" rate of interest simultaneously provides an answer to the contention of those who see in the fact that there is more than one "market rate" for different types of loans, as well as

On the other hand, the sellers (or lenders) may decide that the outlook for general business has improved so much that those buyers (or borrowers) whose ability to use profitably the resources advanced to them may previously have been a matter of doubt, are now clearly in a position to use them to advantage. In this case, what has really happened has been a shift in the demand curve upward, the supply curve remaining as before. The "fringe of unsatisfied borrowers," that is to say, is now "satisfied" not because of a change in the practice of the suppliers of funds, but because of a change in the credit position of the formerly "unsatisfied" borrowers themselves.

Both cases, it will be observed, would still represent instances in which the amount borrowed will depend upon the relation of bank rate to the anticipated profit to be made by the use of a bank loan. It is a pity that Mr. Keynes himself did not make this clear, instead of contenting himself with an assurance that "it is not difficult" to establish the precise nature of the relation of the "fringe of unsatisfied borrowers" to his general theory of bank rate.[52] His failure, indeed, to deal satisfactorily with the simple issues involved has given rise to more than a little confusion, particularly in the light of his suggestion that it would be only in a "perfect market"—in the sense of a market in which there was no "fringe of unsatisfied borrowers"—that we should expect to witness the realization of his earlier proposition that "given the demand-schedule of borrowers, the effective bank-rate . . . must uniquely determine . . . the volume of investment" (that is, the volume of borrowing from banks).[53]

In fact, however, the issues are simple enough. It should be obvious, for example, that when, as in the first case described above, an "expansion" or "contraction" of the "size" of the "unsatisfied fringe" is translated into a shifting of the supply curve, it *represents* a change

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for borrowers of different credit standings, a decisive argument against the traditional doctrine with respect to the *modus operandi* of bank rate. In reality, of course, there is in this fact merely an argument for the further development of the "traditional doctrine" along the lines which are suggested by the translation of the term "market-rate" into "the structure of market-rates": as Wicksell himself implied when he suggested that not only the "natural rate" but also the "market rate" was a "rather vague conception" (*Interest and Prices,* 120; cf. below, p. 239, n. 18). See, also, in this connection, the comments of Myrdal, "Der Gleichgewichtsbegriff, etc.," *loc. cit.,* pp. 402 f.; and cf. what is said on pp. 239 f., below, with respect to the relationship between the long-term and the short-term markets for money loans. A detailed examination of these matters, as well as of their bearing upon certain concepts of a "natural rate," must again be left for another occasion.

[52] *Treatise,* I, 213. Cf. also Ellis, *German Monetary Theory,* 424.

[53] *Treatise,* I, 212. On the meaning to be assigned to the phrase "volume of investment," in the present instance, see pp. 280 ff., below; and for an example of a discussion of the relation of the concept of an "unsatisfied fringe" to Keynes's proposition with respect to the "unique determination" of the "volume of investment," in the sense indicated, see Ellis, *German Monetary Theory,* 424; cf. also p. 226, n. 56, below.

in the "effective bank-rate."[54] In the second case, on the other hand —in all probability the one more commonly realized in practice—we should have a change in the "demand schedule."[55] It is true that the shift in the "demand schedule" which is involved in this second case is one that must be registered in the minds of the lenders as well as in the minds of the borrowers; but this is true of all cases involving the extension of credit in which a demand that was previously ineffective becomes "effective." The demand of a pauper for a pair of shoes may not be effective because the sellers of shoes do not regard the pauper as capable of paying for them. If business conditions should improve to such an extent that the pauper's prospects for employment become favorable enough to induce the seller of shoes to trust the erstwhile pauper, there is an increase in the effective demand for shoes: the relevant segment of the "demand schedule" for shoes shifts upward.

The condition for a "unique determination" of the volume of bank borrowing (in Keynes's argument, "the volume of investment") by the relation between bank rate and "natural rate," therefore, is not a "perfect market" in the sense that there must be no "unsatisfied fringe of borrowers" of the kind envisaged by Mr. Keynes. The necessary condition is a willingness on the part of the banking authorities to choose between, on the one hand, a raising of the bank rate to the level of the anticipated profit to be made by the use of a money loan, as estimated by them, and, on the other hand, an increase in the quantity of bank money as an alternative to an insistence upon keeping bank rate low relative to the "natural rate" without increasing the quantity of bank money—in other words, as an alternative to an insistence upon "rationing," in a significant sense of the term.[56]

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[54] It will be observed that in order to establish the fact that a lowering of "effective bank-rate" is actually involved in the satisfaction of a greater part of what was formerly the "unsatisfied fringe," it is not necessary to argue that, even if more funds are lent at the same absolute rate of interest, "the [market] rate of interest is really lower" by virtue of the fact that it includes a larger risk premium. (Cf. Currie, *The Supply and Control of Money*, 7.) There are grounds for believing, in fact, that an easing of the terms of credit is likely to take the form of a literal lowering of the rate of interest to borrowers of higher credit ratings, at the same time that "more money" is lent at the old rate to borrowers who previously obtained none at all. The risk premium, in other words, often remains unchanged, in such cases, *as between different borrowers;* and it would seem more consistent with the objective facts of the situation to speak of a lowering of the "effective bank-rate" only when the rates are literally lowered, leaving for translation into a shift of the demand schedule the consequences of a change in estimates of risk which makes the bankers willing to lend more at the old rates.

[55] A similar interpretation—though on the basis of a different type of factor affecting demand—is implied in Currie's discussion of the "unsatisfied fringe." See his comment on the "postponement of new issues" by security dealers, *The Supply and Control of Money*, 7.

[56] Contrast, in this connection, Ellis's discussion of the "unsatisfied fringe" (cf. above, p. 225, n. 52), which includes a reference to "rationing" (*German Monetary Theory*, 425), and from which it would therefore appear, if

It will be noted that the "unique determination" of the volume of borrowing from banks to which reference is here made is not a determination of the former by bank rate taken alone, but by bank rate relative to "natural rate." One of the necessary data for our problem, that is to say, is what Mr. Keynes called "the demand schedule." The other datum, as we have seen, is the supply schedule, which may also shift with an expansion or contraction of the "fringe of unsatisfied borrowers." This, however, is true of a "perfect market" as well as of any other kind of market. When, therefore, Mr. Keynes suggested that "if the supply of credit were distributed in an absolutely free competitive market, these two conditions—quantity and price—would be uniquely correlated with one another and we should not need to consider them separately," he was setting up claims for the theory of the determination of the rate of discount in a "perfect market" which no one has ever advanced for any other case of pricing.[57] A perfect market does not guarantee stability of both demand curves and supply curves; yet such stability would have to be assumed if one is to be able to deduce either quantity demanded or quantity supplied from the mere facts with respect to "price." The only thing, it may be repeated, in any way associated with the notion of a "perfect market" which is relevant to the question as to the relation between Bank-rate and the volume of bank-borrowing is that the "traditional" account of this relationship is intended to apply to conditions under which the banking authorities will not refuse *both* to raise Bank-rate to the level of the anticipated profit to be made by the use of a bank-loan *and* to increase the quantity of bank-money. In all other circumstances, it will still be true that the banks would be able to maintain the low bank-rate only if they were prepared to satisfy the increased demand for funds by increasing *ad hoc* the supply of bank-money.

If, therefore, Mr. Keynes had really intended to argue that an increase in the "quantity of bank-money" was not necessary in order to make a low bank rate "effective" in the sense of maintaining it for a considerable length of time below the "natural rate," he would have been advancing a doctrine which is in emphatic contrast with the "traditional" one. In fact, however, I have been unable to find a single passage in the *Treatise* in which Mr. Keynes was prepared to support such a position in so many words. In one pas-

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I understand the argument correctly, that it is in the possibility of identifying the latter with the case of the "unsatisfied fringe" that one may see a "devastating" comment upon any attempt, such as that imputed to Keynes, to establish the fact that changes in the quantity of bank money need not always be expected to result from changes in the relationship of bank rate to natural rate.

[57] Cf. the *Treatise*, II, 364.

sage, on the contrary, Mr. Keynes, in an effort to show that "Wicksell's expressions . . . can be interpreted in close accordance" with the argument of the *Treatise,* stated explicitly that "the market-rate of interest cannot be continually held even a little below the natural-rate unless the volume of bank-money is being continually increased."[58]

To such a statement it is necessary to add only that it is not a question of "interpreting" the traditional doctrine—as represented, say, in the writings of Wicksell—so as to permit of its translation into these terms: this *is* the traditional doctrine.[59] The only matter that could possibly occasion surprise is that Mr. Keynes could have thought that the relevant passages from Wicksell which support this contention show not only that he was "thinking along the same lines" as those followed in the *Treatise,* but that, in so thinking, he was adopting a point of view essentially different from the "first strand" in the "traditional doctrine"—that strand, namely, which regards bank rate "as a means of regulating the *quantity* of bank-money." [60] There is in all this, at any rate, no reason whatever for interpreting Mr. Keynes as having meant to argue that a "change in bank-rate" could be made "effective"—in the sense of being kept lower than the "natural rate" for a considerable period—even if the quantity of money were not altered.

Mr. Keynes's meaning, in the passage cited above, was certainly not made clearer by his reference to the differences between the argument of Wicksell and that of Cassel with respect to the *modus operandi* of bank rate.[61] That differences did exist between Wicksell's treatment of the concept of a "natural rate" and Cassel's, there can be no doubt.[62]

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[58] *Treatise,* I, 198.

[59] Cf. the references given above, p. 223, n. 47.

[60] Cf. the *Treatise,* I, 198. It will be noted that the statement from Keynes with respect to Wicksell appears in the former's discussion of the "third strand of thought" in the "traditional doctrine"—the strand, that is to say, which appeared to Keynes, at the time of writing the *Treatise,* to come "nearest to what seems . . . to be the essence of the matter" (*Treatise,* I, 190). Cassel, on the other hand (on whom see the discussion which follows immediately in the text), had been cited as one of the leading exponents of the "first strand" (*Treatise,* I, 188).

[61] Cf. the *Treatise,* I, 198.

[62] Again a fuller discussion of this matter—which, as it happens, provides a further instance of the way in which economic doctrines can be distorted in the process of transmission—must be left for the proposed publication mentioned on p. 40, n. 2, above.

Yet there can also be no doubt that the passage which Mr. Keynes cited from Cassel has no bearing whatever upon the truth of the proposition that, in Mr. Keynes's words, "the market-rate of interest cannot be continually held even a little below the natural-rate unless the volume of bank-money is being continually increased." [63] Cassel's reference to the "reaction of the capital market to an undue lowering of the rate of interest" as a reason why there need not be a "continuous rise of prices as long as the lower discount-rate is retained" had reference to his argument elsewhere that, as a result of the effect of a lower discount rate in expanding bank credit, the resulting increase in "capital" due to "forced saving" may itself lower the "natural rate." [64] Yet it should hardly be necessary to emphasize the fact that Cassel's point, instead of constituting an argument against the proposition of Wicksell which is now under discussion, simply amounted to saying that *if* the "natural rate" fell to the level of the low market rate, the discrepancy between natural rate and market rate would disappear, and with it the need for expanding the quantity of money in order to keep the bank rate "effectively" below the "natural rate." [65] Whatever

[63] The passage to which Keynes referred was that to be found on p. 479 n. of the first (1924) English translation of Cassel's *Theory of Social Economy*. Cf. also *ibid.*, p. 416.

[64] Cf. Cassel, *Theory of Social Economy*, 416 ff. The point in question has sometimes been dealt with in continental literature under the heading of the "Cassel-Wicksell controversy." The characterization is, however, hardly an apt one. For one thing, the point made by Cassel in the passage just cited had apparently been made prior to the publication of Cassel's *Theory of Social Economy*, which is usually referred to as having inaugurated the "controversy" in question. See, for example, Wicksell's *Lectures*, II, 198. Wicksell himself did not mention by name, either in the Preface or in the text of the second (1915) Swedish edition of the second volume of his *Lectures*, the "critics" who had raised the point. (The passage from the preface is reproduced by E. Sommarin on p. v of his Preface to the third [1929] Swedish edition; cf. also Ohlin's Introduction to *Interest and Prices*, p. xvii.) Conversely, the point was certainly implicit in Mises's discussion, as early as 1912, of the power of banking operations, through their effect upon the accumulation of real capital by way of "forced saving," to lower the "natural rate." Cf. the first edition of Mises's *Theorie des Geldes und der Umlaufsmittel*, pp. 414 ff. (The relevant passages were reproduced virtually without change in the second [1924] edition, 358 ff., on which the English translation [cf. 349 ff.] is based. See also p. 46 of the same author's *Geldwertstabilisierung und Konjunkturpolitik*.) It is true, on the other hand, that Professor Heckscher, who was chiefly responsible for inaugurating a more extensive discussion of the point by Swedish economists, took Cassel as his starting point. Cf. E. F. Heckscher, "Verkan af för låg räntefot" ("The Effect of Too Low a Rate of Interest"), *Ekonomisk Tidskrift*, XXIII (1921), Supplement in honor of Wicksell, 49.

[65] This was precisely the answer which was given by Wicksell, when, in commenting on Cassel's "criticism," he pointed out that there was nothing in it which invalidated the proposition that "a cause operates in the same direction as long as it persists." (Cf. p. 251 of the English translation of Wicksell's review of Cassell, published originally in 1919, and included in the English version of Wicksell's *Lectures*, Volume I.) This is not to say, of course, that arguments of the type advanced by Cassel—a similar one,

differences, in other words, existed between Wicksell and Cassel, these differences did not concern the proposition that "the market-rate of interest cannot be continually held even a little below the natural-rate of interest unless the volume of bank-money is being continually increased."[66] The only effect, indeed, of introducing a reference to Cassel was to create the impression that Wicksell and Keynes were united on a proposition that was somehow in opposition to the "traditional doctrine," of which Cassel was taken as a representative.[67] In fact, of course, all three were agreed upon a proposition that was in itself an integral part of "the traditional doctrine" on the subject. Once more, therefore, the chief effect of Mr. Keynes's incursion into the field of the interpretation of "traditional" doctrine seems to have been to create a previously non-existent "haze" not only with respect to the content of the "traditional doctrine" itself, but also with respect to the position of writers, such as Wicksell, who regarded themselves, and rightly so, as supporters of the "traditional doctrine" on the subject with which we have been here concerned.

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for example, was at least implied in the *Treatise* itself, as was pointed out by Mr. D. H. Robertson (*Economic Journal,* XLI [1931], 406)—are not important for the validity of certain concepts of the "natural rate." On the contrary, I hope to be able, on another occasion, to show that some of them are important in the extreme. It is equally important, however, to see that this type of argument is quite irrelevant to the present discussion, which proceeds on the assumption that by "natural rate" is meant only "the anticipated profit to be made by the use of a bank-loan."

[66] That this is so is perfectly clear from the fact that Cassel himself did not assert more than that "an extremely low rate of interest" could be maintained only "as long as the shortage of capital-disposal [read: voluntary savings in the form of money] is covered . . . by the issue of further bank means of payment" (Cassel, *Theory of Social Economy,* 415).

[67] Cf. above, p. 228, n. 60.

CHAPTER NINE

The Quantity of Money-Substitutes, *M'* (*Continued*)

I

Interest as "Capitalization-" and "Cost-Factor"

WE SAW in Chapter Eight that any interpretation of Mr. Keynes's proposition that it is wrong to say that "a change in bank-rate affects the price-level because, in order to make the new bank-rate effective, the quantity of money has to be altered" as implying a belief, on his part, that "a change in bank-rate" could be made "effective" even if the quantity of money were *not* altered, must involve a specific understanding as to the meaning of the word "effective." We have now to deal with the interpretation that is established when, by "effective," we mean effective in changing the *general price level.* The proposition would then be that a change in bank rate relative to "natural-rate" could affect the general level of prices even if a change in the quantity of money did not result from the discrepancy between bank rate and "natural-rate." [1]

That this interpretation should have been applied to Mr. Keynes's proposition was, in the light of its actual phrasing (which has to do with the "effect" of a change in bank rate upon the price level), not unreasonable. It can be shown, however, that although Mr. Keynes may have seemed to convey the impression that this was in fact his meaning, he also advanced a series of propositions which made it impossible to believe that he could have meant to defend his

[1] It should again be emphasized that the possibility that bank rate may affect general prices through its effect upon some variable of our Quantity Equation other than *M'* is not here in dispute. Cf. above, p. 186, and especially n. 78 thereto. What is involved here is the power of bank rate to affect general prices "directly," without any change in the magnitude of the stream of money relative to that in the stream of goods.

original proposition seriously; it can be shown also that in fact what happened was that Mr. Keynes was the victim not so much of loose reasoning as of his vigorous efforts to suggest that his own analysis, particularly with respect to the relationship between changes in bank rate and changes in the quantity of bank money, was in some essential respect different from that of the "traditional" analysis upon the subject.

Specifically, the argument attributed to Mr. Keynes was that changes in Bank rate could affect the price level, even though there were no change in the quantity of bank money, by virtue of the fact that the rate of interest may be regarded not only as a "cost-factor," but also as a "capitalization factor."[2] The argument was then supposed to be that a lowering of bank rate would result, through the process of capitalizing a given money yield of capital goods at the lower rate of interest, in a higher price for these capital goods. Conversely, a raising of the bank rate would result, through the capitalization process, in a lowering of the prices of these capital goods. Thus, merely as a result of the capitalization process—so this interpretation of Keynes's meaning ran—the price level could be affected directly by raising or lowering bank rate, even without a supporting change in the quantity of bank money.[3] Our problem, then, is to establish, first, what "traditional doctrine" had to say on the subject; and, secondly, the precise respects, if any, in which Mr.

[2] The articulate statement of the issue in these terms is due, as far as I am aware, to Ellis (*German Monetary Theory*, 303 f., 315, 358, 373, 415 ff.). Cf. also F. Machlup, "Interest as Cost and Capitalization Factor," *American Economic Review*, XXV (1935), 459 ff.

[3] For an example of an interpretation of Keynes along these lines, see the comments of Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 404, on the "curious sentences" in the *Treatise* which led one to suppose that Keynes regarded "the effect of interest-changes on the price of machines as arithmetical and mechanical," instead of regarding these price changes as "the resultant of the mutual impact of the relevant flow of money and the relevant flow of goods." See also Hayek, "Reflections," II, *loc. cit.*, 25, on such passages as that on p. 202 of the first volume of the *Treatise*, in which it was alleged that the connection between bank rate and the "price of capital goods" through the process of capitalization was "immediate, direct, and obvious"; and especially Ellis, *German Monetary Theory*, 417, where Keynes is interpreted as having argued that as a result of the possibilities inherent in the capitalization process, the general price level may advance or decline "even aside from [any change in] the 'monetary factor.'"

Keynes's argument differed from the "traditional doctrine."

It may be regarded as beyond question that "traditional doctrine" was prepared to accept the proposition that "prices" are affected by changes in the rate of interest through the process of capitalization. To deny this would be to allege that adherents of "traditional" economic doctrine would seriously argue that the concept of "capitalization," in and of itself, is an unreasonable and unfounded one, corresponding to no process that occurs in the actual world. Such objections as would be raised to the notion of interest as a "capitalization factor" would have to do, not with the notion of "capitalization" itself, but with the degree of realism which may be held to attach to the account of the process of "capitalizaton" given by those writers who would extend it to every conceivable branch of economic activity, or who would assume that a change in a given loan rate is necessarily an indication of an exactly equal change in the rate of interest used in the "capitalization" process, or who would insist that to regard the rate of interest as a "capitalization factor" necessarily means that it is not also and simultaneously a "cost-factor." Not one of these objections, it will be observed, can be interpreted as denying the reality of the capitalization process, as such. Not one of them, therefore, can be cited against the idea that changes in the rate of interest may be expected to affect "prices" through the process of capitalization, as such.

It goes without saying that the objections indicated are not of a sort that can be dismissed lightly by those in whose minds the process of "capitalization" takes on an overwhelming, if not exclusive, importance. It may not be amiss, therefore, to discuss briefly each of these objections —even at the cost of interrupting our main argument, which has to do with the position both of "traditional doctrine" and of Mr. Keynes with respect to the relation between interest as a "capitalization factor" and changes in the quantity of bank money in any reasoned account of the steps whereby changes in bank rate may be expected to affect prices. The reader who is not interested in the question of the limits which should be set to the proposition that interest is to be regarded primarily as a "capitalization fac-

tor" may, therefore, omit the rest of this section and pass directly to section ii of this chapter.

With respect to the first objection to the theory of interest as a "capitalization factor" which was stated above, it must certainly be granted that one would be justified in entering a demurrer, on grounds of realism, against those accounts of the pricing process which would insist that the phenomenon of "interest" and therefore of "capitalization" is present in every instance of pricing.[4] There can indeed be little doubt that if the element of capitalization is present in all transactions, it is surely of such slight importance in the minds of those engaged in many types of pricing transaction as to be virtually negligible. The most that can possibly be said in this connection, however, is not that it was wrong to suggest that interest acts as a "capitalization factor," but that it was unfortunate that sponsors of the notion that it does so act should have hurt their cause by seeming to make what were bound to be characterized as extreme claims.

The point is worthy of more than passing notice in the present instance, in the light of certain aspects of Mr. Keynes's own exposition. In the first place, the prices which Mr. Keynes regarded as subject to the "direct" influence of changes in the rate of interest were those included in his *P'*, which in turn had been defined formally by Mr. Keynes as the price of "new investment goods."[5] Since "investment goods," in Mr. Keynes's definition, included not only "fixed capital," but also those parts of "non-available output" which were represented by

[4] The best known examples of a universalization of the phenomenon of interest in its "capitalization" aspect are, of course, those provided, first, by the proposition of F. A. Fetter to the effect that there is such a thing as "a prevailing price for timeliness," which pervades the whole economic structure of society, so that it may be said that in everything that a man "can buy" there is a "time-price" which is "capitalized" (see, for example, Fetter, *Economic Principles*, I, 308 ff.); and, second, by the proposition of Irving Fisher to the effect that the "discount principle"—that is, the phenomenon of "capitalization"—applies to "all property and wealth": not only "stocks, land . . . buildings, machinery," but "anything whatsoever," including even such things as "clothing" (Fisher, *The Theory of Interest*, 17, 326; cf. the same author's *The Nature of Capital and Income*, Chapter XIII, *The Rate of Interest*, Chapter XII, and *Elementary Principles of Economics*, 406 ff.).

[5] For Keynes's application of the "capitalization" process to *P'*, specifically, see the *Treatise*, I, 154. For the definition of *P'* as the "price-level of new investment goods," see *ibid.*, I, 137. It is worth noting, however, that on p. 154, where the "capitalization" argument was first presented, Mr. Keynes defined *P'* as the price level of "*capital* goods"—with the results, in the way of uncertainty as to the area over which he believed the phenomenon of "capitalization" to be relevant, that are recorded above.

"unfinished goods in process," as well as such consumption goods as were added to stocks on hand, it is not surprising that commentators on the *Treatise* should have wondered whether Mr. Keynes really intended that the process of "capitalization" should be thought of as extending over the wide area which was encompassed by his "investment goods."[6] There were other passages, to be sure, in which a distinction was drawn between "investment goods" and "capital goods"; and the fact that, in Mr. Keynes's exposition, it was usually "capital goods" to which the "capitalization" process was supposed to be applied, led one to suppose that Mr. Keynes thought of this process as being confined to a much narrower range of products than was suggested by his formal statement of the capitalization process as relating to P', the price of "new investment goods."[7] A further limitation of the effect of the rate of interest upon prices through the process of capitalization seems to be implied in the use of illustrations involving the case of "*fixed* capital": although, since the other element included in "capital goods" was that part of working capital "which will emerge from the productive process as Fixed Capital," it was not unreasonable to interpret these illustrations as a shorthand way of indicating that Mr. Keynes believed that the process of capitalization might be extended to the ingredients of "fixed capital" by anticipating their share in the value of this fixed capital.[8] It will be noted, moreover, that the category of "Fixed Capital" was very broad, since, according to its formal definition, it actually included all "goods in use"![9]

It was something of a surprise, therefore, to learn, from Mr. Keynes's "Rejoinder" to Mr. Robertson, that he had not had in mind, as "typical capital-goods affected by changes in the pure rate of interest," even such forms of "fixed capital" as "machines," but rather such things as "'houses' and 'buildings' generally, or 'roads' and 'railways.'"[10] Just how much of a change in view was represented in this statement may be gauged by comparing it, for example, with the passage in the *Treatise*

[6] For Keynes's definition of new "investment goods" (or "the output or production of Investment goods"), see the *Treatise*, I, 130 f. For his definition of "non-available output," see I, 127; and for the definition of "hoards," which were included within his definition of "investment goods," see I, 129. On the relation of the definition of P' as the price of "investment goods" to the rate of interest as a "capitalization factor," cf. Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 398.

[7] For the distinction between "capital goods" and "investment goods," see the *Treatise*, I, 130, 201. For an example of the application of the "capitalization" process to "capital goods," cf. the *Treatise*, I, 202, and also I, 154.

[8] On the components of "the output of capital goods," cf. the *Treatise*, I, 130. For examples of the application of the capitalization process to "fixed capital," rather than to "capital goods" as such, see *ibid.*, I, 202 ff. An argument not greatly dissimilar from that suggested in the text as a possible basis for applying the concept of "capitalization" to "increments of working capital" was suggested by Adarkar, *The Theory of Monetary Policy*, 48. Contrast, however, Ellis, *German Monetary Theory*, 420.

[9] Cf. the *Treatise*, I, 128.

[10] Keynes, "Rejoinder" to Robertson, *Economic Journal, loc. cit.*, 422.

in which Mr. Keynes had specifically stated that the goods on whose prices the effect of changes in bank rate—as opposed to those in "bond rate"—would, through the process of capitalization, be "quantitatively important," were precisely those "goods of which the future yield will be spread over a very short period."[11] The suggestion involved in this passage, which would have extended the process of "capitalization" to those goods whose subjection to it was most doubtful, should be contrasted with that involved in a passage from Wicksell which has sometimes been cited as providing the clearest instance of the latter's recognition of interest as a "capitalization factor."[12] That Wicksell, as a matter of fact, refused to consider "capitalization" at all, in such cases, is obvious from his statement that "a fall in the discount rate on three months' bills from four to three per cent per annum would, as will easily be seen, directly raise the price of goods purchased [by those receiving funds at the lower rate of discount] by one-quarter per cent at the most."[13] Clearly, the ¼ per cent rise would come about, according to Wicksell, not from an anticipated rise in the price of the product, as in the case of the 25 per cent rise due to a change in the long-term

[11] Cf. the *Treatise,* I, 203.

[12] See, for example, Ellis, *German Monetary Theory,* 303. Ellis cites the passage as it appears in Wicksell's *Lectures* (II, 195 f. of the English translation); but the substance of it is found also in all of Wicksell's better known writings on the *modus operandi* of bank rate. See, for example, "Der Bankzins," *loc. cit.,* 234 f.; *Interest and Prices,* 91 f.; "The Influence of the Rate of Interest on Prices," *loc. cit.,* 215 f. It may be pointed out here that the passage in question was strangely interpreted by Mr. Keynes, who cited it (*Treatise,* I, 198 f.) as proof of his contention that Wicksell was "very explicit" on the different degrees of sensitiveness to changes in bank rate exhibited by "investment," on the one hand, and "speculation," on the other. In fact, of course, Wicksell was contrasting, not "investment" and "speculation," but a case in which capitalization was involved with one in which capitalization was not involved. (On Mr. Keynes's distinction between "speculation" and "investment," see also pp. 283 f., below.) There is, moreover, no justification in the passage for Keynes's statement that Wicksell held "that the rate of investment is capable of being affected by small changes in the rate of interest, e.g., ¼ per cent" (*Treatise,* I, 203). The "¼ per cent" of Wicksell's illustration was the change in *price* that would be expected to come about from the saving in interest cost associated with a fall in rate of interest of 1 per cent; and it was part of an argument designed to *minimize* the importance of the consequences of changes in bank rate, in certain cases—not to emphasize the importance of those changes. The substance of Wicksell's argument on this head, indeed, is precisely that which Keynes himself stated briefly in a footnote on I, 263, of the *Treatise.*

[13] So, for example, Wicksell, *Lectures,* II, 195. The passage in question is regarded by Ellis (*German Monetary Theory,* 303 f.) as evidencing a desire on the part of Wicksell to minimize the importance of interest as a cost factor. Actually, what Wicksell was minimizing was not its influence as a cost factor—which was not under discussion in this passage—but its importance as a "capitalization factor" in the particular type of transaction he was discussing. There is, therefore, no basis for Ellis's suggestion that this passage from Wicksell is inconsistent with the one which Ellis cites as evidencing a belief on the part of Wicksell that interest was important as a cost factor.

rate, but from the savings in interest cost, the assumption being that the *selling price* of the product under discussion would not rise at all as a result of the lowering of the rate of discount.[14]

More important, for our present purpose, is the second objection mentioned above: namely, that all changes in the market rate of interest are not necessarily an indication of changes in the rate which is used for purposes of "capitalization" in those transactions in which the process of "capitalization" is undoubtedly real. That there is great weight in this objection can hardly be denied. For, surely, to be prepared to purchase a long-term investment at a price which would involve a capitalization of anticipated money yields at 3 per cent, simply because the market rate of interest happened to be 3 per cent, would be absurd if in fact the prospect were that the market rate would rise in the near future to 5 or even 4 per cent. The capitalization rate, in other words, must surely be based at the very least upon the anticipated course of market rates over the period during which the investment is expected to be made. It follows, therefore, that unless we are to attribute to those engaged in the pricing of investment goods a degree of irresponsibility which it would be hard to credit, any reliance upon an effect of bank rate, through "capitalization," would be almost certain to be disappointed whenever there is no assurance that a given bank rate will remain unchanged over a period long enough to be of importance for the calculations of those interested in the pricing of investment goods.

It is worth noting, in this connection, that Wicksell, on more than one occasion, emphasized the fact that if low money rates are to be expected to have an appreciable effect of the type indicated, there must be a prospect of their remaining low for a "sufficiently long period."[15]

[14] This interpretation is confirmed by a further passage on p. 95 of *Interest and Prices,* where Wicksell, discussing precisely the same case—namely, one in which "an improvement in the terms of credit enabled our business man to pay a higher cash price for goods which he was going to sell in three months' time"—stated explicitly that the entrepreneur was assumed to proceed upon the assumption that "he was due to receive no more than the normal sale price." Cf. also *Interest and Prices,* 142, where again it is specifically stated that plans are assumed to be made on the expectation that goods will be sold at the "normal" (that is, previously prevailing) prices.

[15] See, for example, the *Lectures,* II, 195; cf. also *Interest and Prices,* 92, and "The Influence of the Role of Interest on Prices," *loc. cit.,* 216. In

Mr. Keynes, on the other hand, gave more than a little trouble to his critics by seeming to suggest, in a passage in which interest as a "capitalization factor" had been pushed into the foreground, that the interest rate would have a "decided effect" even if *"the change in the rate is believed to be a short-period fluctuation."* [16] In fact, however, a careful examination of the passage indicated discloses that Mr. Keynes was discussing the effect, not of interest as a "capitalization factor," but of interest as a "cost factor."

Keynes's argument, in the passage in question, was simply that a high rate of interest, for example, would discourage borrowing precisely *because* it was not expected to last, by virtue of the fact that the expectation of a future fall would cause such "investments" as were "capable of postponement . . . without serious disadvantage" to be postponed in fact until lower rates prevailed in the market.[17] Surely, however, if what were involved were interest as a capitalization factor, the expectation of lower rates would mean the expectation of higher prices for capital goods, so that, so far as long term expectations with respect to selling prices are concerned, borrowing would not be discouraged at all by the temporarily high rate. If it is discouraged, it is only because the prospect of being able to borrow at a lower rate of interest later on will mean that the *cost* of the investment will be less at that time. The same argument applied, obviously, with respect to an expectation that interest rates will rise in the near future. If all that were involved were interest as a capitalization factor, this would mean a prospect of lower selling prices for "capital goods" in the future, and therefore, so far as this factor is concerned, a discouragement to borrowing. If, as Keynes argued, there is an actual encouragement to borrowing in the fact that the rate is expected to be low only temporarily, it must be solely because the *cost* of borrowing is less now than it is expected to be in the future.

The argument with respect to the danger of assuming that changes in bank rate are necessarily an indication of changes

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the light of these passages, as well as of Wicksell's argument with respect to the necessity for maintaining bank rate for a period "sufficiently long" for its influence to be communicated to the long-term market (cf. pp. 239 f., below), it must be obvious that his criticism of Tooke's argument with respect to interest as a cost (cf. pp. 249 ff., below) as involving the "doubtful possibility" that "the same procedure has precisely opposite consequences according as it is applied for a long or for a short period" (see *Interest and Prices,* 99), was directed specifically against the particular argument advanced by Tooke, and was not intended as a general denial of the possibility that different effects might be expected to follow in the short and in the long period.

[16] Cf. the *Treatise,* I, 204 (italics mine). Mr. Robertson, who quoted an earlier sentence (Treatise I, 203) of the passage in which the phrases quoted appear, as part of his discussion of Keynes's treatment of interest as a "capitalization factor," was naturally puzzled. See Robertson, "Mr. Keynes' Theory of Money," *loc. cit.,* 404.

[17] Cf. the *Treatise,* I, 203.

in the rate used in the "capitalization" process obviously applies *a fortiori* when account is taken of the fact that, under certain conditions, the movements in bank rate are not typical even of the movements in those types of "market rate" which apply to the sort of investment the yield of which is being "capitalized." It is, therefore, hardly surprising that one of Wicksell's criteria for deciding how "long" the period during which bank rate must be kept depressed in order to have an effect upon prices was that it should be "sufficiently long" to "influence the rate on long term loans also." [18] Mr. Keynes's own emphasis upon the relation of the short rate to the long rate is too well known to require comment here.[19] It need only be added that the fact that, under certain conditions, the connection between the short-term and the long-term markets may be broken is no reason for arguing either that there is never any connection between the two, or that the whole theory that changes in bank rate may affect prices, through "capitalization" or otherwise, is erroneous. What is "erroneous" is, as has so often been the case, not the statement of the problem that one finds in the writings of "acknowledged masters of the subject," but the oversimplified doctrine with respect to the "effectiveness" of bank rate which is found most commonly in the writings of those self-appointed iconoclasts who set up such oversimplified doctrines in order the more easily to refute them.

It need hardly be added, also, that, in making such a statement, there is no attempt to deny that the whole question of the relation between the

[18] See Wicksell's *Lectures*, II, 195, and the other passages cited on p. 237, n. 15, above. It is true that Wicksell sometimes wrote as if a close interconnection between the long-term and the short-term markets were to be expected under all circumstances (see, for example, *Interest and Prices*, p. 75). On the other hand, he was prepared, even as early as *Interest and Prices* (see, for example, p. 120), to admit that not only the "natural rate" but "the money rate" was a "rather vague conception"; and in the *Lectures* he not only called attention to the fact that there may be a "difference" between interest on short and on long dated loans which is "not completely . . . levelled out by the credit market," but he went so far as to suggest that only "interest on long dated loans" could be expected to correspond to the "natural rate,"—the implication being that he was not prepared to press his argument except on the assumption that it is possible for the banking authorities to affect the long-term rate also. Cf. the *Lectures*, II, 191.

[19] See, for example, the *Treatise*, I, 200 f.; II, 352 ff.

short-term and the long-term markets is one which still requires careful study on both the theoretical and the empirical sides. In fact, of course, such a study is called for not only on its own account, but also for the light which it may be expected to throw upon such questions as the effectiveness of changes in bank rate upon the direction of the use of credit, as well as upon the amount of credit in use, to say nothing of the light it would be expected to throw upon an evaluation of those forms of the "monetary" theory of the business cycle which rely heavily upon changes in the "structure of production" subsequent to changes in bank rate as an explanation of the course of events in the sphere of production and in the money market. The results obtained from a study of the kind suggested would, moreover, undoubtedly reflect back upon the question of the validity of certain concepts of a "natural rate of interest."[20] The point made here is merely that the fact that the theory of interest as a "capitalization factor" involves a careful statement of the ways in which the process of capitalization is associated with changes in market rate generally and with the structure of market rates in particular, constitutes no reason for rejecting the concept of "capitalization" in its entirety.

Most in need of clarification, however, are the issues suggested by the third, and last, of the objections cited above as having been raised to an emphasis on interest as a "capitalization factor"—namely, that such emphasis implies that interest is not also and simultaneously a cost factor. If the emphasis in question necessarily carried such an implication, there can be little doubt that it would be definitely misleading. It is, in any case, easy to demonstrate that it *should* carry no such implication.

The first point in such a demonstration must be the establishment of the proposition that, for our purposes, the process of "capitalization" represents, in effect, a judgment as to what is likely to happen to the value of a given investment upon the basis of assumptions with respect to the prospective money yield of that investment, on the one hand, and the prospective course of the market rate of interest, on the other.[21] Capitalization is, therefore, in the first instance, a factor which affects the *selling prices* of the investments in question.

[20] A demonstration of this proposition, as of the others advanced in the same paragraph, must again be left for the study on "The Natural Rate of Interest" to which reference has so often been made.

[21] The phrase "the prospective course of the market rate of interest" is of course to be understood in the light of the limitations and qualifications stated on pp. 237 f., above.

It is, of course, obvious that since capital goods may be sold for use in the production of other capital goods, an increase in the selling price of some capital goods must mean an increase in the costs of the "investment" using these capital goods. It is also true, as we shall see in section iii of this chapter, that the process of capitalization may be expected to lead to a bidding up of the prices of the factors involved in the production of the "investment" whose anticipated selling price will have risen as the result of capitalization.[22] Yet there is nothing in either of these facts which should lead to a denial of the proposition that capitalization is to be thought of as increasing, in the first instance, the *selling price* of "investments," rather than their cost.

With respect to the first point mentioned, for example, it may be pointed out that, as far as *each investment which is the subject of borrowing* is concerned, the important thing is the effect upon selling price of the capitalization of the yield from the investment in question. Naturally, the cost of the elements involved in this investment will affect the *yield* to be capitalized; and it is of course true that the prices of other capital goods which have themselves been subjected to capitalization will be elements in cost. Insofar as each one of these capital goods is the subject of a loan transaction, therefore, capitalization will affect the selling price in each case. Once the price of any one of these capital goods has been determined by the process of capitalization, however, this price is a datum for the next entrepreneur, who then calculates the yield obtainable from his own projected investment on the basis of the level of costs thus established; and the process of capitalization as applied to the yield so calculated is what will determine the anticipated selling price of this projected investment.

In dealing with the second point, on the other hand, it is necessary to distinguish between the effect of capitalization *per se*, and the effects of what follows from the act of capitalization. The words "in the first instance" were intended to call attention to just this fact. The subsequent steps described in section iii of this chapter will, of course, ultimately affect costs, and therefore the yield to be capitalized. Yet this does not alter the fact that the process of capitalization, as such, is directly relevant to the determination of the selling price of an investment on the basis of the assumption of a given yield.

Establishment of the fact that changes in the rate of interest may be expected, through the process of "capitalization," to affect the *selling prices* of investments does not, however, exhaust the list of the ways in which changes in the rate of interest may be expected to affect the profitability of borrowing. The next proposition to be established, indeed, is that the answer to the question whether it pays to undertake a given investment depends not only upon estimates as

[22] Cf. below, pp. 259 f.

to its future selling price, but upon whether the resources involved in its production can be obtained upon a basis of *cost* which will leave a satisfactory margin of profit.

A further proposition is obvious: *interest* is one of the costs involved.[23] It follows, therefore, that *in every case of new production,* interest is in every respect as important an element in cost as it is in "capitalization."

It is of some importance to stress the fact that, in the problem under discussion, what is primarily involved is the effect of capitalization upon the value of *new*—that is, *contemplated*—investment. Keynes's argument, after all, was essentially that the effect of the process of capitalization upon the value of old investments was important primarily insofar as it encouraged or discouraged the production of new investment goods.[24] This is clearly the way to state the problem when it is a question of tracing the influences affecting the amount of borrowing. It is obvious, however, that a stressing of the effect of capitalization on the estimated value (that is, the selling price) of *new* projects makes even clearer the necessity for taking account of the factors entering into the cost of production of those new projects.

It will be observed also that, in order to demonstrate that recognition of the importance of interest as a "capitalization factor" does not mean denial of the fact that it is also, and simultaneously, a cost factor, it is not necessary to argue, as some have argued, that interest as a capitalization factor may be reduced to the same thing as interest as

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[23] In dealing with certain arguments regarding the effectiveness of changes in bank rate upon the amount of borrowing, it might be just as necessary to emphasize that interest is "*one* of the costs" as it is here to emphasize that it *is* a cost. It would be necessary, for example, to emphasize that it is *one* of the costs involved, both as against those, on the one hand, who have tended to exaggerate the importance of interest cost as compared with other forms of cost, and as against those, on the other hand, who have been so greatly impressed by the importance of other costs than interest cost that they have tended to reason as if interest cost could be ignored in all, or in most, cases. An examination of the arguments involved, and of the bearing of the whole matter on the concept of the "natural rate" must, however, be deferred to another occasion. It is sufficient here to point out that Wicksell himself certainly recognized the importance of other costs than interest cost. This was implicit, for example, in his discussion of the effect of wage costs upon the determination of the "natural rate" (for example, *Interest and Prices,* 141, 150), as well as in his discussion of the effect upon other cost items (such as wages) of a saving in interest costs (see, for example, *ibid.,* p. 95, and cf. pp. 249 f., below). It is hardly surprising, therefore, that he should have been prepared to recognize that different types of business would be sensitive to changes in interest cost in different degree. See, for example, *Interest and Prices,* p. 89; and on the importance of this type of consideration for the identification of the "marginal borrower," cf. what is said on p. 244, below, and n. 28 thereto.

[24] Cf., for example, the *Treatise,* I, 203.

a cost factor, on the grounds that "increased capitalization" is merely the "present expectation of continued low cost." [25] The "present expectation of continued low cost" is, rather, one of the elements which, since it forecasts greater earnings, gives a greater expected yield to capitalize. The capitalization of this greater expected yield is itself, however, a further process, the effects of which have every right to be described and judged independently.

The importance of distinguishing between the two types of factor becomes obvious, indeed, as soon as account is taken of the fact that, when once the prospect of a rise in selling price as a result of the capitalization process begins to become widely accepted as reasonable, there will be a tendency to bid up the cost of other factors of production, so that actually the absolute yield on the investment may be no greater than it was before the rate of interest fell.[26] If the process of capitalization represented merely the "present expectation of continued low cost," it must be obvious that the effect of the process just described would be to force down the price of "investments" to the level at which they were before the interest rate fell, since the *total* cost would no longer be "low." Yet it is the essence of the concept of interest as a "capitalization factor" that, even if absolute yields remain at the level at which they were before the fall in the rate of interest, the effect of a fall in the capitalization rate must cause a rise in the price of "investments." The mere fact, therefore, that in practice a fall in interest rates, for example, may cause the value of "investments" to rise by causing *both* their degree of capitalization *and* their yields to rise should not be allowed to obscure the central fact that two effects are operative rather than one. For to do so would be to ignore the fact that different undertakings which present the same yield for "capitalization" may present great differences in the extent to which that yield is sensitive to interest cost.[27]

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[25] So, for example, Ellis, *German Monetary Theory*, 420.

[26] The tendency referred to is again that which is discussed in section iii of this chapter. It is, of course, also the tendency which lies at the heart of the argument of those who put great stress upon change in the relation of the prices of producers' goods to the prices of consumers' goods as a result of a change in the rate of interest, and the subsequent effects upon the "structure of production." It will be observed, however, that one can accept the general account of the "bidding up" process without necessarily accepting that part of the accompanying argument which rests upon the assumption that if the original change in the interest rate came from an increase in savings, no mistakes of entrepreneurial judgment will be made, whereas if it came from an increase in the quantity of bank money, such mistakes will be made.

[27] The point is therefore of wider significance than is implied by the suggestion of Ellis (*German Monetary Theory*, 420) that it is primarily for the sake of those goods "to which capitalization is inapplicable by reason of their transitory character" that it is necessary to retain emphasis upon interest as a "cost-factor," since in all other cases "capitalization and cost" may be regarded as "the concave and convex sides of the same shield."

The thing to be said, therefore, with respect to "interest as a cost factor" is not that it is of no importance relative to interest as a capitalization factor, but that it is possible to demonstrate, *a priori,* that the financial structure of different enterprises may be such as to make interest charges a "cost factor" which is of comparatively slight importance to *certain particular enterprises.* The theory of the forces determining the degree of sensitiveness to changes in the market rate of interest which will be shown by different types of enterprise is, in fact, anything but the simple problem it has seemed to some writers. It is, moreover, a theory which, when stated with some degree of precision, can be shown to have very serious consequences, indeed, for certain ideas widely held in what is generally called "interest theory," and therefore for certain concepts of a "natural rate of interest." The important thing to be observed, however, is that the issues involved are not such that they can be adduced in support of the contention that interest is of *no* appreciable importance as a cost to *any type of borrower.*[28]

With these principles established, we may pass to a discussion of the question whether the Keynes of the *Treatise,* who has been interpreted as having drawn a "sharp distinction" between "the operation of the market interest rate as the *capitalization* factor" and its operation as a "*cost* of production," is also to be interpreted as having argued that interest operates *solely* as a capitalization factor, and never, or rarely, as a cost factor.[29]

That there are certainly passages in the *Treatise* which would seem to indicate that Mr. Keynes definitely believed that interest was of virtually negligible influence as a "cost factor" can hardly be open to

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[28] The broad principle involved is of course simply that of the so-called "marginal analysis." The failure of critics of Wicksell, and indeed of critics of the whole "traditional" analysis, to recognize the truth of this simple proposition has led to an amount of confusion so great that it is not easily forgivable even when account is taken of the fact that Wicksell sometimes expressed himself so carelessly as to provide some reason for the error of interpretation involved—as, for example, when he substituted "the *average* profit on capital" for the "marginal profit" (cf. "The Influence of the Rate of Interest on Prices," *loc. cit.,* 214, 216). An examination of the literature involved, however, as well as of the deep-lying issues that are stirred when one examines the nature of the forces involved in the determination of the position of "marginal borrower," must again be left for another occasion.

[29] For an example of the first of these interpretations, in particular, see Ellis, *German Monetary Theory,* 303.

doubt.[30] Yet again we must be sure that we are not confronted with another of those instances of a conflict between profession and practice which did so much to obscure, for readers of the *Treatise,* the true scope of Mr. Keynes's intentions.

In one case, for example—that represented by Keynes's well-known critique of Hawtrey's account of the effect of changes in the rate of interest upon the amount of borrowing through the effect of these changes upon "the increased costs of business"—he did, to be sure, charge Hawtrey with having presented "a very incomplete account of the normal *modus operandi* of a higher bank-rate."[31] A "very incomplete account," however, is not necessarily an erroneous account, as far as it goes; and, for those who are prepared, as Wicksell was, to admit interest as a "capitalization factor" along with interest as a "cost factor," there is no quarrel here.[32] For the rest, moreover, Keynes's quarrel with Hawtrey turned, not upon whether interest was important as a "cost factor," but upon whether it was important as a "cost factor" to those whom Hawtrey singled out for the purpose—namely, the "traders."[33] This, however, is a vastly different thing from arguing that interest was important as a cost to no class of borrowers. When, on the other hand, we seek for evidence that Keynes was actually prepared to defend the latter proposition, what we find is largely a repetition of the type of argument with which we were concerned in the preceding chapter—namely, that it is not the cost of borrowing *taken by itself* which affects the borrower, but the cost relative to the profit to be made by the use of the money loan.[34] As we have seen, however, this is precisely the classical doctrine with respect to the influence of interest as a cost, so far as the total quantity of bank credit demanded is concerned.[35]

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[30] For page references to these passages, see the notes immediately following.

[31] *Treatise,* I, 194.

[32] On Wicksell's emphasis on interest as a "capitalization factor," see Ellis, *German Monetary Theory,* 303. On the suggestion that certain passages in Wicksell would indicate a desire on his part to minimize the importance of interest as a "cost factor," cf. p. 236, n. 13, above.

[33] See the *Treatise,* I, 195, and especially II, 131 ff. For Hawtrey's reply, see the *Art of Central Banking,* pp. 366 ff., and *Capital and Employment,* pp. 116 ff. The precise scope of Keynes's criticism was unfortunately obscured by the fact that, in the first of the passages cited from the *Treatise,* Keynes did not indicate whether the "element" in Hawtrey's theory which he felt could be "confuted" was its supposedly "exclusive" reliance on "the increased costs of business resulting from dearer money" or the fact that Hawtrey had supposedly "assumed without investigation" that the "additional costs" would "materially affect the trader" more than they "affect the manufacturer."

[34] See, for example, Keynes's insistence upon the necessity for taking into account the effect upon the trader and the manufacturer of "the current and prospective rate of off-take for the goods he deals in and his expectations as to their prospective price-movements" (I, 195; cf. also II, 137 ff., 145).

[35] It is, of course, true that the precise nature of the relation of what Keynes called the entrepreneur's "expectations as to . . . prospective price

There remains a passage which, at first blush, seems unequivocal: "A fall in the rate of interest stimulates the production of capital goods *not* because it decreases their *cost of production* but because it increases their demand-price." [36] Actually, however, a closer examination of the passage shows that Keynes's argument not only had to do with the effect of a change in the rate of interest upon the demand price for *certain types of goods*—namely, "capital goods"—but also was actually dependent upon the importance of "interest as a cost": "The effect of easier credit on the *costs of production* should be . . . to cause a change-over from certain forms of production to other forms; namely, from those for which it is a relatively unimportant cost to those *for which it is a relatively important cost.*" [37] This, as we shall see, is precisely the "classical" doctrine on the subject.[38] It can hardly be represented, at any rate, as alleging the unimportance of "interest as a cost."

In fact, however, it is not necessary, in order to demonstrate that Mr. Keynes was prepared to recognize the importance of interest as a "cost-factor," to deduce a conclusion to this effect from arguments in which the issue was involved only indirectly. His direct utterances on the subject were quite explicit. We were told, for example, that it is necessary not only that entrepreneurs should be "able" to expand output, but that they should be "willing" to do so: "and in order that they may be willing, as well as able, to do this, the rate of interest which command over . . . resources *costs* must not be *so high as to deter them.*" [39] We were told also not only that the thing which will determine whether entrepreneurs will tend to expand output was whether "the average price-level of output as a whole . . . corresponds to the average rate of remuneration of the factors of production," but also that one of the major factors which will decide this is whether the *"terms of credit"* are "easier" or "stiffer" than they would have to be in order to keep costs (the "rate of remuneration of the factors of production") at their "equilibrium level": that, indeed, "booms and slumps are simply the expression of the results of an oscillation of the *terms of credit* about their equilibrium position." [40] When attention is called, finally,

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movements" (I, 195; cf. II, 145) to the "natural rate" has been obscured by a careless treatment which does not always distinguish between this element and others that are involved in "the anticipated profit to be made by the use of a money loan." It is enough, however, to call attention again to the fact that there is ample warrant for the suggestion that the latter definition, which, as we have seen (cf. above p. 198, n. 114), can be found in Wicksell, is simply one of the more inclusive definitions that have been given to the "natural rate." It certainly represents a usage of such long standing, in any case, as to warrant its inclusion as part of the "classical doctrine."

[36] *Treatise,* I, 211 (italics mine).

[37] *Treatise,* I, 211 (italics mine).

[38] Cf. below, pp. 248 ff.; and, for a discussion of the misunderstandings to which the passage cited has given rise, see below, pp. 252 ff.

[39] *Treatise,* I, 182 (italics mine).

[40] *Treatise,* I, 183 f. (italics mine). It should hardly be necessary to point out that the last proposition quoted represents a much more ex-

to the passage cited above, in which Mr. Keynes's principal reason for arguing that changes in the rate of interest would be expected to "have a decided effect" on the amount of borrowing "even if the change in the rate is believed to be a short-period fluctuation" was based upon the notion of interest as a "cost-factor" rather than upon interest as a "capitalization factor," it will be seen that, despite all superficial appearance to the contrary, he left the position substantially as it had been before he wrote the pages which have been interpreted as having exalted "interest as a capitalization factor" to a position which amounted to a virtual ignoring of interest as a "cost factor."[41] That position, as we have seen, was that both aspects of interest are to be regarded as important, the degree of importance attaching to each to be decided upon the basis of the data presented in each concrete case.

II

Wicksell on Interest as a Cost, and M'

We are now prepared to resume our main argument, which has to do with the question whether there is anything in the concept of interest as a "capitalization factor" that would warrant acceptance of the proposition that changes in bank rate could be expected to affect the price level directly, even without a supporting change in the quantity of bank money. In the light of this major question, the issues discussed in the preceding section are of altogether minor importance.

It is, in fact, impossible to understand what is involved in the argument regarding the effect of interest as a "capitaliza-

treme position with respect to the rôle of bank rate—and indeed of money and credit generally—in the trade cycle than would be granted by a very large number of students of the problem. It was distinctly not the position of Wicksell. See, for example, his *Lectures,* II, 209 ff.

[41] Cf. above, p. 238, and n. 16 thereto. It will be observed, moreover—though it is certainly true that Keynes himself did not make the point clear—that it is precisely in instances of the kind adduced by Keynes that interest is of *most* importance as a cost. For whenever new flotations—the "waves" of "new borrowers," in Mr. Keynes's phrase (*Treatise,* I, 204)—represent a demand for capital for entirely new enterprises, "borrowed capital," instead of representing the small percentage of total capital which is usually used for purposes of illustration by those who would minimize the importance of interest as a cost, is 100 per cent of total capital. Since, moreover, this "borrowed capital" is "newly" borrowed, interest charges as a whole, for such firms, will vary, as between any two periods at which borrowing might occur, directly with the relevant market rate of interest. The point has much more profound implications both for the general theory of interest and the "natural rate" than is commonly recognized.

tion factor" upon "prices" unless it is recognized explicitly that whatever differences of opinion may exist with respect to the relative importance of interest as a "capitalization factor," as compared with its importance in some other respect (say, as a "cost factor"), these differences are entirely irrelevant to the question whether changes in the rate of interest may affect the general level of prices even when these changes in the rate of interest have no effect whatever upon the quantity of bank money. The best way of demonstrating this, indeed, is to demonstrate that, so far as the "traditional doctrine" is concerned, the argument with respect to the direct effect of interest upon "prices" is precisely the same regardless of whether interest is regarded as a "capitalization factor" or as a "cost factor." As a matter of fact, the clearest statement which we have on the subject—namely, that of Wicksell—was made precisely in connection with interest as a "cost factor." It would be well, therefore, to state the substance of the "traditional doctrine" regarding the "direct" influence of interest upon prices in terms of interest as a "cost factor," and then apply it, *mutatis mutandis,* to interest as a "capitalization factor."

The first element in Wicksell's argument, and the one to which it is of the greatest importance that attention should be called, is his emphatic and reiterated insistence upon the necessity for distinguishing between *relative* prices and something which may properly be referred to as the general level of prices.[42] That this distinction has been abused by monetary theorists in the past, and that Wicksell himself may be charged with having overworked it in other parts of his argument, cannot be denied.[43] Still less can it be denied,

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[42] See, for example, *Interest and Prices,* 1, 23 f., 39, 99 f., 105; "Der Bankzins," *loc. cit.,* 229 f., 235 n.; *Lectures,* II, 154, 158 f., 196. The failure to make the distinction in question was also described, in the Preface to the German edition of the *Lectures* (*Vorlesungen*), p. xi, as representing "an example of the confusion, as common as it is dangerous, of the individual point of view with the social point of view in economic problems" ("des privatökonomischen mit dem volkswirtschaftlichen Gesichtspunkte").

[43] The criticism usually directed against Wicksell, in this case, is, of course, that, in his anxiety to establish the case for a stable level of "general prices," he glossed over the possibilities of a disturbance of the internal price structure which might occur under cover of a "stable" level of general prices. See, for example, Hayek, *Monetary Theory and the Trade Cycle,* 111 ff.; *Prices and Production,* p. 20. It need be added

however, that, for certain problems, the distinction is and must remain one of the very first importance. That the issue under discussion represents one of these problems will be made clear by a consideration of Wicksell's application of it in his treatment of certain supposed "direct" effects, upon "prices," of changes in the rate of interest when interest is regarded as an element in *cost*.

Concretely, Wicksell took, as his starting point, the argument of Tooke, who, not content with pointing to empirical evidence which was supposed to disprove the traditional doctrine with respect to the relation between Bank-rate and prices, attempted to develop a logical argument designed to show that, as a matter of fact, we should expect, not an inverse correlation between Bank-rate and prices—which according to Tooke was demanded by the classical argument—but a positive correlation. The reason advanced was that since interest was an element in cost, any rise in interest rates must have the effect of raising prices, because the entrepreneurs would add the increased cost to prices in an effort to cover their increased outlays.[44]

To this, Wicksell had correctly retorted that of course it was true that higher interest costs would, other things being

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here only that a careful examination of the controversial literature on this subject, including that in Swedish, to which Wicksell himself contributed, will show that the wrong of the dispute was not always on his side. It must be obvious, moreover, from the discussion in the text as well as from Wicksell's place in the history of the concept of "forced saving" (on which see especially Hayek, "A Note on the Development of the Theory of Forced Saving," *Quarterly Journal of Economics*, XLVII [1932], 132 f.) that Wicksell cannot be said to have been blind to the possibility that the internal price structure, as well as the price "level," may be greatly affected by monetary factors, even if what he had to say on the subject was only "incidental" to the accomplishment of other purposes (cf. Hayek, *Monetary Theory and the Trade Cycle*, pp. 112 f.).

[44] See, for example, Tooke, *An Inquiry into the Currency Principle*, pp. 81, 124 (number fourteen of the famous seventeen "conclusions"; cf. Tooke's *Thoughts and Details on the High and Low Prices*, I, 165; *History of Prices*, VI, 636 f.; also III, 166 f.). Curiously enough, this aspect of Tooke's position, which he himself obviously regarded, in the words of Wicksell (*Lectures*, II, 183), as the "foundation and forefront" of his logical—as opposed to his empirical—argument against what he held to be the classical position, was not mentioned by either Keynes or Hawtrey in their discussions of Tooke (see the *Treatise on Money*, I, 195 f., and *The Art of Central Banking*, 366 f.), although attention had been called to it by Professor Gregory in his summary of Tooke's views on the relation between the "rate of interest" and prices (see Gregory's Introduction to the 1928 reprint of the *History of Prices*, 27).

equal, be reflected in a rise in the prices of those goods in which interest was a large element of cost *relative to the prices of goods in which interest was a smaller element in cost;* but that this constituted no reason for supposing that the *general level* of money prices would be raised.[45] The thing that would decide this, Wicksell insisted, would be, not the height of the "interest rate" as such, but the height of the market rate relative to the "natural rate": for it was only a discrepancy between the two which would make possible the expansion in the quantity of bank money that was necessary to support the higher level of *general* prices.[46]

Of course it is possible that an increase in the cost bills of a given group of entrepreneurs might make them *seek* more bank credit to cover their increased outlays. That this was a possibility which Wicksell would have readily admitted is obvious from his statement of the relation of a rise of costs generally to entrepreneurial applications for a greater amount of bank credit. From his discussion of this issue, it is clear that Wicksell was not only prepared to admit, but actually to insist, that the rise in money costs would be "previous" to the application for, and therefore the possible issuance of, additional bank credit.[47] A careful reading of

[45] For Wicksell's refutation of Tooke, see the former's "Der Bankzins," *loc. cit.,* 235 n.; *Interest and Prices,* 93, and especially p. 99; *Lectures,* II, 182 ff., and cf., in addition, the comments in the Preface to the German (1922) translation of the second volume of the *Lectures* (*Vorlesungen*), x–xi.

[46] It is the first importance that the reader should bear in mind that, in the argument that follows, the term "natural rate" is used solely in the sense of the "anticipated profit to be made by the use of a bank loan." A discussion of the complications which are introduced when certain other definitions of the "natural rate" are used, as well as of the complications that are introduced in the case of "direct inflation," must be left for another occasion.

[47] See, for example, *Interest and Prices,* 144 and 166, on the time-relation between a rise in "money wages and money rents" and the issuance of a greater amount of bank credit. It will be obvious, from the discussion in the text, that an interpretation of passages of this type as indicating that Wicksell "inferred from the rise in the price of instruments and productive services a corresponding rise in price of *all products,*" is without foundation. For such an interpretation, see, for example, G. Masci, "Variazioni dei salari e dei prezzi," in *Economia Politica Contemporanea* (Essays in honor of Professor C. Supino), Vol. I (1930), 383 ff. Professor Masci's own argument that "it is not that the expansion of productive activity leads to an increase in prices, and that this leads to an expansion in the quantity of circulating media, but that expansion in productive activity leads to an extension of credit and of the circulation, and

the passages in question shows, however, where he felt that the real determinants of price-rise lie. It must, indeed, be clear that whether an increased amount of bank credit will in fact be extended to the entrepreneurs who, faced by the prospect of an increased cost bill, apply for a greater amount of such credit, will depend in part upon the judgment of the bankers as to whether the members of this particular group would be able to sell their products at a higher price, and thus be able to make a profit from the use of the loan over and above their increased interest costs.[48] Since there is no reason, however, to suppose that those to whom interest was not so important an element in cost would *lower* their selling prices, a rise in the prices of the products of those for whom a rise in interest cost demanded a significant rise in selling price would be possible only if the *total amount of money-spending power were increased.*[49] *In the absence of any force increasing the total amount of money-spending power,* clearly, the entrepreneurs in question would not be able to raise their prices. So long as they are unable to do

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this causes [general] prices to rise" (*op. cit.,* 385 n.) is, indeed, strictly in accordance with the Wicksellian analysis. The same thing is true of Masci's further contention that in the absence of a separate argument, in terms of the variables of the "equation of exchange," as to why general prices should be expected to change, the sole effect that may be expected is a change in relative prices (Masci, *op. cit.* 386).

[48] See, for example, *Interest and Prices,* 144, where it is made clear that a condition for continued profitability is that there will be a *"rise in all prices"* sufficient to cover the increased cost bill (italics Wicksell's). Cf. also *ibid.,* p. 95.

[49] In the example given on p. 144 of *Interest and Prices,* this condition is implicit in Wicksell's assumption that "the increase in the demand for loans" will be *"met by the banks"* (italics mine). The context shows, moreover, that Wicksell must have regarded the banks as creating *ad hoc* the funds which they lend. See, for example, his references to the financing of the transactions involved by means of "bank-drafts" (p. 144), which he had previously (p. 139) discussed in a context—specifically, in connection with the "quantity of means of exchange" which banks may "issue"—that shows that he thought of the banks as "creating" the amount of media of exchange demanded. Wicksell would of course have been prepared to admit that, in a setting in which banks were unable to create money substitutes, the "increase in the total amount of money-spending power" to which reference is made in the text, might take the form of an increased velocity of circulation resulting from an increase in bank lending. See, in this connection, *Interest and Prices,* 138 f.; and, on the general relation to the present argument of an admission that a discrepancy between natural rate and market rate might affect other factors in our Quantity Equation besides *M',* see above, p. 186, and n. 78 thereto.

so, the anticipated profit to be obtained by the use of a bank loan (the "natural rate") would be less than the rate of interest which they would have to pay in order to obtain the loan (the "market rate").[50] They would, therefore, either abandon voluntarily their loan applications or be forced to do so by bankers whose interpretation of the "natural rate"–"market rate" relationship with respect to their businesses would be better founded than theirs.

It must be obvious, from the discussion just presented, that we are again provided with a demonstration of how greatly beside the point are the contentions of those who would argue that simply because a rise in the cost bill of certain entrepreneurs may, under certain circumstances, precede the expansion of bank credit (M'), and because costs are, after all, prices, we may say that the expansion of bank credit cannot be the "cause" of the change in general prices: "The circulating medium is not a cause of prices; it is only a convenient means of exchanging goods after the price has been already fixed."[51] Surely one need not be accused of reviving outmoded controversies over the definition of a "cause" in being prepared to insist that the *necessary condition* for any sustained price rise, in cases of the type discussed above, is the increase in the quantity of bank money, a failure of which to expand in response to a "demand" for it must immediately bring to an end all prospects of a continuance of "prices" on the level which is supposed to have been "fixed" in complete independence of what happens to the quantity of money. It follows also that there is not the slightest reason whatever for interpreting in the sense indicated above, Wicksell's own statement to the effect that "abundance or scarcity of money . . . is . . . imbued with a merely secondary importance" in cases in which "such factors are to be regarded as consequences of changes in the demand for instruments of exchange brought

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[50] The use of the term "the natural rate" as a synonym for "the anticipated profit to be obtained by the use of a bank loan" means, of course, that each enterprise will have its own "natural rate." Actually, of course, most of the better-informed users of the latter term imply, not without reason, that the "natural rate" with which they are concerned is some kind of "marginal" rate. The location of the "margin," in turn, has usually implied a further criterion with respect to the dimensions of the supply of loanable funds. For our present purpose, however, which is the description of the forces that give M' the magnitude which it has, the "margin" is to be thought of as being determined in every case by reference to the market rate itself. According to the usage suggested—which, it must be repeated, is adopted only as a temporary stopgap until the whole complex of problems associated with the concept of a "natural rate" will have been given an adequate treatment—the "natural rate" of a given entrepreneur will be infra- or intramarginal, depending upon whether it is below or above the market rate.

[51] The quotation is from J. L. Laughlin, *The Principles of Money*, 317.

about by changes in the level of prices."[52] Such an interpretation would be ruled out automatically by the categorical nature of Wicksell's judgment on the central position of the Banking School, even if it were not ruled out by the fact that the statement quoted appears in connection with Wicksell's account of the reasons for believing that the banks may control prices by adjusting the position of the market rate to the "natural rate."[53] Still less ground, therefore, is there for interpreting this statement as implying a desire, on Wicksell's part, to minimize the importance of the quantity of bank money as a factor affecting prices.[54]

It is obvious that precisely the same type of argument as that developed above with respect to the effect upon "prices" of a raising of interest costs will apply to the case of a lowering of interest costs. The one thing that is certain, in such cases, is that a single cost item—interest cost—will have been reduced. In order, however, to be able to argue that this will result in a general lowering of prices through a *general* lowering of costs, it is necessary to demonstrate, first of all, that the lowering of interest costs will have no effect upon other costs. In fact, of course, there is every probability that other costs will be affected.[55]

Let us assume, in order to put the best possible face upon the argument under discussion, that the entrepreneurs to whom interest is important as a cost will lower their selling prices by an amount proportional to the reduction in interest

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[52] *Interest and Prices,* 167.

[53] *Interest and Prices,* 167. For Wicksell's critique of the Banking School position, see *ibid.,* pp. 81 ff.; *Lectures,* II, 173 ff. Cf. also his comments (*ibid.,* II, 154) on what he ironically called "the modern reasoning" according to which money is "regarded as a kind of amorphous, infinitely elastic, or plastic mass which adapts itself without any pressure to any price level and is therefore entirely passive in relation to the pricing mechanism." See finally, and especially, his summary comment, in "Hinauf mit den Bankraten!" *loc. cit.,* 751 f., on the controversy as to whether "the increased note-issue is the cause or the result of the rise in prices": ". . . it is neither one nor the other, in my opinion: the rise in prices, and as a rule, also the increase in the quantity of notes in circulation, are both to be regarded as consequences of the *too cheap credit* which is granted by the banks of issue."

[54] It is to be observed, moreover, that Wicksell himself went on to say explicitly that, in certain cases, of which he proceeded to give examples, the sequence of events may certainly start with a change in the "abundance or scarcity of money." See *Interest and Prices,* 167, and cf. above, p. 184.

[55] Cf. Wicksell, *Interest and Prices,* 98 f.; "The Influence of the Rate of Interest on Prices," *loc. cit.,* 215; *Lectures,* II, 180, and especially 183.

costs.[56] In such a case, unless the demand for their products is completely inelastic, they will find that they are able to sell more of these products.[57] They will therefore have a greater incentive to expand operations than will those entrepreneurs to whom interest is not so important as a cost. This means, of course, that they will be prepared to offer a higher price for the materials of production than would those who have not been benefited equally by the reduction in interest costs. The particular materials of production involved will, as a result, tend to rise in price. All users of these materials must, therefore, if they wish to avoid loss, raise the price of their product in proportion to the rise in the price of materials. In the end, consequently, even if we assume an initial fall in the prices of the particular goods in the production of which interest is important as a cost, this fall will be balanced by a rise in the price of other goods, so that there need be no change in general prices.[58]

[56] It is obvious that in the case in which selling prices are *not* lowered, the effect upon costs other than interest costs may be expected to be even more immediate, as a result of the fact that the entrepreneurs concerned are able immediately to devote money-spending power which would otherwise have gone to pay interest charges to expenditure upon the materials of production. It is noteworthy, however, that, as early as *Interest and Prices* (see for example, p. 91), Wicksell was prepared to consider a situation in which the sellers of the goods produced at lower interest cost would be "forced by competition" to lower their prices somewhat. He even went so far as to point out that, in cases in which goods were manufactured for future delivery, "on the basis of previously arranged prices," it is conceivable that the entrepreneurs promising such delivery may fail to anticipate the rise in costs other than interest cost, and so reduce their prices in proportion to the reduction in interest cost. See, for example, *Interest and Prices,* 93, 98; and cf. "Der Bankzins," *loc. cit.,* 235 n., and the *Lectures,* II, 184. Occasionally, therefore, Wicksell would use illustrations which assumed that the lower interest cost would be partly passed on in lower prices, as the result of "competition" among sellers, and would partly accrue to the entrepreneurs as an extra profit which could be used immediately to bid up the price of the materials of production. See, for example, *Interest and Prices,* 91.

[57] It will be noted that, for purposes of the problem in hand, it is not necessary to assume that *all* entrepreneurs to whom interest is important as a cost are dealers in products the demand for which is elastic. It is quite sufficient that *some* of them should be; for, in that case, we should have the greater incentive for some producers to expand operations which is required by the argument.

[58] In fact, of course, the producers who will have reduced their selling prices in proportion to the reduction in interest cost will find it necessary sooner or later to raise their selling prices to cover the increase in the cost of materials for which their own intensified bidding has been responsible. There would still remain, however, the differential between their prices and the prices of those to whom interest was less important as a cost.

In this second case as in the first, therefore, the thing which will determine the absolute extent of the compensating rise in the prices of goods dealt in by the less favored entrepreneurs will be whether or not there is a change in the total quantity of money-spending power; and this, in turn, will depend upon the relation between bank rate and the anticipated profit to be made by the use of a money loan by each entrepreneur involved. It may happen, for example, that the rise in the cost of materials to the less favored producers may be so rapid and so appreciable as to convince the bankers that these producers are not likely to be able to use bank loans profitably even at the lower rate of interest. In that case, the entrepreneurs in question will be deprived of credit, and, *if the bankers do not choose to issue an equivalent amount of credit to the more favored entrepreneurs,* we shall indeed have a case in which a "lowering" of interest costs will have led to lower general prices. In such a case, however, it must be obvious that the real cause of the lower level of general prices is the fact that, low as interest costs may be absolutely, they are too high as compared with the profit that can be made by the use of a money loan by an appreciable number of entrepreneurs.

It is, of course, much more likely that the banks, if they withdraw credit from the less-favored entrepreneurs, will reissue it to the more favored entrepreneurs, whose "natural rate"–"market rate" ratio will remain favorable.[59] In that case, the prices of the materials involved may be expected to continue to be bid up as long as the profit to be made by the use of a bank loan is such as to permit the borrowing of funds at the going rate of interest. When that point is reached, prices of the commodities in which interest is more important as a cost will, of course, be lower than those in which interest is less important. Whether, however, *general* prices will have fallen will depend upon what has happened to the quantity of bank money; and this, in turn, will depend upon what has happened to the relation between the market rate of interest and the profit to be made by the use of a money loan. In the last analysis, therefore, an argu-

[59] On the usage of the term "natural rate" which is here involved, see n. 50 to p. 252, above.

ment such as that of Tooke, which attempted to demonstrate the existence of a direct correlation between movements in bank rate and movements in prices because of the influence of interest as a "cost," instead of leading to a minimizing of either the influence of bank rate on the amount of borrowing from banks, or the influence of the quantity of bank money upon prices, not only turns out to be altogether illusory in itself but actually strengthens the logical force of the "traditional" argument with respect to both the "natural rate"–"market rate" relation and the quantity of bank money, which are thus again seen to be part and parcel of a single coherent argument.

There can be little doubt that, in arguing along the lines indicated above, Wicksell believed himself to be arguing along the lines which were clearly implied by the substance of "traditional" doctrine.[60] The only passages of which I am aware in which a contrary impression might seem to be conveyed are the passages, in the second volume of his *Lectures,* in which Wicksell discussed those parts of Ricardo's doctrine with respect to the effect of the rate of interest upon prices that Wicksell himself regarded as "much less convincing" than other parts of Ricardo's exposition.[61] In these passages, Wicksell actually went so far as to seem to attribute to Ricardo precisely the doctrine that Tooke had adopted in an attempt to refute what he regarded as the "commonly received opinion"—the doctrine, namely, that a lowering of the rate of interest would tend to lower general prices rather than to raise them. In fact, however, as Wicksell himself went on to show, to interpret Ricardo as having held such a view would be to interpret the latter as having turned his back completely on some of the fundamental propositions whose truth he himself had done so much to establish.[62] There are reasons, therefore, for questioning whether Wicksell, ordinarily the soul of generosity in the treatment of his intellectual forebears, did not do Ricardo less than justice in accusing him of having advanced, in connection with the issue under discussion, "a vague and

[60] It must be remembered that Wicksell was explicit in recognizing that the theory which he had developed was "very much on the same lines" as that of Ricardo. Cf. above, p. 174, n. 45, and the references there given.

[61] See Wicksell's *Lectures,* II, 179 ff. There is also a brief comment, in "The Influence of the Rate of Interest on Prices," *loc. cit.,* 215, to the effect that "Ricardo, in his earlier writings, seems to have believed" that the substance of the argument which appeared later in Tooke's writings was sound.

[62] See, for example, Wicksell's comments upon the inconsistency of the doctrine attributed to Ricardo with the latter's statement of both the principle of comparative costs and the reciprocal relation of profits and wages (*Lectures,* II, 180).

partially erroneous argument, which could not fail to exercise an unfavourable influence on the subsequent discussion of the subject"; though it is only fair to add that Wicksell's final judgment of the passage in Ricardo was that it was "a hasty interpolation" that "has no connection with his general point of view," whereas, as Wicksell pointed out, "in Tooke it is the foundation and forefront of his theory."[63]

As it happens, an interpretation of the troublesome passage in Ricardo in the Marshallian spirit of Ricardian interpretation makes it unnecessary to fall back upon the hypothesis that the passage was not only a "hasty interpolation," but actually represented an internal inconsistency in Ricardo's argument. In point of fact, as Wicksell himself recognized, Ricardo, in the passage quoted, was satirizing the argument of those who, simply by printing more bank notes, would reduce the rate of interest virtually to the minimum dictated by the necessity for covering the costs of carrying on the business of banking.[64] All that we have to do in order to make the whole of the passage consistent with Ricardo's general system is to adopt the inherently reasonable hypothesis that the satire did not end with what Ricardo had to say about the ability of banks to lower the rate of interest permanently to such a level, but went on to include a typical example of reasoning which Ricardo himself obviously thought absurd. The particular example happened to be the supposed influence of a lowered interest rate in reducing prices, and thus improving the competitive international situation of the country with the supposedly lower interest rates. It is significant that Ricardo's exclamation: "To what absurdities would not such a theory lead us!" *follows* this additional example, instead of preceding it; and it is equally significant that, in what follows, no trace of what later came to be the heart of Tooke's doctrine can be found.[65]

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[63] Wicksell, *Lectures,* II, 182 f.

[64] Wicksell himself characterized Ricardo's argument on this head as a conscious *reductio ad absurdum.* See the former's *Lectures,* II, 179.

[65] The passage, which was quoted by Wicksell on pp. 179 f. of the second volume of his *Lectures,* is from *The High Price of Bullion* (p. 36 of *Ricardo's Economic Essays,* as edited by Gonner; the version given in the English translation of the *Lectures* is a retranslation of Wicksell's own translation from the English). It may be added that the "peculiarity" of the argument which Wicksell found in the fact that "at the beginning, and subsequently, he [Ricardo] refers to a lowering of business profits, but at the end he seems to be referring to the possibility of raising them" (Wicksell, *Lectures,* II, 180) tends to confirm the interpretation given above. For the supposed reduction of "profits" as a result of the lowering of the rate of interest was part of the argument which Ricardo obviously regarded as absurd. The statement, on the other hand, that "the increase of bank notes . . . cannot add to our profits nor lower interest" comes *after* Ricardo's exclamation "To what absurdities would not such a theory lead us!" and is obviously Ricardo's way of suggesting his own opinion concerning the relation between interest and profits—an opinion which emphatically did not involve a belief, despite many statements to the contrary by historians of doctrine, that "interest" and "profits" were identical. The latter point is extremely important for an understanding

It need only be added that there is no evidence that Tooke derived his argument from the particular passage in Ricardo under discussion. Given Tooke's antipathy to the Ricardian position generally, and the fact that he was not exactly given to a careful study of the great master's writings, this is hardly surprising.[66] There is, therefore, every reason for doubting whether Wicksell was right in implying that, by virtue of the passage in question, Ricardo exercised "an unfavourable influence on the subsequent discussion of the subject"—particularly since, as far as I am aware, the passage in Ricardo was ignored not only by Tooke but also by writers other than Tooke in their support of an argument similar to the latter's. The truth seems to have been rather what Wicksell himself implied in the rest of his discussion: that Ricardo's central proposition with respect to the nature of the *modus operandi* of bank rate upon prices was "very much on the same lines" as the theory which Wicksell himself had developed; that the materials for the answer to an argument such as that of Tooke lay ready at hand in the Ricardian system itself; and that, in articulating the argument on the basis of the available material, Wicksell was himself both supporting and advancing the "traditional doctrine" on the subject.

III

The Argument Applied to Interest as "Capitalization Factor"

The application of Wicksell's general theorem as to the differences between the effects of changes in the rate of interest upon general prices, on the one hand, and their effects upon relative prices, on the other, to the argument with respect to interest as a "capitalization factor" should be immediately obvious.[67] For the sake of argument, let us assume that changes in the rate of interest have, in a significant degree, that direct effect of raising the prices of capital goods, through the effect of the changes in the rate of interest upon the "capitalization" of the yields of those goods, which

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of the development of the theory of the forces determining the rate of interest and of the part played therein by "real capital," on the one hand, and pecuniary factors, on the other. The point cannot, however, be developed here.

[66] Cf., in this connection, what is said on p. 194, above, with respect to Tooke's failure to quote Ricardo at any point in his argument against the "commonly received opinion" with respect to the effect of bank rate upon prices.

[67] For Wicksell's discussion of interest as a "capitalization factor," see *Interest and Prices,* 91 f., 95 f.; *Lectures,* II, 195 f.; "The Influence of the Rate of Interest on Prices," *loc. cit.,* 216.

Keynes, among others, has attributed to these changes.[68] By the terms of the Wicksellian argument, as long as the quantity of bank money was prevented from expanding *by the market rate's being kept equal* to the "natural rate," all that could happen would be a change in *relative* prices, in accordance with the relative amount of capitalizable income attaching to each type of good.

It is easy to see why this should be so. It is to be expected that, in the first instance, the producers who anticipate a rise in the selling prices of their products as a result of the capitalization of the expected income from them at a lower rate of interest will apply for more credit at the banks, since, *for them,* the anticipated profit to be made by the use of a bank loan will have risen. Granted the permanence of the data upon which the capitalization procedure is based, their expectation of a rise in the price of their product is not unreasonable, and there is no reason why the banks should not satisfy this demand. Before we can conclude, however, that the increase in the amount of credit granted to these favored entrepreneurs will represent an increase in the total amount of money-spending power, it must be demonstrated that nothing has happened to change the credit position of the producers of goods to which smaller or virtually negligible capitalizable yields attach. Actually, as Wicksell pointed out, the higher selling prices on which the more fortunate producers are able to count may be expected to increase the amount that they are willing to pay for the services of the particular factors of production which they need.[69] This must mean a rise in the price of these services. It follows, therefore, that the less fortunate producers who happen to be in need of these particular services are bound to be faced by rising costs, whereas nothing has happened, in their case, to justify the expectation of a rise in the price of their product.

It is precisely the absence from Keynes's *Treatise* of passages such as those to be found in Wicksell with respect to the effect of the bidding-up process which is associated with interest as a "capitalization factor," as

[68] On the limits within which Wicksell was prepared to regard the "capitalization" process as realistic, see above, p. 236.

[69] See, for example, *Interest and Prices,* 91 f., 96; *Lectures,* II, 196.

well as the absence of similar passages in which the bidding-up process is associated with different degrees of sensitiveness to interest as a cost, which led critics such as Mr. Robertson to wonder how Keynes's emphasis upon interest as a "capitalization factor" was to be tied up with the concept of a "mutual impact of the relevant flow of money and the relevant flow of goods."[70] It also constituted the basis of criticisms such as those of Hayek with respect to the effect of changes in the rate of interest upon the "yield of fixed capital" through their effect on the amount of purchasing power directed against, and therefore the prices of, the commodities included in the "circulating capital" required for coöperation with this "fixed capital."[71] The *Treatise* did, of course, deal explicitly and emphatically with one type of "bidding-up process"; it confined itself, however, to the case in which what was involved was a bidding-up of the factors of production to *all* industries, no attention being paid to the degree in which *different* businesses would be affected by the process.[72] In the few cases in which a distinction was made between the degree of profitability in different types of industry, no reference was made to the fact that this different degree of profitability might be associated with different degrees of sensitiveness to the "bidding-up process."[73] As far as most of the explicit exposition of the *Treatise* is concerned, in other words, the fact that entrepreneurs "bid against one another for the services of the factors of production" was regarded as having significance only because it could be expected to drive up the *general* level of costs, and thus affect the *general* profit situation.[74]

[70] See Robertson, "Mr. Keynes's Theory of Money," *loc. cit.*, 403 f. For examples of passages in Wicksell dealing with the bidding-up process as associated with interest as a "capitalization factor," see the references given in the preceding note; and for examples dealing with the bidding-up process as associated with interest as a "cost-factor," see *Interest and Prices*, 143, 149.

[71] See Hayek, "Reflections," Part II, *loc. cit.*, 25 f.

[72] The passages in question are those that have to do with the process by which a "profit-disequilibrium"—represented, according to the argument of the *Treatise*, by a positive or negative value for $(I\text{-}S)$—"reacts . . . on the *first* term of the Fundamental Equation, eventually causing the money-rate of earnings . . . to rise or fall" (*Treatise*, I, 153 f.; cf. also I, 181, 264 f., 269).

[73] On I, 181 of the *Treatise*, for example, the different degree of profitability in the production of "investment-goods," on the one hand, and the production of "consumable goods," on the other, was assumed first as given, and then as changed by the *fall in the selling price* of the more profitable category when output was "changed over" to this category: there was no suggestion that the difference in profitability might have been due both in the first instance and subsequently to the "bidding-up" of relevant costs in each case. Similarly, on I, 268 f., the emphasis is upon the differential movement of P' and P, and the relation of both to the *same* cost factor—namely, E/O—rather than upon differential movements in the respective cost-factors.

[74] For an example of the use, in the *Treatise*, of the idea that entrepreneurs may "bid against one another for the services of the factors of production," in a context of the kind indicated above, see I, 269.

It may be observed here that it is not necessary, strictly speaking, to assume, for the purpose of Wicksell's argument as stated above, the full utilization of resources, though it is true that Wicksell often made such an assumption for the sake of simplifying the exposition.[75] The argument, as stated above, does, to be sure, demand acceptance of the proposition that the favored producers will force up the prices of materials to the less favored producers; and it is also true that the possibility that they will not do so is made at least plausible by the assumption of a less than complete utilization of available resources. That this type of consideration is not decisive against the argument under examination, however, is apparent from the following considerations:

1. There are reasons, to be dealt with in more detail in Volume II of this study, for objecting to the suggestion that the mere existence of "unused resources" in general is a guarantee that increased monetary demand will not result in a rise in the price of the particular resources against which this demand is directed.
2. The argument with respect to the transference of credit from the less favored to the more favored producers can be stated—though it was not so stated above—simply in terms of a greater prospect of profit in some lines of business than in others, rather than in terms of a complete lack of profitability of some businesses.
3. It must be obvious that, with the progressive approach to a full utilization of resources, the case approaches the one outlined above; so that, at best, the introduction of the consideration that costs may not be raised immediately against the less favored entrepreneurs means merely that attention is being paid to an earlier stage in a process which must, if it continues, result in the situation described above.
4. The really decisive consideration is that, in all cases, the possibility of a rise in *general* prices will depend upon whether or not the supply of bank money is increased; and this, in turn, will depend upon the "natural rate"-"market rate" relation in all sectors of the economic system.

From the point at which some entrepreneurs begin to be faced with rising costs, the argument is precisely the same as that considered above, in connection with interest as a "cost-factor." The less fortunate producers, faced by the cer-

[75] See, for example, *Interest and Prices,* 90, 143; *Lectures,* II, 195. The argument in the text should, of course, not be taken to imply that Wicksell's usual assumption that resources were fully employed, or very nearly so, had no consequences for *other* parts of his argument, with which we are not here concerned. It certainly had consequences, for example, for the connotations of the concept of a "natural rate of interest" when the latter is defined as the rate which would be set if real capital were lent *in natura.* The issues involved need not, however, concern us here.

tainty of higher costs, may also apply at the banks for a greater amount of credit, on the ground that their cost bill has increased. Whether, however, they would get more bank credit would again depend in part upon the judgment of the bankers as to whether this particular group would be able to sell its products at a higher price, and thus be able to make use of the loan in such a way that the proceeds of the project which it finances would cover their increased factor costs. Moreover, since the prices of the products of the more fortunate producers will have risen, the less fortunate producers, *unless the total amount of money-spending power is increased,* will be actually faced with the prospect of a *fall* in the price of their product.[76]

The point to be observed, at any rate, is that, in the absence of some force leading to an increase in the total of money-spending power, the anticipated profit to be made by the use of a bank loan by the less fortunate producers will fall. Given this result, one of two things may happen. On the one hand, there may be a tendency on the part of the bankers, if they are foresighted, to shift their loans in the direction of those producers whose "natural rate"–"market rate" relation is more favorable than that of those whose request for increased credit is made at a time when their "natural rate"–"market rate" relation is becoming increasingly unfavorable. In this case, the increased demand for credit on the part of the more favorably situated producers will be accompanied by a decline—forced by the bankers, if necessary—in the demand for credit from the less favorably situated; there will be no necessary increase in the total amount of money-spending power, and the pessimistic ex-

[76] It is, of course, perfectly possible that the producers involved may anticipate the unfavorable cost-selling-price relation and restrict production accordingly, with the result that the diminution in the stream of money expenditure upon the goods in question, which is virtually inevitable if the total amount of money-spending power is not increased, will be met by a diminished stream of goods, so that prices will not actually fall. (The more general consequences of this type of anticipation will be discussed in Volume II.) It would still be true, however, that the bidding up of the prices of the resources used by the less fortunate producers will have made the latter's prospects for profit less favorable than they were, and less favorable than the propects for profit of the more fortunately situated producers. This is all that is needed for purposes of the present argument.

pectations of the bankers with respect to the "natural rate"–"market rate" relation of the less favorably situated producers will be realized. On the other hand, the bankers may continue to lend to the less favorably situated producers, on the assumption that so easy a credit policy will be pursued by the banking community generally that the resulting increase in the quantity of bank money created may permit even the prices of the products of the less favorably situated producers to rise, although not so much as the prices of the products of the more favorably situated producers. In effect, this second possibility amounts to a favorable interpretation of the "natural rate"–"market rate" relation of the less fortunate producers.[77] In all cases, therefore, the fate of *general* prices depends upon the total amount of bank money issued. This, in turn, depends upon the relation of bank rate to "natural rate," in the sense of the anticipated profit to be made by the use of a money loan. The whole argument, therefore, reduces to the Wicksellian proposition that all that could happen, as the result of the effect of interest as a "capitalization factor," would be a change in *relative* prices, as long as the quantity of bank money was prevented from expanding *by keeping the market rate equal to the "natural rate."*

It follows, therefore, that the question whether Mr. Keynes, in presenting his argument with respect to the effect of interest as a "capitalization factor," was departing from the "traditional" doctrine on the subject, turns entirely upon whether he left out the words italicized in the final portion of the preceding sentence. Interestingly enough, however, it was notably characteristic of Mr. Keynes's exposition that he was careful, at its crucial point, not only to insert this condition, but also to argue that, as long as the condition was observed, there would be merely a change in relative prices—concretely, a change in the prices of "investment goods" as compared with those of "consumers' goods"—*without* any change in general prices.

[77] At the risk of being charged with an insistence that is quite unnecessary, I venture to call the attention of the reader again to what is said in n. 50 to p. 252, above, on the meaning to be assigned to the concept of a "natural rate," for purposes of the present argument.

Thus, Mr. Keynes stated explicitly that "if the change in the market-rate coincides with a change in the natural rate . . . the change in the price-level of investment goods . . . is equal and opposite to the change in the price-level of liquid consumption goods . . . so that the price-level of output as a whole remains unchanged."[78] That passages of this type should have been overlooked in the attempts of commentators to ferret out Mr. Keynes's meaning is not surprising in view of the fact that Keynes himself, in his "Rejoinder" to Robertson, instead of calling attention to the type of passage in the *Treatise* just quoted, insisted that what he "denied" and what he believed Robertson to "affirm" was precisely that, "assuming no change in the propensity to hoard," the prices of "consumption-goods" and of "investment-goods" must be related to one another "like buckets in a well," that, in other words, "the one must go down when the other goes up."[79] Actually, of course, Mr. Keynes's argument, in the *Treatise,* was precisely that the two types of prices *would* move "like buckets in a well" except under conditions which, though Keynes himself summarized them under the head of a "discrepancy between Saving and Investment," are, as we shall see, perfectly capable of statement in terms of the variables of the older Quantity Equations.[80] It is certainly true, at any rate, that Mr. Keynes's position with respect to the effect of changes in the rate of interest upon the two groups of "prices" *in cases in which the market rate was equal to the "natural rate"* was that the two groups of prices would move precisely "like buckets in a well," and this is all that is needed for our present purpose.

Where, then, was the conflict with the "traditional" doctrine? Not in the terms of Mr. Keynes's argument itself, surely; for Keynes's argument on this point was precisely the argument of Wicksell, who, in this respect, certainly regarded himself as the defender of traditional doctrine. It is true that Mr. Keynes's exposition, at more than one point, was such as to lead an unwary reader to suppose that he had abandoned the logic of his formal argument, to the point of contending that the change in the price of investment goods, for example, as a result of changes in the rate of interest, would be accompanied by a movement of the prices of consumers' goods in the same direction, regardless of what was

[78] *Treatise,* I, 205.

[79] See Keynes's "Rejoinder," *loc. cit.,* 419. The situation was, of course, complicated by Mr. Keynes's unfortunate proposition, in the *Treatise,* to the effect that "the price-level of consumption-goods is entirely independent of the price-level of investment-goods." See the *Treatise,* I, 136; and cf. the comments by Robertson, *loc. cit.,* 398. Cf. also what is said on this matter on pp. 531 f., below.

[80] See especially, in this connection, pp. 525 ff., below.

happening to the quantity of money.[81] A careful reading of the doubtful passages, however, will show that at this point in his argument, at any rate, Mr. Keynes envisaged a movement, in a similar direction, in the prices of consumers' goods, only as a result of *a disturbance in the relation between the market rate and the natural rate.*[82] It must be obvious, upon reflection, that the insertion of this clause can have meaning only if it is supposed that a difference between the two rates will result in a change in the stream of money relative to the stream of objects offered against money. It must be equally obvious, therefore, that there was nothing in Mr. Keynes's argument to support the interpretation that, in arguing that it is wrong to allege that "a change in bank-rate affects the price-level because, in order to make the new bank-rate effective, the quantity of money has to be altered," he was arguing that bank rate could be "effective" in changing the general level of prices, even if the quantity of bank money were not altered.

It will be observed that the statement that, for purposes of the present problem, "a disturbance in the relation between market rate and natural rate" can have meaning only if it is supposed that a difference between the two rates will result in a change in the stream of money relative to the stream of objects offered against money, leaves room for the possibility that bank rate may affect variables in our Quantity Equation other than M'.[83] It is, however, the forces determining the magnitude of M' that are here under discussion; and my position is merely that there was nothing in Keynes's argument itself to suggest that he was prepared to deny that the discrepancy between market rate and "natural rate" would involve a change in the quantity of bank money (M'). As it happens, Mr. Keynes implied the direct contrary of such a

[81] See, for example, the *Treatise,* I, 209. It was precisely passages of this type which gave Mr. Keynes's commentators so much trouble. See, in this connection, the comments of Robertson, "Mr. Keynes's Theory of Money," *loc. cit.,* 401 f. It is of course obvious that, for the purpose in hand, we are abstracting from the fact that changes in market rate relatively to "natural rate" may affect other variables in our Quantity Equation" than M'. Cf. what is said in the text, below.

[82] Thus, the passage on I, 209, cited in the preceding note, was preceded by an explicit statement on I, 208, to the effect that the only kind of change in bank rate which was under discussion was a change that would "cause the market-rate of interest to diverge from the natural rate."

[83] On this matter, which, as we have seen, was involved in Keynes's criticism of the "traditional doctrine" with respect to the *modus operandi* of bank rate, see above, pp. 207 ff.

suggestion in parts of the *Treatise* which were concerned with the issues under discussion. In one passage, for example, the statement that a "lower rate of interest will stimulate the production of capital-goods by raising their prices"—in other words, the description of the effect of interest upon prices when the former is regarded as a "capitalization factor"—was included as part of a description of the "route" by which the "injection of an increased quantity of money into the monetary system" would "bring about a new equilibrium at a changed price-level."[84] In such passages, obviously, there can have been no thought of arguing that—abstracting from the possible effects of a discrepancy between market rate and natural rate upon the other variables of our Quantity Equation—interest, when regarded as a "capitalization factor," could be regarded as affecting general prices even if the change in the rate of interest had no effect upon the total amount of bank money issued.[85]

If this is so, however, it becomes literally impossible to see either why Mr. Keynes should have inserted at all the troublesome statement with respect to the "quantity of money" which gave his commentators such difficulty, or why—both by his general method of exposition and his specific implication that a conflict existed between his "third strand" in the theory of bank rate (as represented, say, by the "capitalization factor") and the "first strand" of the traditional doctrine, with its emphasis upon the importance of changes in the quantity of bank money—he should have been at such pains to establish differences between his own analysis and the "traditional" analysis with respect to the "*modus operandi* of bank-rate" where in fact none existed. In Chapter Five above, we had an opportunity to observe one type of consequence of the sin against "traditional doctrine" which is represented by charging one's predecessors with faults of which they were not guilty. In that case, the consequence took the form of a series of counter charges, some of them unjustified, but others only too well justified, alleging that the substitute apparatus proposed by the critic was open to precisely the charges of which the older apparatus was in fact quite innocent. In the present instance, the sin consisted of misrepresenting the substance of received doc-

[84] *Treatise,* I, 262 f.

[85] The passage in question, in fact, closes with a sentence which makes it perfectly clear that Mr. Keynes intended to convey no such impression. See the *Treatise,* I, 265.

trine in order to emphasize supposed divergences from Mr. Keynes's own doctrine, despite the fact that no real difference existed between his doctrine and traditional doctrine for the purpose of the special problem in hand. It is hardly surprising that this sin should have brought the consequence that some of his critics took his formal statements in this respect at their face value and, imputing their implications to the details of his argument, rejected the argument itself. To those, however, who are interested in establishing the precise degree of validity attaching both to received doctrine and to analysis advanced as a substitute for received doctrine, it can only be an occasion for rejoicing that the proposed substitute was in fact only the old truth hiding behind a series of misleading slogans which, it is to be hoped, will be speedily consigned to the oblivion they deserve.

The reader is again reminded that the "special problem in hand," Keynes's treatment of which is held to reveal no really significant conflicts with the treatment accorded to the problem by the "traditional doctrine" on the subject, is the relation of changes in bank rate to changes in general prices through their effect upon the quantity of M'. It is not necessary, therefore, to attempt a "reconciliation" between Keynes's definition of the "natural rate" as "the rate at which saving and the value of investment are exactly balanced," when the terms "saving" and "investment" are given the meanings assigned to them in the *Treatise,* and similar definitions of the "natural rate" when the terms "saving" and "investment" are used in a more conventional sense.[86] For one thing, as we have already seen, and as we shall see again in the following chapter, Keynes's argument with respect to the relation between changes in bank rate and changes in general prices can be stated without introducing the terms "saving" and "investment"; and our concern has been merely to demonstrate that, when so stated, the supposed conflicts between his argument and the traditional one are largely nonexistent. For another thing, the paraphrasing of older definitions of the "natural rate" in terms involving the relation between "saving" and "investment" is itself a post-Keynesian phenomenon, and therefore does not claim a degree of attention on the part of those whose primary concern is the defense of the "traditional doctrine"—as opposed to the defense of all current offshoots therefrom—comparable to that which may fairly be claimed by the details of an argument with respect to the *modus operandi* of bank rate in relation to changes in general prices.[87]

[86] For Keynes's formal definition of the "natural rate," in the *Treatise,* see I, 155; and cf. what is said on this matter on pp. 199 f., above.

[87] This is not to say, of course, that the particular offshoot in question is not deserving of examination on its account. Such an examination cannot, however, be attempted here.

CHAPTER TEN

The Quantity of Money-Substitutes, *M'* (*Continued*)

I

Bank Rate and Keynes's Fundamental Equations

IT WILL be remembered that Mr. Keynes's apparent minimization of the importance of changes in the quantity of "bank-money" as a link in the process whereby changes in bank rate may be expected to affect general prices was part of a broader attack upon the usefulness of the familiar "Quantity Equations" for tracing this process, as well as of an argument designed to demonstrate the superiority of the Fundamental Equations for just this purpose.[1] Something is to be said, therefore, for examining the claims to superiority thus presented on behalf of the Fundamental Equations for the special purpose of tracing the *modus operandi* of bank rate. Since the argument involved is necessarily in the nature of a counter attack, this chapter, like Chapter V, may be omitted by those who are concerned only with the major purpose of this study, which is the defence of the substance of received doctrine on the subject of the Theory of Prices against its principal detractors. What follows, nevertheless, may be regarded as having value not only for its own sake, but also because Mr. Keynes's claim that the equations of the *Treatise* were superior for the special purpose of tracing the *modus operandi* of bank rate were accepted, at the time of the publication of the *Treatise* and in the years immediately following, not only by avowed defenders of the apparatus of the *Treatise,* but also by critics who have since shown themselves anything but sympathetic to the general trend of Mr. Keynes's later writings.[2]

[1] See especially, in this connection, the *Treatise,* I, 155.

[2] See, in this connection, not only the remarks of writers such as Adarkar (*The Theory of Monetary Policy,* 51 f.), but also those of

II

"Capitalization," Interest as a Cost, and the Fundamental Equations

It has already been pointed out that since the equations of the *Treatise* did not pretend to include a specific term for bank rate, any more than the familiar Quantity Equations did, a large part of Mr. Keynes's argument with respect to the *modus operandi* of bank rate must be held to "lie behind" his equations as truly as a large part of the "traditional" argument may be said to "lie behind" the Quantity Equations.[3] If, therefore, the Fundamental Equations were to be held to be superior to the Quantity Equations for the purpose of tracing the *modus operandi* of bank rate, it must have been because they were supposed to give a specific place to variables upon which bank rate operated directly, and which in turn directly affected prices, that are not given a specific place in the familiar Quantity Equations.

What would these variables be? If the argument has reference to Mr. Keynes's own emphasis on the rate of interest as a "capitalization factor," the superiority of the Fundamental Equations would reside, presumably, in the fact that the specific introduction of the concept of a "plurality of price-levels" would make it possible to trace the changes in *relative* prices which were held to follow from changes in bank rate.[4] This in itself, however, would hardly justify a claim for superiority on behalf of the Fundamental

Pigou, in the *Nation and Athenaeum,* XLVIII, 544, to the effect that Keynes was enabled, *by the use of his Fundamental Equations,* "to give an account of the *modus operandi* of bank rate much superior . . . to previous discussion."

[3] Cf. above, p. 180, and see again Keynes's own comment in the *Treatise,* I, 185.

[4] This seems to be implied in the statement of Adarkar that the "critics of Keynes" are wrong in supposing that "the Marshallian or Fisherian method would help us just as well to know how the *various price-groups* are affected by changes in bank rate" (Adarkar, *The Theory of Monetary Policy,* 51; italics mine). It is noteworthy, at any rate, that the passage from the *Treatise* cited on p. 268, n. 1, above, with respect to the superiority of the Fundamental Equations over the "ordinary Quantity Equation," is preceded by an account of the effect of interest as a "capitalization factor" upon *P'* (*Treatise,* I, 154); and that the authority of "the Fundamental Equation" was appealed to in that part of Mr. Keynes's account of the "General Theory of Bank Rate" which had to do with changes in *P* and *P',* respectively (*Treatise,* I, 206).

Equations of the Treatise over "quantity equations" of, say, the *general Fisherine form;* for, as we shall see, the concept of a "plurality of price-levels" had been recognized as consistent with, and indeed had been stated by the help of, such equations prior to the appearance of the *Treatise,* at least as articulately as in the case of the Fundamental Equations of the *Treatise.*[5]

If, on the other hand, the argument has reference to the significance of the rate of interest as a "cost factor," an appearance of superiority might seem to have been given to the Fundamental Equations by virtue of the fact that these equations, unlike the older Quantity Equations, included a specific term for costs—namely, W_1 in the equations when written in the form $\Pi = W_1 + (I-S)/O$. In the first place, however, it is anything but clear that Mr. Keynes would have been prepared to claim superiority for his Fundamental Equations on any such grounds. For one thing, as we have seen, he was so unsympathetic to the suggestion that changes in interest costs represent an important part of the *modus operandi* of bank rate that he did not bother, in attempting to rebut the particular argument associated with the name of Hawtrey with respect to the effect of interest as a cost factor, to make it clear whether the element in Hawtrey's argument which he regarded as easily "confuted" was the latter's contention that it is the "traders" who may be expected to be most sensitive to interest costs, or whether it was the emphasis upon interest as a "cost-factor" itself.[6] For another thing, as we have also seen, the importance of interest as a "cost" among the "costs" included in W_1 seemed so slight to Mr. Keynes at the time of writing the *Treatise* that, by defining W, in the expression $W_1 = 1/e \cdot W$, as "the rate of earnings per unit of human effort," he actually managed to convey to some of his critics the impression that he did not regard interest costs as being included in W_1 at all.[7] It is true, as we saw, that interest costs were in fact included in the W_1 of the Fundamental

[5] On the concept of a "plurality of price-levels" in monetary literature, see below, pp. 496 ff. and 512 ff.

[6] Cf. above, p. 245.

[7] Cf. above, p. 111 and especially n. 26 thereto.

Equations. Yet there are at least two reasons why this circumstance cannot be adduced in support of a contention that the Fundamental Equations provided a satisfactory skeleton for tracing the *modus operandi* of bank rate as a factor affecting general prices when bank rate is regarded as a "cost-factor."

The first of these was unwittingly made clear by Mr. Keynes himself, in the course of an argument designed to show that a lowering of the "cost of production" through "cheaper borrowing" could not be expected to provide an inducement for increased borrowing on the part of all entrepreneurs.[8] His argument was, in fact, little more than a corollary of the basic assumption underlying the Fundamental Equations—namely, that the "costs" which are relevant for an answer to the question whether costs and selling prices are in equilibrium are only those costs that represent income to the factors of production; or, as Mr. Keynes himself put it, that "aggregate costs of production" are simply "the aggregate incomes of consumers . . . under another name."[9] If this assumption is accepted, then of course it follows that a general reduction in "interest-costs" will merely reduce incomes, so that entrepreneurs, unless they are guilty of "mistaken forecasting," will realize that the total monetary demand for, and therefore the selling price of, their products, will not be great enough to warrant the type of expansion of operations that would call for an increase in aggregate borrowing.

If, however, we drop the basic assumption underlying the Fundamental Equations, and include in the costs which enter into the calculations of entrepreneurs with respect to the advisability of expanding operations not only costs that represent current payments to the factors of production, but also those costs which are too high to permit the utilization of the factors to which they attach, it becomes obvious that a reduction in such costs, so far from entailing a simultane-

[8] See the *Treatise*, I, 210 f. Mr. Keynes put the argument in terms of the inducement to entrepreneurs to increase their "output," rather than to increase their "borrowing"; but it is obvious that he had in mind such increases in output as might be expected to result from increased borrowing.

[9] Cf. the *Treatise*, I, 211.

ous reduction in incomes, will actually cause an increase in them because more of the relevant factor will be used at the lower cost.[10] The principle is nothing more than that which is involved in the standard refutation of the argument that a reduction of wage *rates* is under all circumstances to be rejected because such reductions lead to a decline in "purchasing power." In the present instance, the element of novelty was merely that an ancient error could be shown to be the direct consequence of what has been recognized as the basic—and erroneous—assumption upon which the Fundamental Equations were constructed.

It happens that, in the passage indicated, Mr. Keynes was guilty of the further sin of failing to make clear whether or not the reduction of the rate of interest, the consequences of which he was tracing, was a reduction of the market rate which was accompanied by a decline in the "natural rate," or was a reduction in the market rate *relative* to the natural rate.[11] In the former case, his argument that "the effect of easier credit on the costs of production should be, not to stimulate production all round, but to cause a change-over from certain forms of production to other forms; namely, from those for which interest is a relatively unimportant cost to those for which it is a relatively important cost," is, in fact, nothing more nor less than the classical argument with respect to interest as a "cost-factor," in the form in which it was used by Wicksell against Tooke.[12] In the second case, however, it must

[10] The point was put by Mr. Robertson in the form of an accusation that Keynes's argument involved "a confusion between 'costs per unit of output' and 'aggregate costs'" ("Mr. Keynes' Theory of Money," *loc. cit.*, 405). This is, of course, one way of stating that there is a difference between "*rates* of remuneration" and "aggregate remuneration," which is certainly involved in the point under discussion. It fails, however, to make clear the precise nature of the relationship between Keynes's "error"—such it undoubtedly was—and the apparatus represented by the "Fundamental Equations." For, as long as the relevant "*rate* of remuneration" continues to be conceived of as being equal to E/O, it is not easy to see how a reduction in this magnitude through a reduction in E is consistent with an accompanying rise in E, when the latter is regarded as "*aggregate* money remuneration." If, on the other hand, it is recognized that the "*rate* of remuneration" involved was not included in the E of E/O, it is perfectly easy to see that a reduction in this "rate of remuneration" may result in an accompanying increase in E, when the latter is regarded as "aggregate money remuneration."

[11] The present instance, therefore, provides some justification for the charge of Hayek that, in some parts of Keynes's analysis, "one is not clear whether he is speaking of the effects of *any* change in the Bank Rate, or whether what he says applies only the the effect of the Bank Rate being different from the market rate." (Hayek, "Reflections," II, *loc. cit.*, 24. The context would indicate that Hayek, in speaking of the "market" rate, had in mind the "natural" ["equilibrium"] rate.)

[12] See above, pp. 249 ff.

be obvious, as Mr. Robertson pointed out, that the reduction of interest cost will necessarily lead to precisely that "immediate increase in the volume of bank-money" which will automatically provide the increased monetary demand in the absence of which Mr. Keynes could see no inducement for entrepreneurs to increase the amount of their borrowings.[13]

The second reason why the mere fact that interest rates were included in the W_1 of the Fundamental Equations cannot be regarded as having demonstrated the superiority of the Fundamental Equations as a skeleton for tracing the *modus operandi* of bank rate, when the latter is regarded as a cost factor, is much more decisive. This second reason is a corollary of the argument developed in Chapter V of this study: namely, that it is possible to accept the introduction of the cost item $W_1(=E/O)$ into the Fundamental Equations only if we are prepared to give up the interpretation of these equations as providing at the same time an apparatus for tracing the steps whereby an enlarged or diminished stream of money goes against a stream of goods, in such wise as to determine the prices of these goods.[14] It follows, from this general principle, that an argument in behalf of the Fundamental Equations which would point to the fact that interest costs, as such, are included in the E/O of the Fundamental Equations automatically removes the possibility of interpreting these Equations as showing us,

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[13] See Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 405 f. Robertson's statement of the argument was unnecessarily weakened by his suggestion that the "increase in the volume of bank money" which is in question is "normally" associated with the lowering of bank rate. The word "normally" is hardly a substitute for the condition "whenever bank-rate is lowered relatively to the anticipated profit to be made by the use of a money loan"; indeed, as we have seen, it was just such loose statements which seemed to give some support for Keynes's caricature of the "traditional doctrine" in this respect (cf. above, pp. 189 ff.). When Robertson's statement is understood in the sense indicated, however, there can hardly be doubt as to its correctness; nor is there any doubt that Robertson was right in suggesting that Keynes's argument on this head was "inconsistent with much of the rest of Mr. Keynes's analysis" (Robertson, *op. cit.*, 406). It is, indeed, indicative of the lack of any fundamental relationship between the Fundamental Equations and "the rest of Mr. Keynes's analysis" with respect to the *modus operandi* of bank rate that when he followed out strictly the implications of the Equations he was led to an argument which was both erroneous and inconsistent with those parts of his argument that can be stated without any reference whatever to the Fundamental Equations.

[14] Cf. above, pp. 124 ff.

simultaneously, the relation between such changes in interest cost and the answer to the question as to "why and in what circumstances credit money gets into circulation," with the inevitable effect upon prices that may be expected from the use of the concept of a "mutual impact of relevant flows." [15] It is, as we have seen, precisely the interpretation of the Fundamental Equations as including a specific term for "costs"—and therefore interest costs—which makes it impossible, under any but a highly special set of conditions, to interpret the Fundamental Equations as simultaneously representing a "mutual impact of relevant flows" of money and of goods, respectively.

III

$I-S$ as "Profits," and Bank Rate

Actually, however, it was not the inclusion, in the Fundamental Equations, of a term representing the "cost" factor —including interest costs—which Mr. Keynes regarded as the principal, or even a major, basis for the claim that these equations were superior for dealing with the problem of the *modus operandi* of bank rate. There can be little doubt, indeed, that the part of the Fundamental Equations which he regarded as important for the purpose of tracing the *modus operandi* of bank rate was the expression $I-S$, or $I'-S$. Yet the nature of the supposed relation cannot be made clear unless it is recognized that the expression $I-S$, like each of the Fundamental Equations themselves, was supposed to carry a twofold implication. It will be well, therefore, to divide the argument into two parts, each of which will deal separately with the implications involved.

The first of the implications was that $I-S=Q$, in which Q represented "profits," as defined in the *Treatise*.[16] That

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[15] It is significant that precisely such a claim was made on behalf of the apparatus of the *Treatise* by Adarkar, whose treatment of "Keynes's Libra" was cited above as an example of the claim that the Fundamental Equations simultaneously performed the dual task indicated in the text. See Adarkar, *op. cit.*, 52; and cf. above, p. 124, and n. 58 thereto.

[16] Cf. the *Treatise*, I, 137 f., 151.

Mr. Keynes attached great importance to *Q* in his account of the *modus operandi* of bank rate, there can be little doubt. We were told, for example, that what gave "significance" to the Fundamental Equations, and "saved" them "from the character of being mere identities" was precisely the introduction into these Equations of a term for "Profits," or, more precisely, of the "*fact* from the real world" which this term summarized.[17] This "fact" was that it was the "influence of positive or negative profits" which, in combination with the "abundance or scarcity of the bank-credit at their disposal," would induce entrepreneurs to change the scale of their spending on the factors of production.[18]

What was not so clear, however, was precisely the way in which Mr. Keynes's discussion of the connection between the "natural rate"–"market rate" relation and the amount of borrowing was associated with *Q*, as defined above. From the formal definition of the "natural rate" as the rate "at which saving and the value of investment are exactly balanced," it seemed to follow that, whenever *Q* had a positive or negative value, the "natural rate" would be regarded as being respectively above or below the market rate.[19] Only slight reflection, however, is required in order to show that this proposition carried us very little nearer to something that could be regarded as either a measure of the "natural rate"—as one might have been led to believe by the suggestion that it was the magnitude of *Q* together with the "abundance or scarcity of credit" at the disposal of entrepreneurs which would determine the magnitude of entrepreneurial borrowing—or as a measure of the *extent of the difference* between natural rate and market rate. One knew only that when *Q* had a positive or negative value, the "natural rate" was respectively above or below the market rate. There was in the Fundamental Equations, that is to say, no term for the "natural rate" any more than there was for "market rate"; in this respect, again, the Fundamental

[17] *Treatise,* I, 156 f. (italics Keynes's).
[18] *Ibid.,* 152.
[19] Cf., in this connection, the *Treatise,* II, 204. For the formal definition of the "natural rate" as the rate "at which saving and the value of investment are exactly balanced," as well as for the relation between *Q* and (*I*—*S*), see *ibid.,* I, 155.

Equations of the *Treatise* and the older Quantity Equations were entirely on a par.[20]

It was, furthermore, a fair conclusion from the argument of the *Treatise* that the existence of "profits" or "losses"—in other words, of a positive or negative value for Q—which was originally the *result* of a discrepancy between "natural rate" and market rate, would be expected to react upon the "natural rate" itself, if the latter were understood to represent the profit to be made by the use of a bank loan.[21] There was certainly an analogue to this suggestion in the older concept of a "price-premium" which would affect the profitability of borrowing.[22] Yet there can be little doubt that the concept of a "price-premium," which makes it possible to distinguish between the inducement to increased borrowing that results from an initial discrepancy between "market rate" and "natural rate" and that which results from the subsequent price rise, is a much better weapon for dealing with the *modus operandi* of bank rate than the $Q(=I-S)$ of the Fundamental Equations.[23] For, obvi-

[20] Cf. what is said on p. 179, above, with respect to the place of "Bank-rate" in the Fundamental Equations and in the older Quantity Equations.

[21] See, in this connection, the statements in the *Treatise* that a "disparity between investment and saving" *sets up* "a disequilibrium in the rate of profit" (I, 153); that "the profits or losses of the producers" *depend upon* "the value of investment relatively to the volume of savings" (I, 182); and that the motive underlying changes in bank rate which are "deliberately designed to provoke a (temporary) disequilibrium between Savings and Investment" is the intention to grant to entrepreneurs "abnormal profits" which may be expected to induce them to extend the scale of borrowing and entrepreneurial spending (I, 210).

[22] The use of the term "price-premium" in a sense which is relevant to the present discussion is principally due, so far as I am aware, to Mises. See, for example, the latter's *Geldwertstabilisierung und Konjunkturpolitik*, 47 f. The idea, however, that price movements, once inaugurated, may increase the profitability of borrowing by a premium greater than that which was offered by the initial discrepancy between market rate and "natural rate" was clearly recognized by Wicksell, though he does not seem to have been aware of the fact that this called for a revision or abandonment of certain definitions of the "natural rate" which appear in his writings. See, at any rate, *Interest and Prices*, 97; *Lectures*, II, 207; and cf. "Der Bankzins," *loc. cit.*, 239 f.

[23] This is not to deny, obviously, that the concept of a "price-premium" is itself capable of a much more precise statement than has often been given to it. One has only to think, for example, of the question as to *what* prices shall be included in the computation of the price premium, when not all prices are changing equally, to obtain some idea of the complexities involved. These complexities cannot, however, be dealt with here.

ously, the very emergence of *Q* is possible only when the "price-premium" appears.[24] To suggest, therefore, that the "abnormal profits" which are held to be necessary to induce more borrowing are represented by *Q*, is to leave out of account the first step in the *modus operandi* of bank rate—namely, the emergence of a difference between bank rate and "natural rate" which encourages the borrowing that in turn gives rise to *Q*. To have obscured this fact still further, as Mr. Keynes did, by regarding the "price-premium" as *part* of the "natural rate," instead of distinguishing the two, can hardly be regarded as fortunate.[25] That he should have done so, however, was virtually inevitable in view of the twofold circumstance that he had advanced claims to superiority on behalf of his *Fundamental Equations* for the purpose of showing the *modus operandi* of bank rate and that, so far as the *motives* for borrowing from banks are concerned, his *Q* could throw light only upon that particular motive for borrowing which had been inherent in the earlier concept of a "price-premium."

There are, moreover, other reasons for arguing that the use of *Q* as a means of tracing the *modus operandi* of bank rate was an extremely clumsy and inadequate way of handling what is in fact a problem of great complexity. In the first place, the term *Q* represented the difference between *aggregate* selling prices in a community and *aggregate* costs in that community. The use of the term *Q*, therefore, as part of an attempt to gauge the effect of a difference between costs and selling prices upon the inducement to borrow from banks made it impossible to deal with a matter which is vital

[24] See especially, in this connection, Hawtrey, *The Art of Central Banking*, p. 349, where it was argued that "the windfall loss or gain" (*Q*) is "one only among several consequences" of the lending policy of banks, "and *neither the earliest nor necessarily the most important*" (italics mine).

[25] For evidence that Keynes regarded the "price-premium" as part of the "natural rate," see, for example, the *Treatise*, I, 212. It is of course true that the "price-premium" makes part of the "natural rate" when the latter is defined as the profit to be made by the use of a money loan. The point made here is merely that it is precisely in connection with the *determinants* of this "profit to be made by the use of a money-loan" that the literature on the subject of the "natural rate" is most seriously in need of critical examination and further development, and that it was precisely this type of problem which the aspect of Keynes's treatment now under discussion tended to obscure.

for any discussion of the incidence of a given change in bank rate—namely, the effect of price movements upon the profit position of particular classes of entrepreneurs and of particular types of transaction. It is, after all, the *marginal* profit to be obtained by the use of a bank loan, and not the global amount of "profits" in a community, or the "average" amount of profits, which is relevant to the *modus operandi* of bank rate.[26]

In the second place, the use of Q, in the sense defined by Keynes, makes it impossible to deal with the factor of "anticipations" as a factor affecting the amount of bank borrowing. For, despite Mr. Keynes's frequent concern, in the pages of the *Treatise,* with the factor of "anticipations," there is, as was pointed out by Mr. Hawtrey and other writers, simply no place for anticipations in the type of setup envisaged by the Fundamental Equations.[27] "Costs" are actually incurred costs, not anticipated costs; and profits (Q) are themselves actually realized profits, not anticipated profits. For the special purpose, therefore, of determining the magnitude of the "anticipated profit to be made by the use of a bank-loan," which is the magnitude to be compared

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[26] The "average rate of profit," as opposed to global profit (Q), would, of course, be represented by $(I-S)/O$ or Q/O. See the *Treatise,* I, 183. The fallacy of concentrating attention upon *aggregate* "profits" (Q) to the exclusion of an emphasis upon the importance of "marginal returns (in respect of a given outlay)" was pointed out by Pigou, "Mr. Keynes on Money," *loc. cit.,* 544. See also Hayek, "Reflections," I, *loc. cit.,* 274 f.; though it may be added that the classification of "profits" according to the "stage" of production at which such profits are realized itself involves a very great simplification, to be justified only for special purposes for which such simplification is not fatal. The important point for our present purpose, at any rate, is that when Mr. Keynes discussed "the gains of one category of producers" as compared with the gains of another, he did so only on the assumption that Q ("profits as a whole") would "remain at zero" (*Treatise,* I, 205).

[27] See Hawtrey, *The Art of Central Banking,* 344 and Myrdal, "Der Gleichgewichtsbegriff," *loc. cit.,* 384. Cf. also B. Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," Part I, *Economic Journal,* XLVII (1937), 65, on the "profit concept in Keynes' *Treatise*" as "an ex-post concept." There can be no question, on the other hand, that Keynes, even at the time of writing the *Treatise, wished* to include the element of "anticipations" among the factors which would affect the amount of borrowing. See especially the *Treatise,* I, 159 ff.; also I, 212, on the relation of "expectations of a tendency towards rising prices" on the "natural-rate of interest," which Mr. Keynes insisted, in his "Reply to Dr. Hayek," *loc. cit.,* p. 11, he had conceived of as "taking account of the existing psychology of the market, including errors of forecasting."

with bank rate for an understanding of the "effectiveness" of the latter in encouraging borrowing, the $Q(=I-S)$ of the Fundamental Equations is merely the loosest of symbols for a complex of considerations which are dealt with much more satisfactorily by a body of analysis that is to be regarded frankly as lying "behind" the older Quantity Equations.

The third reason for refusing to admit that the inclusion of a term for "profits" $(Q = I-S)$ in the Fundamental Equations justifies the claim that those equations provide a more satisfactory skeleton for tracing the *modus operandi* of bank rate than is offered by the older Quantity Equations is, however, the really decisive one. This reason is again a corollary of the proposition, demonstrated in Chapter Five, that it is impossible, under any but a highly special set of conditions, to accept the Fundamental Equations of the *Treatise* as providing, *at one and the same time,* an apparatus for dealing with the emergence of "profit" (Q) *and* an apparatus for dealing with the factors determining the breadth of the stream of money spent upon goods. What this means, of course, is that, even if it could be shown that the term for "profits" $(Q = I-S)$ in the Fundamental Equations is directly and unambiguously relevant to the complex of problems which are associated with the "natural rate"–"market rate" relation, the demonstration would represent a futile victory; for the very acceptance of an interpretation of the Fundamental Equations as representing the forces making for a difference between costs and selling prices necessarily involves a renunciation of an interpretation of the Equations which would permit their use in tracing the steps by which the emergence of such profits is translated into that change in the dimensions of the stream of money expenditure which is the factor directly affecting prices.

IV

$I(-S)$ as "Investment" and Bank Rate

We are thus led to a consideration of the usefulness of the Fundamental Equations for a study of the *modus oper-*

andi of bank rate when the expression $I-S$ in those equations is deprived of all association with the concept of a relation between "costs" and selling prices and is regarded solely as summarizing the forces leading to a change in the dimensions of the stream of money expenditure upon goods. Here, at last, is a fair test of the relative merits of the two sets of apparatus; for, as we have seen, it was only with reference to their fitness for dealing with the forces determining the relative breadth of the streams of money and of goods that claims have been made on behalf of the older Quantity Equations as such. The issues involved can, of course, be treated with some degree of completeness only as part of a general examination of the relation between the argument in the *Treatise* as to the way in which a discrepancy between "Saving" and "Investment" may be expected to affect the price level, and the "traditional" argument on the subject.[28] It is possible, however, on the basis of the analysis thus far presented, to indicate at least the nature of the relation of the present argument to the broader problem.

It was Mr. Keynes's contention that changes in bank rate affect general prices by affecting "the rate of investment," or the "value," or "volume" of investment—by which, of course, was meant the rate, or "value," or "volume" of investment relative to that of saving.[29] Mr. Keynes, however, had already defined "the rate of Investment" as "the net increment during a period of time of the capital of the community."[30] Taken literally, what this would mean would be that banking operations would have an effect upon general prices only insofar as they result in a "net increment . . . of the capital of the community." This is a palpably absurd proposition; for what it would mean, among other

[28] This matter will be dealt with in some detail in Volume II of this study.

[29] See, for example, the *Treatise*, I, 153, 182; II, 211, 346, 348, 350, 387.

[30] *Treatise*, I, 126. See also I, 172, where it was alleged that "Investment" is "measured by the *net addition to wealth* whether in the form of fixed capital, working capital, or liquid capital," and I, 274, where it was stated that "a high rate of investment . . . must necessarily by definition be associated with a high rate of increment of *accumulated wealth*" (italics mine in both cases). Cf. also II, 125.

things, is that when projects financed by bank credit result in a wastage of real resources, so that there is no "net increment" added to the "capital of the community," the bank credit thus issued would either have no effect at all upon prices, or would, in some mysterious way, affect them very differently from the way in which it would affect them if some "net increment" of capital did result.

One way, to be sure, of rescuing Mr. Keynes from such a conclusion would have been to assume that, by an increase in "Investment," he meant, not an increase in the *real* capital of the community, but in the *money value* of all existing "capital," including "capital" in the form of consumers' goods.[31] Quite apart, however, from the fact that Mr. Keynes himself insisted that the concept of Investment "relates to units of *goods,*" so that an increase of Investment would have to mean an increase in the number of "units of goods," the interpretation suggested would amount to a translation of the proposition that banks affect prices by affecting the rate of Investment into a proposition to the effect that banks affect prices by affecting prices. What he must have meant by his concept of "Investment," in this context, therefore, was some such thing as the "total of entrepreneurial spending upon goods and services."[32] Yet, in at least one passage in the *Treatise,* he wrote in terms which would suggest that when he spoke of the power of the banks to affect "the value of investment," he meant their power to control "the aggregate expenditure on output," which, taken literally, would include not only entrepre-

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[31] It should be remembered that "investment," in the *Treatise,* included investment in "liquid capital" (cf. above, p. 280, n. 30) which was made up of goods "capable of being used *or consumed,* at any time" (*Treatise,* I, 128; italics mine). Even so, it will be observed, the total spending on "investment" will not necessarily be equivalent to the total of spending out of funds acquired from banks, which is what is required if "investment" is to be identified with that part of the stream of money expenditure upon goods and services which arises from bank borrowing; for, as Keynes himself pointed out, "Liquid Capital is only possible when goods will 'keep'" (*Treatise,* I, 128).

[32] The phrase "upon goods and services" is added merely by way of emphasizing the fact that the question of a "hitch-up" of the funds advanced by banks is not relevant to the problem under discussion. On this matter, see below, pp. 529, and 583 ff.

neurial spending, but spending of all types, including spending by governments in the way of a dole![33]

Mr. Keynes's way of meeting this latter difficulty, to be sure, was to regard those governmental expenditures which could not by any stretch of the imagination be regarded as resulting in "investment," as "negative saving."[34] The point to be made again, however, is that what determines whether general prices will be raised as the result of borrowing from banks is not whether the funds borrowed are used in such a way as to result in an increment in the real capital of the community, but the simple fact that the funds are spent in the purchase of goods and services.[35] As Mr. Hawtrey pointed out, what was *"essential"* to Keynes's theory with respect to the *modus operandi* of bank rate was that there should be an "alteration in the flow of money."[36] To have obscured this fact by insisting that it was the effect of bank rate in altering the volume of "investment," when "investment" had already been defined in such a way as to associate it with an "increment of real capital" in the community, can only be regarded as having tended to obscure, rather than illuminate, the essential element in the process of general price change.

Nor can it be said that the problem of interpreting Mr. Keynes's proposition that the key to the *modus operandi* of bank rate is to be sought in its effect upon the amount of "investment" was made easier by his incursions into the history of doctrine upon the subject. He re-

[33] Cf. the *Treatise,* I, 182; and for a discussion of the bearing of governmental expenditures on the proposition that banks affect prices through their effect upon "investment," see Currie, *The Supply and Control of Money,* 7 f.

[34] See Keynes's "Rejoinder" to Robertson, *Economic Journal,* 1931, 420.

[35] It is difficult to believe, for example, that Mr. Keynes intended to argue that the spending by "the artificial borrowers" who appeared in the market in 1927-1928 (cf. the *Treatise,* II, 379 ff.) would, if it had continued, have had any less effect upon prices than the spending of entrepreneurs upon "investment." The spending of the former certainly represented in many cases expenditure upon commodities and services, but, just as certainly, it did not represent, in all those cases, anything that could properly be regarded as "investment" according to Mr. Keynes's formal definition of the latter term.

[36] Cf. *The Art of Central Banking,* 363. Cf. also the comment of Hayek, "Reflections," II, *loc. cit.,* 40, to the effect that, despite Keynes's effort to introduce the concept of *"real* investment," what he was "really interested in" was "merely the shifts in the money streams and the consequent changes in price levels."

ferred, for example, to Wicksell as representative of a "third strand of thought" in the development of the theory with respect to the *modus operandi* of bank rate—this "third strand" involving an emphasis upon bank rate as "influencing in some way the rate of investment."[37] But if anything is certain with respect to the interpretation of Wicksell, it is precisely that if he had used the word "investment" at all in this connection, he would have thought of it as an increase in the total of entrepreneurial spending upon goods and services.[38]

In the second place, Mr. Keynes made much of the distinction between an effect of bank rate upon "Investment" and its effect upon "speculation."[39] An initial obscurity was introduced into this part of the discussion by the fact that it was by no means clear in just what the difference between "speculation" and "investment" was supposed to reside. Mr. Keynes had previously made it clear that he regarded what one would have thought was a typical "speculators'" operation—namely, the building up of commodity stocks in anticipation of a price rise—as a form of "investment" in "liquid capital."[40] It is clear, at

[37] Cf. the *Treatise,* I, 190, 196.

[38] This fact, unfortunately, has been obscured by those interpreters of Wicksell who have insisted upon translating phrases which he used in his account of the *modus operandi* of bank rate, like "the demand for loan-capital"—as, for example, in the formal definition of the "natural rate" in his *Lectures* (II, 193)—as being equivalent to "real investment." Actually, so far as I am aware, Wicksell himself never provided any direct justification whatever for such a translation.

[39] Cf. the *Treatise,* I, 192 f., 198. In support of the distinction, Keynes cited Wicksell as having been "very explicit" in arguing that "the influence of the rate of interest on the price-level operates by its effect on the rate of Investment, and that *Investment* in this context means *Investment* and not speculation" (*Treatise,* I, 198). Actually, however, as far as I am aware, no passages can be found in Wicksell's writings which would support such a contention on any interpretation that would make it relevant to the problem in hand. For one thing, Wicksell did not use the term "investment" in contrast to "speculation." For another, his references to "speculation" had to do either with the comparative sensitiveness to interest cost of different types of borrowers (*Interest and Prices,* 90) or with the nature of "anticipations" made by such borrowers with respect to price-movements (*ibid.,* 97 f.), and not with an implication that "investment" is unlike speculation in that the former "causes an increased demand for actual goods for use, and not for 'speculative' purposes," in a sense which would carry the further implication that only "investment" could be expected to result in an "increased actual demand which sends up prices" (*Treatise,* I, 199). On the contrary, it is clear, from the second of the passages cited above from *Interest and Prices* (p. 98), that Wicksell regarded speculative spending as having the greatest effect upon prices. That Wicksell was far from drawing the sharp line between "investment" and "speculation" attributed to him by Keynes is clear also from the passage in the *Lectures,* II, 185, in which he discussed "the element of speculation which necessarily enters into all business transactions and into all capitalistic production."

[40] On "investment" as including "investment" in "liquid capital," see the *Treatise,* I, 172. Cf. also Keynes's characterization of Hawtrey's argument with respect to the *modus operandi* of bank rate as involving "one particular kind of investment, namely, investment by dealers and middlemen

any rate, that if "investment" is to be regarded as the equivalent of "entrepreneurial spending upon goods and services," there is no reason whatever for distinguishing between the effect, upon the price level, of borrowing for "investment" purposes, on the one hand, and borrowing for "speculative" purposes, on the other, so long as entrepreneurial spending on goods and services is involved in both cases. It was certainly not correct, therefore, to imply, as Mr. Keynes did, that the money borrowed from banks for "speculative" purposes does not cause an "increased demand for goods . . . which sends up prices."[41] Whether the "actual goods" thus bought by the speculator are immediately devoted to "use" is indeed relevant to the question as to the magnitude of the effect of the borrowing operation upon prices; but it is relevant only because, in *addition* to the price-raising effect of the spending by those entrepreneurs who buy goods to hold for a further price rise, there is the further price-raising effect of a decline in the rate of sale of these goods.[42] To imply, therefore, that banks are unable to affect the price level unless the borrowers from banks exert "an increased demand for actual goods for use [that is, for "investment"], and not for 'speculative' purposes," is to advance a proposition the foundation for which it is extremely difficult to discover.[43] No one could deny that certain types of "speculator" may, under certain conditions, be *less sensitive to interest cost* than certain types of investor.[44] It must be obvious, however, that the very fact of the comparative insensitiveness of these "speculators" to interest cost during a boom period must mean that they will be taking a proportionately greater share of bank credit, and thus exerting a *greater* influence upon prices than those whose operations are in the nature of "investment."[45] It is difficult to see,

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in liquid goods" (I, 193). Despite this explicit characterization of Hawtrey's argument as involving "one kind of investment," however, Keynes proceeded to find the "classical refutation of Mr. Hawtrey's theory" in an utterance of Tooke with respect to the effect of bank rate on "speculation" (I, 195).

[41] Cf. the *Treatise,* I, 199. Mr. Keynes cannot have had reference to the possibility of a "hitch-up" of funds otherwise unavailable for spending on goods and services which may be said to be involved in *stock-market* speculation (on which see below, pp. 585 ff.). For in all the cases cited—those of Marshall, Hawtrey, Tooke, and Wicksell—what was involved was *commodity* speculation.

[42] This was, in fact, explicitly pointed out by Wicksell, whom Keynes had cited in support of his "distinction" between "speculation" and "investment." See, for example, *Interest and Prices,* 88. On the concept of a "rate of sale," as well as of the "number of middlemen's sales," which, in certain types of "speculation," might have the effect of tying up a certain amount of cash balances, and therefore changing the breadth of the stream of money directed against goods, see below, pp. 554 ff. and 563 f.

[43] Cf. the *Treatise,* I, 199.

[44] This point was stressed by Keynes himself, *Treatise,* I, 199. Yet his stressing of the point was followed immediately by the troublesome statement with respect to "increased investment" causing "an increased demand for actual goods for use and not for 'speculative' purposes" and thereby "sending up prices," which is here under discussion.

[45] This was, in fact, clearly implied by Wicksell, *Interest and Prices,* 97 f.

therefore, what there is in "speculative" operations as such to alter the conclusion that whenever there is a distinction between "investment" and other forms of entrepreneurial spending on goods and services, it is the fact that there is entrepreneurial spending—or, rather, that there is *spending* of any kind upon such goods and services—and not the "investment" character of such spending, which will decide whether or not the banking operations involved may be expected to affect general prices.

It is, however, when we have passed the initial hurdle of translating Mr. Keynes's "investment" into terms whose relevance to the determination of general prices becomes at least *a priori* plausible, that we realize that our difficulties have only begun. What factors combine to make the excess of "investment" over saving—when "investment" is interpreted, say, as "the total of entrepreneurial spending"—*as large as it is?* It is, indeed, precisely at this point that Mr. Keynes's argument may be said to have given rise to the greatest confusion and also to have unwittingly provided a demonstration of the impossibility of "dispensing," in any genuine sense, with the older Quantity Equations.

The first element of confusion was introduced into the discussion by Mr. Keynes's suggestion that an emphasis upon "investment" as the link by which changes in bank rate would be expected to affect prices—his "third strand" in the development of doctrine with respect to the *modus operandi* of bank rate—is somehow in conflict with, or at any rate makes superfluous, the "first strand" in that doctrine—namely, that bank rate is a "means of regulating the *quantity* of bank-money," the "creation of additional purchasing power" thus involved being the force which "raises prices" in "accordance with the Quantity Equation." [46] We were told, in fact, that to accompany an emphasis upon the "third strand" with an argument which "reduced to practically the same thing as the first strand . . . , namely, that the level of Bank-rate determines the volume of bank-money and hence the price-level" must result in "obscurities" which would be "overcome" only by exclusive emphasis on the "third strand." [47]

[46] See the *Treatise,* I, 187 f., 192. On the significance to be attached to Mr. Keynes's italicizing the word "quantity" in the expression "the *quantity* of bank-money," cf. above, pp. 208 ff.

[47] See Keynes's commentary on "the obscurities" in "Wicksell's theory," in this respect, *Treatise,* I, 196 f.

It must be obvious, however, that such a suggestion was misleading in the extreme. When "investment" is interpreted as it must be for purposes of describing the steps by which changes in bank rate may be expected to affect general prices—that is, as the "total of entrepreneurial spending" —it is surely clear that "investment," so defined, is a function of the amount of bank money created by banks for the purpose of financing such entrepreneurial spending. Mr. Keynes himself certainly argued, in the *Treatise,* that the "second term of the Fundamental Equation"—that is, *I*—*S* —was affected by the "quantity of money." [48] In fact, we were told explicitly that the "banking system governs the value of investment" by "varying the price and quantity of bank-credit." [49] It was, therefore, a reasonable conclusion that any theory which was designed to explain why the *I* of the Fundamental Equations is as large as it is, must necessarily include an adequate account of the forces determining the "quantity of money"—in this case, of course, the quantity of "bank-money." It involved, that is to say, the whole of the theory "lying behind" the *M'* of the older Quantity Equations.

Yet it was an outstanding characteristic of Mr. Keynes's contributions to the debate which grew up about the argument of the *Treatise* that he should have seemed to bend every effort to prevent the drawing of such a conclusion. When, for example, Mr. Robertson attempted to introduce into a discussion of the *Treatise* the question as to the effect upon general prices of the creation of "new money" by a "Government or banking authority" in the way of a dole, he was rebuked by Mr. Keynes for suggesting that the "outcome" would depend on "the additional quantity of money associated with this policy" *instead of* upon "the net effect of the Government's policy on the relation between savings and investment." [50] Again, in his Reply to Professor Hayek's criticism of the argument of the *Treatise,* Mr. Keynes, instead of attempting to clear up what confusion existed with respect to his understanding of the relation

[48] Cf. the *Treatise,* I, 269.
[49] *Treatise,* I, 182.
[50] See Keynes's "Rejoinder" to Robertson, *loc. cit.,* 420.

between changes in the "quantity of bank money" and such discrepancies between "investment" and saving as might result from changes in bank rate, retorted only with a sharp denial that he had ever alleged that "the amount of . . . monetary expansion exactly measures the excess of investment over saving." [51] It must, of course, be obvious that the phrase "exactly measures" was a purely gratuitous detail which was irrelevant to the main issue. This main issue was simply the question whether an adequate account of the forces determining the extent of a discrepancy between "investment" and saving, in a sense in which such a discrepancy could be shown to be relevant to the problem of the determination of general prices, could be presented without doing full justice to the forces determining the "amount of monetary expansion" which is involved in the change in the total of "investment."

It is no answer to issues of this type to argue that one cannot predict the effect of a given expansion in M' upon prices without taking account of possible changes in other variables which can also be shown to affect prices. As we have seen, the suggestion that changes in bank rate may affect variables in the Quantity Equation other than M' does not involve a denial of the necessity for tracing the effect of such changes upon M' itself.[52]

Still less is it an answer to the issues involved to suggest that a concern with what happens *after* the quantity of bank money and the total of entrepreneurial spending has been altered is just as important as a concern with the forces determining the amount of bank money. Whether the general argument of the *Treatise* and its Fundamental Equations were superior to the received apparatus for tracing these subsequent steps—for example, the effects upon profits and losses of producers; upon the earnings of the factors of production; and even upon such remote variables as the "volume of savings" and the "volume of goods coming forward available for consumption"—is a question which is partly answered by our examination, in Chapter Five, of the formal validity of the Fundamental Equations

[51] Cf. Keynes's "Reply to Dr. Hayek," *loc. cit.*, 5.
[52] Cf. above, pp. 208 ff.

when interpreted as providing a framework for the simultaneous accomplishment of all these tasks under changing conditions, and will be more completely answered in those parts of the following chapters which undertake to present the traditional method of dealing with these subsequent phases of the process of price change.[53] The important thing to be observed here is that these are matters which have to do with events occurring *after* changes have been effected in the quantity of bank money and the total of entrepreneurial spending ("investment"). The fact remains that Mr. Keynes's own use of the Fundamental Equations for the purpose of tracing the *modus operandi* of bank rate was made obscure by his insistence upon starting with a variable—"Investment"—which was neither defined in such a way as to make it precisely relevant to the problem of the determination of general prices, nor, when so defined, capable of displaying the nature of the forces which can in fact be shown to be strictly relevant to the problem of tracing the steps by which changes in bank rate may be expected to modify the stream of money expenditure on goods and services and therefore the prices of those goods and services. The fact remains, also, that the older Quantity Equations, as far as the *modus operandi* of bank rate is concerned, are perfectly capable of accomplishing this task.

In short: so far from its being true that "when we . . . attempt to analyse the actual monetary problems of the day" we are "compelled to discard" equations of, say, the Fisherine type, we find that it is precisely for such problems that we *cannot* discard these equations; and so far from

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[53] For Keynes's account of these later steps in the "causal sequence," see the *Treatise*, I, 182, 193. It must be obvious, in the light of the argument presented in the text, that Mr. Keynes's insistence that the doctrine on which he was "brought up"—namely, Marshall's emphasis upon changes in bank rate as affecting prices by resulting in the "creation of additional purchasing power"—"did not bring home" to his mind "any clear idea" of these subsequent steps, is quite irrelevant to the questions which he was actually discussing—namely, the relation between the "first strand" and the "third strand" in the "traditional doctrine," and between the quantity of "additional purchasing power" created and the total of "investment" as the immediate factor affecting prices. On the fitness of the Quantity Equations for tracing the steps whereby part of a given increase in "purchasing power" may be "held up" before it is spent upon commodities, see, in addition to pp. 527 ff., below, p. 209, above.

its being true that the older Quantity Equations are "quite ineffective for handling the elements which most matter" in problems concerning the effect of banking operations upon prices, they make it possible to provide precisely the type of "simple and direct explanation why a rise in the Bank rate tends . . . to depress price-levels" which Mr. Keynes had denied to them, and had claimed—on grounds the validity of which we are now in a position to judge—for the Fundamental Equations of the *Treatise*.[54]

[54] For the passages cited, see the *Treatise*, I, 155, 222.

CHAPTER ELEVEN

"Velocity of Circulation" (*V*)[1]

I

The Concept of "Velocity" in Monetary Theory

THE introduction into the "Theory of Prices" of the factor which, almost from the very beginning, was called the "velocity" (or "quickness") of circulation of money was accomplished, as we have seen, as early as the days of Petty and Locke.[2] Yet the introduction of a concept is a vastly

[1] The reader is asked to observe that while the length of the discussion of the velocity of circulation of money presented in this chapter and the chapters immediately following would hardly be justified if it did not deal with questions of much broader interest than the mere question of the rightness or wrongness of Mr. Keynes's detailed analysis on the subject, these chapters are not to be regarded as an attempt to deal with the problem of velocity with the comprehensiveness and detail it deserves. I hope to return to the problem in a later publication.

[2] Cf. above, p. 96, and n. 55 thereto. The expression "quickness of circulation" is to be found in Locke. Cf. Holtrop, "Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *loc. cit.*, 504. This article, which reproduces with some modifications the substance of Chapter I of the same author's *De Omloopssnelheid van het Geld* (Amsterdam, 1928), should be contrasted, as a performance in the writing of doctrinal history, with the unfortunate incursions into the field by such writers as H. Hornbostel, *La vitesse de la circulation de la monnaie* (Paris, 1930). The latter, making no mention of Locke, and desiring to disprove the "general belief" (!) that "the work of economists on this fundamental question [of velocity] goes back only about thirty years," advances, as the result of "personal researches," the name of Cantillon as a case in point (Hornbostel, *op. cit.*, 40). Unfortunately, the kind of treatment of the literature found in Hornbostel's monograph is more typical of what has been the general practice until very recent years than is that found in Holtrop's. It is not uncommon, for example, to meet with statements which dispose of the literature on the subject of velocity of circulation by remarks that *John Stuart Mill* had called attention to the necessity for taking account of the "velocity" factor, and that *Irving Fisher* had proposed a mathematical formula in accordance with this suggestion! (So, for example, J. Grunzel, *Geldwert und Wechselkurs*, Vienna, 1923, 31.) Similarly, R. Liefmann (*Geld und Gold*, 1916, 46) cited, from writers earlier than Fisher, only an incidental and commonplace remark on the subject of velocity by Philippovich, which Liefmann chose to take as typical of the current state of monetary doctrine, adding the tart comment that "this is all that is said in the whole of monetary theory" concerning the phenomenon that is usually described under the heading "velocity of circulation"!

different thing from its full development and critical utilization.

As long ago as 1811, Dugald Stewart regarded "an examination of the circumstances by which the [velocity of] circulation of money is liable to be affected" as being "still a *desideratum* in the theory of commerce." [3] At the end of the nineteenth century, however, so little had been done in the way of systematic restatement of received doctrine upon the subject that writers who must themselves be regarded as major figures in the development of interest in the problem could complain that, despite its importance for a "proper appreciation of monetary questions," the problem of velocity had been so "little studied" by monetary theorists, and was so "very scantily treated in most economic text-books." [4] In not one of the decades of the twentieth century, moreover, have there been wanting writers of eminence to complain of the "stepmotherly" treatment accorded to the concept of velocity, and of the "apparent inability" of the problem to "catch the interest of economists" over a period sufficiently long to yield adequate results.[5]

The result of this failure to gather up the materials that have been left at the wayside by successive generations of isolated workers into a critically constructed and consistent whole which would, as Wicksell hoped, leave little to be desired from the standpoint of "detail and clearness," was

[3] Dugald Stewart, "Notes on the Bullion Report" (published as an Appendix to Sir William Hamilton's edition [1877] of Stewart's *Lectures on Political Economy*), 433. Stewart was not alone among the writers of his day in regarding a study of the factor of velocity as "still a *desideratum* in the theory of commerce." The anonymous author, for example, of *The Theory of Money; or, A Practical Inquiry into the Present State of the Circulating Medium,* London, 1811, in protesting that while much had been "ably written and eloquently delivered" on the subject with which his book was concerned, "some points have been too slightly discussed," declared particularly that "a fuller consideration of the Effects of the *Velocity of its Circulation* upon the *Value* of Money is still wanting" (page i; italics in the original).

[4] See, for example, P. des Essars, "La vitesse de la circulation de la monnaie," *Journal de la Société de Statistique de Paris,* 1895, 149; and Wicksell, *Interest and Prices,* 51.

[5] See, for example, A. Landry, "La rapidité de la circulation monétaire," *Révue d'economie politique,* XIX (1905), 155; J. Schumpeter, "Der Sozialprodukt und die Rechenpfennige," *loc. cit.,* 667; D. Davidson, in the *Ekonomisk Tidskrift,* XXII (1920), 121, and XXXII (1930), 236; H. Neisser, "Der Kreislauf des Geldes," *Weltwirtschaftliches Archiv,* XXXIII (1931), 365.

what might have been expected: the opening of the field to attacks upon the very concept of "velocity" which were as violent as they were ill-founded.[6] Already in 1883, for example, it was declared that all attempts to discuss the forces affecting the "demand" for money in terms of something called its "velocity of circulation" represented a reliance upon "a thoroughly empty and meaningless formalism," and that "the whole theory [that is, the very concept of velocity] remains something altogether unfruitful, purely academic."[7] Again in 1901 we were told that "the theory [that is, the concept] of the rapidity of circulation [of money] is one which . . . by bringing in a fictitious cause to account for certain of the phenomena of money and prices, tends to obscure our view of the causes that really explain them," and that "by no sort of twisting and turning can any valid signification whatever be attached to the theory [that is, to the concept of velocity]"; and in 1914 we were told flatly that the concept of velocity of circulation "signifies nothing."[8]

The last decade was particularly notable for contributions of a positive nature to our understanding of the problems which are summarized by the concept of "velocity of circulation," as well as for an extremely welcome monograph—that of the Dutch writer M. W. Holtrop—which represents the first serious attempt to synthesize the results thus far achieved on the subject.[9] Yet it was precisely during this

[6] For Wicksell's comment, see his *Lectures*, II, 160. The list of instances of an attack on the very concept of velocity which are cited in the sentences of the text which follow is, of course, in no sense complete. Such an attack was certainly implicit, for example, in the argument of those who, at recurring intervals during the last half century, have contended that changes in what had been called the "velocity of circulation of money" could not be expected to affect prices, since every such change would be accompanied by an equivalent change in the "velocity of circulation of goods." For references, see my article on "The Relation between the Velocity of Circulation and the 'Velocity of Circulation of Goods,'" in the *Journal of Political Economy*, XL (1932), 290 ff.

[7] So R. Hildebrand, *Theorie des Geldes* (1883), 40, 42.

[8] Cf. W. W. Carlile, *The Evolution of Modern Money* (1901), 163, 165 f., and L. Deschesne, "Pour la théorie quantitative de la monnaie et du crédit," *Révue d'économie politique*, XXVIII (1914), 409.

[9] Cf. above, p. 291, n. 2. The same author's "Die Umlaufsgeschwindigkeit des Geldes," contributed to the *Beiträge zur Geldtheorie* (1933) edited by F. A. Hayek, represents a reworking of part of the material contained in the earlier Dutch monograph. The only works of monographic dimen-

decade that the concept of "velocity" was subjected to attacks which, for violence, surpassed anything that had gone before. The proposition that too much reliance had been placed upon the element of velocity in attempts to explain the facts of a given historical situation was the mildest of the allegations advanced in these attacks.[10] It was alleged,

sions of which I am aware, on the subject of velocity, prior to the publication of Holtrop's book, are those of E. Kellenberger, *Geldumlauf und Thesaurierung* (1920) and J. F. Feilen, *Die Umlaufsgeschwindigkeit des Geldes* (1923). The first of these is of some historical importance as representing the first attempt to deal with the problem of the velocity of circulation of money and with the history of doctrine concerning that problem on a scale commensurate with its importance. Its effectiveness in directing attention to the problem was, however, greatly impaired by its misleading subtitle (*Grundsätze der Notenbankpolitik*): so much so that the book was not even mentioned by either Feilen or Holtrop. Feilen's book is likewise of some historical importance because of the influence—not always for the best—which it exerted on Holtrop and other writers in some aspects of their treatment of the history of doctrine on the subject. Nor is it by any means without value for bibliographical purposes, particularly as far as the later German literature is concerned. It suffers, however, from very serious faults on the analytical and historical side. (For a contrary judgment, see H. Haenel, "Geld und Kredit," *Zeitschrift für die gesamte Staatswissenschaft,* LXXIX [1925], 313, 315, 317, etc.; and H. Honegger, *Volkswirtschaftliche Gedankenströmungen der Gegenwart* [1925], p. 67. For a judgment, however, that is much more nearly in accordance with that expressed above, see the reference to L. Bortkiewicz in my article "Zur Dogmengeschichte des Begriffes einer 'Umlaufsgeschwindigkeit der Güter,' etc.," *Zeitschrift für Nationalökonomie,* IV, 192, n. 4.) The three monographic treatments of the subject known to me as having appeared after that of Holtrop have not succeeded in shaking the position of Holtrop's book as the best extended treatment now available. Hornbostel's *La vitesse de la circulation de la monnaie* (1930) can hardly be said to make up, by analytical contributions of importance, for the shortcomings of its treatment of the earlier literature (on which see above, p. 290, n. 2, and my "Léon Walras and the Cash-Balance Approach, etc.," *loc. cit.,* 591, n. 51). A. Sternschein's *Das Wesen der Umlaufsgeschwindigkeit des Geldes* (1933), while it is saved from the egregious shortcomings of Hornbostel's book by the author's acquaintance with Holtrop's work—an acquaintance denied to Hornbostel—is, on the historical side, largely repetitive of Holtrop, and, on the analytical side, must be ranked considerably below the latter. Much the same thing must be said of G. Masillo's *La velocità di circolazione della moneta* (Naples, 1937), although it shows a commendable degree of familiarity with some of the literature that has appeared since the publication of Holtrop's work.

[10] Cf., for example, the articles by B. Harms on the Rentenmark in *Wirtschaftsdienst,* IX (1924), especially 841, 992. A very long list of citations could be given from the writings of avowed "anti-quantity theorists" who have insisted that the "quantity theorists," when confronted with "facts" which are not consistent with "the quantity theory," have attempted to cover themselves by putting on the concept of "velocity" a burden much greater than it can bear. An adequate treatment of this type of contention and of the context in which it appears at the hands of the various writers involved would require very much more space than can be accorded here; cf., however, what is said on this matter on pp. 347 ff., below.

more generally, that the factor of velocity was not only relatively "unimportant" for the general Theory of Prices, but actually "never has influence upon prices."[11] Still more generally, it was charged that the very concept of "velocity" is one which, since it purports to describe something that "does not exist," is a "perverted" notion, which "never carried conviction to the ordinary intelligence, and never will, even if much more carefully stated."[12] From such a position it was a short step to the contention that the very concept of "velocity," having already wrought immense harm in the field of monetary theory, ought to be completely "erased" from discussions of problems in that field.[13]

II

Attacks on "Velocity": Appearance and Reality

Even if we admit, however, as we must, that part of the responsibility for such confusion as appears to exist on the subject of "velocity" is to be attributed to the failure of economists, until very recently, to attempt to weave the scattered elements of received doctrine into a consistent and articulate whole, it does not follow that the extent of disagreement is actually as great as it can be made to appear by a random selection from the writings of those who have attacked the concept. The question, for example, of whether too much stress has been placed on "velocity" as an

[11] The contention that the factor of velocity is essentially "unimportant" for the Theory of Prices not only has been made explicitly (cf., for example, P. Schröder, in the *Jahrbücher für Nationalökonomie und Statistik,* CXX [1923], 178), but is of course implicit in the argument of those who insist that the concept is "superfluous" and may be dispensed with entirely. See, for example, the discussion on pp. 344 ff., below, of this aspect of certain variants of the "income approach," and the references there given. The contention that the factor of velocity "never has influence on prices" (so, for example, S. Asch, *La circulation monétaire* [Paris, 1924], 55, 109, *et passim*) is likewise implicit in all arguments of the type referred to on p. 292, n. 6, above. See the references given in my article there cited.

[12] Examples of this type of contention may be found in K. Kirmaier, *Die Quantitätstheorie* (1922), p. 61; F. Bendixen, "Bemerkungen zur Geldschöpfungslehre," *Jahrbücher für Nationalökonomie und Statistik,* CXIII (1919), 127; and E. Cannan, *An Economist's Protest* (1927), p. 385. Cf. also the unsympathetic comments by L. Mises, "Die Stellung des Geldes im Kreise der wirtschaftlichen Güter," *loc. cit.,* 313 f.

[13] Cf., for example, C. Gide, quoted by S. Asch, *La circulation monétaire,* 44 n.

explanation of price movements in a given historical situation is one which, in the nature of the case, can remain in dispute only so long as the empirical basis for measuring "velocity" during the period under discussion remains inadequate; it can hardly be regarded as relevant to the question of the validity of the concept itself, or of the importance attaching to it for purposes of monetary analysis.[14] The latter question, in turn, is not to be settled by apodictic utterances as to the "importance" or "unimportance" of the concept; for if there are authors who have insisted upon its "unimportance," there are others, including some writers of considerably greater authority in the eyes of competent judges, who have insisted that the concept of velocity is in fact the "pivot of the whole of monetary theory," and still others who have not hesitated to summarize certain discussion by British monetary theorists during the last decade—discussions the importance of which no one conversant with the field and its possibilities will deny—as actually representing an attempt to transfer to velocity of circulation, considered as a factor affecting the price level, the position of preëminence formerly occupied by the quantity of money.[15]

Above all, however, it is necessary, as Cairnes argued in his reply to Comte's charge that the whole of economic writing was " 'metaphysical'—that is to say, vague, 'personal,' full of 'sterile and illusory controversies' "—to "dis-

[14] It is true that a considerable number of those who denied any validity to the concept of "velocity" have based their argument upon the supposed impossibility of measuring it statistically. It is, however, characteristic that the writers concerned should not have bothered to take account of the measures of "velocity" provided by writers such as Fisher, Snyder, Burgess, and others, to say nothing of bothering to develop an argument designed to show why the work of these writers cannot be improved upon.

[15] For an example of the first type of statement, see J. Marschak, "Volksvermögen und Kassenbedarf," in the *Archiv für Sozialwissenschaft und Sozialpolitik,* LXVIII (1933), 385—although, for reasons which are not explicitly stated and the nature of which, in the light of the argument presented in Chapters III and IV of this book, it is not easy to imagine, Marschak thought it necessary to qualify his statement with respect to the crucial importance of velocity by suggesting that it had this importance primarily for monetary theory "in its simpler static form." For examples of the second type of statement, see J. P. Lazard, "Les récentes théories monétaires anglaises," *Révue d'economie politique,* XL (1926), 799, and A. Aftalion, *Monnaie, Prix, et Change* (1927), 144.

tinguish." [16] It is necessary, in the first place, to "distinguish," as Cairnes did, between the utterances of the "crowd of unqualified persons," on the one hand, who have been attracted to the discussion of economic topics, and, on the other, "the doctrines of the science as expounded in the works of acknowledged masters." In the present instance, for example, a perusal of the list of those who have questioned the justification of the very concept of "velocity" will demonstrate that, with some notable exceptions for which, as we shall see, an adequate explanation can be found, the list is more impressive by reason of its length than by reason of the importance of the names which it contains. This is not to say, obviously, that whenever the "opponents" of the concept of velocity advance arguments which are at once intelligible and sufficiently influential, they should not be answered. What it does say is that the mustering of a varied roll of heretics without regard to their technical qualifications for an intensive discussion of the points at issue proves nothing with respect to the "confusion" that may be said to exist in the minds of qualified monetary theorists on the subject.

Nor, from the fact that writers of universally recognized competence either have been characterized by historians of doctrine as "opponents" of the concept of velocity, or have apparently given clear warrant for this interpretation of their position by declaring, with every evidence of satisfaction, that the whole concept of velocity of circulation "has been entirely scrapped," are we warranted in concluding that there is an unbridgeable gap between the position of these writers and the position of those who, with all recognition of the inadequacy of much of the available analysis respecting velocity, unrepentantly cling to the general concept itself.[17] To those, for example, who have penetrated to the real nature of the relation between the concept of "velocity" and the so-called "cash-balance approach," it is

[16] See, again, Cairnes, "M. Comte and Political Economy," *loc. cit.*, 285 f. Cf. also what is said on pp. 204 f., above, in connection with the present status of discussion with respect to the concept of a "natural rate of interest."

[17] The quotation with respect to the "scrapping" of the concept of "velocity" is from Cannan, *An Economist's Protest*, 385.

nothing short of absurd that writers such as Menger, Mises, Hawtrey, and Cannan should be regarded as irreconcilable "opponents" of the concept of "velocity." [18] What we have here, at the worst, is an example of the type of episode in the development of doctrine which not only can be accounted for in Hegelian terms, but can be parallelled by innumerable instances in the development of the natural sciences: a point of view which seems at first sight unrelated and even contradictory to that which has previously prevailed, is found, as time goes on, to fit with perfect ease into an improved and elaborated statement of the older doctrine. To present lists of "opponents" of the concept of velocity, therefore, which make no clear distinction between the "crowd of unqualified persons" discussed above and the "qualified persons" who happen to have stated the basis for the "cash-balance approach" in terms that have turned out to be not entirely fortunate, is misleading in the extreme.[19]

We must distinguish, finally, between that which is to be regarded as the essentially unchallenged and definitive core of received doctrine and the problems on the periphery of our subject which are still in need of exploration and clarification. It is no sin to err, if the error is committed in fields largely uncharted; it is not surprising, therefore, that econo-

[18] For a characterization of Menger as an "opponent" of the concept of velocity, see, for example, Feilen, *Die Umlaufsgeschwindigkeit des Geldes*, 23, 26 f. A similar characterization of Mises is at least implied by Sternschein, who (*Das Wesen der Umlaufsgeschwindigkeit*, 4) lumps the former with Bendixen—an avowed "opponent" of the concept of velocity—and others in whose works, it is alleged, no trace of the concept appears. For a characterization of Menger, Cannan, and Hawtrey as "opponents" of the concept of velocity along with such writers as Hildebrand, Liefmann, Bendixen, and K. Elster, supposedly upon the basis of Holtrop's discussion of the writers concerned, see F. A. Hayek, in the *Zeitschrift für Nationalökonomie*, II (1930), 143. It is, however, important to record the fact that Holtrop himself called attention to the rôle played by the three writers first mentioned in the development of the "cash-balance approach" to the problem of "velocity." Cf. Holtrop, *Omloopssnelheid*, pp. 29, 69; and on general relation of the "cash-balance approach" to the concept of velocity, cf. below, pp. 417.

[19] Cf., in this connection, the heterogeneous list of "opponents" of the concept of velocity which is given by Feilen, *Umlaufsgeschwindigkeit*, 23 ff. The same type of indiscriminateness is involved in the assertion by Asch (*La circulation monétaire*, 44), that the concept of velocity has been "rejected" by the "majority of German economists." Cf. also the preceding note.

mists of the eminence of Charles Gide or H. J. Davenport have appeared on the side which to a majority of later writers has not seemed to be that of the angels.[20] Nor is it surprising that writers of competence should complain that certain aspects of the concept of velocity, despite the fact that it is so "glibly employed in economic literature," are "vague," and that in some respects the problem of velocity is "among the most disputed issues of contemporary monetary theory." [21] This would be a source of reproach only if the aspects which are now "vague" should remain forever so; or if it were in fact true that nothing that could be regarded as a consensus of informed opinion prevailed with respect to any part of the subject. It can, however, be demonstrated—and an attempt at such a demonstration will be made in the following chapters—that the extent of agreement among competent workers in the field is vastly greater than that which is implied by a comment to the effect that, on this subject, "unanimity prevails only with respect to the proposition that there is such a thing as 'velocity of circulation of money.' " [22]

III

"Velocity" in Keynes's *Treatise*

It must be admitted, on the other hand, that to one who is really anxious to emphasize the thickness of the "haze" enveloping the "more sophisticated" discussions of problems in the Theory of Prices, the present status of the problem of velocity, presenting, as it does, so many superficial indications of confusion and disagreement, would seem to have provided a golden opportunity. It would not have been strange, therefore, if Mr. Keynes had seized upon the con-

[20] Cf. the references to Gide and Davenport given in my article "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " *loc. cit.*, 295 f., nn. 21 and 22.

[21] Cf. F. D. Graham, *Exchange, Prices, and Production in Hyper-Inflation: 1920-23* (1930), 107, and Holtrop, "Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *loc. cit*, 503.

[22] So S. Budge, in a review of Holtrop's *Omloopssnelheid*, in *Zeitschrift für die gesamte Staatswissenschaft*, LXXXVII (1929), 423. Cf. also the comments, with respect to the degree of "unanimity" thus far obtained, by F. Lutz, "Über die Umlaufsgeschwindigkeit des Geldes," *Jahrbücher für Nationalökonomie und Statistik*, CXLIV (1936), 386.

cept of "velocity" for his most determined attacks upon the substance of received doctrine on the subject of the "Theory of Prices."

As it happens, a superficial reading of the *Treatise* and of Mr. Keynes's contribution to the discussion to which the *Treatise* gave rise might easily lead one to suppose that he definitely intended to launch such an attack. It would be easy, for example, to class him with those writers cited above as violent opponents of the very concept of "velocity" because of the fact that he ventured to characterize the notion as an "omnibus conception." [23] Yet what Mr. Keynes was actually attacking here was not the general concept of "velocity," but certain broadly inclusive variants of that concept.[24] If the characterization of *certain variants* of the concept of velocity as "omnibus conceptions" is enough to warrant calling the authors of such characterizations "opponents" of the very concept of velocity, the list of such "opponents" would have to include some who have contributed most to an understanding of our subject.

It is true, also, that, as in the case of the "quantity of money," Mr. Keynes gave no specific place to "velocity of circulation" either in the formal statement of the "Fundamental Equations" or in the numerous long-hand transcriptions of these equations to be found in the *Treatise*; and that he put "velocity of circulation" on a par with the "quantity of money" as being typical of the "old-fashioned" approach to the problem of the determination of prices which he wished to abandon in favor of his "newer" approach.[25] In the first place, however, it will be recalled that our examina-

[23] See the *Treatise*, II, 5. Paradoxically enough, Mr. Keynes proceeded at once to write a "Quantity Equation" of his own in which the "omnibus conception," "velocity of circulation" (V), was replaced by the "complex notion" V', which included not only the effect of all the forces that would make it "of a similar character to the traditional velocities of circulation" but also the effect of a lack of "balance between Saving and Investment"! Just how much more "complex" the notion of V', as thus defined, is than the rejected "omnibus conception" V will be apparent from our discussion of the relation between "Saving" and "Investment" in Volume II of this study. Cf. also p. 410, n. 52, below.

[24] Cf., on this matter, pp. 403 ff., below.

[25] Cf. above, p. 3, n. 3, and p. 15, n. 20. For the interpretation of the statement, on I, 185 of the *Treatise*, that "the velocities of circulation" are "elements in the Fundamental Equation," cf. above, p. 13, n. 13, and also below, pp. 411 ff.

tion of the actual argument of the *Treatise* showed that it is anything but clear that this argument can be regarded as having made superfluous either the older "quantity equations" or the emphasis on the "quantity of money"—whether money of ultimate redemption or "bank-money"—which is included in the older equations. In the second place, there is nothing in the actual treatment of "velocity" in the *Treatise* which corresponds in degree to the gingerly anxiety with which Mr. Keynes seemed to wish to avoid the specific introduction of the "quantity of money" as a major factor affecting prices. On the contrary, the concept of "velocity" was not only specifically introduced as part of the positive analysis at several points in the *Treatise*, but was discussed in a spirit and with an amount of detail that led some of his commentators to point to the *Treatise*'s discussion of "the theory of velocity" as one of the respects in which the work, by "treating thoroughly parts of monetary theory heretofore neglected," surpassed its theoretical predecessors, and led these same commentators to group Mr. Keynes with Holtrop and others in a list of those who have done most to advance our understanding of the problems which the concept of "velocity" was intended to summarize.[26]

The thing to be said of Mr. Keynes's treatment of the factor of "velocity" in the *Treatise*, indeed, is not that it represents an attack upon the concept of velocity, but rather that it represents the kind of mixture of acceptance and rejection of various parts of received doctrine on the subject which is, and should be, characteristic of work designed to build further upon the foundations that have been provided

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[26] See, for example, H. Neisser, "Kredit und Konjunktur nach J. M. Keynes," *Weltwirtschaftliches Archiv*, XXXIV (1931), 1*, and E. Lederer, in *American Economic Review*, XXVI (1936), Supplement, 158. Even so generally unsympathetic a commentator as Professor Hayek was moved to regard those parts of Mr. Keynes's argument which the former believed capable of translation into terms of a change in "velocity" as representing "in many respects, the most interesting part of his theoretical analysis" (Hayek, "Reflections," Part II, *loc. cit.*, 34). The extent to which the phenomena in question are capable of translation into changes in "velocity of circulation" will be examined in Volume II of this study. It is sufficient here to call attention to those parts of the analysis of the *Treatise* which introduce the concept of "velocity" emphatically and without equivocation. See especially, in this connection, Chapter XXIV of the *Treatise*; also *ibid.*, II, 79 ff.

by earlier generations of investigators. If there are grounds for regret with respect to Mr. Keynes's exposition, they are, first, that the nature of the relation of his own position to that of earlier writers was by no means always made clear, with a resulting lack of certainty as to the precise position of the doctrine as a result of his own treatment of the issues involved; second, that the positive analysis presented was neither as systematic nor as complete as one may reasonably expect from an attempt to integrate the scattered fragments of received doctrine into a consistent and unified whole; and, finally, that the points at which he undertook to depart from something that is properly to be regarded as more authoritatively held doctrine were by no means always such as to represent agreement with the growing consensus of informed opinion on the subject. All three of these circumstances are such as to warrant, for those who are interested in a restatement of received doctrine on the subject of the "Theory of Prices," an examination of the treatment of the factor "velocity of circulation" which is to be found in the pages of Mr. Keynes's *Treatise*.

CHAPTER TWELVE

The "Income Approach" to the Theory of Prices: Its History

I

KEYNES'S *Treatise* AND THE INCOME APPROACH

THE first set of problems with respect to the concept of "velocity" which may be said to be inherent in the argument of the *Treatise* derives from the fact that, according to the interpretation assigned to this argument by commentators thereon, the Fundamental Equations of the *Treatise* represented an example of the "income approach" to the problem of the value of money.[1]

That Mr. Keynes himself would have been prepared to acquiesce in such a characterization of the apparatus presented in the *Treatise,* there can be little doubt. Yet there can be just as little doubt that a reader of the *Treatise* who was unfamiliar with the literature on money could hardly have been expected to learn, from Mr. Keynes's own account, just where he broke genuinely new paths and where he was in effect merely reproducing—often, to be sure, with

[1] For an explicit characterization of the Fundamental Equations of the *Treatise* as "really but an elaboration of the old income equation of Aftalion," see Hansen and Tout, "Investment and Saving," *loc. cit.,* 125. Cf. also C. Clark, *The National Income, 1924-1931,* 128, where, in what purports to be a summary of the Fundamental Equations of the *Treatise,* it is stated that "the recorded national income . . . $= E + Q$," and p. 134, where the second of the Fundamental Equations is written in the form $E + Q$ [that is, "national income"] $= O\text{II}$; also Hawtrey, *The Art of Central Banking,* 335, where the equations of the *Treatise* are rewritten in the form "$E + Q = PR + P'C$ = value of output (consumers' income)." See, finally, Bernstein, *Money and the Economic System,* Chapter XI, in which (252 ff.) the Fundamental Equations are presented, along with summaries of the argument of Aftalion and Hawtrey, as examples of an approach which is summarized by the chapter heading "Income and Expenditure, and the Price Level." On the association, by some commentators, of Keynes's earlier "cash-balance" equation $n = pk$ with the "income approach," see below, p. 362.

admirable clarity—what was, in fact, the flowering of a long tradition in monetary theory.

It is easy, for example, to show that when Mr. Keynes cited Marshall as the single representative of that particular variant of the "income approach" which insists upon regarding "the" purchasing power of money as "the power of money to buy the goods and services on the purchase of which for purposes of *consumption* a given community of individuals spend their *money income*," he did anything but justice to those other writers who had advanced a similar proposition with more emphasis and articulateness than Marshall had.[2] It is easy to show, also, that more than a little misrepresentation was involved in Mr. Keynes's characterizing as the "traditional" approach to the Theory of Prices that which would limit itself to "setting out from the total quantity of money irrespective of the purposes on which it is employed," without going on to concern itself with "the flow of the community's earnings or money income." [3] It is easy, finally, to provide a direct refutation of Mr. Keynes's charge that "the forms of the Quantity Theory [read: Quantity Equation] . . . on which we have all been brought up" took no pains—"any of them"—to distinguish the "types of expenditure" which are to be regarded as "fundamental" from those involved in "economic transactions which are capable of deriving any significance" only from "their having some relationship, sooner or later" to transactions of the sort involved in the "expenditure of consumers." [4]

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[2] Cf. the *Treatise*, I, 54 (italics mine), and the note thereto; cf. also *Memorials of Alfred Marshall*, 207. For other instances in which a proposition similar to that quoted by Keynes from Marshall was advanced, see the references to Tooke, Wicksell, Wieser, Schumpeter, and Lindahl on pp. 314, 326, 339, n. 111, and 328, respectively, below.

[3] See the *Treatise*, I, 134; and cf. the references to Tooke, Wagner, Johannsen, and Hawtrey, on pp. 314, 316, 320, 333, and 340, respectively, below.

[4] See the *Treatise*, I, 133 f. That Mr. Keynes was discussing forms of the Quantity *Equation* rather than forms of the Quantity *Theory* is clear from the fact that the passages cited occur in his discussion of the Fundamental Equations and their supposed superiority over the "statical equations" which he rejected. See also the *Treatise*, I, 76, where the same charge is levied specifically against "the Quantity *Equations* which have been in use hitherto" (italics mine). For an example of a "Quantity Equation" of the "income" form prior to the publication of the *Treatise*,

Surely no belittlement of Mr. Keynes's own contributions, in the way of both articulation and emphasis, to something that is properly to be called an "income-approach" to the *Theory of Prices,* is implied in the suggestion that—at least for those who wish to establish the fact of continuity in the development of monetary theory—some interest attaches to a description of the main elements of the "income-approach" as they existed just prior to the publication of Mr. Keynes's *Treatise,* and to an indication of the chief landmarks in the history of that "approach." As it happens, no really adequate account of the historical development of the "income approach" is at present available. There is, to be sure, a sufficient number of such accounts in existence to make more than a little ridiculous the claim by some of the more recent protagonists of an "income theory of prices" to have provided an entirely "new monetary theory."[5] Yet it must certainly be admitted that the treatment of the literature which one finds even among those who have shown themselves to be aware of the fact that a literature on the subject exists leaves much to be desired both from the standpoint of range and of emphasis.

This is true, for example, of the most ambitious attempt in this direction which has thus far been made—namely, that of W. Mildschuh, who, writing in 1927, built his summary of the "Historical Development of Monetary Theory" upon a framework of which one of the two main pillars was what he designated as "Income Theories."[6] That Mildschuh's discussion should suffer from the same faults of mistaken emphasis and a concern with what are essentially irrelevancies as do certain forms of the "income approach" itself was virtually inevitable in view of the fact that his very distinction between "income theories" and other types of approach to the problem of the value of money is based on one of these irrelevancies—namely, on the "conception of the nature

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it is, of course, not necessary to go beyond the case of Schumpeter (cf. below, p. 339, n. 111; and see also the reference to the first of Lindahl's two formulations on p. 328, n. 78, below). If precedents are desired for an algebraic "income equation" which, like the Fundamental Equations of the *Treatise,* does not give an explicit place to the "quantity of money," one may of course cite also Aftalion (cf. below, p. 306, n. 11) and the second formulation of Lindahl (cf. below, p. 328, n. 78).

[5] So J. S. Robertson, *The Income Theory of Prices* (1935), prefatory note.

[6] Mildschuh, "Geld (Geschichtliche Entwicklung der Geldtheorie)," *Handwörterbuch der Staatswissenschaften,* 4th ed., IV, 718, 725 ff.

[*Wesen*] of money which underlies" these theories.[7] Much the same thing must be said of the chapter in T. Greidanus's *The Value of Money* (1932) entitled "The Income Theories," which is confined almost entirely to the issues associated with a question which, despite the very large amount of attention it has received, must nevertheless be regarded as one of distinctly minor importance for the central problems of monetary theory—namely, the question as to the rôle played by "the marginal utility of . . . *money income*."[8] In the few cases in which the writers who have either designated themselves as sponsors of an "income-theory" or have been so designated by later writers have concerned themselves with the history of doctrine on the subject, the results have not, as a rule, been happy. Zwiedineck, for example, writing in 1909, reported "an overwhelmingly negative result" from his study of the literature on the subject of "income" as a "factor determining the value of money."[9] The principal reason for his lack of success, in this respect, undoubtedly was that, like most German writers of the day, Zwiedineck had not consulted Wicksell's *Geldzins und Güterpreise*, which would certainly have called his attention to the utterances of Tooke.[10] Similarly, Aftalion, whose variant of the "income-approach" has been most

[7] *Ibid.*, 718. The secondary criterion introduced by this author—namely, the treatment of the "passiveness" of prices and the passiveness of the "quantity of money," respectively—can likewise be shown to be essentially an irrelevancy. Cf. below, p. 309, and especially n. 22 thereto.

[8] Greidanus, *The Value of Money*, 119 ff. The italics are Greidanus's. On the reasons for regarding the discussions with respect to the rôle played by the "marginal utility of money income" in the theory of money and prices as having been concerned largely with what are after all irrelevancies, see the brief remarks on p. 309 below, and the forward references given in n. 21 thereto.

[9] O. von Zwiedineck, "Die Einkommengestaltung als Geldwertbestimmungsgrund," *Schmollers Jahrbuch*, XXXIII, 133, n. 6. The references given by Zwiedineck to writers such as Schmoller, Philippovich, Cassel, Menger, and Neurath as representing instances of a "more penetrating consideration of the significance for price-formation which is to be attributed to income" are hardly in point; for, as Zwiedineck himself remarked (*loc. cit.*), none of these writers attempted to make a "genuine application" of their treatment of the concept of income to "the theory of the value of money." (The attempts in this direction which are to be found in Philippovich [see, for example, I, 305 f., 311 f. of the 16th [1921] edition of his *Grundriss der politischen Oekonomie*] were added under the influence of Wagner, Zwiedineck, and particularly Wieser [see the references to these writers on p. 313 of the edition cited; and contrast I, 238 ff., 242 ff., 247, of the 7th [1908] edition].) Much more relevant to the problem are Zwiedineck's references to Cantillon and others, cited on p. 307, n. 13, below.

[10] On the relation of Wicksell to Tooke, in the matter under discussion, see below, pp. 313 ff., and 324 ff. That Zwiedineck was entirely unfamiliar with the *Geldzins und Güterpreise* is obvious from the jejune nature of his discussion ("Die Einkommengestaltung," *loc. cit.*, 157) of "the dogma as to the necessary causal nexus between a lowering of the rate of interest and a rise in prices," in which references were made to Helfferich and R. Hildebrand, but not to Wicksell. Cf. also, in this connection, p. 313, n. 30, below.

widely popularized, not only made no mention of either Tooke or Wicksell, but, although he wrote as late as 1925, mentioned neither Schumpeter nor Hawtrey, referring instead only to Wieser.[11]

The present chapter is intended to provide, within a modest compass, a broad sketch of the historical development of the "income approach" which it is hoped may be free of some of the shortcomings of its predecessors. The reader who is not interested in the historical development of the issues associated with the "income approach" which are dealt with in later chapters, should omit the pages which follow and proceed directly to Chapter Thirteen, where we begin our discussion of those aspects of the "income approach" which are directly concerned with the problem of "velocity."

II

Substance and Shadow in the Income Approach

That the history of the "income approach" does not begin with the contributions of writers of our own day is certainly clear if we choose to date the beginnings of that approach from the time when its various *components* began to appear in the literature. This is undoubtedly true, for example, of the particular component of the so-called "income approach" which is implied in the proposition that the "advantage" of the "income-theory" is that it "views the problem less mechanically" than do the cruder forms of the "quantity theory," according to which "we might expect that a certain increase in money income would increase the general level of prices regardless of which sections of the community receive the increased income," whereas "according to the income theory the increase in prices will be different, depending upon who the individuals are whose money-income is increased," the increase of incomes thus not affecting prices "mechanically, or automatically, but

[11] A. Aftalion, "Les expériences monétaires récentes et la théorie du revenu," *Révue d'économie politique,* XXIX (1925), 838 ff. (cf. the same author's *Monnaie, Prix et Change* [1927], 164 ff.). In Aftalion's later "Die Einkommenstheorie des Geldes und ihre Bestätigung durch die gegenwärtigen Phänomene" (*Die Wirtschaftstheorie der Gegenwart,* II, 376 ff.), there was still no reference to any earlier writers other than Wieser.

through the desires of those whose incomes are raised," so that the "income formula" may be said to give "a more dynamic approach to the problem of price fluctuations." [12]

For it should at once be obvious that this "advantage" must be held to inhere also in all those contributions making part of what has for years been called "monetary dynamics" which, as we have seen, have dealt with issues of this type at least since the middle of the eighteenth century.[13] Cantillon, for example, insisted as emphatically as any contemporary defender of the particular component of the "income approach" here under discussion that "the proportion of the dearness which the increased quantity of money brings about in the State will depend on the turn which this money will impart to consumption and circulation"; that, while it is true that "through whatever hands the money which is introduced may pass it will naturally increase the consumption," this consumption "will be more or less great according to circumstances," since "it will be directed more or less to certain kinds of products or merchandise according to the idea of those who acquire the money"; with the result that "market prices will rise more for certain things than for others however abundant the money may be: . . . the price of meat might be tripled while the price of corn went up only one fourth." [14] Indeed, if the criterion for the inclusion

[12] So F. B. Garver and A. H. Hansen, *Principles of Economics* (1928), 366 f. (338 f. of the 2d [1937] edition). Cf. also T. E. Gregory's Introduction to the reissue (1928) of Tooke's *History of Prices*, 22: ". . . an Income Theory of Prices enables one to think of the price-level more easily as a moving system of forces, with changes in the relations of its parts to one another, than is the case with the Quantity Theory. The result is that the tendency of the Quantity Theory is to emphasise the trend of prices as a whole, and the tendency of Income Theories is to emphasise the dispersion of prices over the field."

[13] Cf. p. 84, above, and n. 30 thereto. It is worthy of note that Zwiedineck ("Die Einkommengestaltung," 155) referred to the writers concerned, whom he discussed in connection with what, following Hoffmann (cf. p. 132, n. 2 of Zwiedineck's article), he calls "monetary dynamics," as having provided "an element which is unmistakably valuable, indeed indispensable, for the whole investigation." Cf. also Hayek, *Prices and Production*, 8 ff., on the relation of analysis, such as that of Cantillon and his successors, which "traces the effects of an increase of money to its influence on individual decisions," to the later "income-theories."

[14] Cf. pp. 235 f. of Cantillon's *Essai* (p. 179 of Higgs's translation). It may be remarked, in passing, that if our model for an "income" approach is Hawtrey's use of "consumers' income and outlay," including the relation of the latter to the theory of the effect of monetary "demand" upon

of a given writer in an account of the historical development of the "income approach" is his responsibility for emphasis upon components of that approach which have a genuinely constructive relevance for a satisfactory "income theory of prices," a writer like Cantillon will be found to have contributed more than those self-designated "income-theorists" who have muddied the waters by their factitious concern with such questions as the "nature" of money, the application of "utility analysis" to the concept of money income, the supposed "passivity" of prices, or any other of the numerous irrelevancies which bulk so large in the writings of even some of the most often cited protagonists of the "income approach."

The association with the "income-approach" of the question as to the "nature" of money is to be found principally in German writers on monetary theory of the post-Knappian period.[15] Zwiedineck, for example, did not hesitate to attribute his difficulty in finding precursors who had accorded what he regarded as adequate recognition to the rôle of income in price determination to the fact that "the theory of the value of money is still too deeply imbedded in the views underlying the metallist theory of money," and specifically credited Knapp with having, through his *State Theory of Money*, opened the way to the construction of a satisfactory theory.[16] R. Liefmann, whose continued reiteration of the proposition that "it is not money which buys goods—it is incomes which buy them" would make it impossible to leave his name out of any account of the development of a recognition of the rôle of income in the Theory of Prices, found it necessary to object even to Zwiedineck's exposition on the ground that the concepts employed, including the concept of "money," were too "materialistic."[17] Mildschuh not only based his differentiation of "income theories" from other "theories," as we have seen, on the concept of the "nature" of money which he attributed to the writers concerned, but allowed his preoccupa-

employment, Cantillon, with other writers before as well as after him, must be regarded as a forerunner of this aspect of the "income approach" also. Cf., for example, pp. 215 f., 221 of the *Essai* (pp. 163, 167 of Higgs's translation).

[15] It is true that Aftalion, in his "Die Einkommenstheorie des Geldes," *loc. cit.*, p. 378, characterized the "income-theory" as being in conflict not only with "the quantity theory" but also with "metallist doctrines." Much less stress, however, was laid upon the latter than upon the former; and one surmises that the introduction of the reference to "metallist doctrines" was itself a result of the writings of the German authors referred to above.

[16] Zwiedineck, "Die Einkommengestaltung," *loc. cit.*, 133 f.; cf. also 139, 150 ff., 187, 189.

[17] For Liefmann's emphasis on the central rôle of income in the Theory of Prices, see his *Geld und Gold* (1916), pp. 63 n., 71 ff., 77 ff., 85 ff., 169 ff., 181, 184 ff. The comment on Zwiedineck is to be found on p. 78 of the same work.

tion with the latter type of issue to lead him to such distortions of emphasis as are involved in the omission of the name of Wicksell from the list of "income-theorists," the placing of the name of Schumpeter in a limbo between the "true" income theories and their "opposite," and the devotion of a considerable amount of space, in the discussion of the "income theory," to the writings of Heyn, Knapp, and other "nominalists" on the ground that they were important for the development of the income approach.[18] For those, to be sure, who would attach more importance to discussions concerning the "nature" of money than I can bring myself to accord to them, some significance may be held to attach also to the fact that the two writers who have done most to provide us with an "income theory" in a really usable form—namely, Schumpeter and Hawtrey—happen also, in their discussions of the "nature" of money, to have been "nominalists." I can, however, only repeat my conviction that this circumstance is entirely irrelevant, in the sense that it is perfectly possible to construct an apparatus designed to show the place of money income in the determination of prices without once introducing pseudophilosophic discussions as to the "nature" of money.[19]

The association of the "income-approach" with the problem of the relation between "utility" analysis and the rôle of income in the determination of prices was introduced almost simultaneously by Zwiedineck and Wieser, and has seemed to some writers the only thing about the "income approach" that is worth discussing.[20] Again, however, I can only record my emphatic dissent from this position, leaving for later pages the few remarks that seem to be called for as far as the place of "utility analysis" in the construction of a satisfactory apparatus for dealing with the determination of money prices is concerned.[21]

The association, finally, of the "income approach" with the question of the "passiveness" of prices—that is, with the possibility that "prices" may "cause" changes in the "quantity of money," as well as *vice versa*—has seemed to some historians of doctrine to be so close as to warrant the suggestion that one of the major differences between the "income-theorists" and their opponents is that, whereas the latter would argue that the "general level of commodity prices depends upon the relationship between the quantity of media of payment and the quantity of commodities," the former would insist that the "quantity of media of payment adjusts itself to the demand, and therefore cannot, as a rule,

[18] Mildschuh, "Geld," *loc. cit.*, 724, 725 ff.

[19] Cf., in this connection, my remarks on the relative degrees of validity which may be held to attach to what has been called "monetary metaphysics," on the one hand, and, on the other, what I have called "problems of mechanism and sequence," in my review of Hawtrey's *The Gold Standard in Theory and Practice,* in *Quarterly Journal of Economics,* XLII (1927), 143 ff.

[20] Cf. Zwiedineck, "Die Einkommengestaltung," *loc. cit.*, 148 ff., and F. von Wieser, "Der Geldwert und seine Veränderungen," *Schriften des Vereins für Sozialpolitik,* CXXXII (1909), 507 ff. Cf. also, in this connection, the reference to Greidanus, on p. 305, above.

[21] See especially pp. 491 f., below.

have an independent influence upon the value of money." [22] There are, unfortunately, a number of instances of just such an association. One of them was provided, for example, by B. M. Anderson, in 1917.[23] The most extensive elaboration of the connection, however, was undertaken in 1925 by Aftalion, who insisted upon relating the "income approach" to the problem of the "causal" relation between variations in exchange rates and variations in domestic prices.[24] The question of the relevance of this type of association for the future development of monetary theory will, of course, be decided largely in accordance with one's convictions as to the importance of the type of dispute involved for monetary theory generally. Again I can only express my conviction that the type of controversy in question, like all disputes with respect to the "truth" or "falsity" of the "quantity theory," should by this time have been regarded as completely outmoded.[25] In any case, it is anything but clear that the lines on which monetary theorists who reject the suggestion that "prices" are more "passive" than "money" would separate themselves from those who believe the opposite, would coincide with the lines on which self-styled "income" theorists would separate themselves from those who reject the really constructive suggestions which may be held to be inherent in a satisfactory form of the "income theory." The point made here is merely that there are grounds for arguing that, as far as the history of these latter suggestions is concerned, writers like Cantillon, who antedated by many years the conscious articulation of an "income approach," have contributed at least as much to an illumination of the central issues as have those writers who have been too anxious to include, as part of the so-called "income-theory," problems with which the issues that are really central in a satisfactory form of that "theory" can be said to have only the remotest connection.

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[22] So, for example, Mildschuh, "Geld," *loc. cit.*, 718. Cf. also p. 312, n. 28, below.

[23] See Anderson, *The Value of Money*, 90, n. 2, 307 ff. Wieser's acceptance of certain aspects of the Banking School argument—for example, the so-called "Fullarton principle" with respect to the passiveness of changes in note issue in the face of a changing "demand" for notes—has sometimes been cited also in discussions of the relation between "income theory" and its alternatives. See, for example, Ellis, *German Monetary Theory*, 179 f. Cf. also the comment on Tooke by E. Petersen, cited on p. 315, n. 38, below.

[24] Cf. Aftalion, "Les expériences monétaires récentes et la theorie du revenu," *loc. cit.*, 813 ff., 823, 828 ff.; also *Monnaie, Prix et Change*, 148 ff., 168 f., 176 ff. Cf. also Aftalion's "Die Einkommenstheorie," *loc. cit.*, pp. 379, 386 f.

[25] It is hardly necessary to rehearse here at length the arguments against the proposition, for example, that the mere fact that under certain circumstances a movement in prices may occur before the corresponding movement in the quantity of money proves that in such cases prices are the "active" factor and money the "passive" factor (though see, by way of illustration, what is said on this matter on pp. 252 ff., above). In view, however, of the fact that the argument with respect to the "active" nature of prices has been associated with the "income approach" through the proposition that "incomes" are determined by "prices," attention may be called to what is said on p. 383, below (especially n. 88 thereto).

III

Tooke and the Income Approach

It must be admitted, on the other hand, that emphasis upon individual components of what has come to be known as the "income approach," even if the particular components involved are distinctly relevant to a satisfactory "income theory of prices," is hardly enough to warrant our attributing to the writers concerned that degree of responsibility for the development of such a theory which properly belongs to those who have explicitly emphasized the element of "income" as a central concept in the Theory of Prices. From this point of view, we should have to begin, not with writers such as Cantillon, but with a writer such as Tooke, by virtue of the latter's adoption of the position which was summed up in the thirteenth of his "conclusions" with respect to the determination of prices.[26] "It is the quantity of money, constituting the revenues of the different orders of the State, under the head of rents, profits, salaries and wages," said Tooke, "that alone forms the limiting principle of the aggregate of money prices. . . . As the cost of production is the limiting principle of supply, so the aggregate of money incomes devoted to expenditure for consumption is the determining and limiting principle of demand."

The rôle played by Tooke in the development of what has come to be called the "Income Theory of Prices" had been commented upon by German writers of our own day even before Professor Gregory, in his *Introduction* to the 1928 reissue of Tooke and Newmarch's *History of*

[26] See Tooke's *Inquiry into the Currency Principle*, 124 (cf. the *History of Prices*, III, 276). The argument which the "thirteenth thesis" was intended to summarize is to be found on pp. 71 ff. of the *Inquiry*. It is quite possible that a closer study of the literature prior to the publication of Tooke's *Inquiry* would disclose statements which have as great a claim as Tooke's "thirteenth thesis" to be regarded as adumbrations of modern variants of the "income theory." See, for example, the remarkable passage from Thomas Joplin's *Outlines of a System of Political Economy* (1823), cited by Hayek, *Preise und Produktion*, 47 n., with respect to the rôle of the "income and expenditure" of individuals in the determination of prices. The other aspects of Joplin's argument which relate it to certain current variants of "the income theory of prices" clearly derive, as is pointed out on p. 314, n. 35, below, from Adam Smith; and it is perfectly possible that other writers may have been similarly influenced. So far as I am aware, however, there is no parallel, in the case of Joplin, to the discussion of Tooke's suggestions with respect to the rôle of income in the determination of prices by a later writer of the standing of Wicksell.

Prices, pointed out that Tooke had "evolved a theory which, in its general tendency, is singularly close to those Income Theories of Prices which in recent years have been adumbrated by Wieser, Hawtrey, Aftalion, and others."[27] In at least one of these cases, however—namely, that of Mildschuh—the force of the citation of Tooke among those who had evidenced some recognition of individual aspects of the "income approach" was more than a little weakened by the generally mistaken emphasis of the particular account of the development of the "income theory" in which the reference to Tooke occurred.[28] Nor were matters greatly helped by the fact that, in the case indicated, the author cited as of equal importance in the development of the "income theory" a passage from John Stuart Mill which is, in fact, entirely irrelevant to the problem.[29] It is, indeed, fair to say that, prior to Professor Gregory,

[27] See, for example, in addition to the references to Spiethoff and Altmann given on p. 313, n. 30, below, K. Diehl, *Über Fragen des Geldwesens und der Valuta während des Krieges und nach dem Kriege,* 76 ff. of the 2d (1921) edition; and Mildschuh, "Geld," *loc. cit.,* 725. For the quotation from Gregory, see the latter's Introduction, 21 f. Cf. also G. Kepper, *Die Konjunkturlehren der Banking- und der Currencyschule* (1933), 7, n. 1, 31 ff., 39 n.; and A. K. Cairncross, "The Victorians and Investment," *Economic History,* III (1936), 287, n. 5.

[28] Characteristically, also, Mildschuh cited the twelfth (erroneously designated as the second) of Tooke's "seventeen theses" as being of equal importance with the "thirteenth thesis" for the construction of an "income theory." In fact, of course, the proposition presented in the twelfth thesis—namely, that "the prices of commodities do not depend . . . upon the amount of the circulating medium; . . . on the contrary, the amount of the circulating medium is the consequence of prices"—is so far from being peculiar to the "income-theory" that its citation can only have tended to confuse, rather than to clarify, our understanding of the precise content of that "theory." Cf. above, p. 310.

[29] The context in which the passage from Mill appears (Mill's *Principles,* Book III, Chap. VII, sect. 3, p. 487 of the Ashley edition) shows clearly that all that Mill was concerned with was an application of his general proposition that "there cannot . . . be intrinsically a more insignificant thing, in the economy of society, than money" (p. 488). One suspects, indeed, that Mildschuh was induced to include the passage from Mill more because of the latter's reference to money as representing "a sort of ticket or order" which a man "can present for payment at any shop he pleases"—in other words, because of Mill's supposed conception of the "nature" of money—than for any other reason. As it happens, it is possible to cite, from writings of Mill other than his *Principles,* an occasional instance of recognition of the rôle of "income" in the determination of money prices which would warrant our assigning to him at least the minor place in the history of the "income approach" which is accorded to him in the "Genealogy of the 'Income-Approach'" appended to this chapter (p. 343, below). In his review of Tooke's *Inquiry,* for example, in the *Westminster Review,* XLI (1844), 588 f., Mill quoted with approval the substance of Tooke's "thirteenth thesis," although his suggested emendation of Tooke's proposition so as to have it apply to "gross incomes" rather than "net incomes," would indicate that he had something less than an incisive grasp of the central issues involved. There are likewise a few passages in Mill's correspondence which run in terms suggesting the importance of an emphasis upon money *income* for an adequate Theory of Prices. See, for example, *The Letters of John Stuart Mill,* edited by H. S. R. Elliot (1910), I, 191; II,

the one writer who called attention with adequate emphasis to the "income" aspects of Tooke's own "theory of prices" was Wicksell.[30]

Wicksell's own example, moreover, provides proof that recognition of Tooke's emphasis on the rôle of income in the Theory of Prices as "a piece of positive elucidation" which "does really provide a starting-point from which a theory of the value of money and prices can be developed" does not necessarily mean unqualified admiration of Tooke's own use of his central proposition. Wicksell himself, indeed, regarded Tooke's "method of elucidation" as being "unfortunately almost as obscure and in need of elucidation as the phenomenon under discussion." Yet it is typical of Wicksell's treatment of his predecessors that he should have undertaken to defend Tooke's statement of the central proposition itself against the comments upon it by its less intelligent defenders—such as Launhardt—who had discussed it in such terms as to open the way to the suggestion that it represented nothing more than a particularly bad example of "circular reasoning." [31] And, indeed, justice to Tooke demands recognition of the fact that, apart from the statement of the problem which is represented by his "thirteenth thesis," there are details in his treatment which are, in Professor Gregory's phrase, "singularly close" to the details of the argument advanced by contemporary "income-theorists." Tooke was, as we have seen, one of those who protested with great emphasis against the suggestion that "it is not necessary to explain, at any length, in what manner excessive issues of currency tend to raise the general prices of goods." [32] If, in so protesting, he was merely carrying on a tradition which antedated the formulation of an "income-theory" of the kind that is suggested by his "thirteenth thesis," it will at least be granted that the *combination* of the presentation of this "thesis" with the details of mechanism in which Tooke was interested justifies the attribution to Tooke of a place in the history of the "income approach" which it is not easy to accord to earlier writers.

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256 f. None of these passages, however, were cited by Mildschuh. On Mill's supposed use of the concept of "income-velocity," see below, p. 358, n. 30.

[30] See especially Wicksell's *Interest and Prices*, 44 f. It is significant that the few German writers of the pre-war period who ventured to mention, even in passing, the "income" aspect of Tooke's argument, showed an acquaintance with Wicksell's book. See, for example, A. Spiethoff, "Die Quantitätstheorie insbesondere in ihrer Verwertbarkeit als Haussetheorie," *Festgaben für Adolf Wagner* (1905), 252 (cf. the references to Wicksell, 254, 265); and S. P. Altmann, "Zur deutschen Geldlehre des 19. Jahrhunderts," in *Die Entwicklung der deutschen Volkswirtschaftslehre im 19. Jahrhundert, Festgabe für G. Schmoller* (1908), I, vi, 45.

[31] Wicksell, *Interest and Prices*, 45. That Wicksell was on this point defending Tooke's proposition rather than attacking it is obscured by the nature of Cairncross's reference to Wicksell, in this connection. See Cairncross, "The Victorians and Investment," *loc. cit.*, 287, n. 5. On Launhardt's place in the history of the "income approach," see below, pp. 315 and 346, and notes 40 and 4 thereto, respectively.

[32] Cf. above, p. 172, n. 38.

Among these details, for example, one would include the emphasis on "demand" which, when combined with the "thirteenth thesis," is certainly "singularly close" to the emphasis on "demand" in combination with that on changes in "consumers' income and outlay" which is to be found in Hawtrey.[33] Nor is this all. One would certainly have to regard as prophetic of much that has since been argued for under the head of the "income approach," Tooke's emphasis upon the prices of "objects of immediate consumption" as the prices which "may be considered with greater propriety than any other description as general prices," particular in view of the fact that Tooke himself deduced this proposition from his central contention that "the power of purchase by consumers depends upon their incomes."[34] One would have to include also—for all its shortcomings—his acceptance of that proposition of Adam Smith with respect to the division of "the circulation of each country" into "the circulation of the dealers with one another, and the circulation between the dealers and the consumers" to which Mr. Keynes referred as having adumbrated his own distinction between "income deposits" and "business deposits."[35]

It is true that Tooke's emphasis upon the importance of concentrating attention on "expenditure in objects of immediate consumption" led him to arguments with respect to the unimportance of what he called "intermediate transactions" which can be shown to be extremely misleading, if not clearly fallacious.[36] In view, however, of the treatment accorded to "intermediate transactions" by some of the most eminent of contemporary representatives of the "income approach," Tooke's errors in this respect are surely forgivable.[37] It is true also that Tooke was

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[33] See, for example, Tooke's *Inquiry into the Currency Principle*, pp. 71 f. (cf. p. 69 of the same work), and see also the *History of Prices*, IV, 55, 197 f. The treatment of the relation between "Incomes" and "demand" is, if anything, more marked in those parts of the *History of Prices* which owe their final form principally to Newmarch. See, for example, the *History*, VI, 189 ff., 209 ff., 806 ff.

[34] See Tooke, *Inquiry*, 71; and cf. pp. 74, 136 of the same work.

[35] See Tooke, *Inquiry*, Chapter VII (33 ff.; cf. the *History of Prices*, IV, 227 ff.). See also the quotation from Joplin given by Hayek, cited on p. 311, n. 26, above. On the value of the distinction itself, when properly articulated, and its place in the "income-theory" of prices, see pp. 405 ff., below.

[36] See Tooke, *Inquiry*, 74 (also p. 36 of the same work); and cf. the following note.

[37] For examples of the type of treatment to which reference is made in the text, see below, pp. 384 and 555 ff., and the references there given. Tooke's own reasons for minimizing the importance of "intermediate transactions," instead of involving, as do many of the current reasons, one aspect or another of the concept of "velocity," partly derived from too literal an acceptance of a proposition of Adam Smith with respect to the relation between the "value of the goods circulated between the different dealers," on the one hand, and "the value of the goods circulated between the dealers and the consumers," on the other (Tooke, *Inquiry*, 34, 71; cf. Hayek, *Prices and Production*, p. 43), and partly represented a further aspect of his hydra-headed distinction between the circulation of "capital"

guilty of seeming to associate the "income-approach" with the particular irrelevancy that has since been discussed under the head of the "passiveness of prices."[38] As we have seen, however, some of the best known of contemporary defenders of the "income-approach" have been no less guilty in precisely this respect; and it can at least be said of Tooke's formulation that it was free of contamination with other irrelevancies—such as the question of the "nature" of money and the relation of "utility analysis" to changes in the level of money incomes—which have bulked so large in later discussion.[39]

IV

Tooke and Adolf Wagner

One cannot help wondering what course monetary theory might have taken if the influence of Tooke, which in its negative aspects was of course enormous, had been felt also with respect to the "piece of positive elucidation," as Wicksell called it, which was represented by his "thirteenth thesis," and which had to do with the rôle of income in the Theory of Prices.[40] As it happens, one historian of the "income approach" has accorded to Adolf Wagner—the most devoted, as well as the most eminent, of Tooke's Con-

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and that of "currency" (see, for example, Tooke, *Inquiry,* 36, 62; *History of Prices,* IV, 227 ff.).

[38] See, for example, Tooke's *Inquiry,* 73—a passage which is particularly unfortunate because it appears in the midst of Tooke's discussion of the importance of income in the determination of prices—and cf. also the "twelfth thesis," p. 123 f. of the same work. For an example of the association of Tooke's argument respecting the "passiveness" of prices with a characterization of him as an "income theorist," see E. Petersen, *Den moderne kvantitetsteoris gyldighet, etc.,* 4.

[39] On the association of the discussion with respect to the "passiveness" of prices with the "income theory" by later protagonists of the "income approach," see above, p. 310, and notes 22-55 thereto.

[40] Outside of Wicksell, and apart from the passage in Mill's review of Tooke's *Inquiry* to which reference is made on p. 312, n. 29, above, I have been able to find only one instance of a possible influence of Tooke's "thirteenth thesis" upon later writers—namely, the case of W. Launhardt. The latter, to be sure, made no reference to Tooke; but there are certainly striking resemblances between the latter's "thirteenth thesis" and the proposition of Launhardt that *"the total annual production of consumers' goods and the amount of use-goods used up annually* [*die Jahresnutzung aller Nutzungsgüter*] *is purchased by the total annual income,"* the latter being held to consist of *"interest on the money value of all capital* [*den Zinsen aller Geldkapitalien*], *wages, and entrepreneurial profit,"* so that it may be said that "the *level of the prices of all commodities depends strictly upon the annual income of all individuals*" (W. Launhardt, *Das Wesen des Geldes und die Währungsfrage,* 1885, 36, 42; italics Launhardt's). Unhappily, certain of the less fortunate of the negative aspects of Tooke's position are also to be found in Launhardt. Cf. below, p. 346, n. 4.

tinental admirers—a place equal to that of Schumpeter, whose importance in this connection can be denied by no one.[41] Yet if Wagner deserves a place in the history of the "income approach," it is not primarily because there is to be found in his writings an articulate emphasis upon the rôle of income in price determination, even in the degree represented by Tooke's "thirteenth thesis." On the contrary, there is, in all of Wagner's writings, as far as I have been able to discover, no mention whatever of Tooke's "thirteenth thesis," to say nothing of any explicit indication that the thesis in question was to be regarded as being of particular significance. Indeed, it is only in the very latest of Wagner's writings on money that it is possible to find fairly explicit indications that he would have been prepared to assign an important rôle to income in the determination of prices.

Thus, it was not until the publication, in 1909, of Wagner's *Sozialökonomische Theorie des Geldes und Geldwesens* that his distinction between "consumers' money," on the one hand, and "producers' money," or "business money," on the other—the significance of which, for the "income approach," is discussed below—was associated in a reasonably articulate fashion with a distinction between, on the one hand, the "receipts and payments of 'consumers'" (that is, consumers' income and outlay) and, on the other, those receipts and payments which appear "in the circles represented by the world of producers, or the business world," and that it was explicitly stated that "consumers' money consists mainly and in the ordinary course of affairs of the part of the *income* of an economic subject (*Haushaltssubjekt*) which has been received in the form of money or has been converted into money," this income flowing "to this subject regularly as a result of his *position in the economic process of production.*" [42]

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[41] See Mildschuh, "Geld," *loc. cit.*, 724. That this author should have classified both Wagner and Schumpeter as being typical of an approach which does not deserve the appellation "income-theory" as clearly as do those to which he accords this distinction, can hardly be regarded as a misfortune for either writer in view of the criteria which Mildschuh sets up for distinguishing the "true" income-theories from their rivals. Cf. above, p. 305, n. 7, and p. 310, n. 22. Although, moreover, Mildschuh mentions Tooke as a forerunner of the true "income-theory," he paradoxically gives no indication of the relation between Tooke and Wagner with respect to the issues under discussion.

[42] Wagner, *Sozialökonomische Theorie des Geldes und Geldwesens* (*Theoretische Sozialökonomik*, II, 2), 159. Italics here, and in all passages quoted in the following pages from Wagner's *Sozialökonomische Theorie*, are Wagner's own.

In justice to Wagner, however, it must be said that his final formulation was in many respects remarkably prophetic of later writing on the subject. From more than one passage, for example, it is clear that Wagner was fully aware of the importance of that distinction between "consumers' income and outlay," on the one hand, and "traders' turnover," on the other, which occupies so central a place in the analysis of Mr. Hawtrey.[43] "*Money outlays and money receipts*," wrote Wagner, "thus appear in the case of producers' money, as they do in the case of consumers' money. But this *external identity* of the processes involved does not permit us to overlook the *economic difference* between the rôles which producers' and consumers' money play in our problem. Consumers' money is associated with the satisfaction of (private) needs. . . . Once the money is given out for this purpose, it does not return of itself, but must be replaced by other new money-income. Producers' money, on the other hand, is capital and as such may be directed to any desired use, from which it returns of itself to the producer when he sells his product." [44] The similarity between the analysis of Wagner and Hawtrey in this respect extends even to emphasis upon the point that *part* of the "money receipts of the producer which the latter obtains from the sale of his product"—namely, that part which represents "the *income* of the capitalist entrepreneur as capitalist, landowner, and personal labor-factor"—is to be associated with the producer in his capacity "as a private consumer," and is therefore to be associated with "the category of *consumers' money*." [45]

There were other respects, also, in which Wagner's final formulation allowed the rôle of income to appear more clearly than it had in his earlier writings, even though the new formulation may be regarded as making explicit what, in his earlier formulations, had been merely implicit. This is true, for example, of the description, in his latest works, of the process of "diffusion" of "business money" and its transformation into "consumers' money," which was now described in such a way as to bring out more clearly its relation to the formation of "money income." [46] It is true also of his discussion of the relation between

[43] See, for example, Hawtrey, *Currency and Credit*, 1st ed., 41, 47, (cf. 46, 56 f. of the 3d ed.); and *The Art of Central Banking*, 84, 89, 107. It is worth noting that a particularly emphatic statement of the distinction in question had been given as early as 1823 by Thomas Joplin: "It is not all the money that passes through the hands of a merchant or trader . . . which forms his own particular stream of circulation. In general, only a very small part of it does so. That part alone which becomes his, in the shape of profit, belongs to his own current. With the rest, he is only an agent, by which it is advanced forward in the respective streams to which it belongs . . ." (cf. Hayek, *Preise und Produktion*, 47 n.). As far as I am aware, however, there is no evidence that Wagner knew Joplin's work.

[44] Wagner, *Sozialökonomische Theorie des Geldes*, 163.

[45] *Ibid.* See also p. 174 of the same work, on the relation of "business profits" to the "purchasing power of the business world in its character as a consuming public," and cf. Hawtrey, *Currency and Credit*, 1st ed., 41 (3d ed., 46): "The trader, of course, gets his true income out of the profits of his business, and this figures in the total of the consumers' income."

[46] See, for example, *ibid.*, 171.

"larger money-income" and expenditure on consumers' goods and therefore the prices of these goods, as well as of the reciprocal effect of higher "money prices" upon incomes and outlay from incomes.[47]

Yet it may be that Wagner's failure to emphasize explicitly the rôle of income in the Theory of Prices was not an unmixed loss; for if Wagner had been so articulate in this respect as to have struck historians of doctrine as an outstanding representative of the "income approach," it is not impossible that some of these historians, with such models in mind as are typified, say, by Aftalion, would have found Wagner's importance for the "income approach" to lie in such irrelevancies as his acceptance, from Tooke, of arguments against the supposed "passiveness" of prices, or —if the model of Aftalion were strictly followed—in his arguments with respect to the dependence of prices upon fluctuations in foreign-exchange rates and his emphasis upon the rôle of "psychological" factors—such as "confidence"—in price determination.[48] In fact, however, Wagner's importance for the "income approach" does not lie in an association of the "income-approach" with such propositions. An association of this kind could, after all, hardly be attributed to Wagner himself, since, as we have seen, he himself did not stress the rôle of "income" in price determination until very late in his career, and then not with great emphasis, whereas most of the other propositions thus far cited are to be found in his earliest writings.

Wagner's importance, for our present purpose, lies rather in his interest in certain details of analysis the relationship of which to a useful form of the "income-approach" has be-

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[47] *Ibid.*, 173 f., 210.

[48] On the "passiveness" of prices—that is to say, the question whether changes in the quantity of money are to be regarded as the "cause" of changes in prices, or vice versa—see Wagner, *Beiträge zur Lehre von den Banken,* 127 f.; *Die Geld- und Kredittheorie der Peelschen Bankakte* (1862) (p. 17 of the reprint of 1920, edited by Plenge). On the relation of changes in foreign-exchange rates to changes in the domestic price level, see, for example, Wagner's *Die russische Papierwährung* (1868), 100; and on the rôle of "psychological" factors, including the factor of "confidence" (*Vertrauen*), see his *Sozialökonomische Theorie des Geldes,* 117 f., 184. For Aftalion's treatment of the "passiveness" of prices in relation to the "income-theory," see above, p. 310, and n. 24 thereto. For his treatment of "psychological" factors, and for a discussion of the relation of such factors to the "income approach" generally, see below, pp. 492 f.

come clearly apparent only in our own day. One of these details, stressed by one historian of doctrine in support of his characterization of Wagner's analysis as containing at least some elements of the "income-approach"—namely, the division of "goods" into various classes, each with its own "price-level," such as the classes of "consumers' goods" and "producers' goods," respectively—undoubtedly looms large in certain variants of the so-called "income-theory." [49] The emphasis attached to this division in Wagner's writings, however, can hardly be regarded as marked.[50] What does appear is another type of distinction that Wagner employed so persistently, and with a reiterated emphasis so striking, that it is difficult to understand why it was so little discussed by his immediate followers.[51] The distinction in question was nothing more nor less than a further development of the

[49] Cf. Mildschuh, "Geld," *loc. cit.*, 724; and on the concept of a "plurality of price-levels" generally, see below, pp. 496 ff.

[50] In his earlier writings, indeed, what Wagner had to say on the subject of a "plurality of price-levels," insofar as it did not amount to an emphatic statement of the general proposition that under certain conditions we may expect "the greatest inequalities in price-change as between the different parts of a given state" with resulting "great inequalities in the position of the different classes of the population" (so, for example, *Die russische Papierwährung,* 111), had to do with the different price groups which should be distinguished in any attempt to trace the effects upon prices of fluctuations in foreign exchange rates (see, for example, *ibid.,* 100 ff.). It is only in the *Sozialökonomische Theorie des Geldes,* and then not with great emphasis, that we find the distinction between "consumers' money" and "producers' money" associated with the idea that expenditure out of "producers' money" will be directed against "different categories of goods and laborers than is the case with consumers' money," the latter being spent "upon objects representing the needs of private consumption and upon those offering personal services," whereas in the case of "producers' money" expenditure will be directed toward the "concrete objects representing the demand for the real means of production which are required by the branch of production in question, and toward the workers employed in the latter" (Wagner, *op. cit.,* 175; cf. also *ibid.,* 181).

[51] The only instance, indeed, of which I am aware in which a pupil of Wagner's made use of the distinction, is represented by the case of S. Jacobi, *Versuch einer volkswirtschaftlichen Unterscheidung der Bankdepositen* (*Tübinger Staatswissenschaftliche Abhandlungen,* 24. Heft), 1912, 60 ff. Unfortunately, however, Jacobi merely repeated what Wagner had to say on the subject, without in any respect advancing the analysis. Among historians of doctrine, the only one, so far as I am aware, who seems to have thought it worth while to summarize at any length Wagner's distinction with respect to "consumers' money" and "producers' money" is H. Döring, *Die Geldtheorien seit Knapp* (1921), 170 f., 175 ff. Mildschuh ("Geld," *loc. cit.,* 724) mentions the distinction as one to which Wagner attached "great importance," but Mildschuh's own brief comment on the distinction shows that he can have had no genuine appreciation of its significance for the construction of a usable "income" theory of prices.

idea which, as we have seen, Tooke had received from Adam Smith—namely, the conception of the money stock of a country as being divided into two main segments, which Wagner himself, after some vacillation as to terminology, called "consumers' money," or "consumers' purchase money" *(Konsumentengeld, Konsumenteneinkaufgeld)*, on the one hand, and "producers' money," or "entrepreneurs' money," or "business money" *(Produzentengeld, Unternehmergeld, Geschäftsgeld)*, on the other.

A study of the treatment accorded by Wagner, in his successive writings, to the distinction which he took over from Smith and Tooke, is illuminating not only because of the persistence with which Wagner can be shown to have clung to it and the degree of importance which it can be shown to have had in his own eyes, but also because of the light that it throws upon the way in which a suggestion that, in its original form, may be so vague and crude as to be almost unusable, may be subjected to a series of subtle transformations of great importance—transformations, indeed, which, if they do not, even in the end, yield a tool of the desired degree of precision, at least show so great an advance over the concept as it was first received as to suggest that a similar amount of attention to the suggestion in question by later writers would have brought it to the desired point of articulateness and usefulness. Some of the references to the distinction in Wagner's *Beiträge zur Lehre von den Banken* (1857), for example, seem at first to do little more than to echo Tooke's distinction between the circulation of "capital" and the circulation of "currency."[52] In the very same volume, however, the distinction began to take on a sharper outline by virtue of the fact that it was related to a classification of the various "types of *deposit*," the two types being designated as "capital *deposits*" and "money-*deposits*," respectively.[53] A further step in advance was represented, in the same volume, by Wagner's subdivision of "capital deposits" in a way which is remarkably prophetic of some of the most recent efforts in this direction —his classification running in terms of (1) "cash-balances" (*Cassenvorräthe*), (2) "reserve funds," and (3) "Hoards," in a sense derived

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[52] See, for example, Wagner, *Beiträge*, 45: "The payments which banks effect between, say, industrialists, are transfers of capital [*Capitalübertragungen;* cf. Tooke, *Inquiry*, 36]; those between consumers have the character of dealings in money (*Geldgeschäften*)." In a footnote to p. 57 of the same work, moreover, a specific citation was made to the distinction between the "circulation of capital and of currency" which is to be found in Tooke.

[53] Wagner, *Beiträge*, 57. The advance represented by the new designation was of course that it made clear that the distinction of Smith and Tooke was to be understood as being concerned with a division of the total stock of money-spending power into what would now be regarded as two aggregates of cash balances.

from that in which the latter term was used by Fullarton.[54] In Wagner's *Die russische Papierwährung* (1868), the distinction between "capital deposits" and "money deposits" reappeared in the form—hardly a happy one, from the terminological standpoint—of a distinction between "cash-balances for capital purposes" (*Capitalcassen*) and "cash-balances serving the purposes of circulating medium" (*Umlaufsmittelcassen*).[55] What is more to the point, however, is that the distinction was actually used, in more than one instance, to describe the steps—including the transformation of *Capitalcassen* into *Umlaufsmittelcassen*, and vice versa—whereby an increase, for example, in the quantity of money would be expected, in the words of Mr. Keynes, to "work its way into the economic system."[56] Indeed, it was Wagner's articulate insistence upon the importance of his distinction for an understanding of what he himself called the "mechanics" (*Mechanik*) of price change, that would justify our regarding his *Die russische Papierwährung* as representing a marked advance over his earlier treatment of the distinction which derived originally from Smith and Tooke.[57]

For a realization, however, of just how far Wagner had gone beyond the original suggestion of Smith and Tooke, it is necessary to consult the treatment appearing in his *Sozialökonomische Theorie des Geldes und Geldwesens* (1909). Wagner was fully aware that his repeated emphasis upon the distinction between the various types of cash balance had not resulted in its becoming "generally customary" in economic literature.[58] He ventured, therefore, in recognition of the fact that his older terminological usage—based on the distinction between money as "medium of circulation" and money as "disposable capital"—was "less clear" than it should be, "and in other respects, also, inappropriate and easily misleading," to suggest a different terminology, the meaning of which, though also not self-evident, he hoped to make clear by "a simple explanation," and which he felt at least emphasized the "chief criterion for the distinction" he wished to make between the "chief parts of the money-stock."[59] This terminology—"consumers' money," on the one hand, and "producers' money," or "business money," on the other—he

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[54] Wagner, *Beiträge,* 61 ff. On modern instances of a use of the type of distinction involved, see what is said on p. 463, n. 10, below.

[55] See, for example, *Die russische Papierwährung,* 9 ff.

[56] On Keynes, in this connection, see especially pp. 171 f., above, and the references there given.

[57] See, for example, Wagner, *Die russische Papierwährung,* xii (summary of Chapter VII), and p. 99, where it was specifically stated that an understanding of "the way and manner in which paper-money, metallic currency, and bank-notes get into trade [*Verkehr*]" depends on "the distinction between money . . . as medium of circulation and as capital."

[58] Wagner, *Sozialökonomische Theorie des Geldes,* 157. Cf. also p. 172 of the same work, where Wagner complained that the distinction between "consumers' money" and "producers' money" had previously either not been made at all, or had not been made sufficiently often, and that the failure to do so was "the source of many errors."

[59] Wagner, *Sozialökonomische Theorie des Geldes,* 158.

regarded as "new." [60] "New" in effect, also—though Wagner himself did not so characterize it, possibly by way of tacit recognition of the fact that one may find in Tooke not only the germs of the distinction between "consumers' money" and "producers' money," but also the type of argument which was implicit in Tooke's "thirteenth thesis"—was the association of the distinction between "consumers' money" and "producers' money," on the one hand, with the element of "income," on the other; the emphasis upon income, as we have seen, really appearing for the first time in the work of Wagner's with which we are here concerned.[61] Yet there was sufficient of the older material to show the descent of Wagner's analysis from the original suggestions of Smith and Tooke. There was still emphasis on the importance, for an understanding of "*causes* as well as *effects*," of the distinction between "consumers' money" and "producers' money," as a preliminary to a study of their mutual "interaction" (*Wechselwirkung*), which was described in some detail; "producers' money" was subdivided into its various parts, much as "capital deposits" had been subdivided more than fifty years before in Wagner's own *Beiträge zur Lehre von den Banken;* and that association of the movements in the quantity of "producers' money" with the mechanism of the introduction of new money into the economic system which, as we saw, had appeared in *Die russische Papierwährung*, was reproduced and sharpened, particularly by the placing of greater emphasis upon the rôle of the rate of loan interest.[62]

It would be idle to pretend that Wagner's treatment of the range of problems which is summarized by his distinction between "consumers' money" and "producers' money" was without its faults. On the contrary, it suffers, for example, from most of the faults which would be expected from a writer who did not think instinctively in terms of what has come to be called the "cash-balance approach." [63] Yet it would be

[60] The only possible rival, as far as I am aware, to the claim of having originated the terminology in question is J. J. O. Lahn (N. Johannsen). See below, pp. 334 f.

[61] Cf. above, p. 316, and the references there given. On the possible influence of Johannsen on Wagner's exposition on this point, see below, pp. 333 f. It is only fair, however, to point out that the emphasis on "income" had begun to appear also in the writings of others who were familiar with Wagner's teachings, so that it is perfectly possible that the new emphasis in Wagner's own exposition was one to which he had come independently. See, for example, A. Spiethoff, "Die Quantitätstheorie," *loc. cit.*, 257, 259.

[62] The relevant passages will be found in Wagner, *Sozialökonomische Theorie des Geldes,* 172, 169 ff., 176, 181, 189; 166 ff.; and 174 ff., 180 ff., 184 ff., respectively.

[63] These faults are particularly obvious, for example, in Wagner's treatment of both the "supply" of, and the "demand" for, money, and in his acceptance of the practice of drawing a sharp distinction between money "in circulation" and money "out of circulation." See, for example, Wagner, *Sozialökonomische Theorie des Geldes,* 165 ff., 186, 205 ff., 211 f., 215 f. Wagner's treatment of the distinction between "circulating" and "non-circulating" money, and its association with the concept of "hoards," was, of course, directly derived from Fullarton and Tooke. See, for

just as idle to pretend that no significant contribution to monetary theory was represented by Wagner's insistence that for a study of the actual mechanism of price determination, some such distinction as that represented by his own differentiation between the two parts of the money stock would have to be substituted for an undifferentiated "quantity of money."[64] The same thing must be said of his virtually pioneer attempt to trace the actual process of diffusion whereby one type of "money"—that is, one type of cash-balance—becomes transformed into another type, the whole being related both to the question as to the process by which new money may be expected to be introduced into the economic system and to the successive impacts upon the price structure that would be expected to accompany this process of diffusion.[65] Nor can one overlook such details as his implication that, under certain conditions, much significance, for the determination of prices, might attach to the fact that money may be "tied up" in certain forms of "business money."[66] In these respects, as well as in others to which attention will be called at later points in this study, Wagner's analysis, for all its

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example, his *Beiträge,* 44, 62, 70; also his *Russische Papierwährung,* 99. It must be remembered, on the other hand, that "cash-balances," as such, played an important rôle in the very earliest, as well as the latest, of Wagner's writings on money; and it could easily be shown that much of his positive analysis in this respect is not only reconcilable with the methodological implications of the "cash-balance approach," but may be regarded as having called attention to instances in which a conscious application of that approach would show its greatest usefulness. Cf., on this matter, p. 406, below.

[64] See, for example, Wagner *Sozialökonomische Theorie des Geldes,* 190: "The quantity-theory, moreover, is not categorically false. It is merely too one-sided; and its chief fault is that of operating with the *whole* quantity of money, instead of operating *separately* with the quantities represented by the two parts of the money-stock and the changes in *these* quantities."

[65] Cf. the references given on p. 321, n. 55 and p. 322, n. 62, above. In the light of the pioneer nature of Wagner's analysis, it is hardly surprising that in many respects it leaves much to be desired from the standpoint of clarity and precision. Yet, if one is not prepared to agree with the judgment that Wagner's description of the "effects of an increase in the quantity of money" represents "an exposition which goes into the finest details," analyzing "most precisely" (*auf das Genaueste*) the mutual interactions involved (so Döring, *Die Geldtheorien seit Knapp,* 175, 177, n. 2), one would certainly be warranted in regarding as less than fair a lumping of Wagner with authors such as Schmoller and Lexis in whose writings "the stress on concrete mechanism is very weak" (so J. W. Angell, *The Theory of International Prices,* p. 326).

[66] See, for example, Wagner *Sozialökonomische Theorie des Geldes,* 164: " . . . a *certain amount* of money-capital must . . . be held on hand for all purposes, whether it is held by its owners themselves, but split up among the individual producers or business men, or whether it is concentrated in banks. This amount is . . . *tied up* [*gebunden*] and is withdrawn from immediate use as money capital in the production process of the business concerned." The point is again one on which Wagner may have been influenced by Johannsen. See below, p. 334, and especially n. 102 thereto.

faults, is significant not only for the advance which it represents over Tooke's rudimentary use of Smith's distinction between "the circulation of the dealers with one another, and the circulation between the dealers and the consumers," but also for the really remarkable way in which it anticipated so much that is of most enduring value in the similar analysis to be found in later writers such as Hawtrey and Keynes.[67] If, finally, it is true that Wagner himself did not associate his analysis, with all desirable explicitness, with something that was specifically characterized as an "income theory of prices," it is also true that in other respects—for example, in his discussion of the relation of his analysis to the "quantity theory"—he did less disservice to the task of constructing a usable form of the "income-theory" than has been done by those who have insisted that the two are entirely irreconcilable, and who, even if they deserve recognition for having emphasized the importance of income in the Theory of Prices, provided nothing in the way of analytical detail corresponding in importance to Wagner's distinction between "consumers' money" and "business money" which can be used in the construction of a satisfactory "income theory of prices."[68]

V

Tooke and Knut Wicksell

The next figure of importance who developed Tooke's suggestions with respect to the construction of a satisfactory "income-theory"—namely, Knut Wicksell—did not seize upon the distinction between "consumers' money" and "producers' money" which had seemed so important to Wagner.[69] Had Wicksell done so, there can be little doubt

[67] Cf. below, pp. 406 ff., and pp. 554 ff.

[68] On the relation between the "quantity theory" and the "income theory," see below, pp. 346 ff. With respect to Wagner's own understanding of the relation between his specific contributions and the "quantity theory," it may be pointed out that, so far from arguing that these contributions showed "the quantity theory" to be "categorically false," he insisted that the most that could be said against it was that it was "too one-sided"; that a "*complete rejection* of the quantity theory" would mean going too far in the other direction; that what he protested against was merely a "*purely mechanical* conception and explanation" of the processes of price change; and that, in general, his own positive suggestions toward an explanation of these processes were to be regarded as representing an attempt to look behind the formula of *ceteris paribus* and to give a realistic content to the conditions which that formula concealed. See, for example, Wagner, *Sozialökonomische Theorie des Geldes*, 190; 204, 184; 210 ff.

[69] It must be remembered that both of Wicksell's principal works on monetary theory—*Interest and Prices* and the second volume of his *Lectures*—appeared prior to the publication of Wagner's *Sozialökonomische Theorie des Geldes*, in which the most articulate statement of the distinction between "consumers' money" and "producers' money" is to be

that we should have possessed, as early as his *Interest and Prices,* a formulation of the "income theory" which, by combining analysis concerning the rôle of "consumers' balances" and "traders' balances" ("income deposits" and "business deposits") with an emphasis upon the importance of income in the Theory of Prices, would not only have closely resembled the formulation ultimately provided by Hawtrey and Keynes, but would also have established a direct continuity with the fragmentary suggestions with respect to the components of an "income theory of prices" that were to be found in Tooke. There can be little doubt, at any rate, that much of Wicksell's own analysis is to be regarded as being directly descended from that of Tooke. As we have seen, Wicksell himself not only accepted the essential part of Tooke's "thirteenth thesis" as representing "a piece of positive elucidation" that "does really provide a starting point from which a theory of the value of money and price can be developed," but undertook to demonstrate this proposition "later on" in the *Interest and Prices* by the terms of his own positive analysis.[70] What is more striking, how-

found. This fact, in combination with Wicksell's understandable lack of sympathy with the less satisfactory parts of the Banking School position, to which Wagner, particularly in his earlier works, acknowledged allegiance, and the further fact that Tooke's original loose distinction between "money as medium of turnover" and "money as capital" had been grotesquely misapplied by other writers (see, for example, Wicksell's *Lectures,* II, 86) make it easy to understand why Wagner's suggestions do not seem to have appealed to Wicksell. On the other hand, the fact that Wicksell's *Interest and Prices* appeared very late in Wagner's career made it easy to understand why Wagner was not greatly influenced by Wicksell, even if the two writers had not been divided by other fundamental differences. Wagner did, to be sure, refer to Wicksell's *Interest and Prices* in his latest work (see Wagner's *Sozialökonomische Theorie des Geldes,* 112, though both the title and date of publication are inaccurately given). It is difficult, however, to believe that the improvements which are found in Wagner's latest formulation—for example, his more articulate relation of his argument to the element of income, as well as to the rôle of the rate of interest—can be attributed in any significant degree to the influence of Wicksell.

[70] Cf. above, p. 313, and n. 30 thereto. It is perhaps unfortunate that Wicksell himself did not, in presenting the details which appeared "later on" in *Interest and Prices,* make clear just how they were related to Tooke's "thirteenth thesis." It is, indeed, only occasionally in Wicksell's writings that one finds a really articulate statement with respect to the relation between "the sum of money incomes" and "the money value of the consumption goods annually produced." See, for example, the *Lectures,* II, 193.

ever, is that virtually every important element in that part of the argument of *Interest and Prices* which would warrant its being characterized as employing an "income theory of prices" can be shown to have been adumbrated by the details of Tooke's own extremely fragmentary exposition.

Tooke had suggested, it will be recalled, that it was the prices of "objects of immediate consumption" which are to be "considered with greater propriety . . . as general prices" than are prices of "any other description."[71] Wicksell used almost exactly the same words, even if he referred, in support of his contention, to writers other than Tooke: "It seems to me . . . that the ideally correct procedure for observing and measuring the general price-level is to confine the calculation to objects of (direct) consumption."[72] What Wicksell added, in this respect, was a more articulate emphasis, and also a less objectionable—though itself essentially undeveloped—suggestion as to the method for dealing with the troublesome problem of "intermediate transactions."[73] Tooke also, as we have seen, included in his argument an emphasis on "demand" which, when combined with the implications of his "thirteenth thesis," certainly suggested the type of association between "demand" and "consumers' income and outlay" which is to be

[71] Cf. above, p. 314, and n. 34 thereto.

[72] *Interest and Prices,* p. 16. Wicksell's references, in this connection, were to Pareto (p. 16) and to Edgworth (p. xxix). For further instances of Wicksell's concern with the prices of consumers' goods, see *Interest and Prices,* 14 ff., 103, 146, 148 f. The emphasis upon the prices of "objects of (direct) consumption" is much less marked in Wicksell's *Lectures* (see, for example, II, 137 ff.)—a circumstance which may help to explain the failure of the Swedish economists, until very recently, (cf. Myrdal, "Der Gleichgewichtsbegriff, etc.," *loc. cit.,* 416) to emphasize this aspect of Wicksell's analysis, though it is true that a few non-Swedish writers have cited the *Lectures,* either with or without the *Interest and Prices,* in connection with the rôle assigned to the prices of consumers' goods by Wicksell's argument. See, for example, H. Kirchmann, *Studien zur Grenzproduktivitätstheorie des Kapitalzinses* (Greifswald, 1930), 31, and Mills and Walker, *Money,* 91. For further comments on Wicksell's general position with respect to the desirability of working with a "plurality of price-levels," see below, p. 496.

[73] Justice is not done to Wicksell's treatment of "intermediate transactions"—for all its brevity—by implying that he disposed of the whole matter by regarding "the inclusion of wholesale prices as a useless double-counting" (so, for example, H. Kirchmann, *Studien,* 31). Actually, on the same page on which Wicksell discussed "double counting" (*Interest and Prices,* 15), he pointed out that "increased activity in the sale of houses and sites . . . increases the need for money and to this extent may occasion a change in the value of money," and he pointed out further that the same thing would happen "when the same commodity changes hands several times before entering into consumption." As we shall see, also, in our discussion of a "plurality of price-levels," Wicksell was very far indeed from suggesting that no prices other than the prices of consumers' goods were significant for the major problems of monetary theory. Cf. below, pp. 496 f., and especially notes 28 and 29 thereto.

found in Hawtrey.[74] Again Wicksell's reiterated emphasis on the rôle of "demand" in the determination of prices, in combination with his acceptance of the substance of Tooke's "thirteenth thesis," may be regarded as amounting to a vigorous driving home of Tooke's suggestion.[75] Indeed, it would not be unfair to say of these parts of Wicksell's exposition, that they represent essentially a more articulate utilization of the suggestions which Tooke had advanced, these suggestions being, at the same time, freed from some of their more serious irrelevancies—such as Tooke's argument with respect to the "passiveness" of prices—and supplemented by additional elements, such as a treatment of the relation between incomes and "costs of production," and therefore "profits," which may be said to have the merits of Keynes's later treatment in the *Treatise*, without any of the narrow stereotyping of the relation which, as we have seen, did so much to vitiate the Fundamental Equations of the *Treatise*.[76]

VI

The Influence of Wicksell and Wagner

In view of the fact that the fundamental importance of Wicksell's *Interest and Prices* for so many of the deeper-lying issues of monetary theory was for many years recognized by only a comparatively small number of writers on monetary theory, it is hardly surprising that the parts of his work which may be regarded as representing an "income-

[74] Cf. above, p. 314, and n. 33 thereto.

[75] See, for example, *Interest and Prices*, 27, 41, 90, 96, 106, 144, 148, 151, 153, 166; cf. also Wicksell's *Lectures* II, 160, 194 ff.; and his "Hinauf mit den Bankraten!" *loc. cit.*, 752. It is of some importance to observe that Wicksell by no means confined to *consumers'* demand the concept which he himself translated into English as "the moneyed demand." See, on this matter, p. 497, n. 29, below.

[76] On Wicksell's acceptance of the general proposition that "costs of production and money incomes are really only two aspects of the very same thing" without simultaneously accepting the type of assumption with respect to the time element that is involved in the double function attributed to the Fundamental Equations of the *Treatise*, see above, p. 130. Wicksell's own use of the proposition had to do with the sequence of events by which a change in the rate of entrepreneurial spending upon the "factors of production" would be transformed not only into a changed level of incomes earned by those factors, but also into a changed level of costs, which would in turn affect the profitability of borrowing. See, for example, especially pp. 149 ff., 166 of *Interest and Prices*. In the light of these passages, it is difficult to understand the statement of Mr. Keynes, in the *Treatise* (I, 197, n. 3), that Wicksell had failed to "bring out" the fact that a continued price-rise following upon the expansion in the quantity of money which results from a money rate below the "natural rate" may be expected to "involve . . . a continual rise in the rate of money-earnings."

theory of prices" went unrecognized also. The neglect of these aspects of his argument cannot, however, be attributed solely to a lack of familiarity with the *Interest and Prices* itself. This is clearly evident from the fact that the Swedish economists themselves, while they certainly adopted Wicksell's argument with respect to the "natural rate of interest," have come to recognize in only very recent years the importance of what Wicksell had to offer for an "income approach" to the Theory of Prices.[77] When, for example, as late as 1929, Lindahl presented what is certainly to be regarded as a variant of the "income approach"—including not only an insistence upon regarding "the price level of consumers' goods" as the "price-level" which has "most general significance," but also an algebraic "income" equation—he referred to writers such as Schumpeter and Hawtrey, and even Foster and Catchings, but not to Wicksell.[78] It was only later that Lindahl's argument came to be char-

[77] It is worth noting, for example, that Professor Ohlin, in protesting, as late as 1926, against the evidence of neglect of Wicksell's contributions by English writers which was implied by their failure to take note of the similarity between the analysis of Wicksell and that, say, of Hawtrey, referred to Wicksell's rôle in the development of the "modern theory of discount policy," rather than to the attempt by Wicksell to bridge "the gap between price theory and monetary theory" which is represented by his contributions to what might be regarded as an "income theory of prices." (See, for example, Ohlin's obituary notice of Wicksell, *Economic Journal*, XXXVI [1926], 506 f., and contrast p. xiv of Ohlin's Introduction to the English version of *Interest and Prices* [1936]. The latter, it may be observed, was published after the monograph of Myrdal referred to on p. 329, n. 79, below, and is in effect a summary of Myrdal's remarks on the subject with which we are here concerned.)

[78] For Lindahl's characterization of "the price-level of consumers' goods" as the "price-level" which has "most general significance," see p. 13 of Lindahl's *Penningpolitikens mål* ("The Aims of Monetary Policy"). (Although it was completed in 1924, only a few copies of this work were printed in that year, the whole being reprinted in 1929.) For references by Lindahl to Schumpeter, Hawtrey, and Foster and Catchings, as well as for a "consumers' outlay" equation in the form $M_2V_5 = P_4T_4$, in which M_2 represents "the total of cash-balances held by all enterprises and individuals," V_5 "the number of times the quantity of money is used, on the average," by consumers in payment for "goods and services ready for consumption," P_4 the "price level" of such goods and services, and T_4 their total quantity, see Lindahl's *Om förhållandet mellan penningmängd och prisnivå*, 11 f. For an income equation of the form $E(1-s) = PQ$, in which E represents "the total nominal [that is, money-] income"; s, the fraction of this income which is saved"; P, the "price level for consumers' goods"; and Q, "the quantity of this kind of goods," see the same author's *Penningpolitikens medel* ("The Methods of Monetary Policy"), Malmö, 1930, 12.

acterized as "completing" a line of analysis which was to be found already implicit in Wicksell.[79]

With the failure of Wicksell's suggestions regarding the "income-approach" to take hold even in the stronghold of the Wicksellian tradition, the only hope for a continuation of the line of descent from Tooke lay in a possible offshoot from the analysis of Adolf Wagner. There was, indeed, such an offshoot in the writings of that strangely original spirit who, with only a modicum of academic blessing, published his works under the names of J. J. O. Lahn and N. Johannsen.[80] Despite the fact, however, that, by one of the strangest accidents in the development of economic doctrine of which there is record, some of the essential elements of an "income theory" which were to be found in Tooke seem to have been ultimately brought to the notice of Mr. Keynes through Johannsen, it is beyond question that, as far as explicit awareness of origins is concerned, the main line of descent from Tooke came to an end with Wagner and Wicksell.[81]

The remarkable similarity, in many respects, between the analysis of Johannsen and that of Keynes will be pointed out in later parts of this study.[82] Here it is necessary to touch only upon Johannsen's relation to Wagner and the external details of a bit of literary history that has certainly very few parallels.

It would not be true to say that the only recognition of Johannsen's work at the hand of academic economists is represented by the sympathetic introduction that Wagner wrote for Johannsen's *Der Kreislauf*

[79] See especially Myrdal, "Der Gleichgewichtsbegriff, etc.," *loc. cit.* (1933), 378 f. There is no corresponding passage in Myrdal's "Om penningteoretisk jämvikt ("On Equilibrium in Monetary Theory"), *Ekonomisk Tidskrift,* XXXIII (1931), on which Myrdal's monograph of 1933 was very largely based.

[80] The principal works of Johannsen are his *Der Kreislauf des Geldes und Mechanismus des Sozial-lebens,* published in Berlin, 1903, under the name of J. J. O. Lahn; *A Neglected Point in Connection with Crises* (New York, 1908); and *Die Steuer der Zukunft, und ihre Einwirkung auf die geschäftliche Depressionen und volkswirtschaftliche Verhältnisse* (Berlin, 1913), the last two being published under the name of N. Johannsen. How far Wagner influenced other writers, such as Spiethoff (on whom see above, p. 313, n. 30, and also below, p. 337, n. 107), whose analysis was certainly very similar in some respects to that of writers who have been accorded an important place in the history of the "income approach," it is not easy to determine.

[81] Cf. the *Genealogy of the Income Approach to the Theory of Prices* appended to this chapter (p. 343, below).

[82] See especially p. 406, below, and n. 45 thereto.

des Geldes, and the equally sympathetic references made to it in Wagner's own *Sozialökonomische Theorie des Geldes.*[83] For one thing, Johannsen's work was referred to in by no means uncomplimentary terms by a number of historians of doctrine in the field of monetary theory. F. Hoffmann, for example, devoted four pages of his *Kritische Dogmengeschichte der Geldwerttheorien* (1907) to a summary of the argument of *Der Kreislauf des Geldes,* which he not only characterized as "an extremely interesting attempt at a solution of our problem," but ventured also to compare to Wicksell's *Interest and Prices.*[84] Zwiedineck, if he showed no real appreciation of the true relation between Johannsen's analysis and the problem with which his own essay was concerned, at least referred to *Der Kreislauf des Geldes* in sympathetic terms.[85] Again, H. Döring, who was familiar with the writings of both Wagner and Hoffmann, pointed out, in his *Die Geldtheorien seit Knapp* (1921) the similarity between the analysis of Johannsen and that of Wagner.[86] For another thing, those aspects of Johannsen's work which were not directly related to the issues with which we are here concerned were honored by comments—of varying degrees of enthusiasm, to be sure—from some of the most eminent economists of our generation. Spiethoff, for example, commenting upon the treatment of the idea of the "circular flow" (*Kreislauf*) "of capital" in economic literature, which he traced to Quesnay's *Tableau Economique,* not only bracketed Johannsen with Marx as one of the two writers of the "present" who had devoted intensive investigations to the problem, but actually rated the "homely (*schlicht*) exposition" of Johannsen higher than the "predominantly unfruitful" discussion of Marx, on the ground that Johannsen's observations had a much more realistic content.[87] In 1909, J. B.

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[83] Cf. Wagner, *Sozialökonomische Theorie des Geldes,* 112, 170.

[84] Hoffmann, *Kritische Dogmengeschichte,* 275 ff. The point of similarity which Hoffmann attempted to establish between Johannsen and Wicksell (Hoffmann, *op. cit.,* p. 278)—namely, that both, in presenting a theory of the "value of money," had simultaneously presented a theory of the business cycle—was hardly well chosen, if for no other reason than because Wicksell can be clearly shown to have believed that the roots of the cycle lay in what may be characterized as "non-monetary" factors. Yet Wicksell has been so persistently misinterpreted on this point by so many writers that Hoffmann's error on this head is easily forgivable. As far as Johannsen is concerned, moreover, it is noteworthy that, apart from the cases cited in the text, most of the references that have been made to him by subsequent writers have concerned that part of his argument which has to do with cycle theory, rather than those which have to do with the Theory of Prices proper (cf. below, p. 331, n. 89). It is, therefore, not altogether surprising that Hoffmann should have failed to pick out the one element that Wicksell and Johannsen had in common—namely, the place they accorded to income—which is really of most significance for the construction of a satisfactory Theory of Prices.

[85] Zwiedineck, "Die Einkommengestaltung," *loc. cit.,* 156.

[86] Döring, *Die Geldtheorien,* 170, n. 4.

[87] See Spiethoff, "Die Lehre vom Kapital," in *Die Entwicklung der deutschen Volkswirtschaftslehre im neunzehnten Jahrhundert* (*Festschrift für G. Schmoller*), I, iv, 47 f.

Clark reviewed Johannsen's *A Neglected Point in Connection with Crises* in terms which it is impossible to read today without being struck by their resemblance to some of the discussion that followed the publication of Keynes's *Treatise;* and in the same year Johannsen's book was credited by J. A. Hobson with having provided "the first clear exposition" of the "process of investment in trade depressions." [88] Nor have references to Johannsen's work been wanting in more recent works on business cycles.[89] The thing to be said is not that Johannsen's work was entirely ignored by later writers, but that, between Wagner and Keynes, there was no writer of distinction who undertook to show how Johannsen's suggestions might be incorporated into a satisfactory Theory of Prices.[90]

As far as Johannsen's relationship to Wagner is concerned, it must be said at once that it is virtually impossible, from the available evidence, to distinguish what, in Wagner's latest formulation, was due to Johannsen and what, in Johannsen's first book, was due to Wagner. For one thing, there can be little doubt that Wagner was more generous in his acknowledgments to Johannsen than Johannsen was to Wagner. This was, indeed, to be expected: for it is a virtually inevitable consequence of a superficial acquaintance with the literature that those who have only such an acquaintance should both stress the "novelty" of their contribution and be somewhat less than scrupulous in acknowledging their indebtedness to those parts of the literature with which they happen to be acquainted.[91] When, for example, Johannsen accused

[88] For Clark's review, see *The Bankers' Magazine,* LXXVIII (1909), 256 f.; and for the comment by Hobson, see the latter's *The Industrial System* (1909), p. 292 n. Cf. the comments on both by Johannsen in his *Die Steuer der Zukunft,* 217 f.

[89] See, for example, W. C. Mitchell, *Business Cycles* (1913), 18 f. (cf. the same author's *Business Cycles: The Problem and its Setting* [1927], 25 n.); A. H. Hansen, *Business Cycle Theory* (1927), 71 n.; W. Röpke, *Crises and Cycles* (1936), 132 f.

[90] The discussion of Johannsen's work by S. Jacobi (*Versuch einer volkswirtschaftlichen Unterscheidung der Bankdepositen,* 60 ff., 65) can hardly be regarded as having fulfilled this task, in view of the fact that the author concerned contented himself, as in the case of his treatment of Wagner, with reproducing almost literally what Johannsen had to say, without developing it further, or even adding an appreciable degree of articulation to Johannsen's exposition.

[91] See, for example, Johannsen's comments on the relation of his own analysis to "prevailing views," as held by "economists" (*Der Kreislauf des Geldes,* 13, 137 ff., 232 ff.). By the time that he came to write *Die Steuer der Zukunft,* he had become much more emphatic in his arrogant insistence that he was entirely "self-taught" and in his bitterness against the "traditionalist professors"—including Adolf Wagner—who, having learned their economics from "books" instead of from life, as he had, refused to meet his arguments only because they feared the consequences for received doctrine. See *Die Steuer der Zukunft,* 8, 21, 85, 217 ff., 428. For an example of Johannsen's ability to judge the degree to which his own analysis represented an original contribution to the literature, see the violent remarks, 93 n., 119 n. of the work cited, on the blindness of "the economists" in having failed to recognize that banks may create "bank-

economists in general of having, in their attempts to explain movements in the price level, persisted in considering only "the total volume of circulating media in a country," and of having failed to consider the "double nature" of money that is implied in the contrast between an "increase in purchase-money" (*Kaufgeldes*) in the hands of income recipients and an "increase in the quantity of capital-money seeking investment," he was certainly ignoring the distinction between the circulation of "currency" and the circulation of "capital" which, as we have seen, had been kept alive by Wagner ever since it appeared in Tooke in the form which the latter had given to the original distinction in Adam Smith.[92] Indeed, Johannsen was forced to admit as much, by implication, when he ventured to compare his "new" classification of the various types of "money" with a classification which he characterized as "the old, and now generally accepted classification," and which not only turned out to be in all essentials that of Wagner, but was also, as Johannsen himself was perfectly willing to demonstrate, easily reducible to his own classification.[93] One contrasts the grudging spirit, typical of one convinced that he is overthrowing a large part of received doctrine, in which these concessions were made, with the generosity with which Wagner wrote of Johannsen's own "three-fold" classification, despite the fact that, by the terms of Johannsen's own exposition, it was the "*two*-fold nature" (*Doppelnatur*) of money that he himself regarded as the kernel of his argument.[94] One notes further that for four or five years prior to the publication of Johannsen's first book he had been in continual contact with Wagner, who bestowed criticism and commentary upon the increasingly complicated "charts" by which Johannsen laid such great store, and one is forced to conclude that, if Wagner's discussions with Johannsen may have influenced in some degree the formulation to be found in Wagner's *Sozialökonomische Theorie des Geldes*, they certainly seem to have influenced Johannsen in much greater degree

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money" "out of nothing"! This is not to say, of course, that Johannsen did not "discover" for himself ideas which he could have found in any number of the "books" that he affected to despise. It is hardly likely, for example, that Johannsen could have read Schumpeter's *Theorie der wirtschaftlichen Entwicklung;* see, however, his comments on the distinction between the "Unternehmer" and the mere "Geschäftsmann" on p. 452 of *Die Steuer der Zukunft.*

[92] See p. 314, especially n. 35, and p. 320, especially n. 52, above; and cf. *Der Kreislauf des Geldes,* 207, 210 ff., 213 ff.

[93] *Der Kreislauf des Geldes,* 47 f. (cf. *Die Steuer der Zukunft,* 450 f.). It is difficult, also, to believe that Johannsen could have derived the notion of "Geldhorten . . . ; im Englischen 'hoards' genannt" (*ibid.,* 36), as he used the term, from any source other than Wagner.

[94] Cf. Wagner, *Sozialökonomische Theorie des Geldes,* 171. For Johannsen's emphasis upon the "two-fold nature" of money—summarized by the distinction between the area numbered 11 and that numbered 14 on his "chart"—see especially *Der Kreislauf des Geldes,* 210 ff.; and for his division of "the money on hand in the country" into "three classes," see *ibid.,* 33, 209, 228 ff.

than one would be led to suppose from the manner of the latter's exposition.[95]

There can be little doubt, on the other hand, that Wagner was doing no more than justice to Johannsen when he credited the latter with much that was "original" as well as "instructive."[96] Without attempting to disentangle the elements due to Johannsen and those due to Wagner's development of his own earlier suggestions, the following aspects of Johannsen's exposition may be noted as relevant to the problem with which we are here concerned:[97]

(1) There can be little doubt that the element of *income* was given a much more articulate place in Johannsen's exposition than it had been given in Wagner's earlier writings.[98] (2) The distinction was clearly made between that part of the receipts of businessmen which was to be regarded as their "income"—and therefore related to "consumers' money"—and that part which was not.[99] (3) The process of "dif-

[95] For a brief account of the nature and the duration of Johannsen's relations with Wagner, as described by the latter, see Wagner's preface to *Der Kreislauf des Geldes,* iii f. It is illuminating to compare the account given by Johannsen himself, in the Preface, written ten years later, to his *Die Steuer der Zukunft,* 4 ff. The two accounts differ in no essential respect as to the external details. The difference is rather one of tone, Johannsen's Preface bristling with hostility to the man who, by Johannsen's own admission, had shown him more personal kindness and sympathy than had been shown him by any other professional economist (p. 8), but who had had the arrogance—in Johannsen's eyes—to disapprove of the Gesellian *Schwundgeld* scheme which Johannsen regarded as the crown of his labors, and to reserve judgment with respect to his theory as to the cause of depressions (pp. 5, 219). Something will be said concerning the two latter aspects of Johannsen's work in Volume II of this study. It is surely not unreasonable to suspect that the difference in generosity shown by the two writers in their personal relationships extended also to the degree to which each was prepared to acknowledge his indebtedness to the other.

[96] Wagner, *op. cit.,* iv.

[97] No attempt is made here to summarize Johannsen's contributions to the problem of the relation between "savings" and "investment." They will, however, be discussed in Volume II.

[98] See especially the section of *Der Kreislauf des Geldes* (114 ff.) which is entitled "Circulation and Income," and the references given in the index (244 f.), under the entries *Einkommen* and *Einkommen-Linie.* Noteworthy also, in this connection, is his relation of what he called "*Purchase Money* in the hands of the working population" (*Kaufgeld in Händen des Arbeitenden Volkes*) but which was in all essential respects the equivalent of Hawtrey's "consumers' balances," to "Income," and its designation as "Income Money" (cf. Keynes's "Income Deposits"). See the carmine sections of the charts included in both *Der Kreislauf des Geldes* and *A Neglected Point in Connection with Crises,* and the references given in the index to the former, under *Einkommengeld* and *Element 14.*

[99] Thus, in the chart prefixed to *Der Kreislauf des Geldes,* the "profits of traders" and "profits of producers" are made part of "income." See also the index to the work cited, under *Elemente 14 B und 14 C.* The point had of course, been made many years before by Joplin. Cf. above, p. 317, n. 43. On Wagner's treatment of the point in question, see above, p.

fusion" of "business money" throughout the economic system, and particularly its transformation into "consumers' money" and "income" was a central element in Johannsen's exposition.[100] (4) The terms "business money," on the one hand, and "consumers' money" or—as Johannsen called it—"income money," on the other, appeared in Johannsen five years before the publication of Wagner's *Sozialökonomische Theorie des Geldes.*[101] (5) There was a much clearer recognition in Johannsen than can be found in even the latest of Wagner's formulations—though Johannsen's formulation itself often left much to be desired from the standpoint of precision—of the possibility that money may be "tied up" in the financing of "intermediate transactions," and thus be "neutral" with respect to (that is, have no inflationary effect upon) prices and production.[102] (6) The relation between the stream of money income and what Wicksell called the "moneyed demand," in relation to both prices and the level of output, appears much more clearly from Johannsen's exposition than from Wagner's.[103]

317, and n. 45 thereto. It is possible that for the purpose of assigning credit for the making of the distinction, some significance may attach to the fact that, in the chart prefixed to *A Neglected Point in Connection with Crises,* there is nothing corresponding to "Elements 14 B and 14 C." If this second chart was the one to which Wagner referred in his preface to *Der Kreislauf des Geldes* (p. iii), as the simple chart which Johannsen first brought to him, it is barely possible that we have here one of the elements in Johannsen's formulation for which Wagner was responsible.

[100] See Figure I in the chart prefaced to *Der Kreislauf des Geldes,* and the references given in the index thereto under *Kreislauf, Grosser,* and *Rother Ring.* (The "red ring" is that which is reproduced in the chart prefixed to *A Neglected Point in Connection with Crises.*)

[101] See *Der Kreislauf des Geldes,* Index, under the entries *Geschäftsgeld* and *Einkommengeld* (the latter being called also *Kaufgeld* [Purchase Money], or—as in the chart presented in *Die Steuer der Zukunft,* p. 426—"money which is available for consumption purposes"). The fact that Johannsen used the term "business money" before Wagner did is, of course, no proof that it was not suggested to him by Wagner, who, it will be recalled (cf. above, p. 322 and n. 60 thereto), asserted that the terminology in question was "new"—that is, original with him. It may be noted that the terms *Geschäftsgeld* and *Einkommensgeld* have since been introduced into German literature, with some slight differences in connotation, by H. Neisser, in an essay which, by a curious coincidence, bears the same title, "Der Kreislauf des Geldes," as Johannsen's first book. (See the *Weltwirtschaftliches Archiv,* XXXIII [1931], 369 ff.) One concludes, however, from the fact that Neisser refers only to Smith and Keynes, that he was not familiar with Johannsen's work.

[102] See, for example, *Der Kreislauf des Geldes,* 210 n., 214, 228 ff. It is possible that the little that Wagner had to say on this point was derived from Johannsen. The unsatisfactory aspects of Johannsen's exposition are typified by his none too happy statement that the "business money" thus "tied up" was a factor which "exerts no influence of any kind upon the course of prices" (p. 228), and also by his tendency to lump together, as being equally "without effect" upon prices, such disparate elements as "business money" and various forms of "hoards" and "reserves" (*loc. cit.*).

[103] See, for example, the references given in the index to *Der Kreislauf des Geldes,* under *Nachfrage.* The effect of the "moneyed demand" upon

On the other hand, Johannsen's exposition unquestionably suffered from serious faults. There can be little doubt, for example, that Johannsen missed one of the main advantages of the "income-approach" in failing to emphasize that a concern with the "mechanism" of the flow of money-spending power through the economic system should not be allowed to degenerate into an account so "mechanical" as to fail to do justice to the "psychological" motivation behind the acts of the individuals through whose hands the flow of money passes.[104] There can be just as little doubt that at other points Johannsen's analysis was so vague as to be positively misleading—as, for example, in his treatment of the sequence of events whereby money flowing into the capital market would, under circumstances by no means carefully described, lead to a "competition" in the production of goods which would result in a lowering of general prices, with serious consequences for the general business situation.[105] At still other points one could wish for clearer exposition —as, for example, with respect to the relation between "income-money" as the total of consumers' balances ("income-deposits"), on the one hand, and "income-money" as the equivalent of what Hawtrey later called "consumers' income and outlay," on the other.[106] Yet, with all

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output—as opposed to its effect upon prices is, however, made clearer, even if the term "demand" is not used, from the argument on pp. 127 ff., and especially pp. 138 ff., with respect to the relation of monetary factors to "the material prosperity of a people." In its essence, his argument reduced to the proposition that the level of this "material prosperity" would be determined—after making whatever allowances may be necessary for differences in "the methods of production of different peoples"—by the following "three factors": volume of money, its velocity, and the price level. See also Johannsen's remarks on the relation between the "volume of money" and the "volume of trade," 23 ff., 141 ff. It was to be expected, on the other hand, that the emphasis on the consequences of a "lack of demand" should have become much more pronounced in those of Johannsen's works which dealt primarily with the cause of depressions. See, for example, *A Neglected Point in Connection with Crises*, pp. i ff., *et passim*, and *Die Steuer der Zukunft*, 287 ff., his argument being summarized by his proposition that the "A[lpha] and O[mega] of all depressions lies in a lack of demand" (pp. 223, 293), this proposition being related to what would now be called the "income approach" by the further proposition that *"the legitimate source of all demand is income"* (p. 314; printed in capitals by Johannsen).

[104] See the remarks by Wagner, in his preface to *Der Kreislauf des Geldes*, pp. v. ff.; and cf. Wagner's *Sozialökonomische Theorie des Geldes*, p. 184—the latter passage, in particular, providing evidence of Wagner's insight in this respect, even if his own constructive suggestions, lacking, as they did, the support which would have been provided by an articulate recognition of the methodological implications of the "cash-balance approach," were of the vaguest.

[105] Cf. the references given in the index to *Der Kreislauf des Geldes*, under *Konkurrenz*. Much could be said also against his use of the concept "unproductive capital" (see the references given in the index to *Der Kreislauf des Geldes*, under *Kapital, unproduktives;* and cf. the comment in Wagner's preface, p. v).

[106] This fault, which, it may be remarked, appears in the exposition of virtually all the earlier users of Smith's original distinction between the

these faults, and others that could be enumerated, it can hardly be denied that if the later sponsors of an "income-approach" had begun with what was offered by Johannsen's use of the traditional elements that had come down through Wagner from Smith and Tooke, the "income theory of prices" would have assumed the shape taken on by the most usable forms of that theory now available much sooner than it actually did.

VII

The Income Approach after 1909

The actual origins of the particular variants of the "income approach" to the Theory of Prices which are most widely current are to be sought, not in Tooke or in a line of descent from Tooke, as in the case of Wicksell and Wagner, but in suggestions advanced, often independently, by a number of writers on various occasions during the past three decades.

The first of these occasions was signalized by the virtually simultaneous, and apparently independent, publication, in 1909, of F. von Wieser's paper on "The Value of Money and Changes Therein," and O. von Zwiedineck's on "Income-

two "branches" of the "circulation," is undoubtedly to be attributed to their failure to combine, with their emphasis on income, an articulate understanding of the issues suggested by the "cash-balance approach," and hence of what is involved in the concept of "money in circulation." In the case of Johannsen, the difficulty was aggravated by his charts, from a superficial inspection of which it might appear that his "income money" was intended to be identified outright with "income." It is only fair to point out, however, that Johannsen himself, who thought of his charts as representing an "instantaneous picture" (*Momentbild*) of the economic process (*Der Kreislauf des Geldes*, 34), wished the "income" portion of his charts to be interpreted as referring to "income-*money*"—that is, to "income insofar as the latter is, *for the moment*, in the form of money in the possession of the person concerned." (*Ibid.*, pp. 42, 44, etc.; italics mine. Cf. also, in this connection, Wagner's usage with respect to "deposits" and "cash balances" [*Cassen*]; see pp. 320 f., above, and especially notes 53 and 55 thereto.) Johannsen, indeed, although his exposition was greatly impaired by his careless use of the concept of a "stream," pointed out explicitly that, in order to obtain a measure of a given change in the magnitude of total money "income," it would be necessary to multiply the addition to "income money" by a coefficient representing the "velocity of money circulation" (43 f.; cf. also 128, 130). If it is true that he sometimes thought of this velocity as a kind of "virtual velocity" (see, for example, *op. cit.*, p. 129 n.) and at other times as the quotient of the money value of the nation's "annual production" divided by the volume of "business money" (p. 128 n.), it is also true that he cannot be charged with having *confused* "money income" with what he called "income money," the latter being in all essential respects the equivalent of Keynes's "income-deposits."

Formation as the Basis for the Determination of Value of Money." [107] If the first of these papers is to be regarded as of much less importance in the history of the "income approach" than is the second, it is not because there was so great a difference in merit between the two. On the contrary, it is extremely doubtful whether Zwiedineck's faults with respect to emphasis and articulation were any greater than those which must be charged against the exposition of his more distinguished contemporary.[108] Nor is the reason for attributing less importance to Zwiedineck's paper the fact that it had no influence of any kind on later writers. The reason is rather that what influence it had was confined almost entirely to writers who succeeded only in driving the

[107] See F. von Wieser, "Der Geldwert und seine Veränderungen," *loc. cit.*, 497 ff. (*Gesammelte Abhandlungen*, edited by Hayek, 193 ff.); and cf. the reference to Zwiedineck given on p. 305, n. 9, above. It may be remarked in passing that neither of these writers, both of whom seem to have regarded as their principal contribution their attempt to tie up the Theory of Prices with the theory of marginal utility ("subjective value"), did justice to the suggestions in this direction which are to be found in the paper by Spiethoff cited in p. 313, n. 30, above, the references to Spiethoff by Zwiedineck (see, for example, Zwiedineck, "Die Einkommengestaltung," *loc. cit.*, 155 n.) being concerned with other matters. Cf. Spiethoff, "Die Quantitätstheorie," *loc. cit.*, 257, 259, 261, 263. It is noteworthy that Spiethoff himself cited, in this context, the few remarks by Böhm-Bawerk which were later examined at some length by Greidanus, in his discussion of "Income Theories" (Greidanus, *The Value of Money*, 137 ff.), and which are notable not only for their adumbration of what Wieser and Zwiedineck were to say more than twenty years later respecting the effect of valuations, in terms of "utility," of increments to money *income*, but also for the courageous way in which Böhm-Bawerk was prepared to deal with the utility of the *cash-balance* as such—or, as he put it, the "value" which individuals attach to the "price-commodity, money" (*das Preisgut Geld*), as such. See Spiethoff, "Die Quantitatstheorie," 257, n. 2; and cf. Böhm-Bawerk, "Grundzüge der Theorie des wirtschaftlichen Güterwerts," 126 ff., 134 of the London reprint.

[108] See, for example, what is said on p. 309, above, with respect to the emphasis on the relation between the "income approach" and "utility analysis," which is to be found in both Wieser and Zwiedineck, and cf., in addition to the references given in n. 20 thereto, what is said on this matter on pp. 491 ff., below. It is true that the question of the "nature" of money was not so closely associated with the emphasis on income in Wieser's paper is it was in Zwiedineck's—though this has not prevented later writers from discussing the former's views as to the nature of money as if they were strictly germane to the subject. (See, for example, the reference to Mildschuh on p. 309, n. 18, above, and cf. Liefmann, *Geld und Gold*, 78 f., 119.) It is true, also, that Zwiedineck's paper does not contain certain suggestions found in Wieser which, if they had been more clearly articulated and better defended, could be regarded as elements that are usable for a satisfactory form of the "income theory." Cf. what is said on this matter on p. 339, n. 111, below.

"income-approach" still further into the bog of irrelevancies into which it had been driven by Zwiedineck's original paper.[109]

The historical importance of Wieser's paper resides in the fact that, if it suffered in many respects from the same kind of lack of articulation and the same sort of concern with blind alleys as did Zwiedineck's, it also exerted a direct influence upon two writers each of whom, for quite different reasons, merits an important place in the history of the "income-approach."[110] Thus, the "income theory of prices" which appeared in Joseph Schumpeter's *Das Sozialprodukt und die Rechenpfennige* (1917) is important not only because it differed from that presented by other writers influenced by Wieser in its freedom from the mass of irrelevancies with which the "income approach" came to be surrounded, but also because, in presenting a concept of the "efficiency" of money which, from the methodological standpoint, is the virtual equivalent of what came to be later called the "income-velocity" of money, it adumbrated a type of discussion that later occupied a central position in the writings of those who accorded to income a central place in the Theory of Prices.[111] The importance of Aftalion's

[109] Cf., for example, the references to Zwiedineck given by Mildschuh, "Geld," *loc. cit.*, 726, and by Liefmann (cf. p. 308, n. 17, above).

[110] In the light of what has been said in this chapter with respect to the work of Tooke, Wicksell, Wagner, and Johannsen, it is obviously going too far to restate this proposition in the form of a characterization of Wieser as "the founder of the income theory" (so Ellis, *German Monetary Theory*, 180). It is, of course, true that writers other than the two indicated in the text were influenced by Wieser. Liefmann and Mildschuh are examples; and the list might be extended by including not only Aftalion and minor German writers (cf., for example, Ellis, *op. cit.*, p. 180), but also such a writer as B. M. Anderson, who, though he had independently "worked out the line of argument" involved, nevertheless acknowledged the relation to Wieser. (Cf. the references to Anderson on p. 310, n. 23, above.) Yet it can hardly be argued that any of these writers deserve the place in the history of the "income-approach" which, for the reasons indicated in the text, must be accorded to Schumpeter and Aftalion. Nor can it be denied that the characterization of Schumpeter and Aftalion as "income-theorists" who were influenced by Wieser is justified in a degree which could not be said to attach to a similar characterization of such a writer, for example, as L. von Mises (so, for example, Döring, *Die Geldtheorien*, 203, 207, and Hayek, *Prices and Production*, p. 10). See, in this connection, Mises's own comments on both Wieser and Schumpeter in his *Theory of Money and Credit*, 136 and especially 422, 424.

[111] On the concept of "income velocity" and the relation of Schumpeter's "efficiency" thereto, see below, pp. 359 ff. Even on this point

discussion, in 1925, on the other hand, derives principally from the fact that, probably as the result of its specific designation by the author as an "Income Theory" (*Théorie du revenu)* and its use of a particularly simple algebraic formulation with respect to the relation between "income" and "prices," it came to be more widely cited than any other "income theory of prices" which had thus far appeared.[112]

Parallel with the line of descent associated with Wieser, through Schumpeter and Aftalion, is another, which falls again into two virtually independent branches.[113] The first

Schumpeter may be said to have been influenced by Wieser, although the "influence" took the form of a transformation by Schumpeter of Wieser's extremely loose and generally unsatisfactory discussion of both the concept of "velocity" and the significance of "intermediate transactions" into a formulation which is at least worthy of serious discussion. (On Wieser's treatment of "velocity," see my discussion in the *Journal of Political Economy,* XL [1932], 291 f., and in the *Zeitschrift für Nationalökonomie,* IV, 209). For Wieser's treatment of "intermediate transactions," see his "Der Geldwert und seine Veränderungen," *loc. cit.,* 516, 520 (*Gesammelte Abhandlungen,* 214, 219); and cf. Schumpeter, "Das Sozialprodukt," 637. On the other hand, at least two of the elements of Schumpeter's formulation—namely, the selection of the price level of consumers' goods as the really important price level, and the concept of an "equation" relating the "sum of incomes" to the stream of consumers' goods (the "social product")—were expressed with reasonable clarity in Wieser, Schumpeter's contribution consisting of the provision of a greater degree of articulation—including a writing out of Wieser's implied "equation" in algebraic terms—and the placing of much less emphasis on such elements as are represented by Wieser's attempt to relate his argument to "utility analysis." See Wieser, "Der Geldwert und seine Veränderungen," 516, 520 (*Gesammelte Abhandlungen,* 215, 219), and cf. Schumpeter, *op. cit.,* 632, 635.

[112] See Aftalion, "Les expériences monétaires récentes et la théorie du revenu," *loc. cit.,* 813 ff., 819 ff. (cf. the same author's *Monnaie, Prix et Change,* 148 ff., 157 ff.). For examples of citations of Aftalion's "income theory," see Garver and Hansen, *Principles of Economics,* 365 ff. (387 ff. of the 2d ed.); Hornbostel, *La vitesse de la circulation,* 112 ff.; A. Pose, *De la théorie monétaire à la théorie économique* (1930), 94 ff.; E. Petersen, *Den moderne kvantitetsteoris gyldighet,* 42 f.; Greidanus, *The Value of Money,* 130 ff.; and E. M. Bernstein, *Money and the Economic System,* 243 f.

[113] We are discussing here only writers whose exposition can either be regarded as distinctly explicit with respect to the rôle of income in the determination of prices, or can be shown to have led to the presentation of such an exposition by other writers. From this standpoint, it is difficult to accord a significant place in the history of the income approach to such a writer as J. A. Hobson, who, despite the fact that he was cited by B. M. Anderson (*The Value of Money,* 308 n.) along with Wieser as one who had made use of the "distinction between volume of money and volume of money income," did very little more than to define "the quantity of money" as "money actually used for purchases"—that is, as "currency and deposits multiplied by their respective velocity" (Hobson, *Gold, Prices and Wages* [1913], 9, 22 n.). Similarly, it is difficult to agree with the

of these—unquestionably the more important for the future development of the Theory of Prices—is provided by the work of R. G. Hawtrey, who, in his *Good and Bad Trade* (1913), published a chapter entitled "The Relations between Money, Prices and Incomes." The claim of this discussion to powerful originality is not in the least diminished by the fact that its argument, apart from its showing an appreciation of the relation between the "income approach" and the "cash-balance approach" that was certainly superior to anything to be found in its predecessors, really amounted, when judged in the light of Hawtrey's later elaboration of its details, to a combination of all that was best in the two lines of descent from Tooke represented by Wicksell and Wagner.[114] That Hawtrey's first formulation, which contains *in nuce* the substance of the argument that he was later to drive home by the process of continued reiteration in successive publications, did not immediately receive the sympathetic attention that was its due is probably at-

characterization as holders of an "income theory" of such writers as Böhm-Bawerk and G. M. Verrijn Stuart. (So Greidanus, *The Value of Money,* 137 ff., 123 ff. For an appreciation of the amount of emphasis actually accorded by Verrijn Stuart to the rôle of income in the Theory of Prices, see the latter's comments on Zwiedineck (pp. 31 n., 34, n. 3), on Wieser (p. 39, n. 1), and especially on Schumpeter (p. 75) in Verrijn Stuart's *Inleiding tot de leer der Waardevastheid van het Geld* [1919].) Nor is it easy, finally, to see what there is in the exposition of P. W. Martin, in his *The Problem of Maintaining Purchasing Power* (1931), which would justify the suggestion that his "analysis of factors affecting the price level" bears such "resemblance to that of Aftalion," as summarized in the latter's "income equation," as to warrant the important place among income theorists accorded to Martin by Bernstein, *Money and the Economic System,* 244 ff.

[114] See Hawtrey, *Good and Bad Trade,* 6 ff. The emphasis on "demand" (Wicksell's "moneyed demand") as represented by the "aggregate of all money incomes" (see p. 6 of the work cited), which, as we have seen, was to be found in both Tooke and Wicksell (cf. above, p. 314, n. 33, and p. 327, n. 75) is, in *Good and Bad Trade,* much more clearly marked than the use of the distinctions between "consumers' income and outlay" and "traders' turnover," on the one hand, and "consumers' balances" and "traders' balances," on the other. These distinctions, as we have seen, were the elements in Tooke's presentation which Wagner, and through him, Johannsen, made peculiarly their own (cf. above, pp. 314, n. 35, 321 f., 332, and 334). They were, to be sure, *implied* in Hawtrey's earliest formulation—as when, for example, he distinguished between "the position of a man *qua* consumer and investor" and "his position *qua* producer" (*Good and Bad Trade,* 12 f.); its clearer statement, however, was left to *Currency and Credit.* Cf. the references to the latter work given on p. 317, n. 43, above. All evidence would indicate, of course, that Hawtrey developed his own argument in complete independence of Wagner and Wicksell.

tributable only in part to its appearance on the eve of the Great War, with the disturbance of academic life which the latter occasioned. Even in its second formulation, Hawtrey's exposition was criticized for its lack of "articulation" by a reviewer of unquestioned competence.[115] If, as one rereads the third chapter, say, of *Currency and Credit* (1920), one feels that the criticism was not entirely without justification, it is also true that one wonders how much of the difficulty was attributable to Mr. Hawtrey's exposition —including his failure to provide an algebraic statement which would bring out more clearly the relation of his argument to that which was summarized by the older "quantity equations"—and how much to the fact that most readers, unfamiliar with the historical precedents lying behind the various components of his analysis, could see in the work, as so often happens, only what they already knew, plus a small "dose of novelty" the precise nature of which it was difficult to define.[116]

The other branch of inquiry, developed independently from the argument of Hawtrey, had to do with the concept of "income velocity." The use, by Messrs. Foster and Catchings, of the concept of "circuit velocity" in a sense which, for all its ambiguity of context, certainly resembles the concept of "income velocity" as used by later writers, meant that, sooner or later, Foster and Catchings would have been referred to in discussions of the "income approach."[117] Indeed, it was not long before the concept of

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[115] A. A. Young, in the *Quarterly Journal of Economics,* XXXIV (1920), 521.

[116] The reference with respect to the "dose of novelty" is to Pantaleoni. See my article, "The Monetary Aspects of the Walrasian System," *Journal of Political Economy,* XLIII (1935), 145 and the reference there given. For Hawtrey's algebraic statement of his argument, see *Currency and Credit,* 53 ff., 1st ed. (60 ff., 3d ed.; cf. also *The Art of Central Banking,* 110 ff., 350 ff.). It is perhaps not without significance that whereas Aftalion's simple "income equation" began to be reproduced by other writers very shortly after its publication, Hawtrey's algebraic formulation, although it was first published in 1919, does not seem to have been reproduced by other writers prior to 1933. See T. E. Gregory, "Money," *Encyclopedia of the Social Sciences,* X (1933), 610; and cf. also Bernstein, *Money and the Economic System* (1935), 250 f., and R. B. Westerfield, *Money, Credit, and Banking* (1938), 424 f.

[117] See Foster and Catchings, "The Circuit Flow of Money," *American Economic Review,* XII (1922); *Money,* 174, 301, 306 f., 311 ff., of the 2d

"circuit velocity" was raised to a level of considerable respectability as the result of its explicit recognition at the hands of Mr. Hawtrey and other writers.[118] In the meantime, however, the concept of "income velocity" had been introduced, with considerably more explicitness, by Robertson and Pigou; and there can be little doubt that the example set by these writers exerted an important influence on the subsequent discussion of the concept.[119]

It must at once be obvious from our account of the historical development of what has come to be called the "income-approach" to the Theory of Prices that the propositions which have been advanced under cover of this "approach" are so heterogeneous in their nature that any attempt to deal with them as a unit would involve a departure from the plan underlying this study, which has proceeded from an examination of the general validity and usefulness of "Quantity Equations" as such to an examination of the analysis lying behind each of the terms in one variant of the better

(1924) ed. (first published in 1923); *Profits* (1925), 252 ff.; also H. B. Hastings, "The Circuit Velocity of Money," *American Economic Review,* XIII (1923).

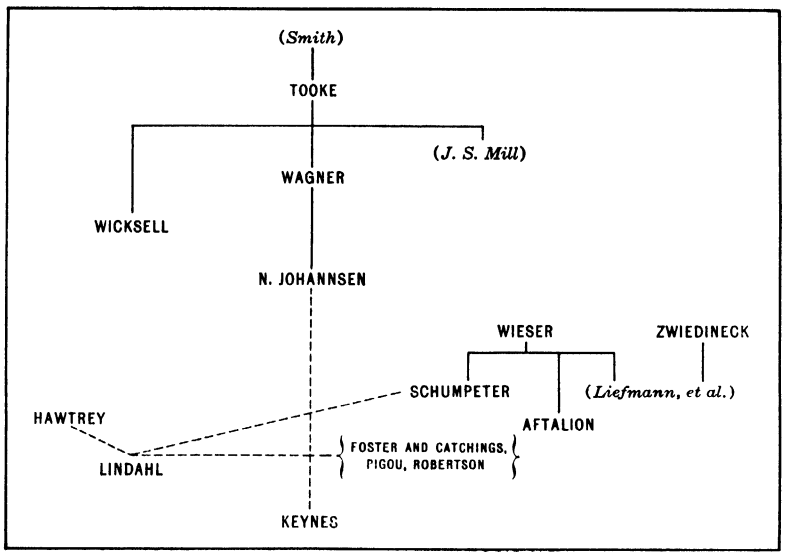
[118] Cf. Hawtrey, "Mr. Robertson on Banking Policy," *Economic Journal,* XXXVI (1926), 422, where Foster and Catchings's "circuit velocity" was related to Robertson's use of a variant of "the Marshallian *K*" (on which see below, pp. 415 ff.), and Hawtrey's *Currency and Credit,* 3d ed. (1928), 59, where "circuit velocity" was compared with Pigou's "income-velocity" (cf. also pp. 253, 257, 456 of the same work). See, in addition, Lindahl, *Om förhållandet mellan penningmängd och prisnivå,* 11 f., where Foster and Catchings's "circuit velocity" was compared with Schumpeter's "efficiency"; Burns, "The Quantity Theory and Price Stabilization," *loc. cit,* 574 n., where Foster and Catchings were mentioned in connection with Schumpeter's "efficiency," which was called "circuit velocity"; Keynes's *Treatise,* II, 25, where Foster and Catchings' "circuit velocity" was mentioned, with some uncertainty as to the precise nature of its relation to the concepts of Schumpeter and Pigou; Neisser, "Der Kreislauf des Geldes," *loc. cit.,* 385, n. 3, where Foster and Catchings were cited, although with reservations as to the quality of their analysis and the nature of the consequences they adduced from the concept, as having provided the precedent for the use of the expression "circuit velocity" (*Kreislaufsgeschwindigkeit*), in the sense of Schumpeter's "efficiency"; J. W. Angell, "Money, Prices, and Production: Some Fundamental Concepts," *Quarterly Journal of Economics,* XLVIII (1933), 43, n. 5, where both Foster and Catchings and D. H. Robertson were cited. For further examples of a use, by later writers, of the expression "circuit velocity," in a sense which would make it the virtual equivalent of "income-velocity" as used by writers other than Keynes, see below, p. 359, n. 33.

[119] See Pigou, *Industrial Fluctuations,* 152 f., and cf. Robertson, *Banking Policy and the Price-Level,* 47 n.; *Money,* 2d ed. (1928), 36, 40. See, more recently, Pigou, *The Theory of Unemployment,* 194 ff.

known types of "Quantity Equations." [120] It is proposed, therefore, to bring the different aspects of the argument that has grown up about the "income approach" into the discussion of each of these terms whenever any one of these aspects can be shown to be relevant to such discussion. Our topic in the chapters that follow is the concept of "velocity"; our first problem in these chapters, therefore, is to examine those aspects of the "income-approach" which bear upon the concept of "velocity" and the analysis which the latter concept was designated to summarize.

[120] An incidental advantage of this procedure is that it can be used to show the extent to which, and the precise way in which, different "approaches," instead of being true alternatives, may be said to supplement each other. See, for example, what is said on the relation between the "income approach" and the "cash-balance approach," on pp. 372 ff., below.

GENEALOGY OF THE "INCOME APPROACH"



CHAPTER THIRTEEN

The "Income Approach" and "Velocity": Income Velocity

I

Income, "Velocity," and the Quantity of Money

THE first of the problems involving the concept of "velocity" which is raised by a consideration of the "income-approach" to the Theory of Prices is associated with the suggestion that the use of this approach makes it possible to avoid altogether the very concept of "velocity." That this suggestion is implied in certain variants of the "income approach" would seem to follow, in the first place, from the fact that, in the exposition of some supporters of that approach, no mention whatever was made of "velocity." It would seem to follow also from the fact that in what is perhaps the best known of "equations of exchange" of the "income-type"—namely, that of Aftalion—no term for "velocity" is included. Most striking in this connection, however, is the fact that certain protagonists of the "income approach" have uttered sharp comments with respect to both the validity and the usefulness of the concept of "velocity," one of the mildest of these comments being that, for users of the "income approach," the concept itself is to be regarded as an element of "superfetation." [1]

The particular phase of the argument for the "income-approach" which is here involved is one which, characteristically, covers a much wider area of controversy than that which would be involved if supporters of the "income approach," instead of using that approach as a club with which to settle almost every controversial question of importance that has ever arisen in the literature of monetary theory, had confined it to a set of propositions clearly differentiated

[1] So Aftalion, *Monnaie, Prix et Change,* 199.

from others which cannot, except at the cost of a tortuous use of language and logic, be regarded as being constructed about the concept of "income." Concretely, the issue is that which is alleged to be involved in the difference between an emphasis upon a *stock* of money and an emphasis upon a *stream* of money payments—the argument being that it is the latter which is really crucial for any study of the pricing process.[2]

So stated, however, the issue is seen to be one that is not only of the greatest antiquity, but one the discussion of which has been shown to lead, not to a minimization of the importance of the concept of "velocity," but to an increased emphasis upon its importance. For it is a matter of established history in the development of economic doctrine that the emphasis on the *stock* of money as the important factor in the determination of general prices—as that emphasis was to be found in, say, Davanzati—was forced to yield to the concept of a *stream* of money precisely *as the result of* the introduction of the concept of velocity—as in the case, for example, of Locke.[3] It was, that is to say, by the very

[2] Ellis, *German Monetary Theory,* 176, actually characterizes the proposition that the "quanta" involved in the Theory of Prices "are flows and not stocks," as one of the "fundamental ideas" underlying the "income theory." From the discussion which follows, it is obvious that this "fundamental idea" has for almost two centuries been part of arguments that otherwise bear no resemblance whatever to "income-theories." For the latter, however greatly they may differ otherwise, agree that it is one particular "segment" of the stream of money payments—namely, *income* payments—that is essential for the Theory of Prices. It cannot be denied, on the other hand, that many supporters of the "income-theory" have implied that the emphasis on the *flow* of money outlay was in some peculiar sense a contribution of, or a feature peculiar to, the "income-theory." Such an implication is certainly involved, for example, in the argument of those who have insisted upon contrasting an emphasis on "income" with an emphasis on the "quantity of money." See below, pp. 346 ff., and the references there given.

[3] For a discussion of some of the broader issues involved in the matter under discussion, see my article, "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " *Journal of Political Economy,* XL (1932), 483 f.; and cf. also my article "The Definition of the Concept of a 'Velocity of Circulation of Goods,' " *Economica,* November, 1932, 441-443. Of the references given in the latter article, those to J. A. Hobson ("The Definition, etc.," *loc. cit.,* notes 25 and 26) are of particular interest for our present purpose, in view of the fact that it is precisely these aspects of Hobson's argument which led B. M. Anderson to bracket Hobson with Wieser as having made use of the distinction between the "*volume of money* and the *volume of money income*" (cf. above, p. 339, n. 113).

introduction of the concept of "velocity" that it became possible at once to *relate* the stock of money to the stream of money payments and to avoid the patently unfounded suggestion that price changes may be expected to vary in strict proportionality to the changes in the money stock. To set up, therefore, as some writers have done, an antithesis between the "quantity theory," in its crudest form, and the "income approach," and then to proceed to berate the sponsors of the concept of velocity for insisting upon using an analytical device which at the worst is meaningless or misleading and at best is an element of "superfetation," is to show a strange degree of insensitiveness to the historical development of our understanding of the reasons *why* a strict "proportionality" between price change and changes in the money stock is not to be expected.

The setting up of an antithesis between the "quantity-*theory*"—understood as holding that price changes may be expected to be strictly proportional to changes in the money *stock*—and the "income theory," is unfortunately to be regarded as having been implied in the argument of Tooke, who contrasted an emphasis on the amount of "revenues . . . of the different orders of the state under the head of rents, profits, salaries and wages, destined for current expenditure" with an emphasis on the "total quantity of money in circulation" as the factor determining the extent of the monetary "demand."[4] The fact that, since the days of Tooke, the "quantity theory" has come to have anything but an unequivocal set of connotations with respect to the degree of "proportionality" it assumes as between changes in the "quantity of money" and changes in prices, can hardly be said to have lent additional clarity to the antithesis between the "quantity theory" and the "income theory." In many cases, indeed, the "contrast" between the "quantity theory" and the "income theory" was concerned only remotely with such issues as the degree of "proportionality" that may be expected to

[4] Cf. Tooke, *Inquiry into the Currency Principle*, 71. In this respect, also, Tooke may have exerted an unfortunate influence upon Launhardt, who insisted that to say that "the level of prices stands in a continuous relation of mutual inter-action (*in steliger Wechselbeziehung*) to the income of all economic groups of human society" was not to say that these prices were *"in any way influenced by the quantity of money in circulation,"* since *"the level of annual income . . . is in no way, or only to an insignificant degree dependent upon the quantity of money"* (Launhardt, *Wesen des Geldes*, 46, 33; italics Launhardt's). On the other hand, it would be clearly unfair to blame Tooke for the argument of Launhardt as to what *did* determine the "level of annual income," with its strange analysis as to the effect of the rate of interest upon this "level." See, on this matter, Launhardt, *op. cit.*, 51 f.

prevail as between changes in the "quantity of money" and "prices."[5] In other instances, on the other hand, the lack of proportionality between changes in the "quantity of money" and changes in "prices" has been advanced as one of the principal reasons for regarding the "income theory" as superior to the "quantity theory."[6] In view of the fact that it is precisely the writers who have made themselves guilty of this type of argument who have regarded the factor of velocity as an element of "superfetation," it was hardly fair to add insult to injury by suggesting that the "quantity theory" involves a "confusion" between "*volume of money* and volume of *money income*," in a context which would suggest that the critic meant to call attention to no more novel a distinction than that between the stock of money and the stream of money payments![7]

It has, to be sure, been argued that the function of the concept of "velocity," as described above, is a purely factitious one, in the sense that the problem which it helps to solve—namely, the problem as to the relation between the stock of money and the stream of money payments—is one that should never have been raised. Given—so the argument runs—an initial emphasis on the money stock as an important factor in the determination of prices, it of course follows that some such device as that which is represented by the concept of "velocity" is necessary in order to bring the initial emphasis into some degree of conformity with the observed facts. The real question, however, it is argued, is whether the emphasis on the money stock was a correct emphasis in the first place. It is contended by defenders of this particular aspect of the "income-approach" that it was not; and that the introduction of the concept of "velocity" is merely a gesture of piety toward received doctrine which becomes entirely unnecessary as soon as we start on a clean sheet with the concept of a *stream* itself, such as is represented by the stream of money income. The trouble, that is to say, is alleged to be that we have not been able to emancipate ourselves from the "tyranny which the

[5] This is true, for example, of Wieser's discussion of the "quantity theory." See his "Der Geldwert und seine Veränderungen," *loc. cit.*, 514 ff.; and cf. Ellis, *German Monetary Theory*, 176 ff., especially 180, 183.

[6] For a particularly explicit argument to this effect, see Aftalion, *Monnaie, Prix et Change*, 170 ff.; and cf. also the same writer's "Die Einkommenstheorie des Geldes," *loc. cit.*, 386, paragraph 2.

[7] See, for example, Anderson, *The Value of Money*, 307 f. The "identification" of a "change in the stock of money with a change in the flow of incomes" is attributed to Fisher also by Joan Robinson, in the *Economic Journal*, XLVI (1936), 298.

concept 'quantity of money' has until recently exercised in monetary theory."[8] Having, in Mr. Keynes's own words, been "brought up in the old Quantity-of-Money, Velocity of Circulation schools of thought," we are alleged to justify the second of these concepts on the ground that it is necessary in order to salvage the first, forgetting that what is really required is an abandonment of both in favor of the fundamental concept of a stream of money payments, such as is involved in the "income-approach" in all its forms.[9]

It has, however, been easy for defenders of the older apparatus to show the weakness of this argument, even when one starts from the proposition—which cannot seriously be questioned—that what is really important, for the process of price determination, is the *stream* of money payments. For it must be obvious, upon very slight reflection, that it is part of the task of monetary theory to explain *why the stream of money payments is larger at one time than another.* For this purpose, it is contended—and it is significant that the point is admitted, though with varying degrees of grace, by some of the best known defenders of the "income approach"—that it is impossible to ignore the

[8] So B. Ohlin, in his Introduction to Wicksell's *Interest and Prices,* p. xiv (cf. the same author's "Some Notes on the Stockholm Theory of Savings and Investment," I, *loc. cit.,* 56). From the context, Professor Ohlin would seem to be objecting particularly to the emphasis on the quantity of *standard* money. For the reasons, however, for believing that Wicksell himself would not have accepted the statement, even when so limited, as a fair description of the drift of his argument, see above, p. 221, n. 43. On Wicksell's treatment of the quantity of *bank money* as a factor affecting prices, see above, pp. 183 ff.; and on his understanding of the relation between the quantity of money and the level of money incomes generally, see below, p. 349, n. 12.

[9] For the reference to Keynes, see above, p. 3, n. 3, and p. 15, n. 20. In fact, of course, Mr. Keynes's own practice—as opposed to his preachment—was, on this point as on so many others, in no essential respect different from that involved in the most nearly satisfactory treatments then available of the relation between the "quantity of money" and the level of money incomes, including the use of a "velocity" factor, in some form, as a bridge between the two. Cf. below, pp. 405 ff. For an explicit example of the suggestion that the concept of "velocity" has been introduced solely in order to "save the quantity theory," see Aftalion, *Monnaie, Prix et Change,* 199, and "Die Einkommenstheorie des Geldes," *loc. cit.,* 384. Cf. also Wieser, *Social Economics,* 265, where the use of the factor "rapidity of circulation" is listed as one of the "changes . . . , more or less forced," which were introduced in order to save an emphasis on the concept of a "stock" of money, and where this type of approach, which was characterized as the "prevailing" one, was contrasted with an approach that would assemble all the "elements" thus indicated "in the one concept of money income."

effect upon the absolute magnitude of the stream of money payments which is exerted by changes in the stock of money.

It is, indeed, of some importance to establish the fact that those figures in the history of the "income approach" to whom we owe most in the way of constructive suggestion, were perfectly clear in their own minds that emphasis upon "income" as a factor determining prices did not mean a lack of concern with changes in the quantity of money, or with the problem of tracing the steps by which changes in this quantity of money would be translated into changes in the level of money incomes. Tooke, to be sure, set a bad example in this respect.[10] Yet it is a tribute to the independence of those who had insight enough to appreciate the value of those aspects of Tooke's analysis which may be regarded as having adumbrated the "income approach," that they did not think it necessary to follow him in adopting the type of antithesis involved in his contrast between emphasis on the "total quantity of money" and the amount of "revenues . . . of the different orders of the state destined for current expenditure." It is certainly true, for example, despite absurd misrepresentations of Wicksell's position in this respect by later writers, that he did not regard his own analysis, including its emphasis upon income, as being in conflict with the "quantity theory," if by the latter is meant an emphasis on the effect of changes in the quantity of money upon prices.[11] The direct contrary is evidenced by the fact that his argument in this respect has been characterized, and with justice, as an attempt to "perfect" the "quantity theory" precisely by including, in his description of the process through which a change in the "quantity of money" becomes translated into a stream of income payments, a sympathetic and in itself extremely valuable analysis of precisely that element of "velocity" which seemed to later "income-theorists" only an element of "superfetation."[12] Wagner, also, as we have seen, was explicit in insisting that his emphasis on the necessity for studying the mechanism of the diffusion of new money through the economic system, including its successive sojourns in the

[10] Cf. above, p. 346.

[11] On Wicksell as an "opponent" of the "quantity theory," cf. what is said on p. 24, above.

[12] The aspects of Wicksell's treatment of the factor of "velocity" which are here in question (see especially *Interest and Prices,* 51-62, and the *Lectures,* 59-67), and which have rightly been described as "basic" (*grundlegend;* cf. H. Neisser, *Der Tauschwert des Geldes,* 15 n.) have nothing to do with his concept of "virtual velocity" (on which see below, pp. 366 ff.). For a characterization of Wicksell's *Interest and Prices* as an attempt to "perfect" the "quantity theory" by "taking account of the velocity of circulation of money," see E. von Philippovich, *Grundriss der politischen Oekonomie,* I, p. 311. See also Spiethoff ("Die Quantitätstheorie," *loc. cit.,* 254 f.) on Wicksell as having, in his *Interest and Prices,* "traced the effects [of an increase in the quantity of money] chiefly through [its influence upon the size of] cash-balances, and the anticipatory or delayed purchase and sale of commodities which results from a change in the size of these cash-balances."

sectors that he labelled "producers' money" and "consumers' money," respectively, did not mean a rejection of that element in the "quantity theory" which insisted upon the importance of changes in the quantity of money for the determination of prices; and, while Wagner's own discussion of the factor of "velocity" did not have the sharpness of outline which would have been given by an adequate appreciation of the implications of the "cash-balance approach," it is nevertheless not without significance that he left an explicit place in his analysis of the forces determining prices for the element of "velocity," instead of characterizing it as superfluous or ignoring it altogether.[13]

It is a fortunate aspect of much of the discussion that has grown up about the "income approach" in its more recent manifestations, that both its ablest protagonists—such as Schumpeter and Hawtrey—and many of those who have commented upon its implications, instead of following the practice of contrasting an emphasis on the importance of changes in the quantity of money with an emphasis on changes in the level of money incomes, have given explicit support to the implicit example of Wicksell and Wagner by insisting that one type of emphasis is not inconsistent with, but is rather a necessary supplement to the other.[14] In some cases, to be sure, an unguarded statement, or a failure to state their position on this matter with all desirable explicitness, by writers the details of whose analysis shows that they cannot have meant to side with the income-theory "opponents" of the "quan-

[13] See, for example, Wagner's *Sozialökonomische Theorie des Geldes*, 213, where "velocity" was discussed as one of the elements concealed in the assumption of "ceteris paribus" which it was necesary to analyze more closely in order to correct the rigid quantity theory and make it more usable. On Wagner's attitude toward the "quantity theory" generally, see above, p. 324, n. 68, and on his relation to the "cash-balance approach," see above, p. 322, n. 63.

[14] See, for example, Schumpeter, "Das Sozialprodukt," *loc. cit.*, 675, where, having in mind the "many authors" who "seem to see a great discovery in the substitution of the sum of incomes [*Einkommensumme*] for the quantity of money" (cf. p. 654), he pointed to the problem as to "the quantitative relationship between the quantity of money and the sum of incomes" as showing "what little novelty one expresses, when one puts the latter in place of the former," and added sharply: "Anyone who, in dealing with the problem of the value of money, puts the element 'sum of incomes' in place of the element 'quantity of money' as a rule is merely, in all innocence, expressing in other words the basic thought of the quantity theory, which he believes that he is thereby superseding" (p. 675; cf. also *op. cit.*, 667). For the position of Hawtrey, see the following note. On the relation between a change in the quantity of money and a change in the level of money incomes, see also Spiethoff, "Die Quantitätstheorie," *loc. cit.*, 257. For instances of an insistence, similar to that of Schumpeter, on the mutually complementary relation of the "quantity theory" to the "income theory," see Neisser, *Der Tauschwert des Geldes*, 12 f. (cf. also the same author's "Die Kreislauf des Geldes," *loc. cit.*, 408); Garver and Hansen, *Principles of Economics*, 366 f. (2d ed., 338); Gregory, Introduction to Tooke and Newmarch, *History of Prices*, 22, 84, 88; A. Nielsen, *Bankpolitik*, II, 137 ff.; G. Haberler, *The Theory of International Trade* (1936), 28, n. 7.

tity theory," has left the door open for continued misunderstanding.[15] Yet against this fact is to be set another, which is much more striking: namely, that, as often as not, precisely those protagonists of a variant of the "income approach" whose argument has been regarded as representing an instance of "opposition to the quantity theory" have found it necessary, whenever what was involved was the question of the factors determining the level of money incomes, to introduce both that element of the "quantity theory" which is indispensable for the purpose in hand (namely, its emphasis upon the importance of changes in the "quantity of money" for the Theory of Prices) and the factor of "velocity" as an element which helps to explain why a given stock of money may serve as the basis for a stream of money payments of a given magnitude.[16] We have here, in other words, another example of the type

[15] The discussion of the "quantity theory," for example, by Hawtrey, in his *Currency and Credit* (cf. the Index under "quantity theory") left at least one commentator in "considerable doubt as to just what he [Hawtrey] really conceives the relationship between money, credit, and prices [one would add: "and incomes"] to be" (so Angell, *The Theory of International Prices,* 185 f.). Even if one cannot accept such a conclusion as fair, it is difficult to regard as other than likely to mislead readers concerning Hawtrey's real meaning certain statements which appear in some of his later writings: for example, the statement that because "what governs demand in any community is the consumers' income . . . and consumers' outlay," it is "not really necessary to introduce the quantity of currency into the analysis at all" (*The Art of Central Banking,* 145). Cf. also Hawtrey's comment in *Economica,* II, N. S. [1935], 464, that, since "credit regulation acts as directly upon the consumers' income itself as upon the effective circulation [MV]," the "latter can be left out of consideration"; and see the similar statement in Hawtrey's *Trade Depression and the Way Out,* p. 7 of the "new" [1933] edition. Actually, of course, there has been nothing in Mr. Hawtrey's writings subsequent to the first brilliant sketch presented in *Good and Bad Trade* which would lead one to suppose that he had abandoned the proposition, advanced in that work, that "the aggregate of money incomes is proportional [that is, bears some proportion, which is not necessarily constant] to the stock of money" (*op. cit.,* 9; cf. also p. 84 of the same work). The true nature of Hawtrey's position is evidenced by his criticism of Aftalion's persistent attempts to set up "a false antithesis between the quantity theory of money and the income theory"—the two, Hawtrey insisting, being "perfectly consistent with one another" (see the *Journal of the Royal Statistical Society,* XCVII [1934], 341; and cf. Hawtrey's review of an earlier work by Aftalion in the *Weltwirtschaftliches Archiv,* XXVIII [1928], 99**, 102**).

[16] Thus, for example, Wieser, "Der Geldwert und seine Veränderungen," *loc. cit.,* 523, stated explicitly that whenever an increase in the quantity of money "increases money-income, without an accompanying increase in real income necessarily resulting, . . . to this extent it must lead to a lowering of the value of money." Similarly, on p. 518, he declared that since the level of monetary incomes will be the resultant of changes in "velocity," as well as of the quantity of money and of "credit media of payment," it was "self-evident" that "every theory of money must concern itself with the question as to how far the formation of money-income is influenced by these elements." (Wieser's statement of the question as to the effect of "velocity" upon prices—as opposed to its effect upon *incomes*—is another matter. See, on this point, the references given on p. 339, n. 111, above.)

of situation which is likely to develop so long as, instead of dealing with specific propositions of unequivocal content, we insist upon hiding behind expressions—such as "the quantity theory" or "the income theory" —which connote quite different propositions to each commentator, with the result that the extent of disagreement is bound to be made to appear much greater than it really is.[17]

It follows, from what has been said, that an emphasis on the importance of changes in the stock of money for an understanding of the process of price determination is not the mere result of blind piety, but is an ineluctable necessity, which is not in any fundamental sense escaped by the device of starting the analysis in the middle by assuming the magnitude of the stream of money payments (or of any segment of this stream of money payments) as given independently of changes in the quantity of money. It follows, also, that to write, for example, as Aftalion did, $R=PQ$, in which R represents the stream of income payments, and then to imply that the use of such an equation avoids all the questions that have traditionally been discussed in terms of the stock of money and its velocity of circulation, is to imply that we are excused from the necessity for going "behind" the variable R in order to see what are the factors which make R as large, in absolute terms, as it is.[18]

Cf. also what is said with respect to Aftalion's treatment of the relation between changes in the "quantity of money" and the level of money-incomes, in n. 18, below.

[17] Cf. what is said with respect to the futility of most discussions with regard to the "truth" or "falsity" of "the quantity theory" on pp. 23 f., above. It is obviously only because of disagreement with respect to what is involved in "the quantity theory," on the one hand, and "the income theory," on the other, that it is still possible to find writers who question the consistency of one "theory" with the other. Cf., in this connection, the remarks by Ellis (*German Monetary Theory,* 183) on Wieser, and by E. Petersen (*Den moderne kvantitetsteoris gyldighet,* 7) on the statement by W. Oualid that Aftalion's theory, despite Aftalion's own emphatic denial (see, for example, *Monnaie, Prix et Change,* 167 f. and cf. "Die Einkommenstheorie des Geldes," *loc. cit.,* 378 f.) "constitutes a complement to and improvement, rather than a refutation of, the quantity theory" (cf. Oualid, *Leçons sur la monnaie et les problemes monétaires* [1927], p. 169 n.).

[18] It may be remarked that Aftalion, like Wieser (cf. above, p. 351, n. 16), was forced to admit that "among the phenomena" which bring about "variations in income" are "variations in the quantity of money" (*Monnaie, Prix et Change,* 157 f.), and even to write the equation $R = a'M$, in which R represents "income" (*revenue*); M, the "quantity of money"; and a', a coefficient establishing the relation between the two (*op. cit.,* p. 191). It is characteristic, however, of the type of argument under discussion that, instead of using this formulation as a starting point for further analysis,

The argument for the abandonment of the concept of velocity as "superfluous," therefore, merely on the ground that the use of a term for "income" makes it possible to "avoid" the concept, is seen to be without foundation. It is of some importance, however, to emphasize the fact that by no means all defenders of the concept of "velocity" would assert that the nature of the forces determining the magnitude of the stream of money *income* is adequately represented by the insertion of a term for "velocity," however defined, side by side with a term representing the "money-stock." This is a method which, as we shall see, the sponsors of certain variants of the concept of "income velocity" have come perilously close to following.[19] It is, however, certainly not a method that is inherent in the use of the concept of "velocity" itself. All that is argued here is that at some stage in the analysis designed to show how a given change in the money stock is translated into a given change in money income, we must introduce the factor of "velocity," in a sense which makes it anything but a covering term for our ignorance—"a fashionable plaster over the inadequacy of our knowledge," as one writer has put it—or an "erratic and unpredictable variable," the chief purpose of which, so it is alleged, is to furnish "excuses for the failures of the predictions" of supporters of the "quantity theory." [20]

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the term a' was left without further definition in economic terms, the final conclusion being that *if* a' *is regarded as being essentially "constant,"* the "accuracy of the expression $R = a'M$ . . . is far from being demonstrated" (*op. cit.*, 192; italics mine).

[19] Cf. below, pp. 365 ff.

[20] Cf. B. Josephy, "Keynes' Geldlehre," *loc. cit.*, 59, and B. M. Anderson, "The Gold Standard versus 'A Managed Currency,' " *Chase Economic Bulletin*, V, No. 1 (March 23, 1925), 5, n. 1. This type of contention has been made so often and in so many different contexts, that a detailed discussion of it must be left for the study mentioned on p. 290, n. 1, above. Here it is necessary only to point out a curious paradox: namely, that while statements of this type have most often been used as part of an attack by "anti-quantity theorists," an examination of the literature shows that it is precisely the "*anti*-quantity theorists" who have most often fallen back upon unmotivated changes in "velocity" in discussions of the relations between money and prices. See, for example, Wicksell's comment on Marx, in this connection, in the former's *Lectures* (II, 150). The paradox finds a particularly striking illustration in the case of Aftalion, who, despite the fact that he characterized velocity as an element of "superfetation" and charged "quantity theorists" with falling back upon it only to save an otherwise hopeless position (cf. above, p. 348, n. 9), himself has repeatedly been guilty of falling back upon unmotivated changes in velocity

II

Income versus Outlay from Income

That the concept of "velocity," when properly employed, is anything but a device to avoid significant analysis will be abundantly demonstrated by our examination, in Chapter XV, of the implications which may be held to underlie the "cash-balance approach." For the present, however, it is important to emphasize another point which is strictly relevant to the question whether the use of the "income-approach" makes it possible to "avoid," in any fundamental sense, the supposedly "superfluous" factor of velocity. This point is the obvious fact that, for the purpose of representing a stream of money *spent against goods and services,* what is relevant is not "income," but *outlay.*

The importance of the distinction between "income" and "outlay" should hardly need to be brought to the attention of a generation that has become familiar with Mr. Hawtrey's use of the concepts of "consumers' income" and "consumers' outlay," and his explanation of the difference between the two by changes in the relative size of consumers' cash balances.[21] If the matter is mentioned here at all, it is only by way of driving home the conclusion that even for those who would begin "in the middle" by taking the level of money income as given independently of changes in the quantity of money, the concept of "velocity" cannot be avoided if what we are interested in is a "stream" equation of the form that is supposed to be represented, for example, by Aftalion's $R=PQ$.[22] When R is defined as "income,"

in discussing the relation between money and prices. See, in this connection, the comments of Hawtrey on Aftalion, *Journal of the Royal Statistical Society,* XCVII, 340.

[21] The point was an integral part of the argument of Hawtrey's *Good and Bad Trade* (see, for example, pp. 11, 14), but was first summed up in the terms suggested in the text by Hawtrey's proposition, in his *Currency and Credit* (p. 41 of the first edition; p. 46 f. of the third edition), that "the difference between the consumers' income and the consumers' outlay will represent a change in the unspent margin." See also Hawtrey's review of Aftalion in the *Weltwirtschaftliches Archiv,* XXVIII, 100**, where it was pointed out that "money spent" out of income may be expected to differ from "income" by the "amount by which *balances* change."

[22] The point under discussion had been made even prior to Aftalion's publication of his "income equation," by German critics of "income-theories" such as that of Liefmann, who was charged by these critics with failing to

and Q is defined as representing the goods on which the income is spent, the expression $R=PQ$ is simply not a true equation under all circumstances.[23] For the "income" which is rightly supposed to be necessary for the construction of a description of the process of pricing, say, consumers' goods, is equivalent to the *expenditure out of income* which belongs in a "stream" equation, *only upon the assumption that income is equal to outlay.*[24] It is the essence of Hawtrey's analysis, on the other hand, that the discrepancies between consumers' income and consumers' outlay, and the changes in the relative size of cash balances to which such discrepancies necessarily give rise, are capable of exerting an effect of an extremely important kind upon prices. This, of course, is merely another way of saying that there

make clear that it is not the level of income, but the level of *outlay* out of income, that determines the level of prices. See L. A. Hahn, *Volkswirtschaftliche Theorie des Bankkredits* (1920), 149, n. 142, and Neisser, *Der Tauschwert des Geldes,* 13 (cf. Ellis, *German Monetary Theory,* 183). It is worthy of note that these writers called attention explicitly to the fact that what was involved was a question of "velocity."

[23] It is worth noting that Aftalion himself admitted that his equation $R = PQ$ "does not present the same degree of accuracy as does the equation of exchange" of the Fisherine type, the former being accurate "only under certain conditions" (*Monnaie, Prix et Change,* 174 f.), though his discussion of the reason for this—a discussion which left much to be desired from the standpoint of emphasis and precision—did not include a treatment of the factor of "velocity," which he had already rejected as unnecessary. (Cf., however, the following note.) See also the remarks by Hawtrey, in his review of Aftalion in the *Weltwirtschaftliches Archiv,* XXVIII, 100**, on the necessity for substituting, in Aftalion's equation, "the total of *expenditure* by consumers" for "the total of incomes" and "goods purchased" by consumers in place of "production," if Aftalion's formula is to become "rigorously correct." Cf., finally, Bernstein, *Money and the Economic System,* 243 f., who, in questioning the "validity" of the "assumptions" underlying Aftalion's equation, pointed out not only that "it is not necessarily true that the money income of a given year is spent in the period in which it is earned," but also that "the entire real income of any year [that is, the Q of Aftalion's equation] is not sold in the period in which it is produced." On the latter point, see also what is said on pp. 540 ff., below.

[24] Again Aftalion was forced to admit this, as when he recognized that the possibility of "variable expenditure" with an "identical income"—due, for example, to a change in the "propensity to save" (*penchant à l'épargne*)—meant that, in such cases, "the formulas of the income theory fit the facts badly" (*Monnaie, Prix et Change,* 210, 213). Again, however, he failed to see any connection between this type of phenomenon and the "element of superfetation" which was all he saw in the factor of "velocity." It may be pointed out here that Aftalion's failure to associate his *penchant à l'épargne* with the phenomenon of "velocity" was prophetic of Mr. Keynes's later failure to provide an articulate association of his "propensity to hoard" with the phenomenon of "velocity." This matter will be discussed in more detail in Volume II of this study.

is no reason whatever for suggesting that the use of an "income approach" in any way releases us from the necessity of accounting for changes in the size of cash balances relative to outlay; and this, in turn, is merely another way of saying that, even if we wished to ignore the rôle of "velocity" in determining the level of money incomes themselves, we could not ignore it in estimating the effect upon prices which may be expected to follow from changes in the level of these money incomes.[25]

III

Keynes's *Treatise* on Income and "Velocity"

It is of some importance to establish the fact that Mr. Keynes, despite those aspects, listed above, of his argument in the *Treatise* and the discussion to which the *Treatise* gave rise, which might have led superficial readers to suppose that he was an "opponent" of the concept of velocity for reasons of the sort examined in the preceding sections of this chapter, in fact was nothing of the kind. Despite, for example, his apparent renunciation of the type of approach that is associated with what he called the "Quantity-of-Money, Velocity-of-Circulation schools of thought," he did not for a moment suggest that we could avoid such concepts by the simple device of starting with the level of money incomes as given independently of changes in the quantity of money. On the contrary, as we have seen, he found it necessary to introduce into his analysis, albeit with something less than complete enthusiasm, the "quantity of money"—the quantity of *bank* money, in particular—as a factor affecting prices, the relation of this analysis to something that may properly be called the "income approach" being subsequently established by the fact that he also thought it necessary to translate these changes in the quantity of money into changes in the level of money incomes.

[25] See especially, in this connection, the remarks of Hawtrey, in the *Weltwirtschaftliches Archiv*, XXVIII, 101**, on Aftalion's treatment of the "speculative factor" as related to income, expenditure, and the size of cash balances relative to outlay, and therefore to "velocity."

It was argued, above, that this process of translation must, at some stage of the argument, introduce the concept of "velocity" in one form or another. This necessity was illustrated in Mr. Keynes's case as it is illustrated in others; and the tangible result is represented by the chapters in the *Treatise* on "velocity" which have received so much praise.[26]

It is, however, of the first importance to make clear the precise nature of the concept of "velocity" which was involved in Mr. Keynes's method of establishing the relation between changes in the money stock and changes in the level of money incomes; and the best way of doing this is to contrast Mr. Keynes's method, with which I am in virtually complete agreement, with another method, which has in fact been more widely used in recent years, but which must be regarded as distinctly inferior from an analytical standpoint. This latter method is that which is summed up by the concept of "income velocity," in any one of the senses applied to the latter term before the term was used in a quite different sense by Mr. Keynes himself in his *General Theory*.[27]

"Income velocity," in the sense in which the writers concerned have used the term, may be defined, in Mr. Keynes's words, as "the relationship between the average stock of money and the national income."[28] As so defined, it is a concept which does not have behind it, despite Mr. Keynes's statement to the contrary, a "very ancient" tradition, dating from "the earliest literature on the subject." [29] Nor is it to be found, as Mr. Keynes suggested, in the writings of John

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[26] Cf. above, p. 300, and n. 26 thereto.

[27] The concept of "income velocity" used by Keynes in his *General Theory* will be discussed in Volume II.

[28] See the *Treatise,* II, 24. The differences among the various definitions that have been given of "income velocity" turn upon the precise meanings given to the expressions "stock of money" and "national income." Some of these differences are noted below. It is sufficient here to call attention to the fact that the various definitions of "income velocity" would agree in the two respects which are important for the present argument: namely, in holding, first, that the numerator of the ratio defining "income velocity" is something which may be called the total of "income payments" rather than the total of money payments of all kinds; and, second, that the magnitude to be compared with "normal income payments" is not the total of what Mr. Keynes called "income deposits" (Hawtrey's "consumers' balances"), but the total of *all* "deposits."

[29] *Treatise,* II, 23.

Stuart Mill.[30] A concept very similar to that of "income-velocity" was, to be sure, used by Mill's contemporary, Thomas Attwood.[31] So far as I have been able to discover,

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[30] *Treatise,* II, 24. The nearest approach to a usage similar to that involved in the concept of "income velocity" which can be found in the writings of Mill, so far as I am aware, is that which appears in Mill's article in the *Westminster Review,* XLI (1844), 590 n. In the passage in question, Mill, in criticizing an argument of Torrens, pointed out that a "million sovereigns may serve, by successive payments, to represent and circulate incomes to the amount of many millions." This, however, is a proposition which would be accepted by all competent economists, including those who would object emphatically to the concept of "income velocity" as an analytical device. It does not, therefore, provide warrant for qualification of the proposition stated in the text. There is no evidence, in any case, that Mr. Keynes had in mind any of Mill's writings other than the *Principles.* On Mill's usage with respect to "velocity" in the latter work, see below, p. 362, n. 40.

[31] See Attwood's *Letter to the Right Honourable Nicholas Vansittart, on the Creation of Money, and on its Action upon National Property,* London, 1817, p. 27: "In the present state of the country, it is probable that the million and a half of money so created, will pass and repass through the hands of labourers every three months; and in the mean while it will be furnishing intermediate markets between one description of capitalists and another, so that in reality it may be presumed, that the issue of this money will furnish six millions of additional wages to the labourers and mechanics *in one year* from its first issue, and probably at least thirty millions more of additional markets to the capitalists and proprietors of stocks." (Italics Attwood's.) So far as I am aware, attention was first called to Attwood's use of a distinction roughly equivalent to "the modern distinction between transaction velocity of money and income velocity of money" by Professor Viner (*Studies,* 200, n. 41). If we may take the total of "wages to the labourers and mechanics" as the equivalent of income, it is obvious that Viner is correct in stating that Attwood estimated his equivalent of "income-velocity" at 4 per annum, since such a conclusion is justified not only by Attwood's statement that money would "pass and repass through the hands of labourers every three months," but also by the ratio of 6,000,000/1,500,000 = 4. The basis for Viner's suggestion that Attwood estimated the equivalent of "transaction velocity" at 50 per annum, is, however, less certain. The figure given by the ratio

$$\frac{30{,}000{,}000 + 6{,}000{,}000}{1{,}500{,}000}$$

is 24—a figure which, by an amusing coincidence, is almost exactly that which has been obtained in some of the more recent attempts to measure "transaction velocity" statistically (cf. the summary given by W. C. Mitchell, *Business Cycles: The Problem and its Setting,* 123-128), just as Attwood's figure for what would correspond to "income velocity" is closer to that given by some recent writers than one might have expected. (See, for example, Pigou, *The Theory of Unemployment,* 196, where the figure given for "income-velocity" is 3, as compared with Attwood's 4.) On the other hand, Attwood was not always entirely consistent. On p. 22 of the work cited, for example, he estimated that money "changes hands . . . in the present state of the circulation, upon the average . . . probably, at least once a week"; and it may be this passage which Professor Viner had in mind in suggesting that Attwood's estimate for what would correspond to "transaction velocity" was "roughly . . . 50 . . . per annum."

however, the concept was not used by a writer of established standing prior to 1917, when it was explicitly introduced by Joseph Schumpeter, whose influence in this respect was reflected in the writings of J. Marschak (1922), M. W. Holtrop (1928), and H. Neisser (1931).[32] Something resembling the concept was introduced independently by Messrs. Foster and Catchings, under the name of "circuit velocity," in 1920.[33] The really important instance of the use of the con-

[32] The denominator of Schumpeter's ratio representing the "efficiency" of money was, to be sure, not the total "stock of money," but the quantity of money "in circulation"—that is, it was explicitly described as excluding not only *bank* reserves but also the "hoards" and "reserves" of private individuals ("money kept in the sphere of hoards and reserves"), as well as money used in what Schumpeter called the "capital-sphere," that is, money used for the purpose of transferring property rights (Schumpeter, "Das Sozialprodukt," 666 f.; on the numerator of Schumpeter's ratio, see below, p. 376, note 70). The question as to the validity of the distinction between money that is "in circulation" and money that is "out of circulation" is discussed below (pp. 459 ff.), as is also the question of the treatment of money used in property transfers (pp. 520 ff., below). Here it is necessary only to point to the element which establishes the kinship between Schumpeter's "efficiency" and the concept of "income-velocity": namely, his explicit insistence that the "quantity of money" which was to be divided into the "sum of incomes" in order to obtain a measure of "efficiency" was to be understood as "the *whole* quantity of money in circulation [that is, all money outside of the "capital sphere" and the "sphere of hoards and reserves"], and *not merely the part, say, which happened to to be in the market for consumers' goods* at any particular point of time" (Schumpeter, *op. cit.*, 675; italics mine). It may be noted that the sharp distinction between "money in circulation" and money "out of circulation" was adopted neither by Marschak, in his discussion of Schumpeter's "efficiency of money" (Marschak, "Die Verkehrsgleichung," *Archiv fur Soz.-wiss. u. Soz.-pol.*, LII [1924], especially p. 348), nor by Holtrop, whose "effectiveness" (*effectiviteit*) of money is clearly derived from Schumpeter's "efficiency" (Holtrop, *De Omloopssnelheid van het Geld*, 113 ff.), though an analogous distinction has been used by Professor Pigou in his more recent writings (cf. below, pp. 390 ff.). For Neisser's treatment of the concept of "circuit velocity" (*Kreislaufsgeschwindigkeit*), which he associated explicitly with Schumpeter, see Neisser's "Der Kreislauf des Geldes," *loc. cit.*, 385 f., 404; cf. the same writer's article "Umlaufsgeschwindigkeit der Bankdepositen," in the *Handwörterbuch des Bankwesens* (1933), 570.

[33] Cf. above, p. 341, n. 117. As Hawtrey has pointed out (*Currency and Credit*, 3d ed., 59 n.), the "circuit velocity" of Foster and Catchings would relate the money stock to *outlay out of income* rather than to *income*. We are here dealing, however, with an ambiguity generally typical of much of the discussion that has employed the concept of "income-velocity." See, on this matter, pp. 369 ff., below. Nor does the ambiguity end here; for much depends upon what is understood by "outlay out of income." When, for example, Hawtrey identified the "outlay" of Foster and Catchings with his "consumers' outlay," he was forgetting, as Lindahl has pointed out (*Om förhållandet, etc.*, 11, n. 4), that the "consumers' outlay" of Foster and Catchings includes only outlay on consumers' goods, whereas the "consumers' outlay" of Hawtrey includes outlay upon investment as well. In one sense, obviously, both types of ambiguity go back in turn to the

cept which, from the standpoint of subsequent influence, deserves to be ranked with that represented by the case of Professor Schumpeter, is its use by Mr. D. H. Robertson and Professor Pigou.[34] It is with the concept as it appears in the work of Schumpeter, Robertson, Pigou, and their successors, therefore, and not as it is supposed to have appeared in "the earliest literature on the subject," that we are here concerned.

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definition that is given to "money income." If, for example, the latter is defined as Schumpeter defined it—namely, as the money value of "goods consumed" (Schumpeter, "Das Sozialprodukt," 635)—it is obvious that "money income" is much closer to the "consumers' outlay" of Foster and Catchings than to either Hawtrey's "consumers' outlay," or to the total of money income *received* by "consumers." It is worth noting, in this connection, that the definition of "income-velocity" given in Robertson's *Money* (2d ed., p. 36) as the "average number of times each piece of money is spent . . . in purchase of the goods and services which enter into ordinary consumption" implies a definition of (real) "income" which would make his "income velocity" virtually identical with Schumpeter's "efficiency" and Foster and Catching's "circuit velocity." Cf., however, what is said on p. 380, n. 79, below.

[34] Cf. above, p. 342, n. 119. For examples of the use of the concept of "income velocity"—or, as it has sometimes been called, "circuit-" or "circular velocity"—by writers subsequent to Robertson and Pigou, see Edie, *Money, Bank Credit and Prices*, 395 ff.; A. Nielsen, *Bankpolitik*, II, 164 n.; J. W. Angell, "Money, Prices, and Production: Some Fundamental Concepts," *loc. cit.*, 42 ff.; "Monetary Control and General Business Stabilization," in *Economic Essays in Honour of Gustav Cassel* (1933), 55 f.; "The 100 Percent Reserve Plan," *Quarterly Journal of Economics*, L (1935), 17, n. 4, 34; "The Components of the Circular Velocity of Money," *ibid.*, LI (1937), 224 ff.; *The Behavior of Money* (1936), see index under "Circular velocity"; L. Currie, "Money, Gold, and Income in the United States, 1931-32," *Quarterly Journal of Economics*, XLVIII (1933), 91 ff.; "A Note on Income Velocities," *ibid.*, 353 ff.; "A Reply to Dr. B. M. Anderson, Jr.," *ibid.*, XLIX (1935), 695 f.; "The Treatment of Credit in Contemporary Monetary Theory," *Journal of Political Economy*, XLI (1933), 66, 76; *The Supply and Control of Money in the United States* (1934), 6; F. A. Bradford, *Money*, rev. ed., 1933, 212, 217 f.; A. H. Hansen, "The Flow of Purchasing Power" in *Economic Reconstruction* (Report of the Columbia University Commission), 1934, 211 n.; J. M. Clark, "Productive Capacity and Effective Demand," *ibid.*, 110 f. (*Preface to Social Economics*, 361 f.); "Cumulative Effects of Changes in Aggregate Spending as Illustrated by Public Works," *American Economic Review*, XXV (1935), 19 ff. (*Preface to Social Economics*, 387 f.); *The Economics of Planning Public Works* (1935), 85, 88, 96 ff.; E. F. M. Durbin, *The Problem of Credit Policy* (1935), 44 n., 149, 205, 250. Characteristic of the treatment of the literature in J. S. Robertson's *Income Theory of Prices* is the author's introduction (for example, p. 23) of the concept of "primary velocity," defined as the ratio between the "quantity of money" and "monetary income," without reference to the use, by earlier writers, of either "income velocity" or "circuit velocity." The differences in the usages of these various writers cannot be discussed in detail here; insofar as they are not touched upon in later pages of this study, they must be left for the book mentioned above, p. 290, n. 1.

It may not be amiss, however, before proceeding to a critical examination of the concept of "income velocity," as thus understood, to indicate briefly the reasons for rejecting Mr. Keynes's generalizations with respect to the rôle played by the concept of "income velocity" in the earlier literature. In support of his statement that "the earlier writers were mainly influenced" by what amounts to the concept of "income velocity," and that the tendency of "monetary theorists" to "oscillate" between "an inclination to regard Velocity (or Rapidity) as a relation between the national stock of money and the national income, and an inclination to regard it as a relation between the stock of money and the total volume of transactions," Mr. Keynes cited as authority the writings of Holtrop.[35] He gave no page references to these writings, however; and, as I have had occasion to point out elsewhere, Holtrop's main dichotomy turns, not upon considerations associated with the concept of "income velocity," but upon the distinction between the "motion-theory" and the "cash-balance theory."[36] That there has been, in recent years, a regrettable tendency to confuse certain variants of the concept of "income velocity" with certain variants of the "cash-balance approach" is pointed out below. This, however, is quite a different matter from arguing that the identification—or confusion—is justified by either logic or precedents which go back to the "earliest literature on the subject." The only precedents that would give even an appearance of support to Mr. Keynes's reference to the general use, in the earlier literature, of the ratio between "the national stock of money and the national income" are those provided by the computations of earlier writers who were interested in the question as to how much money a nation "needs." This, however, is a very different thing from implying, as Mr. Keynes does, that a considerable number, if not a majority, of these earlier writers regarded such a measure as a type of "velocity," which in turn was differentiated sharply from the type of velocity represented by "transactions velocity."[37] Indeed, I have been able to find only one instance in which an earlier writer regarded the ratio in question as a measure of "velocity," namely, the case of Launhardt; and in this instance, it was perfectly clear that the author, instead of *contrasting* his measure of velocity, in the manner of modern users of the concept of "income velocity," with something called, say, "transactions velocity," regarded it as *identical* with the latter—or, as Launhardt himself put it, with the number of times during a year "the individual piece of money passes, on the average . . . to another possessor."[38]

[35] See the *Treatise*, II, 23.

[36] See my "Léon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," *loc. cit.*, 591, n. 50.

[37] It may be pointed out also that the same type of proposition holds as against those writers who have implied that the computations with respect to a country's "need" for currency, of the type indicated, are examples of the "cash-balance" approach. A more detailed examination of such an implication must, however, be left for another occasion.

[38] See Launhardt, *Wesen des Geldes*, 50.

It is also true that certain *terms* which have been used by later writers as the equivalent of "income-velocity," such as "the efficiency of money," go back far in the literature; and so do methods of expression that recur in the writings of those who have made use of the concept of "income-" or "circuit-velocity"—such as what Holtrop calls "the idea of a circuit (*kringloop*) to be covered by money."[39] In order, however, to be able to adduce these facts in support of the proposition that the concept of "income-velocity," in the modern sense of the term, is found in the "earliest literature on the subject," it would be necessary to demonstrate that the terms "efficiency," or "circuit," were used by the earlier writers in essentially the same sense, or in the same context, as that in which they have been used by those writers of our own day whose association with the concept of "income-velocity" cannot be open to question. One surmises, indeed, that, in attributing the concept of "income-velocity" to John Stuart Mill—although, since no page reference was given to Mill, it can be only a matter for surmise—Mr. Keynes, like many other writers, was misled by Mill's desire to substitute for "rapidity of circulation" the term "efficiency," which has been applied by Schumpeter and others to a concept very close to that of "income velocity," into supposing that the substance of the two concepts was identical.[40] In fact, however, the term "efficiency of money" has been used to cover all kinds of concepts, many of which have no more in common with the concept of "income-velocity" than did Mr. Keynes's own use of the term "Efficiency."[41]

A word may be added, finally, with respect to the confusion of certain variants of the concept of "income velocity" with certain variants of the "cash-balance approach," to which reference was made above. The formal association of the two had been implied, prior to the publication of the *Treatise*, by more than one commentator on Keynes's earlier "cash-balance" equation $n = pk$.[42] Unfortunately, moreover,

[39] See, for example, Holtrop's *Omloopssnelheid van het Geld*, 60. A discussion of the implications of "the idea of a circuit" for the theory of the velocity of circulation of money must also be left for another occasion.

[40] For Mill's discussion of the concept "efficiency of money," see his *Principles*, Book III, Chap. VIII, sec. 3 (p. 495 of the Ashley edition). For a discussion of the confusion that has arisen from a failure to observe the simple fact that a use of the *term* "efficiency" does not necessarily mean that the *concept* covered by the term is in any way different from other concepts of "velocity," as well as for a discussion of Mill's meaning, the reader must, however, again be referred to the study indicated on p. 290, n. 1, above.

[41] Cf. the *Treatise*, II, 22.

[42] See, for example, Lindahl, *Om förhållandet, etc.*, 12, where the author groups the Keynes of the *Monetary Reform* with users of concepts such as the "circuit velocity" of Foster and Catchings and the "efficiency" of Schumpeter, on the ground that although, to be sure, "Keynes . . . substitutes, in the manner of Pigou, the concept of a holding of cash-balances for 'velocity,' . . . his line of thought otherwise follows the lines" suggested by the two concepts indicated. Paradoxically enough, Lindahl went on, in a manner suggestive of that adopted by Keynes in his criticism of the concept of "income velocity," to point out that the *total* of cash balances which

some basis for the association can be found in the writings of members of the Cambridge School themselves. Thus Pigou, for example, who elsewhere defined his "circulating period" of money as the "inverse" of "income-velocity," stated that this "period" would "constitute a fraction of the year equal to the fraction of their real income that people choose to hold in money form."[43] Similarly, Robertson, in the course of a discussion of the implications of the cash-balance approach, alleged explicitly that "the proportion of their annual real income over which people wish to keep command in the form of money *is* the income velocity of circulation of money turned upside down."[44] One result of this sort of practice was that, in the discussion of Pigou's "cash-balance" equation, $M/P = kR$ which appears in the *Treatise,* Mr. Keynes himself did not sharply differentiate the issues which are properly associated with the concept of "income-velocity" from those which are properly associated with the "cash-balance approach" as such.[45] It is not altogether surprising, therefore, that when Mr. Robertson undertook to defend the concept of "income-velocity" against the criticisms of that concept which had appeared in the *Treatise,* his defense was really incidental to a broader defence of the " 'Cambridge' method of approach" as embodied in the cash-balance equation of Pigou cited above.[46] As we shall see, however, there is every reason for separating the issues involved in the concept of "income-velocity" from those involved in the "cash-balance approach" as such. It may be noted, in any case, that the possibility of keeping the issues separate was recognized at least in part by Mr. Keynes himself, to the extent that he did not introduce into his own formal discussion of the concept of "income-

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was represented in Keynes's n included not only "consumers' balances" (Keynes's later "Income Deposits"), but also "traders' balances" (Keynes's later "Business Deposits"), so that it was dangerous to draw conclusions from Keynes's formulation with respect to the nature of the relation between Keynes's n and "the total [value] of consumption." Keynes's equation $n = pk$ was discussed in connection with the "income-approach" also by A. Nielsen, *Bankpolitik,* II, 138 (cf. also *ibid.,* II, 126 n.), although not in direct association with the concept of "income-velocity."

[43] Pigou, *Industrial Fluctuations,* 136; and for the definition of the "circulatory period" of money as the "inverse" of "income velocity," see *ibid.,* 152.

[44] See Robertson, *Money,* 2d ed., 40.

[45] See, for example, the *Treatise,* I, 232. It must be said, in justice to Mr. Keynes, that he suggested, at one point, that "the essential advantages of the 'Cambridge' method" could be retained by a redefinition of the M involved in Pigou's formula. In view, however, of the fact that his criticism of the concept of "income-velocity" (on which see below, pp. 388 ff.) was likewise directed against "its tackling the problem as though the same sort of considerations which govern the income-deposits also govern the total deposits" (*Treatise, loc. cit.*), he can hardly be said to have differentiated the issues associated with the concept of "income velocity" with sufficient sharpness from those associated with the "cash-balance" aspects of Pigou's equation.

[46] See Robertson, "A Note on the Theory of Money," *Economica,* August, 1933. Cf. below, pp. 398 ff.

velocity" any reference to Pigou's "cash-balance" equation, which had been discussed elsewhere in the *Treatise*.[47] The point made here is that the association of the two sets of issues is itself a very recent phenomenon, and would hardly justify the suggestion that simply because examples of the "cash-balance approach" can be found in the "earliest literature on the subject," we are warranted in concluding that the concept of "income-velocity" is of an equal degree of antiquity.

IV

The Concept of Income Velocity Examined

Unfortunately for our immediate purpose, the concept of "income-velocity," even as used by the later writers cited above, while it displays nothing resembling the heterogeneity of motivation underlying what has been called the "income-approach," has not always carried the same set of connotations at the hands of all who have used it. It is, however, possible to single out the more important of these connotations for discussion here.[48]

Taken in and of itself, the concept of "income-velocity," as defined above, obviously proceeds from three propositions the soundness of which cannot be open to serious question. The first of these is that, for the purposes of monetary theory, a special importance attaches to those particular money payments which represent income payments—either payments *"into"* income (that is, payments the sum of which represents "money income"), or payments *"out of"* income—as contrasted with the payments involved in transactions other than "income" transactions.[49] The second proposition is one the importance of which we have already

[47] Contrast, in this connection, the *Treatise*, II, 24 f., with I, 229 ff.

[48] I intend to deal at greater length with the various arguments that have been associated with the concept of "income velocity" in the proposed work on the velocity of circulation of money to which reference has so often been made in these pages. Cf., in the meantime, what is said on pp. 561 f., below.

[49] The distinction between those "income payments" which are payments *into* income and those payments which are payments *out of* income has, unfortunately, not always been made clear, with resulting havoc for much of the analysis associated with the concept of "income velocity," and for the good name of the "income approach" generally. In addition to what is said on pp. 354 ff., above, with respect to the relation between "income" and "outlay out of income," and the references to Schumpeter and Robertson given on p. 359, n. 33, above, see what is said on this matter on pp. 379 ff., below.

had occasion to stress—namely, that the magnitude of the stream of money income is greatly affected by changes in the stock of money.[50] The third proposition is that there is no *a priori* reason for supposing that the relation between the stock of money and the total of income payments will remain constant, in such wise that we could use changes in the quantity of money as indicative of changes in the magnitude of money income, and vice versa.[51] It is therefore proposed, by sponsors of the concept under discussion, to introduce a symbol—"income velocity"—which will summarize the forces making for a change in this relation.[52]

The first questions, however, involved in the evaluation of any given concept, are obviously whether the concept in question is of a sufficient degree of precision for the purpose of analyzing the forces which it is designed to summarize, and whether the concept itself throws a sufficient amount of light upon the nature of those forces. From this standpoint, it is possible to raise the question whether the concept of "income velocity," as it has often been used, does not in reality pretend to provide a solution for a problem which, after the concept is adopted, is still left as much unsolved as it was before; and whether, by so doing, it does not encourage either, at worst, hasty solutions that cannot stand up under closer analysis, or, at best, an unfortunate complacency with respect to the present state of our knowledge

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[50] Cf. above, pp. 348 ff.

[51] This proposition has been put somewhat differently by certain sponsors of the notion of "income velocity," in that they have justified the use of the concept by showing that the conditions under which the "sum of incomes" and the "quantity of money" would be *equal*—"income velocity," in such a case, having the value of 1—are not likely to be realized in practice, so that we need the concept of income velocity as a means of expressing the "quantitative relationship between the quantity of money and the sum of incomes." See, for example, Schumpeter, "Das Sozialprodukt," 674 f.; also Pigou, *Industrial Fluctuations,* 136 f., where the concept of a "circulating period of money" (cf. Robertson, *Banking Policy and the Price Level,* 47), which is the inverse of "income-velocity" (Pigou, *op. cit.,* 152), is introduced as a means of taking account of the circumstance that "as a matter of fact, the stream of money appearing as money income during a year is not equal to the stock of money."

[52] See, for example, Pigou, *The Theory of Unemployment,* 197, where the concept of "income velocity" is used in connection with the assumed proposition that "if the total stock of money M is increased by the creation of new bank credits, a consequent increase in the magnitude of I [income] must take place."

concerning some of the most thorny problems that monetary theory has to offer.

The extent to which the concept of "income velocity" actually helps toward a solution of the problems with which we are here concerned may best be judged by considering another concept of "velocity" with which, from the methodological standpoint, it has much in common—namely, the concept of "virtual velocity."[53] Like the concept of "income velocity," "virtual velocity" was introduced in order to relate a stock of money to a stream of money payments which was regarded as relevant for the process of price determination, the only differences here significant being that, in the case of "virtual velocity," the stream of money payments was the *total* of money payments, and the stock of money included only money of *ultimate redemption.*[54] Like the concept of "income-velocity," also, the concept of "virtual velocity" is not without its value for problems of a very broad nature.[55] In the case of "income-velocity," we are dealing with a device which may be taken as summarizing the forces that will lead a given change in the money stock to result in a greater or smaller change in the level of money income. In the case of "virtual velocity," we are dealing with a device that may be taken as summarizing the forces which will cause a given change in the quantity of money of ultimate redemption to result in a greater or smaller change in the total of money payments of all kinds, and therefore in determining the extent of the amount of money of ulti-

[53] Cf. above, p. 186, and notes 76 and 77, thereto.

[54] In the case of "income-velocity," of course, the "stock of money" used as the denominator in the defining ratio is usually the total stock of money of *all kinds* (cf., however, the reference, on p. 359, n. 32, above, to Schumpeter, whose denominator was neither the stock of money of ultimate redemption, nor the total of money of all kinds in the country, but only the stock of money "in circulation"). It may be pointed out, nevertheless, that, from the analytical standpoint, the objections to using the stock of money of ultimate redemption as the denominator of the ratio measuring "income velocity" are not greatly different from those which may be raised against the manner in which the latter concept itself is often used. The argument against the former procedure, presumably, is that to compare the stock of money of ultimate redemption with "money income" would mean that the measure of "income velocity" so obtained would include the effects of too many quite disparate variables. From the argument in the text, however, it should be clear that the difference in the applicability of this criticism to ordinary measures of "income velocity" is merely one of degree.

[55] Cf. what is said on this matter on pp. 385, and 388, below.

mate redemption which a country "needs" in order to carry on a given volume of transactions at a given level of prices.[56]

It will certainly be granted, however, that a summarizing device is valuable only in proportion as it really summarizes, instead of merely providing a name—and then a misleading name—for a relation determined by forces the nature of which are completely unilluminated by the concept itself. From this standpoint, surely, it can hardly be said that the concept of "virtual velocity" adds, in any real sense, to our understanding of the forces determining the relation between the quantity of money of ultimate redemption and the total of money payments. One of these factors, to be sure, is "velocity" in the narrower sense of the term—namely, the factor of repeated spending or "turnover" of a given amount of cash held by individuals.[57] The other factors determining the relation between the total of money payments and the stock of money of ultimate redemption, however, are factors which cannot, by any reasonable stretching of language, be regarded as changes in "velocity." The forces determining the *quantity of money substitutes held as cash balances* (our M'), for example, are of an entirely different nature. The real objection to the concept of "virtual velocity," therefore, is not merely that, as some of its critics have pointed out, it gives rise to strange paradoxes which can hardly be regarded as illuminating—such as, for example, the fact that the smaller the amount of money of

[56] An historical survey—which cannot be attempted here—of the treatment accorded, in economic literature, to the problem as to how much "money" a country needs would, in fact, show that the problem may be said to have advanced toward solution in the degree to which, for mere computations of the ratio between the stock of money of ultimate redemption and the total of money payments, there was substituted detailed analysis of the factors that help to determine this ratio. This, of course, is merely another way of saying that the concept of "virtual velocity," which simply stated the ancient ratio, is, when interpreted in any sense other than that of a shorthand summary for detailed analysis presented elsewhere with respect to the nature of the forces making the ratio what it is, a retrogression as compared with these concepts which represented such analysis.

[57] It must be repeated, in justice to Wicksell, that he himself had much to say, on this narrower problem, that was of great historical importance. The point to be made here is that, in providing discussion of the narrower problem, he was really advancing our knowledge; whereas, in introducing the concept of "virtual velocity," he added literally nothing but a phrase which at best summarized a relation that had already been found to be too all-inclusive to be helpful in problems requiring detailed analysis.

ultimate redemption that is kept from being spent directly, by being added to bank reserves, the higher its "velocity," in the "virtual" sense, is likely to be.[58] The real objection is that whenever, as is often the case, a given factor (such as movements in the rate of interest on money loans) may affect differently components of the concept of "virtual velocity" which the concept itself leaves undifferentiated—such as the rate at which a balance is turned over ("velocity" in the narrower sense of the term), on the one hand, and, on the other, the amount of money substitutes which will be created by banks (our M')—it becomes obvious that the concept of "virtual velocity" is a weapon which at worst may lead to erroneous conclusions and at best must be resolved into its disparate elements before it can begin to throw light upon the problems that really matter.

That the same type of objection applies to "income velocity," when the latter is regarded as a significant analytical device in its own right, can best be shown by adducing at this point a proposition the demonstration of which must be left for the following chapters: namely, that *no concept of "velocity" can be regarded as satisfying which is not in all respects strictly consistent with the methodological principles underlying the body of analysis designed to account for the size of cash balances relative to the outlay against which the cash balances are being held.* In anticipation of the demonstration of this proposition, it is sufficient here to establish the fact that the forces which may be expected to determine the magnitude of "income velocity" go as far beyond the scope of the forces determining the size of cash balances relative to the associated outlay as the forces determining the magnitude of "virtual velocity" go beyond the scope of those which determine "velocity" in the narrower sense of the term just indicated.

In the case of "virtual velocity," we saw that this comprehensive concept summarizes, among other things, the forces determining the magnitude of the stock of money substitutes held as cash balances (M'); and it was argued that the

[58] Cf., in this connection, K. Helfferich, *Money*, 456; Liefmann, *Geld und Gold*, 45; Schumpeter, "Das Sozialprodukt," 674; T. N. Carver, *Principles of National Economy* (1921), 387.

forces determining this latter magnitude were so greatly different from those which determine the size of cash balances relative to outlay—that is, "velocity" in the narrower sense of the term—that only confusion could come from a treatment which, by using the term "velocity" to cover both sets of phenomena, would imply that these forces are similar in nature. It is now to be observed that the case of "income-velocity" is, if anything, even worse in this respect; for, among the forces which will determine the relation of the stock of money to the total of income payments is not only "velocity" in the narrower sense of the term—in this case, a "velocity" measured by the ratio of the total of income payments to the total of cash balances held for the purpose of making payments out of income—but also the *magnitude* and *composition* of the total of *monetary transactions of all types—that is, the magnitude and composition of the PT of our Quantity Equation.*

This proposition is capable of easy algebraic demonstration. If, to provide an initial simplification of the algebra, we write the $(M + M')$ of our Quantity Equation as equal to M, which will now represent the total of cash balances in the country and if, to simplify the exposition still further, we adopt that definition of "income-" or "circuit-velocity" which would define the "stock of money" as equivalent to this total of cash balances, and "income-" or "circuit-velocity" itself as the number of times this "stock" enters into "consumers' outlay," we have the following algebraic statement of the variables which must be studied separately in order to understand why "income-velocity" is as large as it is.

Let $(PT) = (PT)_i + (PT)_{ni}$, in which the subscripts refer to "income" payments and "non-income" payments, respectively.[59] Let $M = M_i + M_{ni}$, in which M_i and M_{ni} represent the cash balances held against each type of payment; let $V_i = (PT)_i/M_i$ and $V_{ni} = (PT)_{ni}/M_{ni}$. We then have

$$M_iV_i + M_{ni}V_{ni} = (PT)_i + (PT)_{ni}. \qquad (1)$$

"Income-" or "circuit-velocity," as defined above, would be equal to $(PT)_i/M$, or $(PT)_i/(M_i + M_{ni})$. From equation (1), it follows that

$$\frac{(PT)_i}{M_i + M_{ni}} = \frac{M_iV_i + M_{ni}V_{ni}}{M_i + M_{ni}} - \frac{(PT)_{ni}}{M_i + M_{ni}}.$$

It is obvious, therefore, that, in addition to the problem of "velocity," in the strict sense, which is summarized by the expression $(M_iV_i +$

[59] It is to be noted that, in the light of the definition of "income-" or "circuit-velocity" here employed, "income-payments" are taken to represent payments *out of* income. On the meaning of "income payments" when "income-velocity" is otherwise defined, see below, pp. 379 ff.

$M_{ni}V_{ni})/(M_i + M_{ni})$, we have the problem of determining why $(PT)_{ni}$ represents a greater or smaller proportion of (PT).[60]

As we shall see at a later point in this study, it is perfectly possible to describe the "demand" for "cash-balances" in terms that will do justice to both the forces determining the demand for cash balances of a given size *relative to outlay* and the forces determining the demand for cash balances of a given absolute height in terms of a number of monetary units.[61] Our point here is merely that it is only the first type of problem that is relevant to the concept of "velocity" in the narrower sense of the term. We have just shown, on the other hand, that "income-velocity" is the resultant not only of forces which are to be regarded as affecting "velocity" in the narrower sense (namely, those determining the ratio of cash balances held against expenditure made from those cash balances), but also of forces of an entirely different nature (namely, those determining what proportion of the total money stock is required as cash balances for the purpose of carrying on a given volume of non-income transactions at a given level of prices).[62] This, it will be observed, is another way of saying that the concept of "income velocity" involves an analysis not only of the forces determining the V of our Quantity Equation, but also of the forces determining the magnitude and composition of T, as well as of the relative height of the "price-levels" that attach to the objects involved in "income" transactions and "non-income" transactions, respectively. As we shall see, these latter problems are of a very much greater degree of complexity than many students of monetary theory have been willing to admit.[63]

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[60] Cf., in this connection, Neisser, "Der Kreislauf des Geldes," *loc. cit.*, 385 f.

[61] In terms of an equation of the Fisherine type, the second type of "demand" would be affected by the magnitudes T or PT—depending upon the nature of the proposition advanced with respect to this "demand" and upon the assumption made with respect to the type of monetary policy being pursued by the monetary authority—as well as by the forces summarized by the V of the Fisher equation. See, on this matter, pp. 444 ff., below.

[62] This latter proportion may, of course, be represented algebraically by the expression $M_{ni}/(M_i + M_{ni})$, which, in turn, since $M_{ni} = (PT)_{ni}/V_{ni}$, will be affected by the magnitudes $(PT)_{ni}$ and V_{ni}, as well as by the magnitudes included in the expression $(PT)_i/M_i$, or V_i.

[63] See especially, on this matter, pp. 484 ff., below.

In the light of these considerations, surely, it is fair to argue that the concept of "income velocity," particularly as used by writers who have followed the practice of substituting, in "income-equations" which are otherwise of the general Fisherine form, a term for "income-velocity" in place of the older "velocity," has conveyed no notion of the degree or even of the nature of these complexities.[64] It has, on the contrary, tended to reduce the problem to terms which have suggested to some writers that nothing more is involved than a *subdivision* of the problem of "velocity" as the latter problem has presented itself in the literature since the days of Petty and Locke. It follows, therefore, that the concept of "income-velocity," whenever it is regarded as a true *alternative* to the older concepts of "velocity," is chargeable with precisely the same type of sin as that which is chargeable against the concept of "virtual velocity." In the latter case, as we saw, the sin consisted of implying that the forces determining the magnitude of the $c = M'/M_r$ of our Quantity Equation were of the same general nature as the forces determining the V of that equation. In the present instance, the sin consists of implying that the forces determining the magnitude and composition of the T of our Quantity Equation are of the same general nature as those determining the V of the equation.

That it is of the utmost importance to keep the two types of problem separate will hardly be denied by those who will admit that the problems of *mechanism*—those examples of a "rational filiation in the succession of events" which, as we have seen, Comte regarded as the product of a truly "scientific" discipline—demand an apparatus which is sufficiently well-articulated to make it possible to trace with some precision the successive steps in the "succession of events." After all, the very use of the concept of "income velocity" implies, or should imply, an interest in a problem of which "mechanism" is the essence: namely, the problem of tracing the steps whereby a change in the stock of money

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[64] For examples of the use of an equation of the type indicated, see Schumpeter, "Das Sozialprodukt," 675; Foster and Catchings, *Money*, 301; Angell, "Money, Prices, and Production," *loc. cit.*, 42 f.

is transformed into a change in the level of money incomes.[65] What is argued here is that, taken in and of itself, the concept of income velocity does very little more than imply an interest. It states the problem; it contributes very little indeed to its solution. For it gives no clear indication of the nature of the magnitudes which may be expected to reflect the successive impacts following upon the change in the money stock as that change is transmitted to and from money incomes.

Nor is this all. It is, of course, obvious that the concept of "income velocity," if it is the resultant of forces that can be lumped with the forces determining "velocity," in the stricter sense of the term, only at the cost of great confusion, is nevertheless also the resultant of forces which are properly to be regarded as determining "velocity" in this stricter sense. The question then arises whether, for the narrower purpose of dealing with these latter forces, the concept of "income velocity" is a help or a hindrance.

An answer to this question must, obviously, be based upon some conviction as to the type of analysis which must underlie any attempt to deal with the phenomena which the concept of "velocity" has traditionally been intended to summarize. As has already been indicated, we shall present, in the following chapters, the proposition that no analysis of "velocity" can be regarded as satisfying which is not in all respects strictly consistent with the methodological principles underlying the body of doctrine designed to account for the size of cash balances relative to the outlay against which the cash balances are being held. If this principle be accepted, then it is easy to show that in one very important respect the concept of "income velocity" is open to grave objection, even when it is regarded as a means for dealing with those forces which are directly relevant to the determination of "velocity" in the stricter sense of the term.

The basis for the demonstration of this proposition is the simple fact that, however the received definitions of "in-

[65] Otherwise, indeed, it is difficult to see why we should not be content, in the manner of Aftalion, to write simply $I = PQ$, the importance of the relation of I to the quantity of money being given no recognition in the equation itself. Cf. above, pp. 348 ff.

come velocity" differ otherwise, they agree in insisting that the denominator of the ratio measuring "income velocity" must be, not the sum of the cash balances held specifically against the necessity for making payments out of income, but the *total* stock of cash balances.[66] The reason for this is obvious if the concept of income velocity is regarded as doing no more than establishing the fact that there is a relation of some kind between the total of income payments and the "quantity of money." The reason is by no means so obvious, however, if the concept is regarded as contributing to an explanation of *why* this relation is what it is, so far as this explanation is associated with movements in something that is properly to be regarded as "velocity." For this problem, it may be repeated, in anticipation of the argument of Chapter XV, it is necessary to use what has been called the "cash-balance approach." As we shall see in Chapter XV, it is of the essence of the "cash-balance approach" that if we are to understand the nature of the forces determining the size of cash balances relative to outlay we must put ourselves in the *position of the administrators of the cash balances which are held against the type of outlay in which we are interested.*[67] We cannot accept, as a satis-

[66] On Schumpeter's usage, see above, p. 359, n. 32. Foster and Catchings arrived at their figure for "circuit velocity" by relating the amount "spent by consumers for new commodities" to "*the total amount of money in circulation*" (*Money,* 311 f.); Pigou stated explicitly that his "income-velocity" was "equal to the aggregate money-income accruing to the community in a year divided by the *aggregate stock of money*" (*Industrial Fluctuations,* p. 152); Angell ("Money, Prices, and Production," *loc. cit.,* 74 f.) obtained his figures for "circular velocity" by relating "national income" to the *total* of "cash and net bank deposits"; Currie's figure for "income-velocity" ("Money, Gold, and Income," *loc. cit.,* 91 ff.) was obtained by relating national income to the "average amount of *money outstanding* during the year." (Italics mine, throughout.)

[67] See below, pp. 417 ff. It will be seen, from the argument there presented, that its central pillar is the methodological proposition, held to underlie "modern" value theory, to the effect that the magnitudes recorded by the action of the market must be referred back to the choices of economizing individuals. In view of the fact that one of the virtues of the "income-approach" has been alleged to be precisely that it makes it possible for us to refer the phenomenon of the market back to the choices of "economizing individuals," there is therefore an element of paradox in the fact that it is precisely a group of "income-theorists"—namely, those who have regarded the concept of "income-velocity" as an analytical device alternative to older concepts of "velocity"—who are open to the charge of having failed to honor at all points the methodological criterion to which they acknowledge allegiance. The paradox is at least partly

factory starting point, the simple fact that the total of cash balances of *all* types bears a given relation to a total of expenditure of *certain* types. What we must do is to take each type of cash balance and study the nature of the forces which establish the relation of each type of balance to the *outlay against which that balance is being held.* It must, however, be obvious that it is precisely this type of apparatus for dealing with changes in "velocity" which the concept of "income velocity," in and of itself, does not offer us.

It is of considerable importance to stress the fact that this argument is not to be interpreted as contending that the use of the concept of "income-velocity" precludes the *subsequent* use of the type of apparatus in question—that is, that type of "cash-balance" analysis which would insist upon relating the aggregate of expenditure of certain types to the cash balances held specifically against such expenditure. All that is argued here is that the concept of "income-velocity," in and of itself, is not only useless for such analysis, but is actually likely to impede it by encouraging the complacent use of analysis which would relate the total of payments out of income to a magnitude—namely, the total of cash balances of all kinds—which is not that which enters into the calculations of those whose decisions with respect to spending and refraining from spending make the total of payments out of income what it is. It is in this sense that the concept of "income velocity" is to be regarded as a cumbersome and inadequate weapon even when the problems to which it is applied are problems of "velocity" in the strict sense of the term.

V

"Income Payments" and Income Velocity

The point of view presented in the preceding section of this chapter is of such great importance, as a matter of

resolved, to be sure, by a demonstration of the proposition that the types of "choice" which the "income-theorists" in question had in mind must also be assigned a rôle in the Theory of Prices. (See, on this matter, pp. 491 f., below.) It remains true, nevertheless, that the methodological criterion indicated was not honored by the writers in question in their treatment of the problem under discussion.

methodology, that it deserves further illustration. For purposes of this illustration, it is again unnecessary to go beyond certain aspects of "income-velocity" that have become clear as a result of the discussion which has already grown up about the concept.

The first of these aspects is revealed as soon as one examines this discussion in the light of the suggestion that the concept of "income-velocity" is to be taken as representing the ratio between the stock of money and the total of "income-*payments*." That this suggestion is implicit in the concept of "income-velocity" is clear from those definitions of the concept which ask us to conceive of the stock of money as "entering into" income a certain "number of times," or as being exchanged against "real income" a certain "number of times" during the given period.[68] What is not so clear, however, is the relation of this suggestion to the definitions of "income-velocity" which amount simply to the statement that the stock of money bears a certain relation to the total of money "income."

The importance of the issue involved becomes obvious when it is realized, in the first place, that the measures of "money-income" that have been used in statistical computations of "income velocity" would lead to the conclusion that the "income" to be compared with the money stock includes not only income in the *form* of money—which is all that it should include if "income" is to be conceived of as a sum of money *payments*—but also the *money value* of "imputed" incomes.[69] The importance of the issue becomes still more obvious when it is observed that not only Mr.

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[68] For examples of both types of usage—often found, unfortunately, in the same author without adequate realization of the importance of the distinction—see below, p. 379.

[69] It is not uncommon to find suggestions to the effect that the elements included in "imputed" income—in the form, say, of accrued income on owned property which does not take the form of a cash payment—may be put on the same plane with income received in the form of money by the simple convention of regarding such accrued imputed income as money income which has been received and then "turned back" into, or spent on, the property in question. There is no intention of denying here that, for some problems, such "conventions" may be adopted without adverse consequences. The reader himself, however, may judge, on the basis of what is said in the text, whether this sort of "convention" may be adopted safely in dealing with the type of problem with which we are here concerned.

Keynes, but some of the best known among the articulate defenders of an "income approach" have advanced an explicit argument to the same effect.[70] There can be no question of entering here upon an examination of all the issues to which the type of usage in question has given rise in the older literature. Our present task is rather to demonstrate that the issues involved in the broader problem provide a further illustration of our contention that, when "income-velocity" is defined as the ratio between the stock of money and the total of "income," it can be shown to include within itself the influence of factors that can be lumped with the factors determining "velocity" in the proper sense of the term only at the cost of underestimating seriously the degree of complexity which actually characterizes the problems with which the concept of "income-velocity" is supposed to deal.

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[70] Mr. Keynes's usage in this respect is typified by his statement, in the *Treatise* (I, 44), that his term for "aggregate money-incomes" should be understood to include not only income receipts in the form of cash, but also "income from the use of fixed consumption capital (e.g., houses)." Typical of Hawtrey's usage is his definition of "consumers' income" as "the total of incomes *expressed* in money," and his explicit statement to the effect that "income paid in kind" should not be excluded from "consumers' income" (*The Art of Central Banking,* 84, 89; cf. also the same author's *Capital and Employment,* 135). Similarly, Lindahl explicitly stated (*Penningpolitikens medel,* 13) that the *Q* of his income equation (see above p. 328, n. 78) "includes consumed goods and services which are not exchanged but still have an exchange value." Cf. also the usage of Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," I, *loc. cit.,* 65: ". . . income has nothing to do with the actual receipt of cash." Unfortunately, not all other protagonists of the "income-approach" have been equally explicit. When, for example, "income-velocity"—or "efficiency"—is defined as the number of times money "enters into" income in a given period (so Schumpeter, "Das Sozialprodukt," 673), one is led to suppose that the "income" referred to is a sum of income-payments in the form of money; yet uncertainty inevitably arises when "money-income" is defined as "merely the *monetary expression of goods consumed*" (*op. cit.,* 635). In other cases, the uncertainty as to a given author's meaning has derived from the fact that although the actual definition of "income-velocity" would lead one to suppose that the "money-income" involved was a sum of actual money payments, the figure for "income" which is used in the statistical computations of "income" velocity is one which includes a very large element of "imputed" income. See, for example, Currie, "Money, Gold, and Income," *loc. cit.,* 91, where, although "income velocity" had been defined as "the number of times . . . [money] is paid over to income receivers," the figure used for "income" contained a considerable amount of "imputed" income. (Contrast Angell, "Money, Prices, and Production," *loc. cit.,* 75, n. 4, and see Currie himself, "A Note on Income Velocities," *loc. cit.,* 353. Cf. also Angell, *The Behavior of Money,* 137 f.)

Concretely, the problem involved is the ancient one of determining the effect, upon the amount of money a country "needs," of the fact that a greater or smaller proportion of goods *produced* may be exchanged against money.[71] It can hardly be denied that if all goods produced were consumed by their producers, the amount of money "needed" by a country would be reduced to the vanishing point. Yet if this proposition is accepted, it follows immediately that the amount of money which a country "needs" to support a national "income" of a given level will also be greatly affected by the proportion of this national income which is accompanied by income payments in the *form of money* to that which is not accompanied by such payments. If the proportion becomes larger, a larger stock of money will be needed to support a given level of income; "income-velocity," that is to say, in one of its senses, will fall.[72]

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[71] See, in this connection, the discussion of the difference between "goods produced" and "goods produced for sale," in my article "The Definition of the Concept of a 'Velocity of Circulation of Goods,' " *Economica,* November, 1932, 450 ff., and the references to the literature there given; and cf. also pp. 540 ff., below. It is obvious also that Hayek's concept of a "coefficient of money-transactions" is relevant in this connection. See my discussion in *Economica* for August, 1933, pp. 293 ff., especially 296 f.

[72] An extremely rough indication of the degree of change which might be involved is provided by a comparison of the figures for "income velocity" given by Currie, "A Note on Income Velocities," *loc. cit.,* 354, col. ii, with the figures given in his earlier "Money, Gold, and Income," *loc. cit.,* 92, col. iii—the difference between the two being attributable to the fact that, in the second computation, the series for income used in the first computation were adjusted—very broadly, to be sure—in such wise as to include what purports to be income actually received in the form of money. If it be objected that the difference is not very considerable, it may be pointed out (1) that over longer periods, or in periods other than the one studied by Currie, it might be very much greater; (2) that the matter might be of very great importance in comparing "income-velocities" in two different countries; and (3) that, at the very least, there is a sufficient difference to warrant uttering a warning against splicing an income series containing imputed income with one omitting such income, for purposes of computing "income velocities" (cf. Angell, *The Behaviour of Money,* 137 f.), since the difference obtained is larger than that often registered by changes in income velocity as between any two years when the same series for "income" is used. For the rest, it should hardly be necessary to remind students familiar with the problems involved in measuring "national income" of the fact that, for practical reasons, many examples of "imputed" income are omitted from national income estimates; and that there is no assurance that if these types of income were included, the two series for national income—and therefore the figures for "income-velocity"—might not show greater divergences, even over short periods, than is shown by the comparison of Currie to which reference has been made.

Yet what a forcing of both language and analysis is involved in regarding the type of change in question as a change in "velocity"! All that has happened has been a change in the magnitude of the T of our Quantity Equation —this T representing the amount of goods and services actually exchanged against money.[73] There is no change in that magnitude—namely, the ratio of cash balances held against money outlay to the total of that money outlay—which, we have insisted, is alone relevant to the problem of "velocity," in the strict sense of the term.[74] We have here, therefore, merely another illustration of the danger, pointed out in the preceding section, of attempting to deal, under the head of "income-velocity," with a type of factor which is not, strictly speaking, a "velocity" factor at all.

The ambiguity in question would, to be sure, be avoided if those who use the concept of "income-velocity" would, in the future, make clear that by "money-income" they mean income *payments* actually made in money, and not income in all its forms, including imputed income. This, however, is very far from saying that we should thereby avoid all the ambiguity which has heretofore attached to the concept of "income-velocity." For it is easy to show not only that the term "income-payments" is itself an extremely ambiguous one, but also that this ambiguity provides a further example of the cumbersomeness of the concept of "income velocity" even when the latter is regarded as a weapon for dealing with the factors influencing "velocity," in the strict sense of the latter term.

Again the point will be made clear by calling attention to a distinction which has already been made—in this case,

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[73] The definition of T thus indicated is certainly the one which has been stated or implied by the better known sponsors of equations of the "Fisherine" type. See, for example, the references to the treatment of "barter" transactions by writers such as Norton and Fisher on p. 66, n. 74, above.

[74] It is of course obvious that a change in this ratio would be registered if, by the type of convention mentioned on p. 375, n. 69, above, we regard "money outlay" as including the money value of imputed real income; for, in that case, if no new factor intervenes to justify the holding of larger cash balances relative to *outlay in the form of money,* these cash balances will decline relative to "outlay" as newly defined. It must be obvious, however, that it would still be true that the change in the ratio indicated would reflect the effect of a type of factor quite different in its nature from "velocity" in the narrow sense of the term.

the distinction between payments *into* income, and payments *out of* income.[75] That both usages have been adopted by sponsors of the concept of "income-velocity" is beyond question. It is equally beyond question, however, that the importance of the distinction between the two usages has been obscured by the fact that, as often as not, "income" has been defined in such a way as to make the definition of "income-velocity" as the number of times money *"enters into"* income equivalent to the number of times it is *spent* out of income.

One way of accomplishing this result, of course, is to define "money income" as the "money value" of "real income." If, for example, in the manner of Schumpeter, we define the "sum of incomes" as being equal to the "money expression of goods consumed," it follows that to speak of money "entering into" income, in this sense, is equivalent to speaking of its entering into *outlay on consumers' goods*, with the result that the definition of "efficiency" as the number of times money "enters into income" becomes automatically equivalent to its definition as the number of times it is spent on consumers' goods—that is, equivalent to Foster and Catchings' "circuit velocity."[76] This, however, is merely

[75] See the discussion on pp. 354 f., above, of the relation between "income" and "outlay." For an example of explicit emphasis upon the distinction in discussions of the concept of "income velocity," see Lindahl, *Om förhållendet, etc.*, 9, and also the reference to Hawtrey's comment on the relation between Pigou's "income velocity" and Foster and Catchings' "circuit velocity," on p. 359, n. 33, above. Mr. Keynes, at one point in the *Treatise*, not only recognized explicitly the distinction between "payments in" to income, and "payments out" of income, but also pointed out, in the manner of Hawtrey, that "the volume of payments out" of income would be equal, not necessarily to "payments in" to income, but to the latter "*minus* any increase (or plus any decrease) in the amount of the income-deposits themselves" (*Treatise*, I, 43 f.; and cf. the references to Hawtrey on p. 354, n. 21, above). This did not, however, prevent Mr. Keynes from adopting a practice which amounted to the insertion of a term for "income" in what was generally understood to be a variant of a "stream" equation representing the stream of money expenditures upon a stream of goods. See the following note.

[76] For Schumpeter's definition of "efficiency" as the number of times money "enters into" money income, see "Das Sozialprodukt, etc.," *loc. cit.*, 673; and for his definition of the "sum of incomes" as being equal to the "money expression of goods consumed," see above, p. 360, n. 33. It may be pointed out, in passing, that this latter usage by Schumpeter, whatever may be said against it on other grounds, at least exonerates him from having committed an error in writing $E = MU$, in which E represents the "sum of incomes," and U his "efficiency" of money (see Schumpeter, *op. cit.*, p. 675). The same cannot be said for Keynes's persistent identification of *his* "income," likewise represented by E, but defined in such a way as to make it equivalent to money income *received*, with his M_1V_1, which, by virtue of the fact that it was used in an equation ostensibly designed to represent a stream of money against a stream of goods, could only have been meant,

another way of saying that there does not appear, in Schumpeter's scheme, a magnitude designed to measure the number of times money enters into "*money* income" when the latter is defined as income actually received in the form of money. It is of some importance to stress this fact, in order that Schumpeter's "efficiency" may not be confused with a concept of "income-velocity," called "income-frequency," and defined as the number of times money is "paid over to income receivers," which has been attributed to Schumpeter by later writers.[77]

That a confusion of this kind is extremely likely to arise as the result of a usage similar to that of Schumpeter may be seen from an examination of the treatment of the concept of "income velocity" at the hands of Robertson and Pigou. Robertson himself, in his *Banking Policy and the Price Level*, had defined his "velocity" as measuring the number of "occasions on which money changes hands *against final income*."[78] It is perfectly clear from the context in which this definition appeared that Robertson could have meant by "final income" only final "real" income. If so, it is obvious that his "velocity," after allowance for the fact that his "real income" presumably includes more than "consumers' goods," is equivalent to Schumpeter's "efficiency," which has to do with the ratio between the stock of money and *outlay* out of money income.[79] On the other hand, the adoption by Professor Pigou, in his *Industrial Fluctuations*, of the concept of a "period of circulation" of money would lead one to suppose that his definition of "income velocity" was intended to be identical with that of Robertson.[80] As it happens, however, Pigou's "period of circulation" had to do, not with

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despite Keynes's definition of "velocity" as the reciprocal of a proportion of income rather than of outlay, to represent *outlay* out of income, rather than income itself. For examples of Keynes's use of the equation $E = M_1V_1$, see the references given on p. 134, n. 79, above; and for the proper algebraic statement of the relation between income *received*, income *spent*, and "velocity," the essential element in which is the use of subscripts to indicate the time "periods" involved, see p. 382, n. 85, below.

[77] See, for example, Currie, "Money, Gold, and Income," *loc. cit.*, 91.

[78] Robertson, *Banking Policy and the Price Level*, 47 n. Cf. also the same author's "A Note on the Theory of Money," *Economica*, August, 1933, 244, where what is obviously intended as the equivalent of "income velocity" was spoken of as "the velocity of circulation of money *against output*" (italics Robertson's).

[79] If, by "real income," we understand only "goods and services which enter into ordinary consumption"—a usage implied in Robertson's *Money* (cf. above, p. 360, n. 33)—it is obvious that Robertson's definition of "income-velocity" is equivalent to Schumpeter's "efficiency" and Foster and Catchings' "circuit velocity" without important qualification. It would seem, however, from the usage cited in the preceding note from Robertson's "Note on the Theory of Money," that there is some evidence of vacillation, on Mr. Robertson's part, as to the precise meaning to be given to "real income."

[80] For Robertson's use of the concept of a "period of circulation of money," see *Banking Policy and the Price Level*, 47; for Pigou's, see the latter's *Industrial Fluctuations*, 136 ff., 152.

the number of times money was spent upon "real income," but with the number of times money "appeared" as money income, in the literal sense of the term.[81] Hawtrey was, therefore, undoubtedly correct in *contrasting* Pigou's "income-velocity" with the "circuit velocity" of Foster and Catchings, on the ground that the latter was concerned with *outlay* rather than with "income."[82] There can be little doubt, on the other hand, that the episode demonstrated again the ambiguity of the usage which has surrounded the concept of "income velocity," even when it is defined as the number of times money "enters into" something called "income." It is, therefore, hardly surprising that Professor Pigou should have thought it worth while, in a later publication, to emphasize the fact that some importance may attach to the fact that money income and expenditure out of income are by no means necessarily the same thing.[83]

It is, indeed, precisely this distinction between payments *into* income and payments *out of* income, which has so often been obscured in discussions of the concept of "income velocity," that is important for an understanding of the forces determining the ratio of cash balances to the outlay against which these cash balances are being held. This becomes immediately obvious when attention is called to a circumstance that has also been emphasized at an earlier point in our discussion—namely, that, for purposes of an "income-equation" which is designed to show *how income affects prices,* it is *outlay* out of income that is immediately

[81] See, for example, Pigou's *Industrial Fluctuations,* 136. The definition of what is obviously intended to be a variant of "income-velocity" as "the number of times per year [the quantity of money] . . . becomes income" appears also in Robertson's "Mr. Keynes' Theory of Money," *loc. cit.,* 402. Robertson seems to have regarded this definition as the equivalent of the "velocity of circulation" of money "against output" (*ibid.*). The two definitions will be identical, however, only if by money "becoming income," we mean its being spent on objects which represent *real* income ("output") —in which case Robertson's definition of "income-velocity" as the "number of times per year money becomes income" is not the same as Pigou's, despite the identity of the wording.

[82] Cf. above, p. 359, n. 33. See also what is said in the same note on the relation between Foster and Catchings' "circuit velocity" and *Robertson's* definition of "income-velocity"—particularly that given in the latter's *Money.*

[83] See *The Theory of Unemployment,* 193. The nearest, however, which Professor Pigou came to associating the point with the concept of "income velocity" is on pp. 196 f. of the same work, where, having estimated the "period of circulation"—that is, the reciprocal of "income velocity"—at four months, an attempt was made to "split this period of four months into two parts, namely (1) the average period between the receipt of money income and its expenditure, and (2) the average period between the expenditure of money income and its reappearance as somebody else's income."

relevant.[84] Given, to be sure, the figure for income *received* in the form of money, it is easy to establish the relation between this figure and that for *outlay* out of income. The relation, indeed, as we have seen, is to be represented by a figure for "velocity" which will be determined by the size of the cash balances held by income recipients relative to their outlay.[85]

This, however, represents only one of the problems with which the concept of "income-velocity" was intended to deal. As we have seen, the other problem—and the one which provides the clearest reason for refusing to accept the argument of those who see an irreconcilable conflict between the "income-approach" and one which emphasizes the importance of changes in the quantity of money for the determination of prices—is that of establishing the nature of the forces which make the level of money incomes what it is. For this problem, we need a type of analysis that is concerned with the movement of money *into* incomes. It follows that the type of "income-equation" needed for this purpose is quite different from that which is ordinarily presented as an "income equation." What we now need is an equation in which "money-income" is the *end term* of analysis—our problem being to establish the nature of the forces which will decide how much of a given stream of money expenditure will enter into final incomes and how much will represent merely "transit" payments.[86] For this purpose, we cannot be concerned—certainly not in the first instance—

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[84] See, in this connection, the comment on Aftalion's "income" equation, on pp. 354 f., above.

[85] If, that is to say, we let M_t represent the cash balances held by income recipients; I_{t_n}, the income which they receive in the form of money during the "period" t_n; and $(M_tV_t)_{t_{n+1}}$ their outlay out of this income during the "period" following the receipt of the income, then the difference between I_{t_n} and $(M_tV_t)_{t_{n+1}}$ will be reflected in V_t during the period when income is disbursed. That is, $(V_t)_{t_{n+1}} = \phi\,[I_{t_n}, (M_tV_t)_{t_{n+1}}]$. Cf. also p. 383, n. 88, below.

[86] A simple variant of the type of equation indicated is represented, for example, by equation (8), on p. 383, below, in which the total of payments into income is represented by the term $(PT)_t$. The usefulness of this equation should, however, be judged, not in isolation, but in the setting provided not only by the discussion of the following chapter, but also by the whole of the analysis presented as "lying behind" the variables of our Quantity Equation.

solely with payments *out of* income. Some payments out of income will, to be sure, enter at once into other incomes; but others will not. Similarly, of the outlay of traders and producers, some will not enter into final incomes; but others will. It is hardly necessary to labor the point that we are again dealing with factors which are not, in any real sense, associated with "velocity" at all.

The point may be demonstrated by a further elaboration of the algebraic formulation presented above. The elaboration consists merely of differentiating payments not only, as heretofore, according to whether these payments are made *out of* income, or *out of* what may be called "traders' receipts," but also according to whether they are made *into* income, or *into* "traders' receipts." Let us write

$$(PT)_i = (PT)_{i\cdot I} + (PT)_{i\cdot NI}, \tag{3}$$

the capital letters used as subscripts indicating the *destination* of the payments concerned, while the small letters used as subscripts continue to indicate the *source* of these payments. Thus, equation (3) tells us merely that payments out of income $(PT)_i$ go partly to aliment other incomes $[(PT)_{i\cdot I}]$ and partly to aliment that part of "traders' receipts" which is not the true income of the "traders" $[(PT)_{i\cdot NI}]$. Similarly, we may write

$$(PT)_{ni} = (PT)_{ni\cdot I} + (PT)_{ni\cdot NI}. \tag{4}$$

If we let $(PT)_I$ represent the total of payments *into* income, and $(PT)_{NI}$ the total of non-income receipts by traders, we have, by definition,

$$(PT)_I = (PT)_{i\cdot I} + (PT)_{ni\cdot I}, \tag{5}$$

and

$$(PT)_{NI} = (PT)_{i\cdot NI} + (PT)_{ni\cdot NI}. \tag{6}$$

Adding equations (3) and (4), and substituting on the basis of equations (5) and (6), we obtain

$$(PT)_i + (PT)_{ni} = (PT)_I + (PT)_{NI}. \tag{7}$$

By equation (1) we have

$$M_iV_i + M_{ni}V_{ni} = (PT)_i + (PT)_{ni}. \tag{1}$$[87]

Equation (7), however, shows us that we may also write

$$M_iV_i + M_{ni}V_{ni} = (PT)_I + (PT)_{NI}. \tag{8}$$[88]

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[87] Cf. above, p. 369.

[88] Since $(PT)_I$ represents total income received in the form of money, it is the same magnitude that was represented on p. 382, n. 85, above, as I. It is this I, as we have seen, which is related to *outlay* made from income during the period following the receipt of income through V_i. It follows, therefore, that if, as before, we let the subscripts t_n and t_{n+1} represent the two periods involved, we have $(V_i)_{t_{n+1}} = \phi[(PT)_{I\cdot t_n}, (M_iV_i)_{t_{n+1}}]$. Similarly, of course, we may write $(V_{ni})_{t_{n+1}} = \phi[(PT)_{NI\cdot t_n}, (M_{ni}V_{ni})_{t_{n+1}}]$. It may be noted, in passing, that this formulation, which represents, by virtue of the subscripts t_n and t_{n+1}, a simple application of so-called "period analysis,"

It will be seen, therefore, that the problem of determining the number of times the stock of money "enters into" money income involves, among other things, since it has to do with the ratio $[(PT)_I]/(M_i + M_{ni})$, the determination of the relation between $(PT)_I$ and $(PT)_{NI}$, this relation, in turn, being very largely affected by factors summed up in the T of our quantity equation.[89] Once we have solved the problem of determining why money income will be at a given level (that is, once we have dealt with the forces that determine the magnitude of payments *into* incomes), the problem of determining the magnitude of outlay *out of* income is reduced to a problem that is strictly one of velocity, namely, that of determining the magnitude of V_i.

That a failure to indicate the degree of complexity characterizing the phenomena which the concept of "income velocity" was intended to summarize should have led to the avoidance of problems which are of the greatest importance is hardly surprising. Nor is it surprising that, in a number of cases in which the concept of "income-velocity" was used, this sin of avoidance become transmuted into one of positive error.[90] It is undoubtedly true, on the other hand, that the

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provides an adequate answer to those who have implied that the "income approach" fails to recognize that, if it is true that incomes "govern" prices, it is also true that prices "govern"—and are indeed the same thing as—incomes. (See, for example, Ellis, *German Monetary Theory*, 184, and the references there given; also B. M. Anderson, Jr., *A Critical Analysis of the Book of Lauchlin Currie, Ph.D., The Supply and Control of Money in the United States* [pamphlet], New York, 1935, 32.) Actually, of course, the fact that it is as correct to say that prices determine incomes as that incomes determine prices had been explicitly recognized by figures of importance in the development of the "income approach." See, for example, the references to Wicksell and Wagner given on p. 313, n. 31, and p. 318, n. 47, above. That this proportion does not involve "circular reasoning" is, of course, immediately obvious when it is observed that the "income" which is "determined by," or "identical with," prices is $(PT)_{t_n}$, whereas the income which "determines" prices is the *outlay* out of this income in the subsequent "period"—that is, $(M_iV_i)_{t_{n+1}}$.

[89] Cf. above, p. 369, and especially pp. 404 ff., below.

[90] I have attempted to demonstrate the presence of error in the arguments of no less eminent sponsors of the concept of "income velocity" than Professors Schumpeter and Pigou, as well as other writers, in the second of my articles on "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " *Journal of Political Economy*, XL (1932), 477 ff. (Appendix C to this article should be read only in conjunction with the "Further Note on Holtrop's Formula for the 'Coefficient of Differentiation' and Related Concepts," *ibid.*, XLI [1933], 237 ff.) I hope, on a later occasion, to deal with the subsequent literature on the subject, including such comments as have been made by other writers on the article cited above. Cf., in the meantime, what is said in the following note, and also on pp. 556 ff., below.

concept *need* not have led to these errors.[91] The argument of this chapter has been, not that the concept of "income velocity," in and of itself, necessarily leads to analysis that is erroneous, but that it does not, in and of itself, lend itself to analysis of the desired degree of precision for a broad range of problems that must be regarded as central in any adequately developed Theory of Prices.

For an understanding of the position represented in this chapter, it is of some importance to stress the fact that it is with respect to the usefulness of the concept of "income-velocity" for the purpose of handling the particular problems indicated that objection is made to the concept here. It is, for example, perfectly possible that a case may be made for the use of the concept in the analysis of other problems. For example, there are certain aspects of the trade cycle in the treatment of which the reasons for variations in the level of money incomes may be regarded as lying outside the scope of the immediate analysis; and in such cases "income-velocity" may be introduced solely to call attention to the fact that these variations in the level of money incomes are somehow related to variations in the money stock.

Much the same thing may be said for a concept such as Professor Viner's "final purchases velocity."[92] One of the virtues of Viner's

[91] It was precisely for this reason that, in the article referred to in the preceding note, which, as its title indicated, was concerned solely with "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,'" I studiously refrained from entering upon a criticism of the concept of "income velocity" as such. The reason for so doing was the conviction that the concept of "income velocity" was involved in the issues there discussed only in the sense that the concept is such as to make the error involved *more likely to occur*. In other words, from the standpoint of the error to the exposure of which the article was devoted, the question as to what happens to "circuit velocity," or "income velocity" is relevant only to the extent that the error in question may be said to have encouraged the conclusion that changes in the "velocity of circulation of goods" would be accompanied by a compensating increase in the "trade-velocity" of money, so that no additional balances need be taken from the "income sphere" to support the larger volume of T. If this were true, "income" need not fall, and "circuit velocity" need not change. If, on the other hand, additional balances *are* needed to carry on the larger volume of "intermediate transactions," a larger total volume of cash balances will be needed to support a given level of money incomes, so that "circuit velocity" or "income velocity," or "efficiency" will fall. It is thus seen that the conclusion as to what happens to "income-velocity" is merely a corollary of the position one happens to hold with respect to "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods'"; the concept of "income velocity" itself is not at stake, except in the sense indicated above—namely, that its use made more likely the commission of the particular error with which alone the articles in question were concerned. Contrast the comments of Ellis, *German Monetary Theory*, 136, 152, 199; and see also pp. 561 f., below.

[92] See Viner, *Studies*, 367 f.

presentation, in addition to the fact that it suggests a healthy dissatisfaction with the uncertainty attaching to the meaning of the "income" involved in definitions of "income-velocity," is that it appears in a context which makes it clear that the author's primary interest is in "the international mechanism." There is, that is to say, no suggestion (in contrast with what one finds in the works of certain contemporary sponsors of the concept of "income-" or "circular-velocity") that the author regards his concept of "final purchases velocity" as a substitute for other devices for establishing the relation between money and prices. On the contrary, this latter problem is left, presumably, for those whose primary interest lies in the specific task of establishing the nature of the forces determining this relation, rather than in the working of the "international mechanism." It is, to be sure, a serious question whether it is wise to speak, in the case indicated, of a "velocity," instead of using a term—such, for example, as "the final transactions *ratio*"—which is not likely to lead, as in the case of the concept of "income velocity," to the impression that nothing more is involved in the determination of the ratio in question than a problem of "velocity" in the strict sense of the term. This, however, is a purely terminological question, which is of entirely secondary importance in comparison with the issues that are involved in the suggestion that the concept of "income-" or "circuit-velocity" is, in and of itself, a tool of sufficient precision for dealing with the relation between money and prices.

We cannot, however, leave the matter thus on a note of negation. As we have seen, the principal problem to which the concept of "income-velocity" was designed to call attention is a real problem. We *are* interested in the nature of the forces which determine the relation between the stock of money and the total of payments into and out of income; and, if we reject the concept of "income velocity" as a means of dealing with this problem, we must be prepared to provide a substitute. As it happens, Mr. Keynes, in his *Treatise on Money,* himself provided what may be regarded as a substitute. As it happens, also, this substitute was not only one which, in its general outlines, is likely to commend itself to future workers in the field, but is also one whose roots go back much further into "tradition" than does the concept of "income-velocity" itself. To those who are interested, as we are in this study, in the twofold task of restating the substance of received tradition on the subject of the Theory of Prices and of evaluating this tradition in the light of the impact upon current discussion of critical judgments such as those of Mr. Keynes, it is of some importance that Mr.

Keynes's contribution to the discussion concerning the concept of "income-velocity" should be evaluated in the light both of its historical antecedents and the controversies which have been aroused by his own contribution to the discussion in question. It is to this task that the following chapter is devoted.

CHAPTER FOURTEEN

The Alternative to "Income Velocity"

I

Income Velocity as a "Hybrid Conception"

MR. KEYNES minced no words, in his *Treatise,* in expressing his opinion of the concept of "income velocity," when the latter is defined as "the relationship between the total annual receipts of income-receivers and the average stock of money held for all purposes." It was, he insisted, "a hybrid conception having no particular significance." [1]

It should be obvious, from the closing paragraphs of the preceding chapter, that I should not be prepared to go so far as to argue that the concept of "income-velocity," no matter how circumspectly used, is a concept of "no particular significance." The concept is indeed of significance in that it calls attention to the fact that a given change in the money stock may result in a greater or smaller change in the level of money incomes. It is hardly possible, therefore, to disagree with Mr. Robertson's contention that, within limits, movements in "income-velocity" may be regarded as throwing light upon the question whether or not action is called for "on the part of the monetary authority," when that authority is interested in controlling the level of money incomes by means of its control over the quantity of money.[2]

To admit, however, that a given device is not without its value as a crude guide to policy is quite a different thing

[1] *Treatise,* II, 24.

[2] Robertson, "A Note on the Theory of Money," *loc. cit.,* 244. As was pointed out on p. 363, above, Mr. Robertson's defense of what amounts to the concept of "income velocity" was really incidental to his defense of the "'Cambridge' method of approach," as embodied in Pigou's cash-balance equation $M/P = kR$. In the present chapter, however, we shall deal only with those aspects of the arguments of both Robertson and Keynes which can be shown to be relevant to the concept of "income velocity," as such, leaving for later chapters a discussion of the "cash-balance" aspects of the so-called "Cambridge approach."

from admitting that it is adequate for the purposes of a type of *analysis* which would lay claim to a reasonable degree of refinement. One will certainly agree with Mr. Robertson that there can be no objection, for certain purposes, to *"expressing"* the "money stock . . . as a proportion of . . . [the] income" of a community.[3] It would certainly not be argued, however, that we can rest content with the mere "expression" of this proportion. We must explain why the proportion is what it is; and the real issue is whether the concept of "income-velocity," when interpreted as a device for dealing with the nature of the forces which help to determine why a given addition to the money stock will result in one degree of change in the level of money incomes at one time, and in another level at another time, can be regarded as an instrument of a sufficient degree of precision for the purpose in hand. It is in the light of this issue that judgment must be passed on Mr. Keynes's contention that the concept of "income-velocity," as thus far defined, is a "hybrid" one.

Unfortunately for an adequate understanding of the central problem involved, it is anything but clear that those who have contributed to a discussion of the concept of "income velocity" since Mr. Keynes launched his attack have agreed as to what aspect of "income velocity" was supposed to give the concept a "hybrid" character. It is therefore of some importance to emphasize again a proposition which was advanced in the preceding chapter: namely, that, for anyone who accepts the methodological implications of the "cash-balance approach," the crucial test as to whether a given concept of "velocity" has or has not a "hybrid" character is whether or not the concept in question relates a given type of cash balance to the *outlay against which the cash balances are being held.* If the concept in question does so, it is not "hybrid" in character; if it does not do so, it is. Every other criterion is, for our purposes, irrelevant.

If the criterion suggested be accepted as the decisive one —and it must be accepted by anyone who accepts the methodological implications of the "cash-balance approach"—it

[3] Robertson, "A Note on the Theory of Money," *loc. cit.*, 243.

can easily be shown that the defenses, and even the modifications that have been proposed by way of protecting the concept of "income velocity" against the charge that it is "hybrid" in character, cannot be regarded as satisfactory. This is certainly true, for example, of the proposal, by Professor Pigou, that the concept of "income velocity" be amended to the extent that the denominator of the ratio, instead of being represented by the "total stock of money," should include only what he has called the "active" part of the money stock, this "active" part including only the "units" of monetary stock which "become income at least once during the period under review." [4] The argument for this amendment, according to Professor Pigou, was that the figure for "income velocity" as newly defined would become a "genuine physical magnitude," in the sense that each of the units included in the "active" part of the money stock would appear "physically" in money income during the period in question.[5]

This reasoning, however, whatever may be said of its appeal to those accustomed to thinking in terms of what Holtrop has called the "motion-theory" approach to the problem of velocity, can hardly be expected to appeal to those who accept the methodological implications of the "cash-balance approach," of which, paradoxically enough, Professor Pigou has on occasion been one of the most distinguished among contemporary protagonists.[6] For it is of the essence of that approach that it is only by studying the forces affecting the decisions of the administrators of cash balances that we can understand why "velocity," in a satisfactory sense of the term, is what it is; and it is a fair corollary of the approach that what enters into the consciousness of the administrators of cash balances is not the "unit" of the currency of which cash balances are composed—say, the pound,

[4] See Pigou, *The Theory of Unemployment,* 194 ff. Pigou did not refer to Keynes's attack on the concept of "income velocity" in his attempt to redefine it; but the context in which Pigou's renewed discussion of the concept appears leads one to suspect that it was Keynes whom Pigou had chiefly in mind when he admitted that what amounts to "income-velocity" as ordinarily defined is "a mere arithmetic ratio without any physical significance" (*op. cit.,* 195).

[5] Pigou, *The Theory of Unemployment,* 195.

[6] Cf. below, pp. 416 ff.

or the dollar—but the whole of the cash balance itself. As long as we hold fast to the concept of a cash balance which is large or small relative to the outlay against which the cash balance is being held, we have something, as Professor Pigou himself argued in describing the advantages of the "cash-balance approach" some twenty years ago, which "brings us at once into relation with volition." [7] So long, on the other hand, as we are content to speak of "units" of currency entering "physically" into money income, we have something which not only "seems at first sight," but remains in fact "accidental, arbitrary, and more or less in the air." [8] If the "accidental" and "arbitrary" quality which attaches to the concept of "income velocity" is to be removed, it cannot be removed by the device of invoking the criterion of "physical" movement. It can be removed only by confining ourselves rigorously to concepts of "velocity" which relate a given type of cash balance to the outlay against which that cash balance is being held.

Indeed, it may fairly be asked whether the effect of Professor Pigou's emphasis upon "physical" movement into incomes as the criterion for the inclusion of "units" of currency in, or their exclusion from, the denominator of his revised measure for "income velocity" is not to increase, rather than to decrease, the "arbitrary" and "accidental" element in the latter concept. By the definition given to the "relevant" or "active" stock of money, as we have seen, any unit is to be included in that stock which "becomes income at least once during the period under review." In practice, however, Professor Pigou's figure for the "relevant stock of money" is derived by taking all parts of the money stock "except the part held as savings." [9] The implication, presumably, is that each "unit" of this stock—including what would correspond to the "Income Deposits," "Business Deposits A," and "Business Deposits B" of Keynes's *Treatise*—"becomes income at least once during the period under review." [10] Actually, of course, there is no reason

[7] See Pigou's *Essays in Applied Economics,* 179, and cf. below, pp. 417 ff.
[8] Pigou, *loc. cit.*
[9] Pigou, *Theory of Unemployment,* 196.
[10] See Pigou, *loc. cit.,* and the reference to Keynes's *Treatise* there given. For Keynes's classification of "deposits," see the *Treatise,* I, 35, 244. It will be observed that Pigou's inclusion of that part of Mr. Keynes's "financial circulation" which was represented by "Business Deposits B" as part of the stock of "active" money prevents the strict identification of his definition of "income velocity," even as amended, with Schumpeter's "efficiency of money," despite the fact that the latter also was supposed to apply only to the active, or "circulating" part of the money stock. Cf., on this matter, p. 359, n. 32, above.

whatever for supposing that all the "units" of currency contained in these balances "enter into income" *physically* "at least once during the period under review."[11] There is no good reason, for example, for supposing that a large portion of the "units" of currency contained in the balances held by "traders" ever enter "physically" into "income" at all; it is perfectly conceivable that they may enter "physically" only into payments between "traders." According to Professor Pigou's formal definition of the "relevant or active stock" of money, therefore, they would be excluded. Yet even if the money stock consisted—in Professor Pigou's own words—of "physically distinguishable pieces," so that it would be practically possible to exclude the units of currency in question from something called the "active" money stock, the "accidental" and "arbitrary" element would not thereby be excluded. It would still be true that whether a given "unit" of currency entered "physically" into money-income would depend upon factors which cannot by any reasonable stretch of the imagination be regarded as entering into the consciousness of the spenders of the units of currency in question.

The "accidental" and "arbitrary" nature of Professor Pigou's criterion becomes still clearer, moreover, when it is observed that, according to his definitions, a unit of currency is to be included in the "relevant" part of the money stock—that is, in the denominator in the revised definition of "income velocity"—if it "becomes income at least *once*."[12] In practice, however, Professor Pigou was reduced to the necessity of providing a figure for "income velocity" which was held to apply, not to each "unit" of currency, but to what he called "a representative £." The "income velocity" of his "representative £," he concluded, was for England, "in recent time," "equal approximately to 3." Has this figure the "physical significance" which Pigou attributes to it? Some "units" of currency enter into income once; some twice; some four times, and more. What, precisely, is the difference between saying that a "representative £" enters into income three times a year, although many of the £'s to which this figure for "income velocity" is attached do not enter more than once, and saying that all of the money stock enters into income twice, although many units in that stock do not, *within the period in question*, "enter into" income at all? Professor Pigou's computation of income velocity, as a matter of fact, really rests upon the latter type of convention; for, as we have seen, there is no reason whatever for assuming that all the "units" of currency which he includes in his statistical estimate of the size of the "relevant money

[11] Unfortunately, some of those who have criticized the concept of "income velocity" have implied that this difficulty applies to *all* variants of that concept. See, for example, B. M. Anderson, Jr., *A Critical Analysis of the Book by Lauchlin Currie, etc.*, 31 f. Actually, of course, it is strictly relevant only as against an argument such as that of Pigou, with its insistence upon "physical" movement. Cf. Currie's "Reply to Dr. B. M. Anderson, Jr.," *Quarterly Journal of Economics*, XLIX (1935), 696.

[12] Italics mine.

stock" enter into income "at least once." If this is so, the exclusion of that part of the "total money stock" from the "relevant" money stock would be justified only on the ground that the excluded portion could never under any circumstances enter into income. It is perfectly clear, from Professor Pigou's own exposition, that he makes no such absurd assumption.[13] Yet the evolution of monetary theory from the days of Montanari and Hume to the present has shown that it is only upon the basis of such an assumption that one can draw a sharp distinction between money held as a cash balance which is "active" or "in circulation," and that which is "inactive" or "out of circulation"; and that the only thing which the distinction can mean is simply that the two types of cash balance are to be regarded as having different velocities of circulation.[14] As we have seen, Professor Pigou's use of the concept of a "representative £" involves just such a basis of differentiation between the different parts of the "active" or "relevant" money stock itself. Is it too much to characterize the very differentiation between the "active" or "relevant" part of the total money stock and the part which is "inactive" and therefore "irrelevant" as involving a degree of "arbitrariness" which, in actual fact, could never be attributed to the concept of "velocity," as such, when that concept is properly defined?

Unfortunately, Professor Pigou is not the only defender of the concept of "income velocity" who, in the attempt to safeguard, and possibly modify slightly, the concept in the light of Mr. Keynes's charge that it was "hybrid" in character, has failed to see that the only adequate safeguard against "arbitrary" and artificial constructions of the kind which the concept of "income-velocity" was alleged to represent is an unyielding attachment to the methodological implications of the cash-balance approach. From the standpoint of this approach, for example, as well as from the standpoint of Mr. Keynes's own statement of the issue which is here under discussion, it is clearly wrong to imply, as did another defender of the kind of concept typified by "income velocity," that the "hybrid" character to which Mr. Keynes objected is removed by the fact that the payments relevant to a computation of "income-velocity" are

[13] See, for example, Pigou, *The Theory of Unemployment*, 197, on the effect of withdrawals of "savings deposits" from, or their addition to, the "income-expenditure circuit."

[14] On the distinction between cash balances "in circulation" and those which are not "in circulation," its place in the historical development of monetary theory, and its relation to the "cash-balance approach," see below, pp. 459 ff.

homogeneous in the sense that, instead of including such payments as those which are involved in "financial" transactions, they represent payments "from consumers to one or more of the various classes and levels of producers and dealers, and thence back to consumers." [15] If this criterion of "homogeneity" were relevant to the question whether a given concept of "velocity" is or is not a "hybrid" one, it would indeed follow, as these commentators have argued, that the older concepts of velocity—that is, those which represent a "comparison of the *total* supply of money with the *total* volume of money-using transactions of all sorts"—are themselves "hybrid" in the extreme.[16] In fact, of course, whatever may be said against these older concepts otherwise, they cannot be said to be "hybrid," so far as the implications of the cash-balance approach are concerned, in any sense other than that which would warrant our characterizing as "hybrid" "almost any summation or average of masses of economic phenomena." [17] The older type of ratio would still represent the ratio of the total of cash balances to the outlay against which *those particular cash balances* are being held; they would, that is to say, represent a "true" velocity.

Unfortunately, this was denied by Mr. Keynes himself in another part of his discussion.[18] The only effect of his denial, however, was to confuse the issues involved in the argument for a subdivision of a global figure for "velocity" which is a "true velocity" into sub-"velocities," each of which is likewise a "true velocity," with those issues which are involved in the proposition that "income velocity" is in a genuine sense a "hybrid concept." To say that "the expression V"—that is, the velocity of what Mr. Keynes called "cash-deposits"—is "an average of two different things, and in a sense is not a true velocity at all," is surely a misleading statement. The "two things" thus "averaged" are really not "different": they are both, by Mr. Keynes's own statement, "true velocities"; and if the average of the two "true" velocities is regarded as being itself "not a true velocity," it can be so characterized only in the sense in which an average that is widely different from the mode—say, an arithmetic average applied to a U-shaped distribution—may be said to be not a "true" indicator of the size of the units averaged. One

[15] J. W. Angell, *The Behavior of Money*, 131 n., 133.
[16] Cf. Angell, *op. cit.*, 131 n.
[17] Cf. Angell, *ibid.*
[18] See the *Treatise*, II, 22 f.

might, indeed, just as well argue that those "velocities" which Mr. Keynes himself characterized as "true" velocities are not in fact "true" velocities because a change in any one of these "true" velocities may come about, not as the result of a change in the velocity of *each* cash balance within the separate groups, but because of a shift in "proportions" between those balances within each group which have a high velocity and those balances which have a low velocity. There is, therefore, nothing at all absurd in the parallel which Mr. Keynes adduced in support of his contention that the velocity of "cash-deposits" is "in a sense not a true velocity at all": "the velocity of transport of London passengers by tram and train might increase without there being any change in the velocities of trams and trains, because of an increase in the proportion of passengers travelling by trains."[19] It would be literally true that "passengers to London" were being "transported" with greater "velocity" as a result of the increase in the proportion of passengers travelling by trains. In this case, as in the case of a change in the velocity of Mr. Keynes's "cash-deposits," we may argue, and we should argue, for a breakdown of the global figure for "velocity" into sub-"velocities"; but as long as we are careful in all cases to relate cash balances to the outlay against which these particular cash balances are being held, it cannot be denied that either the global velocity or any of the sub-"velocities" is "a true velocity."

This, however, is precisely what cannot be said of "income-velocity"; for the concept involves, at best, the comparison of a sum of cash balances with outlay which is not the outlay against which these cash balances are being held. It is, as Mr. Keynes argued, "the product of two quite different things": it is as if one "were to divide the passenger-miles travelled in an hour by passengers in trams by the aggregate number of passengers in trams and trains and to call the result a 'velocity.' "[20] *This* is what confers a "hybrid" character on the concept of "income-velocity," and prevents its acceptance as a tool having the degree of precision required in dealing with the phenomena which the concept itself was designed to summarize.

For unless we do relate a given volume of cash balances to the specific outlay against which the balances are being held, it is obviously impossible, in the first place, to explain

[19] *Treatise,* II, 25.

[20] *Treatise,* II, 25. The passage should be contrasted with that cited just above. It will be observed that the difference between Mr. Keynes's two uses of the analogy of passengers and passenger miles is categorical, despite the fact that in both cases Mr. Keynes used it as part of an argument designed to indicate the nature of a "true" velocity.

the magnitude of "income velocity" in terms of the decisions of the administrators of cash balances with respect to the amount of cash which they wish to hold relative to outlay—a matter which, if the methodological implications of the cash-balance approach are sound, must be studied if we are really to explain why the movements in "velocity" are what they are.

In the second place, we must face squarely the fact that "income velocity," in the sense of a ratio of "income" payments to the total of cash balances held for all purposes, will be greatly affected by a type of factor which has nothing to do with "velocity" in a sense of the latter term acceptable to defenders of the cash-balance approach. Specifically, income velocity will be affected not only by (1) a change in the ratio of cash balances to the outlay against which such cash balances are held, but also by (2) the ratio of income payments to nonincome payments, and by the ratio of the absolute amount of cash balances "absorbed" in holdings against nonincome payments to those held against income payments.[21] The argument against the complacent acceptance of the concept of "income velocity" as a tool having the desired degree of analytical sharpness is precisely that it fails to distinguish with sufficient clarity between these two problems, so completely different in their nature.

It is easy to point to consequences of the failure to distinguish the two elements involved, on the part of protagonists of the concept of "income velocity," which are anything but reassuring. In some cases, for example, sponsors of the concept, in discussing the forces determining the magnitude of "income velocity," have tended to write as if only

[21] In the notation suggested on p. 369, above, the part of the problem with respect to the relation between "income" and the stock of money which is strictly one of "velocity" would be summarized by the formula $(PT)_t:M_t$, in which $(PT)_t$ represents payments out of income, and M_t the sum of cash balances held by administrators of income against these payments. The formula for "income velocity," however, would be either $(PT)_t:M$ or $(PT)_I:M$; or—if we write $(PT)_t = (PT)_{t\cdot c} + (PT)_{t\cdot o}$, in which $(PT)_{t\cdot c}$ represents payments out of income against consumers' goods and $(PT)_{t\cdot o}$ represents payments out of income for all other purposes—it would be $(PT)_{t\cdot c}:M$. It is obvious that a study of the forces determining any of these magnitudes would have to deal not only with those determining the ratio $(PT)_t:M_t$, but also with those determining the ratio of PT to $(PT)_t$, $(PT)_I$, or $(PT)_{t\cdot c}$, respectively, as well as the ratio of $(PT)_{nt}$ to M_{nt}, the latter ratio alone presenting a problem of "velocity" in the sense in which this could be said of the ratio $(PT)_t:M_t$.

the first of the two elements indicated above were involved.[22] Nor can it be said that in those cases in which it has been recognized that something in the nature of the second of the two elements indicated above is involved, the analysis presented has been of the desired degree of articulateness. In some instances, for example, the factors which are associated with each element, instead of being segregated in such a way as to show their relation to the general type of influence which they represent, have been listed in hit-or-miss order, with the effect of obscuring the difference between the types of analysis which are required in each case.[23] In other instances, the analysis is obscured by the practice of arguing that "the term V"—as used, say, in the Fisherine equation—is *determined by* "income-" or "circular" velocity.[24] For those who accept the methodological implications of the cash-balance approach, there is obviously something strange in the suggestion that a magnitude which is directly controlled by the decisions of individuals with respect to the size of cash balance they wish to hold relative to outlay, is *determined* by a magnitude which, in turn, is affected not only by these decisions, but by much besides.[25] Admittedly, this circumstance will not

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[22] See, for example, Pigou, *Industrial Fluctuations,* 154 ff., 240.

[23] See, for example, the list of "factors that alter the circuit time of money" given by W. T. Foster, "The Circuit Flow of Money," *loc. cit.,* 471 ff., and H. B. Hastings, "The Circuit Velocity of Money," *loc. cit.,* 236 ff. It may be remarked, in passing, that the special definition of "circuit velocity" given by these two writers, which would relate it solely to expenditures out of income upon *consumers' goods* ($(PT)_{i.c}$, in the notation suggested on p. 396, n. 21, above) opened the way for the inclusion, in the lists presented, of factors which are still less closely related to those determining the size of cash balances relative to the outlay against which these balances are held than are the factors which are typified by the second element indicated in the text. Both lists, indeed, go still further and include factors which are related neither to the two types of factors indicated, nor to the distribution of expenditure between consumers' goods and other types of good: the whole being a particularly bad example of the degree of precision—or lack of it—which may be expected from the use of concepts of the type represented by "income-velocity."

[24] See, for example, Angell, "Money, Prices, and Production," *loc. cit.,* 45, 75.

[25] Strangely enough, Professor Angell implied the direct contrary when he suggested ("Money, Prices, and Production," *loc. cit.,* 45) that it is the *Fisherine V* which is dependent largely on "the prevailing forms of business organization," such as the degree of integration of industry. The basis for this statement, unfortunately, would seem to be an error which has already wrought havoc in discussions of velocity. Cf. the comment on this aspect of Angell's argument by H. S. Ellis, in the *Journal of Political Economy,* XLIV (1936), 695; and on the error as it appeared in the earlier literature, see my article, "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' II," *Journal of Political Economy,* XL (1932), 486 ff. In a later publication, Professor Angell has done something to correct the impression that he believes "circular velocity" to be essentially unaffected by the "forms of business organization," such as a change in the degree of integration of industry. See Angell, "The Components of the Circular Velocity of Money," *loc. cit.,* 257 f. One is left in continuing doubt, however, as to his understanding of the relation between

seem compelling to those for whom a clearly articulated cash-balance approach is not an absolute prerequisite for a discussion of phenomena which are properly to be regarded as being associated with the "velocity of circulation of money."[26] This, however, is a matter which the reader may be left to judge for himself on the basis of the discussion of the "cash-balance approach" presented in later pages of this study.[27]

II

Robertson versus Keynes on "Income Velocity"

There can be little doubt that, of the various defenses of the concept of "income-velocity" which appeared after Mr. Keynes launched his attack upon the concept, the most impressive was that presented by Mr. D. H. Robertson, to which reference has already been made.[28] It is easy to show, however, that its impressiveness derives, not from a weakness in Mr. Keynes's argument or from the strength of the concept of "income velocity" in itself, but rather from the twofold circumstance that Mr. Robertson's contentions involved: (1) a tacit but nevertheless welcome admission that the concept of "income velocity" must be supplemented by a set of concepts of a much more traditional type if it is to prove useful for purposes of *analysis*—as contrasted with its possible usefulness as a crude guide to a monetary policy; and (2) the imputation to Mr. Keynes's proposed alternative to "income velocity" of weaknesses from which it was actually free.

That Mr. Robertson's argument involved the first of these elements is clear, paradoxically enough, from the case which he adduced in order to demonstrate the supposed advantages of the concept of "income velocity." This case was that of

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"integration" and the *Fisherine V*, so that the episode may still be regarded as an example of the dangers inherent in that type of failure to adhere strictly to the implications of the cash-balance approach which is so greatly encouraged by concepts such as "income velocity."

[26] That Professor Angell would fall within this group, despite an occasional concern with the problem as to the "amount of cash, which producers (and others) require" (see, for example, "Money, Prices, and Production," 140 ff., 162), would seem clear from his extraordinary characterization of the cash-balance approach as "tautological" in nature ("The Components of the Circular Velocity of Money," *loc. cit.*, p. 263). On this latter proposition, cf. below, p. 419, n. 13.

[27] See below, Chapters Fifteen and Sixteen.

[28] Cf. above, p. 388, n. 2.

an integration in business which would involve no change in the "general velocity of circulation of money," but would nevertheless have the effect of increasing the ratio of total money incomes to the total stock of cash balances of all kinds—that is, "income-velocity."[29] By the insertion of the condition that there shall be no change in the "general velocity of circulation of money," we are obviously precluded from supposing that the change in the ratio represented by "income-velocity" could have come about either as the result of a change in the velocity of specific types of cash balance (the proportions of the different types of balance remaining the same) or as the result of changes in the proportions of the different types of cash balance (the "velocity" of each type of cash balance remaining the same). Under the circumstances, the explanation of the change in the ratio represented by "income-velocity" must be found elsewhere; and Mr. Robertson proceeded to find it, quite reasonably, in an increase in *the proportion of PT which is represented by "income-payments."*

More precisely, as Mr. Robertson himself suggested, what has happened as the result of the closer integration of business, is a decline in the "volume" of certain types of transactions—and specifically, in the volume of intermediate transactions. In terms of the concept which was suggested in Chapter XIII, and which will be dealt with again in the chapter following, there has been a decline in the "absolute" demand for cash balances for what may be characterized loosely as "middleman" purposes. A certain volume of cash balances then becomes, in Mr. Robertson's phrase, "redundant." We are again prevented, by our hypothesis of an unchanged "general velocity of circulation of money," from supposing that the cash balances thus released, or made "redundant," will, when added to other cash balances, increase the ratio of these cash balances to total outlay. We must assume, therefore, as Mr. Robertson did, that the "redundant" balances will be "spent [on] or invested" in other things than "middleman" items. If, as in the preceding chapter, we let $(PT)_t$ represent the total of

[29] Robertson, "A Note on the Theory of Money," *loc. cit.*, 244.

"income" payments (in the sense of payments out of income), and $(PT)_{ni}$ represent the total of non-"income" payments, it is obvious that what has happened is that MV has remained the same, and therefore that PT, which is the sum of $(PT)_i$ and $(PT)_{ni}$, has also remained the same. If, therefore, there has been a decline in $(PT)_{ni}$, there must be an increase in $(PT)_i$. $(PT)_i$, however, is the numerator of our formula for "income velocity"; and since the denominator of that formula—namely, the total stock of money—has remained unchanged, we obtain an increase in "income velocity."

No one recognized more clearly than Mr. Robertson that the "initiating change" (that is, the factor which has led to a change in "income-velocity") is not a change in "habits regarding the disbursement of income"—that is, a change in the ratio of cash balances held relative to outlay.[30] As we have seen, however, for any one who accepts those implications of the cash-balance approach which are common to all of its better articulated variants, nothing is to be properly regarded as a change in "velocity" unless it is registered in a change in the size of cash balances relative to outlay.[31] This, obviously, is equivalent to saying that what is involved, in the case under discussion, is no more a change in "velocity" than a change in the quantity of money substitutes created by banks is a change in "velocity." It can surely not be regarded as a virtue of the concept of "income-velocity" that it lumps the type of effect adduced by Mr. Robertson with those which are attributable to changes in "habits regarding the disbursement of income," and, by using the expression "income-velocity," suggests that nothing more is involved than a problem of "velocity" in a sense of the term which would seem proper to supporters of the "cash-balance approach" in its most satisfactory form. On the contrary, it must be clear that the whole of this type of analysis, instead of proving that the concept of "income velocity" is superior to the older type of analysis, involves

[30] Robertson, *loc. cit.*

[31] This is certainly true of those variants of the "cash-balance approach" which run entirely in money terms. On the relation of the "real balances" variants of the cash-balance approach to "velocity," see below, pp. 435 ff.

the tacit admission that if we are to explain why the movements in "income velocity" are what they are, we must fall back upon a type of analysis which would draw a sharp distinction between (1) changes involving a modification of the size of cash balances relative to outlay—whether what is involved is "general velocity" or the "velocity" of specific types of cash balance; and (2) changes involving a modification of the proportions of the total volume of payments which are represented by "income-" and "non-income" payments, respectively, and therefore in the absolute volume of cash balances which must be held against each type of payment.

Once the latter point is seen, it is much easier to evaluate Mr. Robertson's criticism of the positive aspects of the treatment accorded to "velocity" in the *Treatise*. Mr. Robertson correctly insisted that it is dangerous to confine our analysis solely to the forces determining the size of the cash balances which *income recipients* hold against outlay.[32] It is clear, also, that Mr. Robertson was right as to the reason why such a procedure is dangerous—namely, that cash balances which are not, at a given moment, held in expectation of expenditure of a given type, are "potentially" capable of being used for such a purpose, and therefore should be taken into account in our analysis.[33] It is of first importance to observe, however, that this would be a valid criticism of Mr. Keynes's procedure only if in fact he had seriously proposed that we should confine our analysis entirely to the figure represented by the ratio of the total of incomes to the total of "income-deposits." [34] Actually, how-

[32] A similar argument had been presented, in 1929, by E. Lindahl (*Om förhållandet, etc.*, 12). Lindahl's argument, however, which, paradoxically enough, had been directed against what he believed to be the implications of Keynes's earlier "real-balances" equation, led to a conclusion, not in *favor* of the concept of "income velocity," but rather—at least by implication—to one in favor of a type of analysis which would make use of Hawtrey's distinction between "consumers' balances" and "traders' balances"—that is, to a conclusion similar to that advanced in Keynes's *Treatise*, and supported by me in the text, above.

[33] Robertson, *loc. cit.*

[34] The problem is one aspect of the broader problem as to the need for a "plurality" of equations of exchange, if one of the equations used happens to be a "consumers' equation" of the form, say, $M_1V_1 = PO$. See, on this matter, pp. 519 ff., below.

ever, we should take, as Mr. Keynes's contribution to our understanding of the problem, not his simple translation of the term E into M_1V_1 (in which M_1 represents the total of "income deposits" and V_1 the velocity of circulation of these deposits), but the whole of his analysis with respect to the *relation between* the magnitude of "income-deposits" (M_1) and *other types of cash balance.*[35] As soon as this is done, it becomes obvious that an insistence upon segregating the various types of cash balance for the purpose of studying the relation of each type of balance to the outlay made from each does not mean indifference to the fact that the part of the total stock of cash balances with which we may at any one time be particularly concerned is "potentially" capable of being increased or diminished by changes in the relative magnitude of other segments of the total stock of cash balances. In short: Mr. Robertson's defense of the concept of "income-velocity" was in effect an admission that it must at least be supplemented by an apparatus of the type presented by Mr. Keynes as an alternative to "income velocity."

III

Keynes and the Traditional Alternative to Income Velocity

For those who are interested in the process by which received doctrine on the Theory of Prices has reached its present state, the conclusions reached in the preceding section of this chapter cannot be left without some indication of their bearing upon the relation of "received doctrine" to those parts of Mr. Keynes's analysis which may be regarded as representing a distinct advance over that of many of his most distinguished contemporaries. There can be little doubt that Mr. Keynes himself believed that in his positive analysis he was turning his back not only upon most current doctrine, but also upon "very ancient tradition."[36]

[35] For Keynes's own summary of this part of his argument, see the *Treatise*, Book IV, Chap. XVII, sec. ii (I, 265 ff.): "The Diffusion of a Change in the Total Deposits between the Different Kinds of Deposits."

[36] Cf. above, p. 357, and the references there given.

Yet it is easy to show, by an examination both of the degree of justification which may be credited to Mr. Keynes's critical utterances respecting the received treatment of the factor of "velocity," and of the historical precedents which may be held to lie behind the elements that made up the positive part of his analysis, that in this particular instance, Mr. Keynes, whether he realized it or not, was a traditionalist *malgré lui.*

Consider, for example, the validity of Mr. Keynes's charge that "the" concept of "velocity" is an "omnibus" conception. What our analysis has shown is that there are, indeed, certain concepts of "velocity" against which this charge can fairly be levied. It is a sound charge, for example, against the concept of "virtual velocity"; and, if the argument of the preceding section is sound, it is valid also against the concept of "income-velocity." The concept of "virtual velocity," however, has never taken hold in the literature with anything remotely resembling the tenacity that has characterized those concepts of "velocity" which are strictly translatable into a ratio between a cash balance of a given size and the outlay made from that cash balance.[37] As we have seen, also, the concept of "income-velocity," instead of being backed, as Mr. Keynes suggested, by a "very ancient tradition" which dates from "the earliest literature on our subject," is virtually a creation of our own day, and is therefore typical of devices whose final validity and usefulness may be expected to be established only as a result of the type of critical discussion that was provided by Mr. Keynes and may be expected to be provided by others. What Mr. Keynes was attacking, in this instance, was not one of the citadels of received doctrine, but an advanced outpost subject at any moment to recall as the result of well-directed fire from the enemy. If, as we have seen, Mr. Keynes's own shots succeeded at first only in arousing a spirited defense from within the outpost itself, it is also true that the position of the outpost has been seriously threatened, not only by the recruiting of new forces for the

[37] Again a more detailed history of the concept of "virtual velocity" must be left for the publication indicated in note 1 to p. 290, above.

attack, but also by defections from within the outpost itself, in the form of a frank acceptance of the only proposition that really matters—namely, that the concept of "income velocity" is, at very best, "a useful shorthand summary of the final effect of all rates of spending affecting incomes," and is not "very significant for the purpose of a close study of the problem of the value of money." [38] When account is taken of all that such an admission implies as to the nature of the substitute which must be used in place of the concept of "income velocity" whenever "close study" is required, the admission may be regarded as a virtually complete surrender on the central issue.

In its essence, as we have seen, the substitute which must be put in the place of "income velocity" whenever "close study of the problem of money" is involved consists of the proposal to subdivide the total stock of cash balances (and therefore the "velocity" which may be held to apply to this total stock) into significant subgroups of cash balances, each related to its own type of outlay, and therefore possessing its own "velocity of circulation." The "stock of money" is then related to the total of money income and to outlay out of income, not by a single figure for "velocity" which simply measures the ratio of money income to money stock, but by successive steps of analysis which show precisely how a given addition to the money stock becomes transmuted into money income and outlay out of income—the whole process being described in terms of the diffusion of the increment to the money stock through the groups of cash balances previously distinguished, and the outlay from these cash balances.[39] The question as to how far Mr. Keynes's substitute

[38] So Lauchlin Currie, "A Reply to Dr. B. M. Anderson, Jr.," *loc. cit.*, 696. For Anderson's attack on the concept of "income velocity," see pp. 31 f. of his pamphlet cited on p. 392, n. 11, above; and for a similar acceptance of Keynes's charge that the concept of "income-velocity," as ordinarily used, is "hybrid" in character, see Ellis, *German Monetary Theory*, 137.

[39] For Keynes's own summary of this aspect of his argument, cf. the reference given on p. 402, n. 35, above. It may again be observed that Keynes's account lost something in cogency because of the identification of "income" and "outlay out of income" which is involved in his equation $E = M_1V_1$, as well as his failure to provide an explicit algebraic notation for those stages of the process which are associated with *payments into income* (the $(PT)^I$ of our notation). It would be a simple matter, however, to

for the type of analysis—or lack of it—which has been most commonly associated with the concept of "income velocity" is consistent with "traditional analysis" reduces, therefore, to the question as to the nature of the precedents which can be found for (1) the subdivision of cash balances into significant subgroups; and (2) the association of this subdivision with the *income approach,* in the sense of its use as a means of studying the process by which changes in the money stock are related to changes in the level of money incomes.

So far as the first of these elements is concerned, it may be pointed out, to Mr. Keynes's honor, that he himself remarked that "a distinction closely analogous to" his own distinction between "Income Deposits" and "Business Deposits" was to be found in the proposition of Adam Smith that "the circulation of every country may be considered as divided into two different branches; the circulation of the dealers with one another and the circulation between the dealers and the consumers." [40] In truth, however, as was pointed out by one commentator on the *Treatise*—who, as it happens, was not very sympathetic with the type of analytical device in question—the "distinction . . . has turned up again and again in writings on money *since* the time of Adam Smith." [41] What is more to the point, moreover, is that most of the writers since Adam Smith who have made use of the "distinction" in question can be regarded as having been sufficiently aware of the second element in the alternative to "income velocity," as summarized above, to warrant the inclusion of their names in a history of the "income approach" to the Theory of Prices.

The distinction in question appeared, for example—with acknowledgments, of course, to Smith—in the writings of Tooke, who, as we have seen, has come to be regarded as one of the principal ancestors of contemporary protagonists of

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remedy these defects, though the algebraic statement, which can be made to appear very complicated, need not be presented here.

[40] *Treatise,* I, 35 n.

[41] Cf. Hayek, "Reflections, etc.," I, *loc. cit.,* 272 (italics mine). On this occasion, Hayek gave no references to the literature. Cf., however, the quotation from Thomas Joplin given by Hayek in his *Preise und Produktion,* 47 n.

the "income approach."[42] As we have seen, also, the distinction, in the guise of a differentiation as between "consumers' money" and "producers' money," appeared in the writings of Adolf Wagner, who was in turn under the influence of Tooke.[43] It appeared again in the writings of N. Johannsen, who, as we saw, was in close contact with Wagner.[44] It was Johannsen, who, in the "chart" contained in his *A Neglected Point in Connection with Crises*—a book which was known to Mr. Keynes—used the terms "Business Money" and "Purchase Money (mostly Income)" in a sense which makes them virtually identical with the "Business Deposits" and the "Income Deposits," respectively, of Mr. Keynes himself.[45] Surely here is an amazing bit of history of doctrine: a distinction that appeared in Adam Smith, and which provided the foundation for a solution that Mr. Keynes himself regarded as being (except for its crude adumbration in Smith) in conflict with the "very ancient tradition" on the subject, turns out to have reached Mr. Keynes through the writings of a heretic who can be shown to have been in close contact with the one academic economist of his day who in turn can be shown to have maintained unremittingly a tradition frankly derived from suggestions which are to found in Smith and Tooke![46]

[42] Cf. above, p. 314; and see also the reference to Joplin which is there given.

[43] Cf. above, p. 320.

[44] Cf. above, pp. 333 f.

[45] For evidence that Johannsen's book was known to Keynes—though it was not cited by the latter in connection with the distinction between "Income Deposits" and "Business Deposits"—see the *Treatise,* II, 100. There is no evidence, on the other hand, that Johannsen's *Der Kreislauf des Geldes* was known to Keynes. All the more striking, on that account, is Johannsen's attempt—much cruder, to be sure, than that of the later Keynes—to obtain statistical measures of the relative magnitude of the various types of "deposit." See *Der Kreislauf des Geldes,* 126, 128 n.; and cf. Keynes's *Treatise,* II, 7 ff., 27 ff. The extraordinary similarity between other parts of the argument of Johannsen and that of Keynes, both in the *Treatise* ("Saving and Investment") and Mr. Keynes's later writings ("The Multiplier"), will be commented upon in Volume II.

[46] The writers named in the text as having made use of a subdivision of cash balances roughly similar to that of Keynes are selected because they were links in a continuous chain of tradition, in the sense that each writer can be shown to have been influenced by a predecessor in direct succession from the original suggestion in Smith. A list which would pretend to completeness in any other sense would have to include not only such writers as Thomas Joplin (on whom see above, p. 405, n. 41), but also Léon Walras, whose distinction between "monnaie de circulation chez les consommateurs,"

The seeker for precedents for the type of analysis presented by Mr. Keynes is not, however, obliged to pursue ways which, if they extend in an unbroken line to one of the greatest of the fountainheads of "tradition," are nevertheless devious from the standpoint of influences active at the time Mr. Keynes wrote his *Treatise*. There is, after all, the case of Mr. R. G. Hawtrey, the power and brilliance of whose "income theory" of prices is not in the least diminished by the fact that almost all of its separate elements lay ready at hand in the works of earlier writers—especially since, from all indications, Mr. Hawtrey's argument was constructed in complete independence of these writers. Consider, for example, the first of the two elements indicated above as constituting the essence of Mr. Keynes's substitute for the type of analysis often associated with the concept of "income velocity": namely, the subdivision of the total of cash balances into significant subgroups. Mr. Hawtrey's "consumers' balances" are surely Mr. Keynes's "Income Deposits"—just as surely as his "traders' balances" are Mr. Keynes's "Business Deposits." As for the second element—namely, the association of this distinction between various types of cash balance with a variant of the income approach: it can hardly be denied that Mr. Hawtrey's variant of that approach is one of the most useful, if indeed it is not the most useful, now available. In fact, when one considers the sharpness with which Mr. Hawtrey etched in certain of the details of his exposition—the distinction, for example, between consumers' income and consumers' out-

"monnaie de circulation chez les producteurs," and "monnaie d'épargne" (see, for example, Walras's *Théorie mathématique de la richesse sociale* [1883], 59) is certainly relevant in this connection. As it happens, this aspect of Walras's monetary theory, like so many others, exerted very little influence on subsequent writers, though there has been an occasional exception to the general rule. I hope, however, on a future occasion, to demonstrate the possibility of constructing a generalized apparatus for dealing with the theory of money and prices along lines of a distinctly "modern" character, which can be shown to have been adumbrated in Walras's writings; and in this reconstruction and development of Walras's thought, the distinction in question will have an important place. For a further example of a subdivision of cash balances prior to Keynes—in this case, a division suggesting the distinction between Keynes's "Industrial Circulation" and his "Financial Circulation"—see Fanno, *Le banche, etc.*, 220 (cf. the same author's *Die reine Theorie des Geldmarktes, loc. cit.*, 33, and especially 36 n.).

lay, which was glossed over in Mr. Keynes's presentation—and the precision with which he succeeded in joining an "income approach" which was remarkably free from a concern with irrelevancies to a form of the "cash-balance approach" which was likewise remarkably free from irrelevancies, one's admiration for the achievement grows.

It required only a final touch to establish Mr. Hawtrey's claim to gratitude on the part of those monetary theorists who are anxious to establish not only as great a degree of continuity with the writers of the past as is consistent with an unforced interpretation of these earlier writers, but also as great a degree of agreement with contemporary writers as is consistent with the maxim that peace which is purchased at the price of truth is a peace that will not endure. This touch was provided by Mr. Hawtrey's recognition that, while his own positive analysis ran in other terms than those commonly associated with concepts such as "income velocity" and "circuit velocity," these concepts could be used in monetary analysis so long as they are kept within the modest limits proper to them.[47] The nature of these limits should be sufficiently clear from the argument developed above, which may be said to reduce to the simple proposition that the concepts in question must not be regarded as providing an excuse for failing to undertake a detailed description of the forces controlling the creation and the expenditure of money-income and the relation of both to the "stock of money." One may, if one wishes, go further, as Mr. Hawtrey did—so far can respect for tradition cast its beneficent shade!—and insist that the place in the Theory of Prices which is thus assigned to the "quantity of money" is such as to constitute a defense of that battered inheritance which, for all the diversity of connotation associated with it, continues to be referred to as "the quantity theory of money," against the assaults of those "income theorists" who have

[47] For Hawtrey's treatment of the concept of "circuit velocity," even as used by Foster and Catchings, see p. 342, n. 118, above. For a similar recognition of the possibility of using the concept of "income-velocity," within the narrow limits proper to it, by a writer whose own positive analysis runs in terms of a differentiation as between various types of cash balance and their interplay, see H. Neisser, "Der Kreislauf des Geldes," *loc. cit.*, especially 385 f.

regarded the demolition of the "quantity theory" as one of the major achievements of their own forms of the "income approach" to the Theory of Prices.[48]

IV

Keynes's Alternative and the "Quantity Equations"

A final comment is in order, before leaving our discussion of the alternative to the concept of "income velocity" which Mr. Keynes presented in his *Treatise*. It will be recalled that Mr. Keynes, in defending his own abandonment of Quantity Equations of the general Fisherine type, argued that these equations, no matter how carefully stated, did not succeed in "separating out those factors through which, in a modern economic system, the causal process actually operates during a period of change," and that "when we advance to the later stages of the argument and attempt to analyse the actual monetary problems of the day—the problem of the Credit Cycle, for example"—we are "compelled to discard" these older equations.[49] This was distinctly not the position adopted by some of the most eminent among the protagonists of an "income-approach." Professor Schumpeter, for example, had found it possible to present the substance of his own "income-theory," with its related concept of the "efficiency," or "income-velocity," of money, within the framework provided by a "Quantity Equation" which he himself characterized as being in its "external" aspect "completely identical with the Newcomb-Fisher equation."[50] It is of some importance, therefore, that we should examine the consequences of Mr. Keynes's lack of piety in this respect.

There can be little doubt, in the first place, that Mr. Keynes's categorical rejection of the older equations led to

[48] See, for example, Hawtrey's comment, in his review of Aftalion in the *Journal of the Royal Statistical Society*, XCVII, 341, on Aftalion's habit of contrasting the "quantity theory of money" and the "income theory," as involving a "false antithesis."

[49] See above, p. 288.

[50] Schumpeter, "Das Sozialprodukt," 675. Professor Schumpeter of course went on, rightly, to point out that "in fact" a different set of connotations was given his equation by virtue of the differing definition given to "velocity," and "the corresponding limitation of the magnitudes on the right-hand side of the equation to consumers' goods."

misunderstanding as to the precise scope of the strictures which he urged against the concept of "income-velocity." The nearest approach to a translation of his argument into the terms of the older equations, as we have seen, was his translation of the term E ("income"), into M_1V_1, in which M_1 represents the volume of "income deposits" and V_1 the "velocity of circulation" of these deposits. It is in this circumstance, as has already been pointed out, that one must seek the explanation of a charge such as that made by Mr. D. H. Robertson: namely, that Mr. Keynes had not done justice to the fact—which is "of the utmost importance"—that "under certain conditions money which has been imprisoned in what Mr. Keynes calls the 'saving deposits' and 'business deposits' may seep out, raise the aggregate of incomes and 'income deposits,' and drive up P." [51] As we have seen, the charge that the details of Mr. Keynes's *analysis* ignored this type of possibility is completely without foundation. There can be little doubt, however, that the Fundamental Equations, as such, tended to support such an interpretation. For, clearly, the whole of the analysis whose importance Mr. Robertson was at such pains to vindicate was supposed to be hidden away in that most "omnibus" of all "conceptions," the expression $(I-S)$.[52] We shall deal, in Volume II of this study, with Mr. Keynes's contention that "the traditional Quantity Equation" is capable only of dealing with the "monetary," as opposed to what he called the "investment" elements involved in the Theory of Prices. Here it should be sufficient to ask whether, when

[51] Robertson, "A Note on the Theory of Money," *loc. cit.*, 244.

[52] It will be recalled that Mr. Keynes, not content to relegate to the term $(I-S)$ the heterogeneous elements of analysis to which reference is made in the text, offered a new "Quantity Equation," of the form $M'V' = \Pi O$, in which M' represented the "volume of the Industrial Circulation, O the volume of output, and Π the price-level of output," and in which "V' is a complex notion not identical with V, the Velocity of circulation"—which he had just characterized as an "omnibus conception"—but was regarded as being "compounded of two elements;—one of . . . which is of a similar character to the traditional velocities of circulation, and the other dependent on the balance between Saving and Investment" (*Treatise*, II, 5). Strange, indeed, is a purported correction of the older Quantity Equations, with their "omnibus conception 'velocity of circulation,' " by means of the insertion of a new expression—namely $(I-S)$—the "omnibus" character of which, as we shall see in Volume II of this study, far surpasses anything that could be said to inhere in the concept of "velocity" as such!

we consider the elements which are involved in the "income" aspect of Mr. Keynes's analysis, and the relation of "velocity" thereto, it can really be argued that the Fundamental Equations of the *Treatise* actually succeeded in doing what Mr. Keynes insisted the older Quantity Equations were incapable of doing—namely, "separating out those factors through which, in a modern economic system, the causal process actually operates during a period of change." [53]

The truth of the matter, surely, is that, instead of being "compelled to discard" equations of the older type "when we advance to the later stages of the argument," as Mr. Keynes contended, we are once more forced to reintroduce precisely these equations, or some variant thereof. The proof of this, in fact, was provided by Mr. Keynes himself. For when he came to trace the actual process by which a change in the quantity of bank money becomes translated into a change in the level of money incomes, he was forced to revert to the methods of the older "quantity of money, velocity of circulation schools of thought" for which he was later to express such complete contempt.[54] In this instance, the only change that was introduced in the *form* of Quantity Equations of the Fisherine type—despite the important change in substance which the change represented—was the adoption of the simple device of breaking up the total of cash balances into segments, each with its own distinguishing subscript and each with its own "velocity of circulation." [55] In terms, indeed, of our own "Quantity Equation," in order to obtain a formulation capable of "separating out" the particular "factors through which, in modern economic systems, the causal process actually operates during a period of change," and which may be regarded as having been

[53] Cf. above, p. 123, and the reference to the *Treatise* there given.

[54] Cf. above, p. 15, and n. 20 thereto.

[55] See especially the *Treatise*, I, 265 ff., where Mr. Keynes undertook to "consider in more detail by what routes an increase in the total deposits distributes itself between the Savings-deposits, the Business Deposits, and the Income-deposits," from the "first effect of a new loan by a bank" and the associated "increase" in "the deposits of the borrower by the amount of the loan," through the effects registered in "an increased earnings-bill"—that is, the *income* of the factors of production—to increased cash holdings and cash outlay (M_1V_1) by the recipients of income.

"separated out" by those parts of Mr. Keynes's analysis which we have been considering, all that is required is a breakdown of the figure for V, representing a weighted average of the velocities of circulation of the different types of balance, into its components.[56] We thus obtain

$$(M+M')_1V_1 + (M+M')_2V_2 + (M+M')_3V_3 + \ldots (M+M')_nV_n = PT,$$

the subscripts referring to the various types of cash balance which it is felt desirable to distinguish and their respective velocities of circulation.[57] One should not, to be sure, stop at this point, if one is to do justice to the intricacies of the process of the transmutation of an increase in the quantity of money into an increase in the levels of money income and outlay out of that income—even though, as we have seen, Mr. Keynes himself did, in effect, stop just here, so far as his formal algebraic exposition is concerned.[58] At the very least, we should go on, in the manner suggested above, to present a formulation which makes it possible not only to trace the movements of the stream of money expenditure into and out of income, but also to show, by the use of a particularly simple form of what would now be called "period analysis," the nature of the time relation involved in the interconnection of the various segments of the total money stream. The important thing to observe, however, is that at no point in the argument does it become necessary to abandon "Quantity Equations" of the general Fisherine

[56] On V as a weighted average of the V's prevailing in different "spheres," cf. also Neisser, "Der Kreislauf des Geldes," *loc. cit.*, 385, and n. 1 thereto.

[57] It will be observed that the "objection to the Fisher Equation" which Keynes found in the fact that it neglected "to take explicit account of the distinction between Cash-deposits and Savings-deposits" (*Treatise*, I, 236), is hardly a criticism of the *general form* of the Fisherine equation, as one might have been led to suppose by the fact that the criticism occurs as part of the discussion of the older "alternatives" to the Fundamental Equations of the *Treatise*: a discussion which was supposed to show that "when we advance to the later stages of the argument and attempt to analyse the actual monetary problems of the day," these alternative equations become "quite ineffective for handling the elements which most matter." Nor can it be said that the "corrected" formulation of the Fisherine equation which Keynes himself provided (*Treatise*, I, 237,) was calculated to show how little fundamental change was really necessary in order to obtain an equation of the general Fisherine form which would be fitted for handling the type of issue here under discussion.

[58] Cf., for example, what is said on p. 379, n. 76, above, with respect to Keynes's use of the expression $E = M_1V_1$, and what such an expression implies with respect to the relation between consumers' income and outlay.

form. On the contrary, just as in our discussion of the forces determining the quantity of M' and the relation thereof to the "*modus operandi* of bank-rate," we found that the older Quantity Equations, or variants thereof, became indispensable "when we advance to the later stages of the argument and attempt to analyse the actual monetary problems of the day," we find that, in order to deal adequately with precisely those contributions which Mr. Keynes, among others, has made to our understanding of the relation between changes in the stock of money and the level of money incomes, including the rôle played in this relation by the element of "velocity," we are forced to revert to equations of the older type.

The moral, certainly, for an appreciation of the importance of honoring the criterion of continuity in the development of economic doctrine, should be clear. It becomes very much clearer, however, when it is remembered that the "Quantity Equations" with which Mr. Keynes wished to break once and for all included not only the "Fisher Quantity Equation," but the "Cambridge Quantity Equations" as well.[59] The issues thus raised have to do, of course, with the "cash-balance approach," of which the so-called "Cambridge Quantity Equations" represent a variant. In view of the fact that the "cash-balance approach" is absolutely crucial for an understanding of the forces determining the magnitude of the factor of "velocity" with which these chapters are concerned, it will be observed that the issues involved are great ones indeed. An attempt to deal with these issues in the light of the treatment of the "cash-balance approach" which is to be found in Mr. Keynes's *Treatise* is therefore presented in the following chapters.

[59] It will be recalled that the "Cambridge" equations were included among the "Quantity Equations" which Mr. Keynes wished us to abandon in favor of the "new pair of trousers" which were supposed to be represented by the Fundamental Equations of the *Treatise*. Cf. above, p. 15, n. 20.

CHAPTER FIFTEEN

The Cash-Balance Approach

I

KEYNES AND THE CASH-BALANCE APPROACH

IT SHOULD be clear, from the discussion in Chapters Thirteen and Fourteen, that I emphatically agree with those who would attribute a crucial importance to what has come to be called the "cash-balance approach." Our task, in this chapter, is to justify the emphasis thus accorded to this "approach," and to indicate the precise nature of the place it should occupy in an adequate Theory of Prices.

The task is made more necessary by virtue of the extraordinary treatment accorded to the "cash-balance approach" by Mr. Keynes. At the time of writing his *Tract on Monetary Reform,* he was one of its best known proponents, publishing, in token of his adherence, a "real balances" variant of the "cash-balance" type of equation. It is true that in this instance Mr. Keynes did no more than reproduce—unconsciously, of course—an equation published some forty years before by Léon Walras.[1] It is also true, however, that he must be accorded the historic merit of probably having done more to attract the attention of monetary theorists at large to the existence of such a thing as the "cash-balance approach" than did the collective efforts of Walras, Karl Menger, Alfred Marshall, and their respective intellectual descendants, as well as their common forebears. Yet, in the *Treatise,* Mr. Keynes announced abruptly that while he had "formerly" been "attracted by this line of ap-

[1] See my "Léon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," *loc. cit.,* 569 ff. For further details with respect to the history of the "cash-balance approach," and for a much more detailed treatment than can be presented here of the issues raised in this chapter as well as in the chapter following, the reader must be referred to the book on *The Velocity of Circulation of Money* mentioned earlier.

proach," it now seemed to him that it "only causes confusion," and could not be regarded as providing "any real insight" into the problems with respect to the Theory of Prices which he had come to regard as really central.[2]

To those interested in establishing the fact of continuity in the development of the Theory of Prices, it is precisely this kind of episode which is most disturbing. The fact that Mr. Keynes was himself a figure of some importance in the history of the cash-balance approach was bound to confer an unusual degree of authority upon the criticisms which he chose to direct against it. Obviously, however, the question as to the merits of a given analytical device, such as that which is represented by the "cash-balance approach," cannot be settled by merely appealing to the authority of those who happen to have sponsored it in the past; the question must be settled on the basis of the inherent logic of the device itself. By way of disclosing the nature of the "inherent logic" of the cash-balance approach, it is proposed, in this chapter, (1) to state the relation of the "cash-balance approach," and of the particular "quantity equations" in which that approach is embodied, to the variables of the Quantity Equations which are serving as the skeleton for the whole of our presentation of received doctrine upon the Theory of Prices; (2) to examine the statements of Mr. Keynes with respect to the implications of the cash-balance approach, with a view to determining whether these statements are justified; and (3) to draw certain conclusions, upon the basis of the findings thus obtained, with respect to the rôle of the cash-balance approach in an adequate Theory of Prices.

II

The "Marshallian K"

The symbol which best summarizes the substance of the "cash-balance approach" is what Mr. D. H. Robertson has called "the Marshallian concept K." [3] As it happens, Mar-

[2] *Treatise*, I, 229.
[3] See Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 403, n. 1.

shall himself did not use the symbol K, or any other symbol, to summarize the central contention of his argument. This, however, may be taken as an advantage, rather than a disadvantage; for what it means is that, by speaking of the "Marshallian K," rather than Pigou's "K," or Keynes's "K," or even Robertson's "K"—each of which was differently defined—we are able to connote the general point of view represented by the "cash-balance approach" without being confined to any one of the variants presented by adherents of the general notion that is summarized by "the Marshallian concept K."[4] It follows that, within the limits suggested by the general implications of the cash-balance approach, we may give to our own K any definition we choose. Fortunately, we are able to find a precedent for our definition of K in a part of Mr. Keynes's *Treatise* which, interestingly enough, was not directly concerned with the cash-balance approach.[5] For our purposes, K is to be defined simply as the *proportion between outlay of any given type and the cash balance held against that outlay.*[6]

Fortunately, also, Mr. Keynes, in the part of the *Treatise* to which reference has been made, presented a satisfactory statement as to the mathematical relation between K, as so defined, and the "velocity of circulation" of the cash balances to which the K is relevant: namely, that K is simply the

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[4] Sufficient indication of the diversity among the definitions assigned to K by the writers named is provided if it be noted that (1) Pigou's k, as used in his "The Exchange Value of Legal-Tender Money" (*Essays in Applied Economics,* 177 ff.), represents the *proportion* of the "total resources [R], expressed in terms of wheat, that are enjoyed by the community"; (2) Keynes's k, as used in his *Monetary Reform* (84 ff.), is essentially the equivalent, not of Pigou's k, but of his kR, though a further ambiguity is introduced by the fact that not all readers were certain whether Pigou's R is to be thought of in terms of wealth ("resources," in that sense), or of annual real *income* (see, in this connection, Pigou, "The Monetary Theory of the Trade Cycle," *Economic Journal,* XXXIX [1929], 185, and Keynes's *Treatise,* I, 231, n. 2); and (3) Robertson's K', as used in the second edition of his *Money* (p. 195; cf. also "Mr. Keynes' Theory of Money," *loc. cit.,* 403, n. 1), is "the proportion of T ["the real annual volume of transactions"], which people wish to have enough money on hand to conduct." Cf., on the other hand, what is said with respect to the differences within the cash-balance "family" in my "Léon Walras, etc.," *loc. cit.,* 599, n. 74.

[5] See the *Treatise,* I, 43 ff.

[6] Thus, Keynes defined his k_1 as "the proportion of the income-deposits to the income-transactions," and k_2 as "the proportion . . . of the business deposits to the volume of the business transactions" (*Treatise,* I, 48).

reciprocal of the "velocity of circulation" of those balances.[7] Simple as the proposition is, its self-evident character has often been denied—sometimes explicitly, and sometimes by the implicit terms of an argument purporting to establish the "relation" between the cash-balance approach and the type of approach embodied in an equation of the general Fisherine form.[8] Indeed, as we shall see, Mr. Keynes himself may be fairly charged with having forgotten the existence of the simple expression $K = 1/V$ in the formal discussion of the "cash-balance approach" which is to be found elsewhere in his *Treatise*.[9]

The explanation of this extraordinary fact need not be sought further afield than in the simple circumstance that Mr. Keynes did not bother to state, at any point in his *Treatise*, the nature of the implications which may be said to underlie the expression $K = 1/V$. Yet, despite the fact that their neglect is evidenced in a very large number of instances in which writers have attempted to formulate the implications of the cash-balance approach as such, these implications are not only of extreme simplicity in themselves, but have been stated with perfect clarity by some of the most eminent protagonists of the cash-balance approach. Reduced to its simplest terms, the argument which is symbolized by "the Marshallian K," when defined as we have defined it, is as follows:

The "velocity of circulation of money" is—in the words of Mr. Robertson—a "phenomenon of the market."[10] No one, however, who accepts the methodological foundations of "modern" value theory can believe otherwise than that the phenomena of the market, if they are to be explained at all, must be explained as resulting from the decisions of

[7] Cf. the *Treatise*, I, 49.

[8] The list of these instances is so long and the contexts so varied that a discussion of them must be left for my book on Velocity of Circulation.

[9] The "formal discussion" referred to is that which appears on pp. 222 ff. of the first volume of the *Treatise*. See below, pp. 421 ff.

[10] Cf. Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 398. Mr. Robertson was, to be sure, discussing the Fisherine *equation*, rather than the V of that equation; but it should be clear, from the argument in the text, that so far as the general *type* of equation is concerned, the difference between what Mr. Robertson called "the Fisherine type" and what he called "the Marshallese type" should be regarded as turning entirely upon the treatment accorded to the factor V.

"economizing" individuals, whose choices make these market values what they are. It is, indeed, hardly an accident that writers, such as Walras and Menger, who, in their writings on the general theory of value, were insistent upon referring the phenomena of the market back to the choices of "economizing" individuals, should, in their writings on money, have also been protagonists of the cash-balance approach—though it is not without interest to notice that the proposition of Menger, for example, to the effect that a satisfactory account of the forces determining an economic community's "demand for cash" must begin with the forces determining the demands for cash of the *individuals* who make up the "economic community," had revealed itself to the sound sense of the earliest "cash-balance theorists" long before the "methodological principles of modern value theory" had been formally developed.[11] The important point for our present purpose, in any case, is the corollary to be drawn from the general methodological principle when it is applied to the special case in hand. This corollary is simply that if, instead of being content merely to record the fact that "velocity" is at a given level, we wish—in the words of Marshall—to unravel the "*causes that govern* the rapidity of circulation of the currency," we must, as always whenever we attempt to explain how market values are determined, put ourselves in the position of the individuals engaged in

[11] It was, after all, Petty who, in a passage cited by Marshall (*Money, Credit and Commerce,* 47) as an example of the cash-balance approach, had remarked that, in considering the demand for cash of a "whole Nation," we must remember that "the whole Nation . . . is but many particular Men united" (*Quantulumcunque concerning Money,* Qu. 23 [*The Economic Writings of Sir William Petty,* II, 446]). On the relation of Walras's monetary theory, with its emphasis upon the "economic actions of individuals," to Walras's general "conceptions of economic theory," see T. Wessels, *Die Geldtheorie Léon Walras* (Cologne, 1925), 20, 22, 24, 31. On Menger's monetary theory, from this standpoint, see A. Nielsen, *Bankpolitik,* II, 122; also F. A. von Hayek, "Carl Menger," *Economica,* November, 1934, 414 (cf. also p. 406): and for a particularly explicit statement by Menger as to the necessity, in constructing "a theory with respect to the forces determining an economic community's demand for cash," for taking, as a "point of departure," the "demand for cash by *individuals*," see Menger's article *Geld* (*The Collected Works of Carl Menger,* IV, 112). In this respect L. von Mises is a true disciple. See especially Mises's *The Theory of Money and Credit,* 131 ff.; and cf. the same author's "Die Stellung des Geldes im Kreise der wirtschaftlichen Güter," *loc. cit.,* 312 f. On the broad question of the relation of the cash-balance approach to the "general theory of value," see also pp. 440 ff., below.

market processes, and ask what they do which is relevant to the particular market process in which we are interested, and why they do it.[12]

In the present instance, what individuals do is to retard or advance the rate *at which they spend their cash balances.* The movements in "velocity" are the simple resultant of the decisions which the individuals in charge of the administration of cash balances make with respect to the size of the cash balance that they choose to keep relative to outlay. The "Marshallian concept K" is the symbol which we use to summarize the body of analysis designed to explain why these decisions are what they are.[13] As Mr. Robertson has put it, the symbol is designed to call attention to the "phenomenon of the mind" which lies behind the "phenomenon of the market" which, in turn, is represented by the V in equations of the Fisherine form.[14] It follows, therefore, that, in strict logic, every "Fisherine" equation which contains an analytically satisfactory concept of "velocity" can be transformed into a "cash-balance" equation by the simple device of writing the V of such equations in the form of $1/K$.[15] It follows, also, that, whether or not we bother to rewrite the V of the Fisherine equations in the form of $1/K$, we *must* use the body of analysis which is summarized by the "Marshallian concept K" in order to explain why the movements in V are what they are. The "Marshallian concept K," in other words, and all that it symbolizes, is simply part

[12] For the quotation from Marshall, see the latter's *Money, Credit and Commerce,* 43, 48.

[13] It is obvious, therefore, that one cannot dismiss the whole of the cash-balance approach by insisting that it reduces to the "merely tautological proposition" that "the money stock is what it is [in size relative to outlay]" (cf. Angell, "The Components of the Circular Velocity of Money," *loc. cit.,* p. 263). The fact that "the money stock is what it is" relative to outlay—*i.e.,* the magnitude of K—is the *datum* from which cash-balance analysis *starts*; it is the function of the body of analysis summarized by the term K to *explain* why the ratio in question is what it is.

[14] Cf. above, p. 417, n. 10.

[15] Attention may be called to the fact that the concept of "velocity" involved must be "analytically satisfactory." What this amounts to, of course, is the proposition that a given concept of velocity can be regarded as "analytically satisfactory" only if it makes sense when written $V = 1/K$, the K being defined as in the text. An examination of the instances in which an attempt has been made to show that V is not necessarily equal to to $1/K$ cannot be undertaken here. Cf., however, what is said on pp. 432 ff., below.

of the analysis which "lies behind" the V of our Quantity Equations.[16]

Nothing could be simpler; nothing could better demonstrate how "approaches" to the Theory of Prices which may have seemed to their sponsors, and to observers, to be in fundamental conflict with alternative "approaches," are in fact nothing more than a necessary complement to these supposed alternatives.[17] Yet nothing could have been more completely obscured by the details of the treatment that Mr. Keynes accorded to the "cash-balance approach" in the pages of the *Treatise* which he devoted specifically to a discussion of that approach. In the interest of clarity, therefore, it is necessary to examine the treatment accorded by Mr. Keynes to the "cash-balance approach" in the *Treatise*, with a view to disentangling the heart of that approach from the mass of irrelevancies in which Mr. Keynes, along with others, managed, unfortunately, to involve it.

III

Relevance and Irrelevance in the Cash-Balance Approach

The task of separating the unessential from the essential elements of the cash-balance approach may best be handled by laying down a series of propositions which ought never to have been brought into question:

1. *The shortcomings of the concept of "income-velocity" are not to be attributed to the cash-balance approach, as such. On the contrary, as we have seen, the concept of "income-velocity," as most commonly used, violates one of the cardinal principles of the cash-balance approach.*[18]

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[16] It is, of course, true that if K is defined in a different way from that in which we have defined it—say, as equivalent to the K of the Keynes-Walras equation $n = pk$—it may be said to "lie behind" not only the V of our equation, but the T, as well. Cf., however, what is said on this matter on pp. 436 ff., below.

[17] Contrast, in this connection, what is said by Mises with respect to the relation of the "cash-balance approach" to the concept of "velocity" and equations of exchange of the "Fisherine" type generally, in his "Die Stellung des Geldes, etc.," *loc. cit.*, 313, 315.

[18] Cf. above, pp. 368 ff., 373 f., 390 f.

Mr. Keynes's confusion of the issues involved in the cash-balance approach with those associated with the concept of "income-velocity" was due primarily to the accident that such a confusion may, in a sense, be said to have been involved in the particular "cash-balance" equation presented by Professor Pigou.[19] In that equation, the sum of cash balances to which "the Marshallian concept K" was related was the *total* of cash balances of all kinds, rather than the sum of those cash balances which represented what Mr. Keynes called "income-deposits." As we have seen, the volume of payments against which this *total* volume of cash balances must be regarded as being held is the *total* volume of payments.[20] The $(1/P)\cdot R$ of Pigou's equation, however, is not the *total* volume of payments; at best, it is the volume of "income-payments." [21] Pigou's equation, therefore, which involves the same kind of comparison as does the concept of "income-velocity," is open to the same type of objection as is the latter concept.[22] That this fact, however, does not constitute an objection to the cash-balance approach as such, is evidenced by Mr. Keynes's procedure in that part of the *Treatise,* to which reference has already

[19] For Keynes's discussion of Pigou, in this connection, see the *Treatise,* I, 231 ff. Cf. also what is said on p. 363, and in n. 2 to p. 388, above, in connection with Robertson's defense of the concept of "income-velocity."

[20] Cf. above, pp. 373 ff.

[21] The ambiguity as to the meaning of Pigou's equation was, of course, increased by the fact that it was not clear whether $(1/P)\cdot R$, assuming that it was to be conceived of as a sum of "income-payments," rather than as merely a way of measuring the absolute volume of cash balances (that is, as incidental to the statement of the magnitude $(1/P)\cdot kR$), was intended to represent the amount *purchased* by "incomes"—in which case $M\cdot(1/k)$ would represent payments *out* of income and $(1/P)\cdot R$ the amount purchased by these payments—or to represent the amount going *into* incomes—in which case $M\cdot(1/k)$ would represent the payments made to income receivers for the production of R, the latter having the money value $(1/P)\cdot R$. Since, however, Pigou himself undertook to relate his equation to the "Fisherine" equation, it is to be supposed that the first of the two meanings was intended. (It will be observed that I am here using P in Pigou's sense of the *inverse* of the price level, and not as on p. 388, n. 2, above, where the P is the "price-level" itself.)

[22] This argument was more implicit than explicit in Keynes's discussion of Pigou. See, however, Keynes's discussion, in the *Treatise,* I, 232, of the "chief inconvenience of the 'Cambridge' Quantity Equation" as residing "in its applying to the total deposits considerations which are primarily relevant only to the income-deposits, and in its tackling the problem as though the same sort of considerations which govern the income-deposits also govern the total deposits."

been made, in which a formulation was presented showing perfectly clearly that the substance of Pigou's formulation may be retained by the simple device of redefining both the M of that formulation, on the one hand, and, on the other, the "real" element (R) in the "volume of transactions" against which the cash balances, as newly defined, are kept.[23]

2. *There is no reason why the cash-balance approach should be regarded as being relevant only to the problem as to the forces determining the size of cash balances held against payments out of* income.[24] *It is applicable to all cases involving the holding of cash balances of a given size relative to outlay, regardless of the nature of this outlay.*

This proposition follows directly from the fact that not merely the recipients of income, but *all* holders of cash balances, must weigh "the comparative advantages of holding resources in cash and in alternative forms."[25] This was clearly implicit in the arguments of exponents of the cash-balance approach from the earliest times down to Marshall

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[23] See the *Treatise,* I, 44 ff. In the notation suggested above (p. 369), the M of Pigou's formulation would be replaced by M_4, representing the volume of "income-deposits," and the R would be replaced by $(PT)_4$. Alternatively, of course, we may define M as the total of cash balances of all types and substitute for R the inclusive T of the Fisherine equation. See, in this connection, the references to Robertson and Ellis on p. 455, n. 112, below.

[24] An implication to the contrary was provided by Keynes's suggestion, in his discussion of "the 'Cambridge' Quantity Equation," that "the Cambridge method" was to be applied to income deposits "alone" (*Treatise,* I, 232). He referred, moreover, for illustration of this principle, to Chapter X of the *Treatise,* in which it is literally true that the only types of "deposit" which are shown in the Fundamental Equations are "income-deposits"—a practice which not only makes understandable Robertson's misunderstanding of the nature of Keynes's alternative to the concept of "income velocity" (cf. above, pp. 401 ff.), but, coming as it did in conjunction with the statement that "the Cambridge method" was to be applied to the "income-deposits alone," certainly gave countenance to the suggestion that Keynes might have denied the proposition stated in the text, despite his explicit practice to the contrary elsewhere in the *Treatise.*

[25] Cf. the *Treatise,* I, 232. Keynes's own statement, in the same paragraph, that the term representing the "resources" involved "ought not to be interpreted, as it is interpreted by Prof. Pigou, as being identical with current *income*" is in agreement with the argument advanced in the text above. Unfortunately, however, his statement that "the prominence given to k . . . is misleading when it is extended beyond the income deposits," was, when taken in conjunction with the statements cited in the preceding note, certainly bound to create the impression that he would have denied proposition (2), as stated in the text.

and Hawtrey and Robertson.[26] The contrary impression is probably to be attributed to an absurdly narrow interpretation of the implications of the proposition that the weighing of the relative "advantages" of holding cash and of holding wealth in other forms is nothing more nor less than a balancing of the "utility" to be derived from each type of holding—it being alleged, or implied, that calculations of "utility" are relevant only to consumers and consumers' goods.[27]

[26] See, for example, the quotation from Petty given by Marshall, *Money, Credit and Commerce,* 47, in which, from the reference to the desire of holders of cash balances to "turn and wind it [money] into various commodities to their great profit," it is obvious that "the most thriving men" whom Petty had in mind were thought of as "traders." Marshall's own examples, moreover, as Robertson has pointed out, included examples of the administration of cash balances by "traders" as well as by "wage receivers" and the recipients of salaries. (See Marshall, *op. cit.,* 46 f.; and cf. Robertson, "A Note on the Theory of Money," *loc. cit.,* 243.) The same thing may be said of Pigou, whose examples concerned chiefly the choices facing a "business man" and the estimates the latter would be likely to make with respect to "the expected fruitfulness of industrial activity" (*Essays in Applied Economics,* 181 ff.). Hawtrey's application of "cash-balance analysis" to the administration of "traders' balances" as well as to "consumers' balances" is of course well known: see, for example, *Currency and Credit,* 37 f., 41 ff., 110, 188 f., 217, 291 f., 379, of the 1st ed., (42 f., 47 ff., 57 f., 78, 217 f., 245, 289, 456 of the 3d edition); and for an equally explicit distinction at the hands of a protagonist of the "cash-balance approach," see the discussion of the demand for cash balances by both *Erwerbswirtschaften* (Hawtrey's "traders") and *Aufwandswirtschaften* (Hawtrey's "consumers"), in Menger's *Geld,* sec. xiv (A) (*The Collected Works of Carl Menger,* IV, 107 ff.). For examples of Robertson's readiness to apply "cash-balance" analysis to traders, it is not necessary to go beyond his application of this analysis to "turnover" as well as to "income." See, for example, Robertson, *Money,* 2d ed., 41, 195. A survey of the practice of other protagonists of the cash-balance approach would likewise show that, as often as not, the examples were taken from the experiences of "traders." Such a survey cannot, however, be presented here.

[27] It is perhaps not without significance, from the standpoint of a judgment as to the fruitfulness of much of the discussion with respect to the relation of "utility analysis" to the problem of the value of money, that the writers who have insisted that "utility analysis" must be confined to the actions of consumers in relation to consumers' goods have not been "cash-balance" theorists at all. See, for example, Wieser, "Der Geldwert und seine Veränderungen," *loc. cit.,* 516. (On the rôle of "utility analysis" in the Theory of Prices, for purposes other than the theory of the forces determining the size of cash balances relative to outlay, see below, pp. 491 ff.) Conversely, those sponsors of the position that "utility analysis" may be applied in fruitful ways to the problem of the value of money who happened to be "cash-balance" theorists, saw no difficulty whatever in speaking of the choices of "traders" as involving a balancing of "utilities" quite as much as do the choices of consumers. See, for example, the reference to Pigou, in the preceding note; also the quotation from T. N. Carver given by Pigou in the passage cited; and cf. Mises, *Theory of Money and Credit,* 134, where the argument with respect to calculations of utility ("the subjective valuations of individuals") is illustrated from the calculations of "entrepreneurs" as well as from those of consumers.

That this is merely a bit of scholastic concept juggling, however, becomes apparent as soon as it is recognized (1) that nothing more should be implied by the suggestion that calculations of "utility" are involved than that a greater economic advantage is found to inhere in one type of holding than another, and (2) that "traders" are, if anything, more sensitive to the type of calculation with respect to economic advantage which is involved in the administration of cash balances than are consumers.[28] This, again, was demonstrated by Mr. Keynes's own practice, elsewhere in the *Treatise,* when he applied the equivalent of the "Marshallian *K*" not only to the ratio between "income-deposits" and the payments against which these "deposits" are held, but also to the ratio between "business deposits" and the payments against which the latter are being held.[29]

3. *It follows, from the fact that the "Marshallian K" is, mathematically, merely the reciprocal of "velocity," when both the "Marshallian K" and "velocity" are properly defined, that the "price-levels" involved in equations of the "cash-balance" type need differ in no respect whatever from the price levels involved in equations of the Fisherine type.*

This proposition was explicitly denied by Mr. Keynes, who asked us to conceive of a "cash-balance standard"—that is, a price level of a special type which was alleged to be required in a "cash-balance" equation—as something distinct from the "price-level" required in equations of the Fisherine type, whether the latter are concerned with the price levels of consumers' goods, or the price level of something called "transactions."[30] Yet it is easy to show that

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[28] On the general usefulness of the first of these propositions for an understanding of the forces determining the size of cash balances relative to outlay, see below, pp. 480 f. It may be remarked in passing that the fact that "a much nicer calculation of balances" is to be expected in the case of traders than in the case of most consumers (so Hawtrey, *Currency and Credit,* 3d ed., 42; cf. the 1st ed., 37) would constitute an argument for a separation, for purposes of analysis, of traders' balances from consumers' balances, even if such a separation were not called for in the interest of tracing the process of "diffusion" of additions to the stock of money through the various economic groups in a community.

[29] Cf. the *Treatise,* I, 48 f.

[30] Cf. the *Treatise,* I, 76 ff., 223, 226, 228, 238. On the concept of a "Transactions Price-Level" and its implications, see below, pp. 517 ff.

this argument rests upon a series of fallacies of a peculiarly naïve kind.

The first of these fallacies derives simply from a failure to realize that the only "price-levels" which should find a place in an equation designed to explain how prices are actually determined are price levels which record the effect of the impact of a stream of money against a stream of goods.[31] It has, to be sure, been suggested by more than one writer that "cash-balance" equations are not "stream" equations of this type, since—so it is argued—"cash-balance" equations refer only to a "point" of time, whereas "stream" equations refer to a *period* of time.[32] That this distinction, however, is quite without foundation, becomes apparent when it is recognized that the "size" of cash balances involved in all cases in which the cash-balance approach is used to throw light upon the problem of velocity, is "size" *relative to the magnitude of outlay.* The "magnitude of

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[31] It should hardly be necessary to point out that this proposition, while it does mean that the prices included in equations of the type under discussion are actually realized, rather than "anticipated," prices, does not mean that the use of such equations implies an indifference to the effect of "anticipations" upon the determination of prices. The element of "anticipations" will be discussed at greater length in Volume II. Cf., however, what is said on the matter on p. 48, above; and also on pp. 429 ff., below.

[32] Thus, for example, R. F. Harrod, in the *Economic Journal,* XXXIX (1929), 242, argued that equations of the "Cambridge" type "differ from Professor Fisher's in that the time element does not enter into any of their terms" (cf. also G. U. Papi, *Lezioni di economia generale e corporativa* [Padua, 1934], Vol. II, 28). Similarly, J. H. Rogers, in his *The Process of Inflation in France,* 314, argued that Keynes's equation $n = pk$ is "unlike that of Irving Fisher, which deals with *periods* of time," in that it "represents instantaneous cross-sections of the monetary situation." See also A. F. Burns, "The Quantity Theory and Price Stabilization," *loc. cit.,* 574, where it was argued that the Fisherine equations are " 'historical' equations," in the sense that they "embrace a period of time," whereas equations of the cash-balance type may be characterized as " 'cross-section' equations," since they "refer to a point of time." (Cf. also G. M. Verrijn Stuart, *Prae-advies over het wezen der prijsstabilisatie, hare wenschelijkheid en mogelijkheid* ["Memorandum on the Nature of Price-Stabilization, its Desirability and its Possibility"], The Hague, 1929, 130 n.; and Ellis, *German Monetary Theory,* 125 f., 145, 154, 189.) Even so intelligent a defender of the cash-balance approach as D. H. Robertson has written in such a way as to suggest that this approach involves analysis "at a given *point* of time," in contrast with analyses involving "a given *period* of time." See, for example, Robertson, *Money,* 2d ed., 30, 37. In justice to Mr. Robertson, however, it should be pointed out that he did not contrast the two types of *equation* concerned as involving this type of distinction; and that he himself did not hesitate to rewrite the Fisherine equation in "cash-balance" terms (*op. cit.,* 195).

outlay," obviously, is a concept which has no meaning except with reference to a *period* of time.[33] The cash balances which are compared with this outlay are, therefore, as "cash-balance" theorists from Locke to Wicksell and Hawtrey have pointed out, *average* balances over a *period* of time.[34] Once these simple facts are recognized, it becomes obvious that "cash-balance" equations are as truly "stream" equations as are equations of the "Fisherine" form.[35] There is no reason, therefore, why the "stream" of goods which are represented as being "priced" in a given "Fisherine" equation need con-

[33] The failure to recognize this simple point lies at the root of some errors with respect to the possibility of constructing a concept of "velocity" which shall be free from any stipulation with respect to the element of "time." "Unit-of-work" concepts of velocity—such as have been attributed to the two Mills—are examples. It is, of course, true that some writers have seen no difficulty in conceiving of the "total of payments" at a "moment of time." An examination of this position, as well as of "unit of work" concepts of velocity, must, however, be left for another occasion. See, in the meantime, the comment by Cannan, *An Economist's Protest,* 386.

[34] See, for example, the quotation from Locke given by Holtrop, *De Omloopssnelheid van het Geld,* 4 ("Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *loc. cit.,* 505). For examples of Wicksell's usage, see his "Der Bankzins als Regulator der Warenpreise," *loc. cit.,* 231, and his *Lectures,* II, 61 ff. For Hawtrey's usage, see *Currency and Credit,* 1st ed., 48. It is no argument against the usage suggested in these passages to point to the fact that when "we are dealing with an individual decision to hold money *or* something else . . . such a decision is always made at a point of time" (so Hicks, "A Suggestion for Simplifying the Theory of Money," *loc. cit.,* 4). The real issues are, first, whether it is or is not true that the decisions made at a "point of time" are made in view of the events likely to occur over a period of time; and, second, whether, in order to obtain a significant setting for the choices of individuals in the administration of cash balances, it is not necessary to consider these choices as made over a typical period. It may be noted also that the fact that the cash balances involved are *average* balances provides the obvious answer to those who would argue that any formulation—including that represented by equations of the "Fisherine" type—which includes a term for the "stock" of money should be understood as referring to a *"point of time,"* and can introduce a concept of "velocity" which has reference to a "period" of time, only at the cost of a series of artificial or "fictitious" constructions. See, in this connection, Cassel, *The Theory of Social Economy,* 424 f.

[35] This conclusion is expressed by Adarkar, *The Theory of Monetary Policy,* pp. 50 f. Unfortunately, however, this author couples the statement of his conclusion with the further statement that, while both the "cash-balance" and the "Fisherine" approach "relate to the flow of money to the market," "in one case [Fisher's] the flow of money is investigated *after* the transactions are complete; in the other [the Cambridge formula] the same flow is investigated from an *anticipatory and pre-transaction* viewpoint," so that "the difference between the two . . . corresponds to that between anticipation and fact." On the objections to this way of putting the matter, see below, pp. 429 ff.

tain goods in any respect different from those which are represented as being "priced" in a cash-balance equation.

The second of the fallacies involved in the suggestion that the price level included in "cash-balance" equations is necessarily different from the price level involved in equations of the "Fisherine" form derives from a failure to understand what is involved in the construction of an apparatus for dealing with the Theory of Prices which involves a "plurality" of "stream" equations. We shall deal at a later point in this study with the problems underlying the concept of a "plurality" of price levels.[36] At this point, it is necessary to do no more than to indicate, with the help of some very simple algebra, the reasons for objecting to the suggestion that the use of the cash-balance approach somehow makes it impossible to arrive at precisely the same type of price level as that included in equations of the general Fisherine form.

Let $(PT)_i$ represent, as before, payments out of income.[37] It is obvious that it is against these payments that the cash balances of income recipients are held. The "K" of our cash-balance equation, in other words, has reference to this $(PT)_i$. Using the term k_i in a sense close to that in which the term k_1 was used by Mr. Keynes in his *Treatise*,[38] we may, therefore write

$$M_i = k_i \cdot (PT)_i , \tag{1a}$$

or

$$M_i \cdot \frac{1}{k_i} = (PT)_i . \tag{1b}$$

Let us suppose that it is now desired to provide an equation representing the stream of money going against, say, *consumers'* goods—in other words, an equation in which the "price-level" represents the reciprocal of what Mr. Keynes

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[36] See below, pp. 496 ff.

[37] Cf. above, pp. 369 and 383.

[38] *Treatise*, I, 44 ff. It will be observed that our k_i is defined by the ratio $M_i/(PT)_i$—that is, as the ratio of "the amount of the income deposits" to the total of payments *out of* income (Hawtrey's "consumers' outlay"), whereas Keynes's k_1 was defined as the ratio of "the amount of the income-deposits" to "the aggregate annual money-*income* of the community" (Hawtrey's "consumers' income"). See, on this matter, pp. 354 ff., above.

wishes to regard as the "purchasing power of money." [39] We may write

$$(PT)_i = (PT)_{i \cdot c} + (PT)_{i \cdot p}, \tag{2}$$

in which the subscripts c and p indicate the expenditures on consumers' and producers' goods respectively. If, for the sake of ease in exposition, we assume that all payments for consumers' goods are made out of incomes,[40] it follows that we may also write, on the basis of equations (1) and (2)

$$M_i \cdot \frac{1}{k_i} = (PT)_{i \cdot c} + (PT)_{i \cdot p}, \tag{3a}$$

or

$$M_i \cdot \frac{1}{k_i} - (PT)_{i \cdot p} = (PT)_{i \cdot c}. \tag{3b}$$

It will be observed that we now have a "cash-balance equation" which leads directly to a price level of consumers' goods—that is, to $(PT)_{i \cdot c}$. The mere fact that k_i is related directly to, and is expressed as a proportion of $(PT)_i$, which is equal to $(PT)_{i \cdot c} + (PT)_{i \cdot p}$, does not mean that we are prevented from writing an equation which will describe the stream of money going against $(PT)_{i \cdot c}$.[41] Equation (3*b*) shows perfectly clearly the nature of the forces determining the price level of "consumers' goods"—Keynes's "purchasing power of money"—without abandoning, or doing violence to, the central idea involved in the "cash-balance approach," or involving a distinction between the "standard" which is

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[39] Cf. the *Treatise*, I, 53 f. It is to be remembered that Mr. Keynes himself *contrasted* equations which "lead up . . . to the Purchasing Power of Money" with the familiar "Quantity Equations," whether the latter were of the Fisherine *or* the "cash-balance" type. Cf. the *Treatise*, I, 76 ff.

[40] It would be easy to show that the inclusion of payments for consumers' goods from sources other than income would, although greatly complicating the algebra, lead to no change in results which is significant for our present purpose.

[41] It will be observed, incidentally, that the procedure developed in the text shows that there is no reason for arguing, as some commentators on Keynes have, that the segregation of income deposits from "business deposits" is "logically requisite to equations employing specialized producer and consumer price indices," if by this is meant that we are to assume that "income deposits" are thought of as being held against expenditure upon "consumers' " goods, whereas "business deposits" are held against expenditure upon "producers' " goods. (See, for example, Ellis, *German Monetary Theory*, 194.)

represented by Keynes's "purchasing power of money," on the one hand, and a supposed "cash-balance standard," on the other.[42] If, in the past, the "Cambridge formula has been . . . applied to investigate the income value of money" —in the sense of its "value in terms of the goods and services which enter into final consumption"—while "the Fisherine formula" has been applied to other types of price index, this, as has been pointed out on occasion even by supporters of Mr. Keynes, is "merely a coincidence," and is not inherent in the "cash-balance" approach and the "Fisherine" approach as such.[43]

The third fallacy involved in Mr. Keynes's argument touches upon a problem which we have already encountered in another connection.[44] The problem has to do with the treatment to be accorded to anticipated, or "quoted," prices, as contrasted with actually realized prices. Our contention, it will be remembered, was that the mere fact that our price statistics include "quoted" or "anticipated" prices provides no justification for the use of these prices in an equation designed to represent the actual impact of a stream of money against a stream of goods.[45] From this it would follow that the "prices" included in the Fisherine equation, which is a "stream" equation of the type indicated, are actually realized prices. This was, in effect, agreed to by Mr. Keynes.[46]

[42] The statement that equation (3*b*) "shows perfectly clearly the nature of the forces determining the price-level of 'consumers' goods' " should, of course, not be taken to mean that it is proposed to use an equation of this type *in isolation*. Cf. what is said on this matter on pp. 512 ff., below.

[43] Cf. Adarkar, *The Theory of Monetary Policy*, 51. The definition of "the income value of money" given in the text is that of Robertson (*Money*, 2d ed., 19).

[44] See above, p. 48.

[45] We are here speaking, of course, of analytical considerations, not of the practical inadequacy of the available statistics. Cf. what is said on the page cited in the preceding note.

[46] This is certainly a fair deduction from Mr. Keynes's characterization of equations of the Fisherine type as involving a "Cash-Transactions Standard"—the "cash-transactions" clearly referring to transactions *consummated*, rather than *anticipated*. See the *Treatise*, I, 76, 234; and cf. especially 1, 239, where Keynes, unlike Mitchell and the other writers mentioned on p. 48, above, showed that he was perfectly well aware that the prices included in equations of the Fisherine type are not necessarily the prices which are *quoted* currently—or, as Mr. Keynes himself put it somewhat awkwardly, "the prices prevailing at the moment"—but are the prices applying to "the transactions which are being completed to-day," although they may have been *quoted* as "prevailing" prices some time before the transactions were "completed."

The point, however, at which his argument began to involve a clear fallacy is that at which he introduced a second implication, which is by no means self-evident—namely, that the "prices" involved in equations of the cash-balance type are anticipated, rather than actually realized prices.[47] It was from this proposition that he drew the obvious corollary that, particularly "when prices are changing," the price levels involved in "cash-balance" equations would be different from those involved in equations of the "Fisherine" type.[48]

The reason, however, for refusing to accept as self-evident the proposition that the prices involved in "cash-balance" equations are different from those involved in equations of the Fisherine type is simply that such a proposition would run counter to the implications of the expression $V = (1/k)$. What that expression tells us is that the forces determining "velocity of circulation" are the forces which determine the size of balances that individuals choose to keep on hand relative to outlay. In and of itself, it tells us nothing with respect to the nature of the forces determining the magnitude of K. The analysis of these forces, on the contrary, may be said to be part of the body of doctrine which "lies behind" the K—and therefore the V—of our equations as truly as does the analysis of any of the forces which "lie behind" the other variables in those equations. Among the forces affecting the magnitude of K, to be sure, are the anticipations of the holders of cash balances with respect to the future course of prices. There is, however, no reason whatever for identifying these *anticipated* prices, which may be regarded as *influencing* the decisions of holders of cash balances with respect to the size of these balances relative to outlay, with the prices which are to be regarded as being

[47] Again this is certainly a fair deduction from the terms of Mr. Keynes's own exposition. See, for example, the *Treatise*, I, 238, on the difference in *date* as between the price quotations affecting "the cheques being cleared at any time"—that is, the price "quotations" which have now been realized as "actual" prices—and those "which affect the amount of cash-balances required to be held"—that is, the price quotations which are based on anticipations as to the condition of the market when payments come finally to be made. Cf. also the quotation from Adarkar, given on p. 426, n. 35, above.

[48] See especially the *Treatise*, I, 238 f.

influenced by a change in velocity.[49] It is the latter "prices" which are included in our "stream" equations, not the former.[50]

As we have seen, however, the "cash-balance" equations, when properly stated, are as truly "stream" equations as are equations of the Fisherine form. This, of course, is merely another way of saying that the relation of "anticipated" and "realized" prices to the V of the Fisherine equation is in no wise different from the relation of the two groups of prices to the K of cash-balance equations. It follows, therefore, that, for purposes of a theory designed to show how prices are actually determined in the market, we have, in the present instance, merely another illustration of the soundness of Mr. Robertson's contention that "Mr. Keynes' 'cash-balances standard,'" with all that it implies regarding the differences between the prices included in the P of a Fisherine equation and those included in the P of a cash-balance equation, "is an unnecessary and confusing complication."[51]

As it happens, Mr. Robertson went on to concede that, under certain conditions, the difference between "quoted" and actually realized prices, while it did not demonstrate that there was "need for, or help in, a specially constructed index number" for use in cash-balance equations, might nevertheless be regarded as necessitating a type of analysis which would, in effect, "destroy the identity which normally exists between a

[49] This is not to say, of course, that prices actually realized during a given period do not also "influence" the size of cash balances held relative to outlay during that period. Nor is the influence exerted upon the size of these balances by actually realized prices limited to the effect of *experience* upon *anticipation*. It is also possible, as has been pointed out by writers to be discussed on another occasion, that the course of actually recorded prices may affect the size of cash balances relative to outlay by affecting actual *outlays* in a different degree from actual *incomes* (or *receipts*). This possibility, however, has nothing to do with the argument discussed in the text.

[50] It may be pointed out, in passing, that an explicit recognition of the difference between *anticipated* and *realized* prices might have done much to clear up the confusion involved in the century-old controversy with respect to the "passiveness" of prices, in relation to the other variables in the equation of exchange, particularly if the point involved is stated in conjunction with the use of (1) "period analysis"—that is, analysis which makes it perfectly clear in *which* "period" the prices held to be "active" are regarded as being themselves determined, and (2) the distinction between payments *into* and *out of* "income," applied in such a way as to show that the prices which are involved in payments *into* income belong to a different "period" than the prices which are made *out of* the same income. These matters cannot, however, be developed here at length.

[51] Robertson, "A Note on the Theory of Money," *loc. cit.*, 246.

Marshall fraction of the K type and the inverse of the analogous velocity of circulation"—that is, the validity of the expression $K = 1/V$.[52] In fact, however, there are grounds for arguing that this concession, and the conceptual construction on which it is based, is as unnecessary, and in a sense as confusing, as Mr. Keynes's suggestion with respect to the need for a "specially constructed index number."

From an examination of Mr. Robertson's argument, it is clear, in the first place, that the exception which he believed he had found to the expression $K = 1/V$ is in no real sense associated with the differences between "quoted" prices and actually realized prices. What he was concerned with was the effect upon prices, whether quoted or actually realized, of anticipations with respect to changes in the *quantity of money*. Clearly, therefore, there is no genuine connection between the type of issue raised by Mr. Robertson and that raised by Mr. Keynes.

It is easy to show, in the second place, that even the element of anticipations with respect to changes in the quantity of *money* can be regarded as providing an exception to the general validity of the expression $K = 1/V$ only if the Fisherine equation and equations of the "cash-balance" type are subjected to a difference in treatment which is in fact not inherent in either of the two types of equation. In its essence, Mr. Robertson's argument started from the assumption that, in order to show the effect upon prices—whether "quoted" or actually realized—of people's anticipations with respect to changes in the quantity of money, it is desirable to introduce into the Fisherine equation a magnitude which Mr. Robertson himself called "latent money"; whereas it was not necessary—and indeed not desirable—to include a term for "latent money" in equations of the cash-balance type.[53] If this were true, then of course it would follow that, since the other magnitudes in the two equations would be equal, the difference would be reflected in a difference between the magnitude of V and that of $1/K$.

In fact, however, it is difficult to see just why it is more necessary, or more desirable, to introduce a term for "latent money" into the Fisherine equation than it is to introduce it into a cash-balance equation. Mr. Robertson's argument was that the expectation of an increase in the quantity of money will be reflected in a diminution of K, with a corresponding inverse effect upon prices. Yet, in view of the fact that, by virtue of the expression $V = 1/K$, every effect upon K must be reflected in V, it is difficult to see why the effect of anticipations with respect to an increase in the quantity of money may not with equal ease be reflected in a corresponding change in V, without the necessity for having recourse to the concept of "latent money."[54] One

[52] *Ibid.*, 247.

[53] Robertson, "A Note on the Theory of Money," *loc. cit.*, 246 f. On the concept of "latent money," see also Robertson's *Money*, 2d ed., p. 196; and cf. the remarks on the general concept by M. Palyi, "Ungelöste Fragen der Geldtheorie," *loc. cit.*, 480.

[54] See, for example, in this connection, the remarks by Pigou on the effect of changes in anticipations—or, as he put it, in "business confidence"—

might, indeed, argue, with quite as much cogency, that the fact that people "have a confident expectation of being able to get hold of more money by the time they require it" would justify our regarding this "expected" money as constituting a "latent" addition to their cash balances, which would thus be regarded as bearing the same relation to outlay as before. An idea very similar to this, indeed, is involved in Mr. Keynes's contention that the "volume of deposits"—that is, of cash balances—should include not only the cash balances actually held, but the volume of "cash-facilities" as well. There are, to be sure, serious objections, as we shall see, to Mr. Keynes's proposed procedure.[55] Yet the objections to it are of precisely the same kind as those which may properly be urged against Mr. Robertson's concept of "latent money." This, obviously, is merely another way of saying that neither the concept of "latent money" nor that of "cash-facilities," when the latter is regarded as being in all essentials equivalent to actual holdings of money, should be included in either type of equation. Mr. Robertson's own argument is that the validity of the expression $V = 1/K$ is destroyed only by the "intrusion" of "latent money," or of a similar concept, into one of the equations without a simultaneous "intrusion" of an identical concept into the other type of equation. From this very argument, however, it follows that, so long as the Fisherine equation and a comparable "cash-balance" equation are subjected to the same treatment, and until a really convincing argument can be developed by way of showing that they should not be subjected to the same treatment, there is just as little reason for seeing an exception to the expression $V = 1/K$ in the case of anticipations of changes in the money stock as there was for seeing an exception to it in the difference between "quoted" prices and actually realized prices.

4. *The only range of problems for the solution of which there may be said to be an incontrovertible case for the "cash-balance approach" is that which has to do with the phenomena usually treated under the head of the "velocity of circulation of money."*[56] *The mere fact that other problems within the field of the Theory of Prices are not easily handled by means of the "cash-balance approach," or that some variants of the so-called "real-balance" approach may have seemed to imply that the "cash-balance approach" is well-adapted to the handling of problems other than those*

on V, as well as on his k; though it is true that he did not adduce the special case of anticipations with respect to changes in the quantity of money (Pigou, *Essays in Applied Economics,* 178 f.).

[55] Cf. below, pp. 472 f.

[56] The only important exception to this generalization has to do with that component of the "velocity of circulation of goods" which may be characterized as the "rate of sale" of goods. See, however, what is said on this matter on p. 455, n. 111, and pp. 454 ff., below.

associated with "velocity" cannot, therefore, be regarded as arguing for the abandonment of the "cash-balance approach," as such.

As we have seen, Mr. Keynes, in his *Treatise,* announced his formal abandonment of the cash-balance approach which he himself had presented in his earlier *Monetary Reform,* on the ground that its use not only "causes confusion," but also fails to give us "any real insight into the price-making process." [57] In view of the fact that the first of these propositions was based on the allegation that the cash-balance approach necessarily involves "the merging together of all the different sorts of transactions—income, business, and financial—which may be taking place," we need only point to the first of our counterpropositions advanced above, from which it should be clear that the "merging together of . . . different sorts of transactions," so far from being inherent in the "cash-balance approach" as such, is actually inconsistent with one of the central methodological implications underlying the cash-balance approach—namely, that cash balances should be related specifically to the particular outlay against which these cash balances are being held.[58]

On the other hand, Mr. Keynes's reason for believing that we cannot, through the use of the cash-balance approach, "get any real insight into the price-making process," was of quite a different kind. Specifically, his argument was that there is nothing in the "cash-balance approach" which provides an understanding of the rôle played in the price-making process by such elements as "the rate of interest" and "the distinctions between incomes and profits and between savings and investment." [59] It is easy to show, however, that all that this argument amounts to is the extraordinary contention that simply because a given analytical device cannot be expected to throw light upon certain ranges of problems to which it should never have been applied, it is not needed in the handling of problems for whose solution it is actually indispensable.

[57] Cf. above, p. 415, and n. 2 thereto.
[58] Cf. above, pp. 420 ff.
[59] See the *Treatise,* I, 229; and cf. I, 233. Mr. Keynes's disciples have followed him closely in this matter. See, for example, Adarkar, *The Theory of Monetary Policy,* 51.

The particular range of problems for which the "cash-balance approach" is indispensable is, as we have seen, that which deals with the phenomena treated under the head of the "velocity of circulation of money." Once its indispensability for this purpose is granted, it is a matter of quite secondary importance that, *by reason of its relation to the problems associated with "velocity,"* a connection of some kind can be established between the "cash-balance approach" and the other elements—such as "the rate of interest and the distinctions between incomes and profits and between savings and investment"—mentioned by Mr. Keynes.[60] Its connection with these problems is the result of the fact that these problems, in their turn, are related to the problem of "velocity." [61] Nobody, however, has ever seriously suggested that they are *solely* problems of velocity. A proper statement of the claims for the cash-balance approach would have made this clear by insisting that the element in equations of the "Fisherine" form to which "cash-balance" analysis is directly relevant is the V of those equations. It is in connection with other elements in the equations—M', for example—that, as we have seen, the "rate of interest" is chiefly, though by no means solely, important.[62] Similarly, it is in connection with the theory of output, which, as we shall see, is part of the analysis lying behind the T of the Fisherine equations, that "the distinction between incomes and profits" is chiefly important.[63]

[60] On the effects upon velocity of movements of the rate of interest, see, in addition to what is said on p. 483, below, with respect to the effect of interest upon the size of cash balances held relative to outlay, the references to Wicksell, on p. 186, above. The place of "velocity" in "savings-investment" analysis is discussed in Volume II of this study.

[61] It is, of course, *possible* to describe certain of the phenomena associated with a "discrepancy between savings and investment" which are not strictly phenomena of "velocity," in terms of the "real balance" approach. See, however, what is said on this matter on pp. 534 f., below.

[62] See above, pp. 183 ff.; and cf. what is said with respect to the relation between the rate of interest and "velocity" in n. 60, above.

[63] On the theory of output as a "part of the analysis lying behind the T of the Fisherine equations," see below, p. 599; and for the substance of the "theory of output" itself, see Volume II of this study. The "distinction" which is really "important" for the theory of output is, of course, the distinction between *costs* and *selling prices*, and the relation of this distinction to "profits," rather than "the distinction between *incomes* and *profits*." It must be remembered, however, that, according to the apparatus presented in the *Treatise*, "incomes" were the same thing as "costs," while "profits"—

Similarly, also, the theory of "savings and investment," when regarded as part of the apparatus designed to explain movements in general prices, is only in part a problem involving the V of the Fisherian equations. To renounce the "cash-balance approach" solely because, in and of itself, it is not designed to deal with these other problems, is like renouncing the use of a razor because it cannot be used also as a scalpel.

If there is some basis for the suggestion that more has been claimed for the "cash-balance approach" than that it is indispensable for a solution of the problems ordinarily associated with the "velocity of circulation of money," this basis is probably to be sought in the implications which may be said to inhere in the particular branch of the general family of "cash-balance" approaches represented by what has come to be called, by Mr. Keynes as well as by others, the "real balance" approach.[64] The particular "cash-balance" approach which Mr. Keynes had presented in his *Monetary Reform* was, as it happens, an example of the "real balances" variant of that approach; and it is not entirely unnatural that Mr. Keynes should have seemed to regard the "real balances" approach as coextensive in all its aspects with the cash-balance approach.

In fact, however, one of the most eminent of contemporary exponents of the cash-balance approach—Mr. Hawtrey—had not only used, from his earliest writings on the subject, a form of that approach which was completely free of "real balance" implications, but had gone on record, even prior to the publication of Mr. Keynes's *Treatise,* as objecting to the "real balance" variant on the ground that it did not give a realistic picture of the type of calculation which, according to all variants of the "cash-balance" approach, is engaged in by administrators of cash balances in order to determine the size of the cash balance which they wish to keep by them.[65] Even earlier, moreover, objections had

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that is, "windfall profits"—were nothing more than the difference between costs and selling prices.

[64] See especially the *Treatise,* I, 222 ff.

[65] See especially Hawtrey's *Currency and Credit,* 39 f. of the 3d edition; and see also Hawtrey's comment on Aftalion's "criticism that the 'real' balances which appear in Mr. Keynes's formula are less directly involved

been made to Mr. Keynes's own variant of the concept of "real balances" on the ground which, in my opinion, is the most cogent that can be urged against it: namely, that, like the concepts of "virtual velocity" and "income-velocity," it lumps in a single analytical device elements which are sufficiently disparate in character to warrant a separate treatment in each case.[66] Had Mr. Keynes, in his *Treatise,* built his discussion of the concept of "real balances" on the relative advantages of a "cash-balance approach" which was entirely free from "real balance" implications, and one which carried such implications, or even on the relative advantages that may be held to attach to different formulations of the "real balance" approach, he would have contributed at least as much to a clearing of the "confusion" surrounding the problem as he contributed to that surrounding the concept of "income velocity." Unfortunately, however, there are grounds for arguing that his discussion of the relation between the concept of "cash-balances" and the concept of "real balances" merely succeeded in adding to the confusion which already existed on the subject.

It certainly did not add to a clarification of the issues involved in an examination of the difference between the concept of "cash-balances," on the one hand, and of "real balances," on the other, to argue, for example, as Mr. Keynes did, that "the volume of cash-balances depends on the decisions of the bankers and is 'created' by them," whereas "the volume of real balances depends on the decisions of the depositors and is 'created' by them."[67] Neither proposition is, in fact, strictly true. If, for example, by "the volume of cash-balances" we mean, as Mr. Keynes himself meant, the *absolute* volume of cash balances, it follows, from the fact that the "volume of cash balances," as thus defined, will be represented by the $(M + M')$ of our Quantity Equation, that this "vol-

in people's thoughts and intentions than the 'nominal' or 'money' balances" (*Weltwirtschaftliches Archiv,* XXVIII, 100**; cf. Aftalion's *Monnaie, Prix et Change,* 143 ff.). For examples of a defence of the "real balances" formulation, by later writers, on the ground of its "realism," see the references given on p. 446, n. 88, below.

[66] See, for example, Angell, *The Theory of International Prices,* 180; and cf. D. Davidson, in the *Ekonomisk Tidskrift,* XXVI (1924), 172 n. On the aspect of "virtual velocity" and "income velocity" involved, see above, pp. 366 ff. It should be pointed out that the criticisms in question were directed against Mr. Keynes's equation $n = pk$. On other forms of the "real balance" approach which are much less open to criticisms of this type, see below, p. 439, n. 73, and p. 455, n. 112.

[67] *Treatise,* I, 224.

ume of cash-balances" will be determined, not only by the decisions of the "bankers," but also by the decisions of all those whose actions are involved in the processes described in the body of analysis which can be said to "lie behind" the variables M and M'. The volume of M', for example—to go no further—will, in the light of the classical argument with respect to the effectiveness of the rate of discount in affecting the amount of borrowing, depend not only upon the "decisions of the bankers" with respect to the level of the rate of discount, but also upon the decisions of the borrowers in the light of the anticipated profit to be made by the use of a bank loan.[68] If, on the other hand, by the "volume of cash balances" we mean—though Mr Keynes himself did not mean—the size of cash-balances *relative to outlay*, the suggestion that the "volume of cash-balances" is determined by the "decisions of the bankers" is vastly further from the truth than would be the proposition that their "volume" is determined by the decisions of the "depositors" who administer the "deposits" in question.[69]

The proposition that "the volume of real-balances depends on the decisions of the depositors and is 'created' by them" is open to much more serious reservations than is the proposition that "the volume of cash-balances depends on the decisions of the bankers and is 'created' by them." When literally interpreted, indeed, it is simply false. The formula for "real balances" is M/P.[70] An analysis of the forces "determining," in any fundamental sense, the magnitude of these "real balances"—represented by k in Keynes's *Monetary Reform* and by C in his *Treatise*—would have, in the first place, to deal quite as much with the forces determining the M of the formula as with the forces determining the P; and, in view of the fact that the M—"the total volume of cash-balances"—is equivalent to the $(M + M')$ of our Quantity Equation, everything that was said in the previous paragraph with respect to the part played by "bankers" and "depositors," respectively, would still hold true.

This, however, is only the beginning of our difficulties. According to Mr. Keynes himself, "the price-level"—that is, the P of the formula—is the "resultant" not only of the "decisions" which have made the volume of M what it is, but also of the "decisions" which have made the volume of "real balances" what it is.[71] This, of course, is merely another way of saying that in order to analyze the forces which combine

[68] Cf. above, pp. 191 ff.

[69] The former proposition would, of course, not be wholly wrong; for the "decisions of the bankers" could affect the ratio of cash balances relative to outlay by virtue of the power of "the bankers" to provide or withdraw facilities for borrowing, which are, of course, an important factor in the determination of the size of the balances which will be held relative to outlay. Cf., on this matter, pp. 472 ff., below.

[70] Cf. the *Treatise*, I, 224. Keynes himself wrote, as the formula for the "volume of Real-balances" (C), $C = M/P_1$, in which P_1 was the special "Price-level" to which the cash-balance approach was supposed to "lead up." Cf., however, the argument on pp. 424 ff., above.

[71] Cf. the *Treatise*, I, 224.

to determine the volume of "real balances," we must attack *directly* the problem of isolating the determinants of this C. It is precisely at this point, however, that we observe at one and the same time the disparity in nature among the forces affecting the size of "real balances" and the essential absurdity of suggesting that their size will be determined solely by the decisions of the "depositors."

Actually, of course, from our earlier conclusion that there is no logical reason why a "cash-balance" equation need differ from a "Fisherine" equation in any respect other than that V is replaced by $1/K$, in the sense of K indicated in our own analysis presented above, we are forced to the conclusion that the C of the equation $M/P = C$ is really equal to T/V, in "Fisherine" terms, or to KT, in cash-balance terms. It is obvious, in the light of what is said above, that the particular determinant of the size of "real balances" *which is represented by our K* may, in fact, be said to be largely the result of the "decisions of the depositors."[72] To say, however, that the magnitude of T is "determined by the decisions of the depositors" is nothing short of grotesque. The mere fact that T includes, among other things, a term for output, with all that this implies with respect to the need for an apparatus dealing with the whole of the theory of output, shows how much more is involved than the "decisions of the depositors."[73]

Nor is this all. The T of our Quantity Equation includes much more than "output"; and, as we shall see in later chapters of this study, it is right, despite frequent contentions to the contrary, that it should do so.[74] As we shall see, also, these factors other than output which are included in T are not only of an extremely complex character, but are of such a nature that to regard them as being "determined by decisions

[72] Again it may be pointed out that though our K is "largely" determined by the "decisions of the depositors," it will not be *entirely* determined by their decisions. Cf. what is said on p. 438, n. 69, above, and also what is said on p. 483, below, with respect to the rôle of "institutional" factors in determining the size of cash balances relative to outlay.

[73] It is difficult, indeed, to believe that if Mr. Keynes had used a "real balances" equation of a form similar to that used by Pigou—that is, one in which the k or C of his equation was replaced by a term equivalent to Pigou's kR—he could ever have concluded that the magnitude of C was determined by the "decisions of the depositors." See, for example, Pigou's brief discussion of the nature of the forces determining his R, *Essays in Applied Economics*, 180. It is, in fact, something of a commentary upon the degree of precision attaching to the k, or C, of Keynes's "real balances" formulation, that commentators on the argument of *Monetary Reform* should have supposed that the variations in *Keynes's k* were associated with nothing but variations in "velocity." See, for example, Aftalion, *Monnaie, Prix et Change*, 144. An indication, on the other hand, of Mr. Keynes's awareness of the chief difficulties in the way of accepting his proposition that the magnitude of C is determined by the "decisions of the depositors" may be found in his otherwise cryptic suggestion that "there are all sorts of assumptions underlying the argument [concerning the nature of the forces determining the magnitude of real balances] as to what is happening to output, etc." (*Treatise*, 1, 229).

[74] See below, pp. 518 ff.

of the depositors," is simply absurd. It was precisely at this point that Mr. Keynes had a golden opportunity to demonstrate the cumbersomeness of certain formulations of the concept of "real balances," when the latter is regarded as a weapon of analysis. Instead, however, of taking advantage of the opportunity, he accepted, at its face value, a type of implication with respect to the nature of the forces determining the volume of "real balances" which shows in all their nakedness the vices of the concept when there is insufficient appreciation of the complexity of the forces which it summarizes. It is hardly surprising that Mr. Keynes, having identified his particular "real balances" variant of the "cash-balance approach" with the "cash-balance approach," was no longer "attracted" by the possibilities that could be regarded as inherent in its use!

5. *The connection between the "cash-balance approach" and "general value theory" is established by the fact that the former makes it possible to relate the rate at which money is spent, and, in a small measure, the* direction *in which it is "spent," to the choices of individuals, who may be said thereby to act as a result of a weighing of the relative "utilities" to be derived from holding a cash balance and holding other forms of wealth, respectively.*[75] *It is not necessary, and indeed not desirable, to use a "real balance" approach in order to establish a connection between "monetary theory" and "general value theory."*

The "cash-balance approach" in itself provides a refutation of Mr. Keynes's allegation, in his *General Theory,* that when economists "pass, in Volume II, or more often in a separate treatise, to the Theory of Money and Prices, we hear no more of . . . [the] homely but intelligible concepts" that appear in economic writings on the subject of

[75] A relation between the "cash-balance approach," on the one hand, which is of course primarily concerned with a weighing of the relative "utilities" to be derived from holding a cash balance and holding other forms of wealth respectively, and, on the other hand, the *direction* in which money is spent—that is, the problem of *relative* prices—is, of course, established by the fact that the size of the cash balance held relative to outlay may be expected to bear some relation to the relative demands for specific forms of "real" wealth on the basis of the degree of "liquidity" attaching to these other forms. Cf. on this matter, Rosenstein-Rodan, "The Co-Ordination of the General Theories of Money and Price," *loc. cit.,* 262 ff. The *principal* tie-up between monetary theory and utility analysis when the latter is concerned with the determination of *relative* prices is, however, of a much simpler kind. Cf. what is said on this matter on pp. 491 f., below. In any case, it is not the connection between "cash-balance analysis" and the theory of *relative* prices which concerns us here.

the general "Theory of Value." [76] Nor is this aspect of the cash-balance approach one which has appeared only as a second thought to commentators on the "cash-balance approach" after it came to be articulated in its modern form. On the contrary, some of the most eminent among modern creators of what has come to be called "cash-balance analysis"—Walras, Menger, Mises, and Cannan are examples—have, as even Mr. Keynes once said in commenting on Marshall at a time when he was more sympathetic to his Marshallian inheritance than he has since shown himself to be, definitely regarded it as not least among the merits of this approach that it makes it possible to expound the theory of the value of money "as a part of the General Theory of Value." [77]

Having said this, however, it is of some importance to add that the significance of this achievement has proved to be a handicap to further progress in monetary theory as often as it has proved a benefit—so much so that writers of the highest standing have been driven, in recent years, to ask whether the time has not come to "get rid of the apparatus of supply and demand, so useful for one range of problems, but an intolerable bearing-rein" for such problems as the value of money.[78] There can be little doubt, for example, that the discussion of the applicability of "utility analysis" to the problem of the value of money has too often degenerated into a formalism of a peculiarly meaningless kind.[79]

[76] See the *General Theory,* 292; and cf. what is said with respect to the relation between monetary theory and "general value theory" on pp. 176 f., above. The matter will concern us also in Volume II of this study.

[77] Cf. Keynes's memoir on Marshall, in *Memorials of Alfred Marshall,* 29; and on the use of the cash balance approach as having enabled Marshall "to build up a structure of monetary theory which is . . . completely coherent with his general theory of value," see also Pigou's *The Functions of Economic Analysis* (Sidney Ball Lecture, May 27, 1929; Oxford, 1929), 9. On Walras, see, in addition to the reference to T. Wessels given on p. 418, n. 11, above, my "Léon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," *loc. cit.,* 591 ff. For the position of Menger and Mises, see also the references given on p. 418, above. On Cannan, see the latter's "The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency," *Economic Journal,* XXXI (1921).

[78] So, for example, J. Schumpeter, in the *Journal of Political Economy,* XLII (1934), 256 f.; cf. also the same writer in the *Journal of the American Statistical Association,* XXXI (1936), 793.

[79] A detailed account of the developments which have led me to this conclusion would not only require an absurdly disproportionate amount of

For proof of this, one does not have to go beyond the fact that the contributions to our understanding of the rôle, say, of "cash-balances" in the Theory of Prices, which have been made by writers who themselves either denied the applicability of "utility analysis" to the problem of the value of money—as in the case of Wicksell—or who certainly regarded the matter as of entirely secondary importance—as in the case, for example, of Hawtrey—have been at least as concrete as have the contributions made by writers who were anxious to use the cash-balance approach as a bridge between "monetary theory" and the "general theory of value." [80] The association of the theory of money and prices with the "general theory of value" is desirable not primarily because it adds elegance to a set of analytical devices whose heuristic value was never in serious question, but rather because, and in so far as, it can be shown that the association is of specific heuristic value within each of the two fields thus associated.[81]

Concretely, in the case of cash-balance analysis, the advantage of awareness of the implications of "modern" value theory has been that it called attention to the necessity for

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space here, but would, it is to be feared, only help to perpetuate a type of discussion already full of futilities. The reader can, therefore, only be referred to the brief comments in my "The Monetary Aspects of the Walrasian System," *loc. cit.*, 156 ff., and what is said on this matter on pp. 491 ff., below.

[80] For Wicksell's denial of the applicability of "utility" analysis to the problem of the value of money, see his *Interest and Prices*, 29; "Den dunkla punkten i penningtheorien" ("The Obscure Points in Monetary Theory"), *Ekonomisk Tidskrift*, V (1903), 486 ff.; and his *Lectures*, II, 20. For Hawtrey's discussion of the value of money as a special case of the general theory of value—or, as he put it, of the proposition that "money is a commodity" subject to the general "law of supply and demand"—a discussion the conclusion of which Hawtrey himself summed up in the proposition that while "the theory that money behaves like a commodity has the attractiveness of a paradox which completes a generalization," it "melts away under analysis"—see *Currency and Credit*, 1st ed., 168 ff. (3d ed., 197 ff.). It may be noted also that, in his review of Aftalion in the *Weltwirtschaftliches Archiv* (XXVIII, 100**), Hawtrey went so far as to say, of any attempt to expound "the theory of the value of money on the lines of the theory of marginal utility," that "in monetary theory that is really a digression."

[81] This statement will, of course, hardly be accepted by those who would insist that the "Ricardian" example of an emphasis on substance rather than form was a mistaken one. Cf. the comment on my position in this respect, by G. Del Vecchio, in the *Giornale degli economisti*, LXXII (1932), 99, and my reply in "The Monetary Aspects of the Walrasian System," *loc. cit.*, 155 ff.

avoiding discussions of "velocity" in terms which would suggest—to use the words of Professor T. N. Carver—that money itself has "organs of locomotion," instead of moving, or ceasing to move, as a result of the calculations of "economizing" individuals.[82] If this central point is grasped, and its implications carried through to detailed analysis, it makes very little difference whether these calculations are then translated into terms of a balancing of "utilities." To be sure, in order to establish the fact that a calculation of "utility" is involved, it is sufficient to ask those who are skeptical of the applicability of "utility analysis" to the problem of the value of money, why the recipients of cash do not continue to heap it up without limit, just as one may ask why consumers of a given good do not continue to demand the good in unlimited quantities; and one may ask why, if they cease to heap up money and choose to spend it instead, they do so at one point rather than at another, just as one may ask why consumers who shift their demand from one product to another do so at one point rather than at another. Yet, as we shall see when we come to describe the nature of the forces determining the size of cash balances relative to outlay, the statement that these questions are answered by considerations with respect to the "utility" of the cash balance as such, while it is certainly correct as far as it goes, carries us hardly a step nearer to a clear understanding of the nature of the specific forces which determine the size of cash balances relative to outlay.[83] Indeed, it may even be asked whether the insistence upon translation of the concrete considerations involved in the determination of the "size" of cash balances into the terms of formal "value theory" has not actually impeded further progress in the direction of a description of the specific factors involved. There are, in fact, grounds for arguing that it has had just this effect, not only by encouraging writers to regard as a solution of the problem what is in fact only a *statement* of the problem, but also by leading to an argument as to the ineluctable necessity for the adoption of certain forms of the cash-balance approach (such as certain "real balance"

[82] See Carver's *The Principles of National Economy*, 386.
[83] Cf. below, pp. 480 f.

variants thereof) which, as we have seen, are to be regarded as cumbersome in the extreme for the handling of the concrete problem of the determination of money prices.

There can be little doubt, for example, that one of the reasons for preferring the particular "real" variants of the cash-balance approach to which reference has been made has been the desire to describe the "demand" for currency in terms which will be at once all-inclusive and referable to the choices of individuals in administering cash balances. Yet it is certainly fair to ask whether the inclusiveness which is thus obtained is not gained at the cost of both realism and precision. The inclusiveness of the particular "real balance" formulations involved is derived from the fact that they include, among the things affecting the "demand" for cash balances, not only the factors reflected in a change in the size of cash balances relative to money outlay, but also those which are reflected in the T (or R) of the "Quantity Equations." As we have seen, however, the suggestion that the magnitude of T, for example, is determined by the choices of the administrators of cash balances is a grotesque exaggeration of the simple fact that the magnitude of T may be *affected* by the desire of these administrators to increase, say, the size of their cash balances relative to outlay—in which case they may be expected to increase the volume of T to the extent that they will offer other goods in greater quantities than usual against money, for the sake of obtaining the desired ratio of cash balances to outlay.[84] We are still left without an adequate account of those forces affecting T which, so long as there is no attempt to deny the "truth" of equations of the Fisherine type, can be shown to affect general prices, and yet are quite different in nature from the forces determining the demand of individuals for a cash-balance of a given size relative to outlay.

At the very least, therefore, a distinction should be drawn between what may be called the "relative" demand for cash balances—that is, the demand for cash balances of a given

[84] It may be noted, in passing, that the type of adjustment process thus described does not, in and of itself, demand the use of a "real balances" approach *simply because* the element of a change in T is involved. On the contrary, it provides an argument *against* the "real balances" approach. Cf. what is said on this matter on pp. 454 ff., below.

size relative to outlay—and the "absolute" demand for cash balances, in the sense of a demand for cash balances of a given height in terms of a definite number of monetary units. The "relative" demand for cash-balances—that is, the demand for cash balances of a given size relative to outlay—is *one* of the factors which will affect the "absolute" demand.[85] That it is not *identical* with it should be clear if one considers a case in which, with no change in the forces affecting the size of cash balances held relatively to outlay, there is, as a result of an increase in T, an increase in the total volume of cash balances "demanded" in order to carry on the increased volume of transactions at the same level of prices, the result of this increased "demand" being, in the event that the "demand" for cash is not met by an increased supply, a fall in prices.[86] To treat the two types of factor affecting the "demand" for "a store of ready purchasing power" as if they were essentially similar, is certainly not to encourage precision, whatever may be said for the practice on grounds of inclusiveness.[87] This, in itself, would con-

[85] In terms of the equation $M \cdot (1/k) = PT$, the "relative" demand for cash balances will be represented by the element k, which, as the proportion of cash balances held relatively to outlay, will be equal to $M/(MV)$, or $1/V$. The demand for an absolute volume of cash balances of the magnitude M, on the other hand, will be represented by $P \cdot kT$—in which, it will be observed, k is only *one* of the factors determining the "absolute demand."

[86] It may be observed that the statement in the text involves no more "circularity" in reasoning, by virtue of the introduction of the element of *prices* into the concept of a demand for cash balances, than does the ancient dilemma traditionally put before beginners in economics, in the form of a request to "reconcile" the statements that "demand depends upon price" and that "price depends upon demand." In both cases, the "contradiction" is of course resolved by a proper definition of "demand" for the special purpose in hand. In the present instance, it may be said, without attempting to force the proposition into a mold suggested by the simplest propositions of "general value theory," that the "demand" which "depends upon" price is the quantity of money (M) "demanded" *at a given level of prices;* it is therefore equal to $k \cdot PT$, and will vary with P as well as with k and T. The "demand" which *affects* price is kT. If this be an example of "circularity," the "circularity" involved is just as directly chargeable against the concept of "real balances," despite statements to the contrary by some writers, as it is against the proposal here advanced. See, on this matter, p. 450, n. 99, below.

[87] It can, in fact, be shown that this lack of precision has led to confused arguments with respect to the effect of such factors as "population" and "wealth" upon the "size" of the "demand for cash-balances." The matter, unfortunately, cannot be discussed here in detail. Cf., however, what is said on p. 453, below. The failure to draw a distinction corresponding to

stitute grounds for hesitating to regard the concept of "real balances" as superior to a description of the demand for cash balances in the terms just indicated, even if nothing were to be said—although I believe much is to be said—for the contention of Mr. Hawtrey that the whole description of the mental process of the cash balance administrator which is presented in most versions of the "real balance" approach is extremely weak from the standpoint of realism.[88]

Attention is called to the fact that the expressions "the absolute demand" and the "relative demand" for cash balances, as used above, have nothing in common with certain other distinctions which have been made between the "absolute" and the "relative" demand for "money."[89]

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the suggested distinction between the "absolute" and the "relative" demand for cash balances has also proved a source of much confusion, as we shall see in Volume II of this study, in the discussion of "hoarding" in the set of possibilities envisaged under the head of "an excess of savings over investment."

[88] Cf. above, p. 436, and n. 65 thereto. The attempts to reply to Hawtrey's contentions in this respect can hardly be regarded as convincing. When, for example, R. F. Harrod, in attempting to answer Hawtrey (*Economic Journal,* XXIX [1929], 242), asked whether the amount of individual outlay, in relation to the size of cash balance, is not "a function of the value of the pound," he seemed to imply that Hawtrey's statement that "the wealth value of the [monetary] units is itself not part of the calculation," was to be taken as implying that price-movements (the "value of the pound"), actual and anticipated, did not enter into "the calculation." Nothing, surely, could have been further from Mr. Hawtrey's intention. See, for example, his comment on Aftalion in this connection, *Weltwirtschaftliches Archiv,* XXVIII, 100**. The real issue, so far as the question of "realism" is concerned, is whether the element of price change enters the "calculations" of the cash-balance administrator as a matter affecting "his prospective receipts and payments in monetary units," as Hawtrey holds, or whether it enters as part of a kind of "deflation" process—in the statistical sense of "deflation"—represented by the division of a cash balance by a price index. The question, that is to say, is whether, from the standpoint of realism, it is helpful to think of cash-balance administrators as taking "express account of any index number relating their cash to its equivalent in products" (*Currency and Credit,* 3d ed., 40). The argument of Ellis (*German Monetary Theory,* 161), on the other hand, that a "calculus" which "runs in real value . . . proves to be the more realistic" is based upon certain contentions concerning the assumptions with respect to unitary elasticity of demand which are alleged to underlie forms of the cash-balance approach other than "real balance" variants thereof. See, on this matter, pp. 457 f., below.

[89] For examples, on the other hand, of a usage *similar* to that suggested in the text, see Edie, *Money, Bank Credit, and Prices,* 191 f., where the distinction is made between the "demand . . . for . . . [a] certain absolute sum of money" and the demand for a "sum . . . relative . . . to . . . income and expenditure"; and Ellis, *German Monetary Theory,* 155, on the difference between "the total *absolute* magnitude of reserves"—that is, cash balances—and their "*relative* size, *i.e.,* the ratio of reserves [cash balances] to the year's transactions." Ellis's usage in this respect is based on that of

It has certainly nothing in common, for example, with the usage of Adolf Wagner, for whom, while the "absolute" demand for money was the total quantity of monetary units "demanded," the "relative" demand was apparently the quantity of units of money of ultimate redemption demanded relatively to the *quantity of money-substitutes*.[90]

Nor, obviously, does our usage coincide with that of those writers who have spoken of the "absolute" demand for cash balances as concerning *individuals*, whereas the "relative" demand concerns a *society's* "demand" for cash balances, it being argued that society's "demand" for cash balances is "relative" to a given price level, whereas the demand of individuals is not.[91] According to the usage suggested above, each individual has both an "absolute" and a "relative" demand for a cash balance. His "relative" demand concerns the amount held *relatively* to a given outlay—that is, it affects $k = 1/V$; his *absolute* demand is *affected by* his "relative demand," but it is affected *also* by the absolute volume of transactions—that is, by T, as well as by $k = 1/V$. It follows that, according to the usage suggested, there is no difference whatever between the "relative" and "absolute" demands for cash balances by individuals, when the individual demands are summed together, on the one hand, and the "relative" and "absolute" demands for cash balances by "society," on the other, since the latter is nothing but the sum of individual demands, both "relative" and "absolute," for cash balances.[92]

There is thus no conflict, despite statements to the contrary by some writers, between the central emphasis of the cash balance approach on the decisions of individuals as affecting the "demand" for cash balances, and the suggestion, inherent in equations of the "Fisherine" type, that, *so far as the* absolute *demand for cash balances is concerned*, these decisions will be related not only to the V, but also to the T, of these equations.[93] In both cases, it is individuals who demand a cash balance;

Wicksell (see especially the latter's "Der Bankzins als Regulator der Warenpreise," *loc. cit.*, 231). See also, however, Ellis, *op. cit.*, 191 ff., where a distinction is drawn between "absolute" and "relative" "real balances," without specific reference to Wicksell.

[90] Wagner, *Sozialökonomische Theorie des Geldes*, 160 f. Wagner's "relative" demand, therefore, has to do with the ratio M'/M_r, or c, of our Quantity Equation, whereas our "relative demand" has to do with the $(M + M')/([M + M']V) = k = 1/V$.

[91] See, for example, A. Nielsen, *Bankpolitik*, II, 122.

[92] There is, of course, no intention of arguing that one may obtain the monetary demand of "society" by summing the demand of individuals as the latter would be if the individuals adopted no social arrangements for the "economizing" of cash—that is, the demands of individuals as these "demands" would be realized if the individuals lived *in isolation*. The point is that such social devices will affect the "social" demand only by affecting the demands of individuals living under the "social arrangements" in question.

[93] It is, therefore, only with serious reservations that one could accept the proposition of Professor Mises (*The Theory of Money and Credit*, 132), to the effect that "for individual economic agents it is impossible to make use of the formula: Total Volume of Transactions ÷ Velocity of Circulation." The same thing must be said of Edie's proposition (*Money, Bank*

the difference has to do merely with the emphasis, on the one hand, upon the forces which induce the individual to keep a cash balance of a given size relative to outlay (that is, our K) and an emphasis, on the other hand, upon those additional forces which, when taken into account *along with* the forces determining the size of the cash balance relative to outlay, will affect the absolute size of cash balances which these individuals choose to hold.[94] In the light of the analysis presented above, moreover, there is just as little conflict, despite emphatic statements to the contrary by other writers, between thinking of "the demand for currency as being furnished . . . by the number or amount of *transactions*" and thinking of it as being "furnished" by "the ability and willingness of persons to *hold* currency."[95] From the argument presented above, it should be clear that one of the factors determining the "absolute" demand for currency for "holding" purposes is precisely "the number or amount of *transactions*," in the sense that an increase in the "number or amount of transactions" will, at a given level of prices, and with no change in the "relative" demand for cash balances, require the holding of an absolutely larger volume of cash balances by way of preparation for carrying on these transactions.

It will be observed that the use of the concept of an "absolute" demand for cash balances in the way indicated above —that is, its use only in conjunction with the concept of a "relative" demand for cash balances, which is a component of the "absolute" demand—avoids, by virtue of the fact that the distinction in question makes it possible to consider the forces determining T separately from those determining V, any suggestion that our treatment of the forces determining the "demand for cash-balances" must confine itself entirely to the decisions of cash-balance administrators with respect to the relative advantages of holding cash and of holding wealth in other forms, if it is not to fly in the face of the

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Credit and Prices, 191) that the demand of an individual "is not for any certain absolute sum of money, but for a relative sum of purchasing power —relative, that is, to his income and expenditure."

[94] Alternatively, we may express the fact that T, for example, is one of the factors affecting what has been called the "absolute" demand for cash balances by saying that at a given level of prices (P), and with a given set of conditions determining the demand for cash balances relative to outlay (that is, with a given K), a given absolute amount of money will be "demanded" as cash balances by individuals for the purpose of transacting a given pecuniary volume of business, which would be represented by PT.

[95] So, for example, Cannan, "The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency," *Economic Journal*, XXXI (1921), 453. Cf. also Mises, "Die Stellung des Geldes, etc.," *loc. cit.*, 313 f.

principles of "modern" value theory, which would hold that all market actions must be referred to the choices of individuals.[96] Of such a suggestion, all that can be said is that it would distort a methodological principle of unquestioned validity into a bit of formalism which is as grotesque as it is short-sighted.

For it should be obvious that, from the proposition that we must refer the phenomena of the market to the choices of individuals, it certainly does not follow that these choices of individuals are always of one very special kind—namely, the choice as between the holding of wealth in the form of cash and the holding of wealth in other forms. T, for example, *once it is itself determined by factors other than the decisions of cash-balance administrators,* affects the "absolute" demand for cash balances. Insofar, however, as there is included in T a term for *output*, the choices of "individuals" involved are primarily entrepreneurs' choices as between various productive possibilities in the light of a given structure of prices and a given institutional setting.[97] In the case of the other components of T—for example, the volume of "financial" transactions and the number of "middlemen's sales"—the institutional elements take on even greater importance. There is no reason for arguing that a study of the nature and effect of these institutional elements

[96] It must again be pointed out, in justice to certain protagonists of the "real balances" variant of the cash-balance approach, that the same advantage may be said to inhere in those formulations of the "real balance" approach, such as that of Pigou, in which the k *of* Keynes's equation $n = pk$ is resolved into kR, in which R represents "the community's total real resources of commodities [real income?]," and k represents "the proportion of these reserves that it chooses to keep in the form of titles to legal tender." Pigou's account of the nature of the forces determining R (*Essays in Applied Economics,* p. 180) shows as clearly as one would wish the fallacy involved in Keynes's suggestion that *his* k (or C), which *includes* Pigou's R, is determined "by the decisions of the depositors" (cf. above p. 437). Cf. also the reference to Ellis, on p. 446, n. 89, above. When, indeed, forms of the "real balance" approach of the type $M = P \cdot kT$ are used, there is, from the standpoint of heuristic value, little to choose, apart from the question of "realism" (on which see again p. 436, n. 65 and p. 446, n. 88, above), between a cash-balance approach running in "monetary" terms and one running in "real" terms, despite the vigorous denials of some "real balance" theorists. See, on this latter point, pp. 457 f., below.

[97] This is not to say, of course, that there is not also included in T an element which is related to the administration of cash balances. Cf., however, what is said on this matter on p. 444, n. 84, above; and see pp. 455 f., below.

means an abandonment of the principle that we must study the nature and effect of individual choices as they operate within the institutional framework thus described and analyzed. The only result which can follow from a failure to recognize that vastly more is involved in an analysis of the forces determining the magnitude of "real balances" than a comparison between the "utility" of wealth in the form of money and wealth in other forms is a discrediting of "utility analysis" and of "general value theory" far beyond anything that could be charged against detailed analysis which would be interested in introducing individual choices of a given type whenever choices of this type can be shown to be relevant to a realistic description of the economic process, and not otherwise.

A still greater relapse into the bog of mistaken formalism is represented by the argument that a "real balances" approach is required in order to escape a bugbear which has pursued discussions of the application of "utility analysis" to the problem of the value of money ever since the association was first investigated—namely, the charge of "circularity" in reasoning.[98] The "utility of money," it is argued, is the utility of what money will buy; the utility of money, even when held in the form of a cash balance, is still the utility of what money can buy; to explain, therefore, the value of money—that is, "what money can buy"—by reference to the "utility of money," even in the sense of the utility of a "cash-balance," is to argue in a circle. The only escape from the circle—so it is argued—is to conceive of a cash balance which is not "merely money," but is the money representative of a given volume of real resources. In the employment of cash-balance analysis, therefore, "the theoretical *pons asinorum* is the recognition of the necessity of a *real value calculus*." [99]

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[98] See, for example, Ellis, *German Monetary Theory*, 67 ff., 153, 157, 160 ff., 189 ff., 199 f. The earlier literature on the subject of the applicability of "utility analysis" to the problem of the value of money is much too vast to be summarized here. See, however, the references given to such writers as Weiss, Bortkiewicz, and Hirsch, in my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.*, 592, n. 53.

[99] So Ellis, *German Monetary Theory*, 189 (italics Ellis's). In the argument which follows in the text, a conscious effort has been made, in the interest of avoiding the perpetuation of a type of dispute which has already occupied far too great a place in monetary literature, to refrain from raising the question whether, if the "circle" in question were a real one, it can be said to be avoided by a "real balance" approach any more than it is avoided by a variant of the cash-balance approach which is not stated in "real" terms. The interested reader may, however, be asked to consider whether, *for purposes of the problem with respect to "circularity,"* the situation is greatly improved (1) by distinguishing between the utility of

Unfortunately for this argument, there are much more cogent grounds for contending that the true *pons asinorum* in the problem of the relation between "utility analysis" and "cash-balance analysis" is recognition of the fact that the "circularity" of the argument quoted above results entirely from a mistaken use of the proposition that the utility of money is the utility of what money will buy. There is undoubtedly a sense in which this proposition is always true, just as there is a sense in which it is aways true that nothing can have value unless it has utility—this latter proposition being, in a sense, the inverse of the former proposition, which may be translated to read that money has utility only if it has value. The proposition that nothing has value unless it has utility, however, while it is an axiom which is never to be lost from sight, tells us nothing as to *why things have utility*, or why they have more utility for one person than for another. For this, we need a special type of analysis, most of which, in the case of goods other than money, happens to be largely noneconomic in nature; and in this special type of analysis the proposition that "nothing has value unless it has utility" plays virtually no active rôle.

The same type of comment applies to the special case of cash-balance analysis. One could, with much greater justice, designate as one of the *pontes asinorum* of this analysis the proposition that people hold cash balances because cash has more "utility" than would the wealth which the cash could be used to purchase. For it is clear that this proposition has vastly greater significance than a proposition which, by insisting that the utility of a cash balance is equal to the utility of the goods that might be purchased by the expenditure of the cash balance, might lead one to suppose that it is in all cases a matter of indifference whether one holds wealth in the form of cash or in other forms, since the "utilities" would always be "equal."[100] The real question, of course, is *why*

"money" when held as a cash balance, and the utility of "resources in the form of currency" (cf. *ibid.*, 67, 190); or (2) by the fact that the demand for "resources in the form of currency" which is held to determine the price level, needs, in order that a given amount of "money" may be translated into "real" terms, a "price-level" which assigns to "resources in the form of currency" a given "real" value. This second proposition was, indeed, explicitly stated by Mr. Keynes, at the time when he was still an advocate of the "real balance" variant of the cash-balance approach. See *Monetary Reform*, 46 f.: ". . . the amount of cash which the community requires . . . depends on the level of prices." Cf. also the comment of Hawtrey to the effect that "the formulas of Professor Pigou and Mr. Keynes involve comparisons of the wealth value of the unspent margin at different times," and that "in such comparisons a price index is an essential factor" (*Currency and Credit*, 3d ed., 40).

[100] The latter conclusion is, of course, in direct antithesis to the argument of those supporters of the "real balance" approach, such as Walras and Pigou, who have made it particularly clear that they regarded the "utility" of a cash balance, as such, as inhering in the fact that a stock of money had a specific utility as *money*. It is, however, relevant to the position of those who have seen an escape from "circularity" in the exposition of these writers not because they used this notion of a specific utility of a cash balance, which is quite independent of the concept of "real balances," but

a cash balance has more "utility" to an individual at any given time than do other forms of wealth; and it is of some importance to observe that the answer to this question is *not derived from the proposition that the utility of money is determined by that of the goods it will purchase*—or, if one wishes, from the proposition that money has utility because it has value. If it were derived from either of these propositions, then "circularity" of reasoning might be said to be involved. Actually, however, an understanding of the reasons why a cash balance has more "utility" than other forms of wealth can be derived only from a body of analysis which is as specialized, in its own way, as that analysis—again, largely noneconomic in character—which would explain why butter has more "utility" in the eyes of a given individual than carrots. The precise nature of the content of the special body of analysis which helps us to understand why individuals choose to maintain a given cash balance relative to outlay, will be indicated briefly below.[101] The reader will then have an opportunity to judge the relative importance of, on the one hand, perfecting analysis of this type and, on the other, continuing to advance sterile propositions to the effect that we must, if we wish to avoid "circularity" in reasoning, use the notion of "real balances," in complete disregard of the only question which really matters: namely, whether certain variants of the concept of "real balances" can be shown to be in themselves analytical devices of a sufficient degree of precision and freedom from the type of invitation to positive errors in analysis which is almost inevitable whenever we lump together in a single analytical device elements which are really disparate in their nature.

If, to be sure, the problem of "circularity" were, as has recently been alleged, merely an "alternative" statement of the difficulty that arises from a failure to recognize that certain variables in the Fisherine equation have every claim to "exist each in its own right," instead of being merged into categories which hide their separate identity, something could be said for a continued concern with the problem of "circularity."[102] Unfortunately, however, no supporting argument has been advanced on behalf of the allegation that one proposition is merely an "alternative" statement of the other. We are, therefore, left with the final problem of determining which of the two leading variants of the "cash-balance approach" sins more grievously in the direction of lumping into single categories elements each of which has a claim "to exist in its own right."

Concretely, the charge levied against the practice of conceiving of the "size of balances" in terms of their "money content" rather than their "real content" is that it leads to a denial of the separate existence, "each in its own right," of the terms M and V of equations of the Fish-

precisely because they used a "real balance" variant of the cash-balance approach.

[101] See below, pp. 482 f.

[102] For an example of the suggestion that one of these propositions is "alternative" to the other, see Ellis, *German Monetary Theory*, 189.

erine type.[103] "Nothing," we are told, "can be proven with regard to the behavior of *velocity* by looking toward *money* balances."[104] To attempt to do so is to fail "to consider the fact that total money holdings can neither be increased nor decreased by their possessors, since they coincide with $M + M'$, a magnitude regulated by gold production and the banking system."[105]

It must, however, be obvious that this type of argument would have cogency only if it could be shown that those who would "look toward [the size of] *money* balances" for an explanation of movements in "velocity" had actually argued that the change in the ratio of these balances to money outlay would be expected to come about as a result of the changes in the absolute size of cash balances, rather than as a result of changes in the total of *outlay relatively to these balances.*[106] In fact, of course, the whole argument of those who would "look toward [the size of] *money* balances" *relative to the outlay against which these balances are held* for an explanation of movements in "velocity" assumes that the adjustment will normally come about through an acceleration or retardation of the rate of *spending*—that is, through a variation in total *outlay*—and not through a variation in the total stock of cash balances. It is, indeed, precisely this variation in the rate of spending which constitutes the phenomenon of "velocity." If there has been any denial of the right of V, in this sense, to "exist in its own right," it must surely be held to inhere in the argument of those who have failed to see that there is a vast difference between the "size of cash-balances" in the sense of a given absolute volume of monetary units —which is, indeed, a matter affecting $M + M'$—and the "size of cash-balances *relatively to outlay,*" which is the only matter that concerns the "behavior of velocity" directly.

Strangely enough, however, it is precisely the possibility of tracing the "behavior of velocity" to variations in the amount of money outlay relatively to the volume of money balances held against this outlay that has been called into question. "Total *money* outlays," we are told, "are the component (*sic*) of volume of trade times prices; but prices *depend* upon velocity, concerning which nothing is known . . . until the ratio of money balances and *money* outlays is established"; hence there is no

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[103] See Ellis, *loc. cit.*

[104] *Ibid.*, 153 (the italicizing of the word "velocity" is my own; the other italics are Ellis's).

[105] Ellis, *loc. cit.*

[106] It may be pointed out, in passing, that this statement should not be taken to mean that there is no sense in which a change in the ratio could be said to come about "as a result" of a change in the absolute volume of money balances. An example is provided by the earlier stages of certain types of inflation, in which the addition to the money supply has largely taken the form of the holding of large cash balances relatively to outlay. Yet there is much to be said for arguing that, even in such cases, the "reason" for the change in "velocity" is not the increase in the total volume of cash balances, as such, but the decisions of cash-balance administrators with respect to outlay.

possibility of "proving anything" with regard to "the behavior of velocity" by looking at the ratio between money balances and money outlay.[107]

It is easy, however, to show that, in this syllogism, a fallacious major premise has led to a fallacious conclusion. To say that total money outlays are *equal to* "the volume of trade times prices" is not to say that they are the *same thing* as "the volume of trade times prices."[108] The total of money spent by individuals is one thing; what they spend it on is quite a different thing. Velocity is a factor involved in the "total of money spent"; it is "established" simultaneously with the ratio of money spent to the stock of cash balances, since it is identical with that ratio.[109] *Prices* are established as the result of the impact of the total of money spent upon the stream of goods offered against money; they "depend," consequently, upon the magnitude of the components of each of the two streams. Since, therefore, "velocity" is a component of the "stream of money," there is no conflict whatever between accepting the proposition that prices "depend upon velocity" and using a form of the cash-balance approach which runs in terms of the ratio of money outlay to the volume of money balances.

The whole matter would, indeed, be hardly worthy of serious discussion if it were not for the fact that the introduction of the element T (the volume of "trade") into the discussion of the process by which cash balances are brought into a given relation to outlay makes it possible to judge the relative degrees of precision which may be held to attach, for purposes of understanding "the behavior of velocity," to certain variants of the "real balance" approach, on the one hand, and a variant of the cash-balance approach which runs in terms of the relation between money balances and money outlay, on the other. There is not the slightest doubt that, in an attempt to change the size of cash balances held relative to outlay, the administrators of cash balances may

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[107] Ellis, *German Monetary Theory*, 153.

[108] It will be observed that the fallacy involved is precisely that which is represented by the suggestion that the quantity equations are "mere identities," since the second member of the equations is merely a rewriting of the first member. See above, pp. 88 ff.

[109] It may be pointed out that it is more than a little misleading to suggest that those who look to "the ratio of money balances and money outlay . . . believe that velocity is *determined* by this ratio" (Ellis, *German Monetary Theory*, 153; italics mine). "Velocity" is *identical* with this ratio; and the argument with respect to what "can be proven" by the use of the ratio in the discussions of the forces determining "velocity" is merely that the use of the ratio reminds us of the fact that movements in "velocity" are the result of decisions, by administrators of cash balances, with respect to the size of cash balances which will be held relatively to outlay. Once these decisions are carried into effect, it is a matter of complete indifference whether we speak of a change in the ratio or a change in "velocity." There is certainly no suggestion that the change in the ratio "determines" velocity; since the ratio and "velocity" are identical, they are both "determined" by the *decisions of cash-balance administrators with respect to the size of cash balances held relatively to outlay.*

be expected to alter not only the rate at which money is spent against goods, but also the rate at which goods are exchanged against money.[110] In the attempt, for example, to build up large cash balances relatively to outlay, it is quite probable that the administrators of cash balances may not only reduce the rate at which they spend their money receipts and such cash as they happen to have with them in the form of a balance, but may also offer a larger quantity of goods against money than they would normally offer. It is extremely important, however, to see that what is involved, in this case, is not a change in the velocity of *money* at all. On the contrary, in so far as a problem of "velocity" is involved, we are dealing here with that component of what has been called the "velocity of circulation of *goods*" which may be called the "rate of sale" of those goods.[111] The "velocity of circulation of goods," as we shall see, is a component of T, not of V. T, or more precisely, that one of its components which has been called the "rate of sale" of goods, comes into the picture as the *accompanying and intensifying obverse* of the change in the rate of money spending—that is, the "velocity" of *money*.

Surely it is not unfair to point out that this distinction, which is taken care of so neatly by a variant of the cash-balance approach that runs in terms of the ratio of money balances to the money outlay held against these balances, is precisely what is obscured by so many of the "real balance" variants of the cash balance approach.[112] According to

[110] It is of some importance to stress the fact, which has often been ignored, that the two "rates" are by no means necessarily equal and offsetting. See my two articles on "The Relation Between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " in the *Journal of Political Economy*, XL (1932), especially pp. 506 f.

[111] On the concept of a "rate of sale" of goods, see below, p. 563, and the references given in n. 107 thereto. It may be noted, also, that T may change, in the process of adjustment of cash balances, not only as a result of a change in the "velocity of circulation"—in this case, the "rate of sale" —of goods, but also as a result of a change in the proportion of "goods intended for sale" to "goods produced." On this matter, which is important also for an understanding of certain cases which have been subsumed under the head of an "excess of savings over investment," see below, pp. 540 ff.

[112] It is again only fair to point out that the degree to which this fact is obscured varies as between different versions of the "real balance" approach. It is certainly obscured by "real balance" equations of the form of Keynes's $n = pk$; it is not so completely obscured by an equation such as that of Pigou's, in which, since Keynes's k is replaced by the expression kR, it is at least possible to distinguish between k, the reciprocal of "velocity," and other elements changing the size of "real balances." On the other hand, Pigou's R does not include "transactions" of the type indicated in the text; and a formulation of the "real balance" approach which would replace Pigou's kR by the expression kT would be superior on this acount, if for no other reason. For an example of a "real balances" formulation which runs in terms of a proportion of the real volume of *transactions*—or, as Robertson puts it (*Money*, 2d ed., 41), "the proportion of their *annual real turnover* which people wish to have enough money at hand to conduct"—see Ellis, *German Monetary Theory*, 191 ff. (cf. also the following note); and

these variants, it is a matter of indifference whether the "volume of real balances" is increased as a result of a decrease in the ratio of money outlay to the volume of money balances, or as a result of the increase in the rate at which goods are exchanged for money, because of the desire to hold larger cash balances relative to outlay. Yet there is a simple proof of the danger of treating the distinction with indifference in the fact that it is perfectly possible to have a change in the ratio of money outlay to the stock of money—that is, in V—without such a change being accompanied by what was described above as an "accompanying and intensifying obverse" in the form of an opposite change in the rate of sale of goods. This, indeed, is precisely what will happen whenever the efforts of administrators of cash balances to effect a change in the ratio of money outlay to the money stock take the form solely of a change in the rate at which money received is spent, or of a change in the rate at which parts of cash balances which were previously kept unspent, are spent. The "accompanying and intensifying obverse" of such actions comes into play only when the administrators of cash balances carry their desire or aversion for cash balances beyond a change in the rate of disbursing current money receipts and current cash balances to the point of varying the rate of sale of goods. When this happens, it is to be repeated, what is involved is not a change in the velocity of circulation of *money*, but a change in the "velocity of circulation" of *goods;* and it is here argued that the fact that both types of action have the effect of changing the "volume of real balances" is, from the standpoint of the desideration of an analytical apparatus of maximum articulateness and precision, a defect rather than a merit of those variants of the "real balance" approach which fail to draw a sharp distinction between the two.[113]

The argument developed above may thus be summarized as representing an application, to the special problem of the choice between the different variants of the cash-balance approach, of the principle advanced at an earlier point in this chapter with respect to the more general question of the reasons for an interest in the association of the theory of money and prices with the "general theory of value": namely,

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on the more general questions involved in a discussion of the components of T other than the R of Pigou's formulation, see below, pp. 518 ff.

[113] It is worth noting that no mention is made of the element of a "rate of sale" of goods as a component of T in Ellis's argument in favor of a substitution of T for the R of Pigou's formulation, the only element introduced which is associated with the "velocity of circulation of goods" being what may be called the "number of middlemen's sales," involved in Ellis's "problem of industrial differentiation" (Ellis, *German Monetary Theory,* 191). The fact, obviously, that it is possible to have a change in the "velocity of circulation of goods," and therefore in T, even though there is no change in either the "number of middlemen's sales" or the velocity of circulation of money would warrant the suggestion that we have here a further indication of the dangers inherent in the use of a "real balance" approach, even when the latter is expressed in its least exceptionable form—that is, in a form in which explicit notation is given to T.

that the association is of genuine interest only in so far as it can be shown to be of specific heuristic value within the field under investigation.[114] It happens, however, to have been alleged, on behalf of the "real balance" variants of the cash-balance approach, that they *not only* avoid the "circularity" of a cash-balance approach stated in "monetary terms," but also prove to be the "more realistic" of the two types of variant.[115] Since the greater "realism," in this case, is alleged to turn upon the supposed impossibility of obtaining, by means of the use of a variant of the cash-balance approach which runs in "monetary terms," results which are *accurate*, the argument in question cannot be passed over in silence.

Briefly, the argument is that it is only by the use of a "real balances" variant of the cash-balance approach that we are able to represent the demand for cash balances as having an elasticity different from unity.[116] As it happens, it is possible to point to instances—Cannan is an outstanding example—in which a writer whose "cash-balance" analysis has been characterized as being completely free (supposedly to its disadvantage) from the implications of the "real balance" approach, has insisted with all possible emphasis upon the fact that there is no reason whatever for assuming that the demand for cash balances will always have an elasticity of unity.[117] On the other hand, it is precisely writers such as Walras and Marshall, both protagonists of a "real balance" variant, who, in their graphic presentation of the demand for cash balances, used a rectangular hyperbola.[118] The question arises, therefore, as to just why a cash-balance approach which runs in "monetary terms" should lead to the conclusion that the demand for cash balances must be represented by a curve of this latter type.

The argument in question gives an answer that can hardly be regarded as carrying conviction. "Cash balances," we are told, "must be made separate from the mere quantity of money existing within the country": "so long as the calculus runs in terms of dollars," it is impos-

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[114] Cf. above, p. 442.

[115] See Ellis, *German Monetary Theory*, 161.

[116] Ellis, *loc. cit.*

[117] See Cannan, "The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency," *loc. cit.*, 458 ff. On the freedom of Cannan's analysis from the implications of the "real balance" approach, see Ellis, *German Monetary Theory*, 190.

[118] On Walras, see my "Léon Walras and the 'Cash-Balance Approach,' " *loc. cit.*, 578 f. and the references there given. On Marshall, of whose exposition on this point Ellis, interestingly enough, is aware (cf. Ellis, *German Monetary Theory*, 142 n.), see *Money, Credit and Commerce*, p. 282. I have of course not the slightest intention of advancing the converse of the allegation criticized in the text, in the form of a proposition to the effect that the use of the concept of "real balances" necessarily leads to the assumption of unitary elasticity of demand. There is no evidence that either Walras or Marshall had anything more in mind, in representing the demand for money as having an elasticity of unity, than a desire to provide a simple diagrammatic representation in which the demand for the standard metal as *money* would be easily distinguishable graphically from the arts demand for the metal.

sible to conceive of the "psychological make-up of the economic subject" as entering the process of cash-balance administration in such a way as to give us something other than a demand for cash balances of unitary elasticity.[119] The fallacy in this argument becomes apparent as soon as it is remembered that all that is involved in the representation of the demand for money as having the form of a rectangular hyperbola is a graphic statement of a particular variant of the "quantity theory" in which no room is left for such elements as changes in "velocity." The fact, however, that a satisfactory statement of the demand for cash balances in "monetary terms" would insist upon considering the size of cash balances not only *absolutely*—that is, as equivalent to "the mere quantity of money existing in the country"—but also, and primarily, their size *relative to outlay* shows that it is precisely with changes in "velocity" that the cash-balance approach, even when stated in "monetary" terms, is directly concerned. It has been our argument, indeed, that it is precisely the "monetary" variants of the cash-balance approach which run least danger of obscuring the fact that it is with the explanation of changes in *velocity* that the "cash-balance approach" is primarily concerned.[120] In the light of this central fact, to argue that the "monetary" variants of the cash-balance approach are of no help whatever for the purpose of ascertaining "what *determines* the specific volume which the individual decides to hold"; for allowing, to this end, "the psychological make-up of the economic subject to enter the process"; and for reaching the conclusion that the demand curve for cash balances may take a functional form other than that represented by a rectangular hyperbola, is merely to suggest again the dangers of an excessive concern with matters which, when all is said, are of a purely formal nature and have little indeed to do with the issues of substantive analysis which still await definitive solution.

[119] Ellis, *German Monetary Theory*, 160 f.
[120] Cf. above, pp. 433 ff.

CHAPTER SIXTEEN

The Cash-Balance Approach (*Continued*)

I

Money "in Circulation" and the Cash-Balance Approach

AS THE discussion presented in the preceding chapter has shown, the cash-balance approach can hardly be regarded as having been examined, in Mr. Keynes's *Treatise*, with the sympathy and understanding one might have expected from one who was himself a figure of some importance in the popularization of that very approach. Yet this is not all that can be said against the argument of the *Treatise* in this respect. For it can be shown that Mr. Keynes's failure to penetrate to the real meaning of the "cash-balance" approach led him to adopt a series of positive arguments, of an extremely misleading nature, which a clear grasp of the principles underlying the cash-balance approach would have made it possible to avoid.

The first of these arguments involved the sort of reasoning which has appeared in economic literature at least since the days of Hume, in the guise of a distinction between money "in circulation" and money "out of circulation"—or, as Mr. Keynes himself put it, between "coins and notes which were being actually used *as money*," and "hoards." [1] It was Mr. Keynes's contention that this distinction was one which was necessary for the "clarity" of the very conception of "velocity of circulation"; "for, otherwise, an increase (or

[1] Cf. the *Treatise*, II, 20 f. (italics Keynes's). Examples from the literature of the type of usage in question, with arguments virtually identical with those adduced by Mr. Keynes, are so numerous that citation of even the most important of them is impossible here. On the general point involved, however, cf. my articles in the *Journal of Political Economy*, XL (1932), 483 ff., and in *Economica*, 1932, 441 ff.

decrease) in the amount of the hoards would appear as causing a decrease (or increase) in the velocity of the money, whereas what they were really causing was a decrease (or increase) in the supply, or *quantity,* of effective money." [2]

In actual fact, however, a consideration of the implications of the "cash-balance approach" should have shown that what is called for, in the interest of "clarity," is precisely the abandonment of the distinction between money which is "in circulation" and money which is not "in circulation," in the sense in which Mr. Keynes employed the distinction.[3] It is one of the merits of the cash-balance approach that it calls attention to a circumstance the reality of which it is not possible to deny: namely, that "money"—including what Mr. Keynes called "the quantity of *effective* money"—does not, during the greatest part of its existence, "circulate" at all. At any given moment, by far the greater portion of the so-called "circulating" part of the money stock is, in the homely phrase of Mr. Robertson, "money sitting"; only a very small part is really "money on the wing."[4] Once this is recognized, it becomes obvious that the distinction between money "in circulation" and money "out of circulation" is an extremely tenuous one. This proposition was perfectly clear to Simon Newcomb, whose sponsorship of it is particularly noteworthy in view of Newcomb's importance in the history of an emphasis on the distinction between "stocks" and "flows."

". . . when we speak of a *flow,*" wrote Newcomb, "we introduce a conception which does not strictly conform to the actual case, because at

[2] *Treatise,* II, 20.

[3] This is not to say, of course, that the phrase "money in circulation" may not be used in a sense quite different from that employed by Mr. Keynes. There is no objection, for example—apart from the danger of association with the type of distinction to which exception is taken in the text—to speaking of money "in circulation" in the special sense in which it is used in American monetary statistics—namely, as the volume of *money held as cash balances outside of banks.* Cf., on this matter, p. 148, above.

[4] See Robertson, *Money,* 2d ed., 31, 36, 40 ff. The expressions, as far as I am aware, are original with Mr. Robertson. (The appearance of a similar set of terms in the English translation of Wicksell's *Lectures* [II, 21], is the result of a loose—and indeed inaccurate—rendering of Wicksell's words by the translator.) The *point,* however, that at any given moment, by far the greater portion of the stock of "circulating" money is what Mr. Robertson called "money-sitting," has been made repeatedly in the literature. For a contemporary example, see Hawtrey, *Good and Bad Trade,* 10.

no time is money really flowing like a fluid from person to person. Excepting such cases as that of transmission by mail, money is always in possession of some one person, and it passes from one person to another in a moment by the act of payment. It would therefore be more exact to consider the circles [in Newcomb's diagram representing the "flow of currency"] as representing reservoirs of money, and the motion along the arrows [in the same diagram] to take place by sudden transfers from one reservoir to another."[5]

A more emphatic statement of the proposition, however, had been presented some eighteen years before Newcomb wrote, by a critic of John Stuart Mill:

"What becomes of money which (to use the common phrase) is in 'active circulation' in this country? In what condition does it exist? All writers speak of the coins and notes so employed as if they were *in vacuo*—in a limbo where they belong to no one in particular,—as if, like Mahomet's coffin, which is believed to remain suspended between heaven and earth, they existed in a middle state between the payer and receiver, and belonging to neither. This, of course, like many other prevalent notions in monetary science, is a mistake: it is sheer nonsense. The money said to be in 'active circulation' . . . is all in the possession of individuals, who can employ it each in his own way. Every man keeps on hand so many coins or notes for his current wants. He keeps at home so much money for small household wants, and carries so much also in his purse for cabs, dinners, or casual purchases or payments. Even the working-man leaves so many shillings at home, and carries a smaller trifle in his pocket for his own daily wants . . . The many millions of coin and notes 'absorbed in circulation' . . . are the reserve-wealth of individuals for the day or the week, or the fortnight. . . . All Money . . . is reserve-wealth belonging to individuals."[6]

It is clear that this argument is of the greatest importance for a judgment as to whether the ancient distinction to which Mr. Keynes has acknowledged allegiance invites "clarity" in discussion of the concept of "velocity of circulation," or whether it does precisely the opposite. The concept of velocity was designed, as we have seen, to establish the relation between a *stock* of money, and the *stream* of

[5] Newcomb, *Principles of Political Economy*, 319.

[6] R. H. Patterson, *The Science of Finance* (Edinburgh and London, 1868), 76 ff. Since Patterson's day, the contention that "it is quite impossible to draw the line between circulating and non-circulating money" has been advanced by many defenders of the cash-balance approach, of whom Wicksell may be taken as typical. See, for example, the latter's *Lectures*, II, 21; also his "Den dunkla punkten i penningteorien," *loc. cit.*, 491. For an example of the appearance of the contention in current textbooks, see Edie, *Money, Bank Credit and Prices*, 193 f.

money payments which that stock may be regarded as supporting. So long as we adhere to the concept of the stock of money as being made up of a sum of cash balances which rest "sitting" in anticipation of outlay, the "stock of money," and therefore the "velocity" which is regarded as attaching to it, has a definite meaning: that is, the stock of money is the *total* stock of money in a country, minus the amount kept as currency and banking reserves held against forms of money substitutes which, in turn, make part of the total of cash balances.[7]

The moment, on the other hand, that we abandon this concept of the stock of money as "money-sitting," we run into the necessity for a type of construction that is as unsatisfactory practically as it is arbitrary logically. One might insist, if one wished, in the manner of the "income theorists" discussed in Chapter XIII, that the concept of a "stream" of money makes the concept of "velocity" an element of "superfetation"; in that case, the issue becomes merely one of deciding whether, *for purposes of understanding why the "stream" of money is as large as it is,* we can dispense with emphasis upon changes in the "stock" of money, and therefore with the concept of "velocity."[8] What one may not do is to accept the position that the "stock" of money is important and that we cannot dispense with the concept of "velocity," and then set up criteria for describing the stock of money in terms of the amount "in circulation" and the amount "out of circulation" which must continue to be of the highest degree of arbitrariness in the face of the hard fact that most of the "money" of a country, most of the time, is not "circulating" at all. Surely the "stultification" of the concept of "velocity" which Mr. Keynes saw in including, in the stock of money to which the factor of "velocity" is to be applied, "money which was not in circulation at all," comes, not from including, but from failing to include, this money.[9] The failure to do so is resolvable in its turn

[7] The present discussion, it will be observed, provides the logical basis for the particular definitions given to the M and M' of our Quantity Equation on pp. 148 ff., above.

[8] Cf. above, pp. 345 ff.

[9] For the quotation from Keynes, see the *Treatise,* II, 21.

into a failure to see that the distinction between money which "circulates" and money which does not "circulate" should really be stated as a distinction between money which "circulates" more rapidly, and that which "circulates" less rapidly. The factor of greater or less "rapidity of circulation" is precisely what is measured by the coefficient for "velocity"; given this coefficient, the attempt to establish a distinction between money which circulates and that which does not circulate as a difference in *kind* becomes, in truth, an element of "superfetation." [10]

Nor is this all. Mr. Keynes's distinction between money which circulates, and that which is "hoarded" turns, as we have seen, upon the assumption that money which is being hoarded is not "being actually used *as money*." His argument was that such "money" was merely a "store of value," and "therefore had *no* velocity." [11] Yet this is a type of

[10] It will be observed that recognition of the fact that there is no genuine difference in *kind* between money which "circulates" and money which "does not circulate" is not to be taken to mean that it is not permissable, or advantageous, to segregate, for purposes both of analysis and statistical measurement, various types of cash balance, or various parts of a cash balance, which can be shown to differ, among other respects, in their relative "velocities of circulation." The detailed argument for favoring such a procedure, which has been advanced with particular cogency by contemporary German writers, of whom H. Neisser is the most influential, cannot, however, be discussed in detail here. For Neisser's distinction between cash balances as "reserves" (*Kassen***reserven**) and as "working funds" (*Betriebsfonds*) see especially his *Der Tauschwert des Geldes,* 17 ff. and also his "Der Kreislauf des Geldes," *loc. cit.,* 390. For examples of his influence upon later writers, see F. Burchardt, "Entwicklungsgeschichte der monetären Konjunkturtheorie," *Weltwirtschaftliches Archiv,* XXVIII (1928), 136; G. Haberler, in *Economica,* February, 1934, 101; K. Bode and G. Haberler, "Monetary Equilibrium and the Price Level in a Progressive Economy: A Comment," *Economica,* February, 1935, 77. It may be pointed out, in passing, that Neisser's distinction is not a new one. It was, for example, implicit in Adolf Wagner's differentiation of "reserves" from other types of "capital deposit," or of "producers' money." See, for example, Wagner's *Beiträge zur Lehre von den Banken,* 61 f., and his *Sozialökonomische Theorie des Geldes,* 166. An analogous distinction was made also by Menger and Wicksell. See, for example, Menger, "Geld" (*The Collected Works of Carl Menger,* IV, 327), and Wicksell, *Lectures,* II, 71 (cf. *Interest and Prices,* 61). For an example, contemporaneous with Neisser, of emphasis upon the desirability of distinguishing "cash-*reserves*" from other parts of the cash balance, see also Holtrop, *De Omloopssnelheid van het Geld,* 114 (cf. "Die Umlaufsgeschwindigkeit des Geldes," *loc. cit.,* 132).

[11] See, for example, the *Treatise,* II, 21. Mr. Keynes, in assigning to "hoarded" money a velocity of "zero," was following a practice so common in the literature that a representative list of citations would occupy far more space than can be afforded here. So far as the proposition that money which is functioning as a "store of value" is not being "used as

reasoning which cannot be accepted by anyone who accepts the fundamental principles of the cash-balance approach.

For, in the first place, as Professor Mises has shown with the greatest clarity and emphasis, the suggestion that money which is being "hoarded" is not being "used as money" is absurd in the light of the motives for which people hold money altogether.[12] To say that money is used "as money" only when it is being used as a medium of exchange, and not when it is used as a "store of value" is to forget that the "store of value" function of money is *part* of the medium of exchange function.[13] People hold money in "reserve" because money can act as a "store of value" with a much greater assurance against loss in value when the time comes to exchange it for other commodities, than can other forms of wealth; or because, as other writers have put it, it is a "bearer of options" in a degree in which this can be said to be characteristic of no other commodity.[14] To suggest that

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money" is concerned, it may be pointed out that there were passages in the *Treatise* which would seem to indicate that Mr. Keynes was not always convinced as to the truth of this proposition. See, for example, the *Treatise*, I, 3, on money as "that . . . in the shape of which a *store of General Purchasing Power* is held," and II, 289, on the money metal as "the most suitable commodity for holding a *store of value or a command of purchasing power*" (italics mine).

[12] Cf. Mises's *Theory of Money and Credit*, 147 ff., 423; also the same author's "Die Stellung des Geldes, etc.," *loc. cit.*, 313.

[13] See, in this connection, the remarks by Wicksell, on the "store of value" function of money as involving merely the "hoarding" of "a future medium of exchange" (*Lectures*, II, 8; cf. II, 23), and, conversely, on the "medium of exchange" or "means of payment function" as "including" the "storing of value" during "the period between a sale and a subsequent purchase, or, more generally, between a payment received or advanced and a payment by the receiver" (II, 15). See also, and especially, the *Lectures*, II, 142, where Wicksell explicitly stated that "we may regard these two concepts [that is, money as a medium of exchange and money as a store of value] as different aspects of one and the same function."

[14] See, in this connection, H. J. Davenport, *Outlines of Economic Theory* (1896), 243, on the relation of "the option feature" to the "demand for currency"; cf. also the same writer's *Economics of Enterprise* (1913), 269, on money as "an option of use," and his *The Economics of Alfred Marshall* (1935), 73 and 70, on "the option aspect of an intermediate of exchange." For a more extensive use of the concept of money as a "bearer of options," see Anderson, *The Value of Money*, Index, under the entry "'Bearer of options' function of money." Anderson himself "distinguishes" between the "store of value" function and the "bearer of options" function (see, for example, *op. cit.*, 425). In fact, however, the only reason why money may be said to act as a "store of value" in any sense in which the same thing could not be said of any other conceivable form of wealth is that it is a better

money held in reserve is not acting as a medium of exchange is to imply that money so held will *never* be spent for commodities; for if and when it is spent, it is merely fulfilling the second step in the exchange which began when the money was received.[15] What meaning, in the light of the fact that all money held in "reserve" is intended, by all but unbalanced misers (to whom the possession of money is supposed to be a pathological joy in itself), to be spent at *some* time, can be attached to the proposition of Mr. Keynes—and of the many other writers who have followed a similar practice—that this money has "*no* velocity"?[16] The only way in which it could be said to have *no* velocity would be to assume that it would not be spent in all eternity.[17] If it is spent once in a thousand years, it has a velocity of 1/1000 per annum, not a velocity of zero. The theory with respect to the amount which will be held as "hoards" is nothing more nor less than a part of the theory of the forces determining the size of cash balances relative to outlay.[18] And this is merely another way of saying that the theory of

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"bearer of options" than most other forms of wealth. There is every reason for arguing, therefore, that to say that money acts as a "store of value" means, if anything, that it acts as a "bearer of options." On the fact that certain forms of wealth other than money may also act as "bearers of options," see below, pp. 470 ff.

[15] Cf. Davenport, *The Economics of Enterprise*, 256: ". . . the intermediate [that is, money] may be a storehouse of purchasing power. The second half of the barter may be deferred. The intermediate is generalized purchasing power. Delay is one of the privileges which especially the intermediate function carries with it."

[16] It is worth stressing the fact that the only misers' hoards which could possibly be regarded as having "no velocity" are the hoards of misers who are literally "unbalanced," in the sense that they are unlike those whom Adam Smith had in mind when he declared that "even the miser who locks up his gold in his chest has . . . in view" the fact "that as money is the instrument of commerce, a man can change it for the necessities and elegancies of life more easily than anything else." "No man in his senses," Smith added, still speaking of "the miser," "hoards up money for its own sake, but he considers that by keeping money always by him, he has it in his power to supply at once all the necessities of himself and his family." See Smith's *Lectures on Justice, Police, Revenue, and Arms* (edited by Cannan), 200.

[17] On the fact that the "inactivity" of money is not "definitive," and on the relation of this fact to the concept of "velocity," see G. Del Vecchio, *Lezioni di economia pura* (2d ed., 1932), 243.

[18] Cf. Mises, *Theory of Money and Credit*, 148: "Hoarding money is nothing but the custom of holding a greater stock of it than is usual with other economic agents, at other times, or in other places."

"hoards" is part of the theory of the velocity of circulation of money.[19]

II

"Savings Deposits," Cash Balances, and "Velocity"

From this simple argument, which is merely an explicit application of the general principles of the cash-balance approach, it is easy to pass to a set of corollaries which are in direct conflict with other details of Mr. Keynes's usage as found in the *Treatise*. Consider, for example, Mr. Keynes's treatment of "savings deposits." It is a merit of this treatment that it does not follow the example of those who would deny, on grounds which are anything but conclusive, that savings deposits represent part of the money stock of the country. As against writers who have insisted that savings deposits do not make part of the money stock, Mr. Keynes's own position was unequivocal: Savings Deposits, according to the *Treatise*, were definitely part of "Total Deposits," and therefore definitely part of the total stock of "Bank-Money." [20]

The literature on the question whether savings deposits are or are not to be regarded as "money" is so extensive that it is possible to cite here, by way of example, only the argument of Lauchlin Currie, in his *Supply and Control of Money in the United States*. Currie's criterion for deciding whether "time deposits" should be "classified as money" is "whether . . . payment is made by the direct transference of time de-

[19] It is, indeed, striking that the first serious attempt of monographic dimensions to deal with the literature and the problems associated with the concept of the "velocity of circulation of money" was E. Kellenberger's *Geldumlauf und Thesaurierung* (Zürich, 1920), the connection between "hoarding" and "velocity" being established at the outset by the proposition that the one was the "reciprocal" of the other (*op. cit.*, vii).

[20] See, for example, the *Treatise*, II, 7. It is true that Mr. Keynes seemed on occasion to imply a different conclusion—as when, for example, he suggested that "savings deposits" are "scarcely money at all (not much more than *e.g.* a Treasury Bill is)." (*Treatise*, I, 43.) The assumption, however, that "savings deposits" are "money," clearly underlay the whole of Keynes's discussion with respect to that "demand for money for financial purposes" which played so great a rôle in his famous discussion of the effect of "bull" and "bear" positions on the "equilibrium between savings and investments." See the *Treatise*, I, 248 ff. The assumption is implicit also in the suggestion, offered in a related context, that it lies within the power of "the banking system" to bring out a "creation" or "contraction" of "savings deposits" (see, for example, the *Treatise*, I, 142).

posits without the use of demand deposits or cash."[21] There is no intention of denying here that this distinction is important for some purposes; but it must be obvious that those who would regard "time deposits" as "money" might easily retort that if the fact that a "deposit" may have to be changed by its owner into other forms of "money" before it is spent means that the "deposit" is not "money," then a considerable volume of "demand deposits" are likewise not "money," since they, too, may have to be turned into "cash" before they are spent—as, for example, in the case of the internal drain. The argument for holding that "savings deposits" are part of the money stock of the country would stress rather (1) the fact that reserves of money of ultimate redemption must be kept against "savings deposits" as well as against demand deposits, whereas they do not have to be kept against "any property possessing good marketability which by sale can be converted into means of payment," so that in this respect, at any rate, serious reservations must be entered against Currie's proposition that "time deposits . . . do not differ essentially from such forms of property"; and (2) the much more significant fact that very few, if any, of the "forms of property" mentioned by Currie as not differing "essentially" from savings deposits have the peculiarity with respect to "marketability" which attaches to savings deposits—namely, "marketability" with the assurance, in all but the most exceptional circumstances, of no change, either upward or downward, in the dollar value of the principal.[22] The first fact is of very great importance for monetary policy; the second fact is of extreme importance analytically, particularly when viewed in the light of the fundamental problem which it is the task of cash-balance analysis to solve: the problem, namely, why individuals choose to keep a given amount of wealth in the form of money, rather than in other forms.[23]

The part of the treatment accorded to "Savings Deposits" in the *Treatise* against which objections must be raised is not, therefore, that part in which Mr. Keynes argued that "Savings Deposits" represent a segment of the monetary stock. It is rather the part in which he argued against including Savings Deposits in the stock of money *to which the*

[21] Currie, *The Supply and Control of Money*, 14.

[22] This would certainly not be true, for example, of "government bonds," which are cited by Currie in this connection (*op. cit.*, 14). The case of "call loans," on the other hand, which is likewise cited by Currie as being equivalent to "time deposits," would seem to be ruled out of the category of "money" for the same reason that "individual claims on other individuals" have been ruled out as "part of the stock of 'purchasing power'" —namely, that "if valid individual claims to immediate payment are included as means of payment, then individual liabilities to immediate payment should be subtracted therefrom" (so Viner, *Studies*, 247).

[23] Cf. also, in this connection, what is said on pp. 477 f., below, with respect to Keynes's treatment of "cash-facilities."

factor of "velocity" was to be applied. This, he argued, was "tantamount to treating 'hoards' as *cash*," and to "treating as current money that which is really a 'store of value.'" [24] From this it was concluded, naturally enough, that savings deposits were to be regarded as having a velocity of circulation of zero.[25]

As we have seen, however, it follows, from the principles underlying the cash-balance approach, that, so long as the money which is described as acting as a "store of value" is destined to be spent at a later date—and it was of the essence of the chapter in the *Treatise* on "The Proportion of Savings-Deposits to Cash-Deposits" that account must be taken of the possibility that savings deposits may move toward expenditure at any time simply as a result of their being treated by their owners as if they were current accounts—such money cannot be regarded as having a velocity of circulation of "zero." [26] The most that possibly could be said is that savings deposits can be shown to have a *lower* velocity of circulation than so-called "demand" deposits.[27] This in itself, to be sure, provides sufficient warrant for segregating savings deposits from other types of deposit, in computations of Velocity, for the same reasons which argue for the breakdown of any global average into significant subaverages.[28] There are grounds, also, for arguing that

[24] *Treatise,* II, 21; cf. also I, 36.

[25] Cf. the *Treatise,* II, 23, 34.

[26] For Keynes's recognition of—indeed, insistence upon—the fluidity of the relation between "Savings Deposits" and "Cash-Deposits," see not only the chapter referred to (*Treatise,* II, 7 ff., especially pp. 10, 12), but also I, 38.

[27] Cf. Hawtrey's comment on Keynes's statement that savings deposits have a velocity of circulation of "zero," in *The Art of Central Banking,* 371 n. See also, and more generally, the discussion on pp. 87 f. of the same work.

[28] A breakdown of this type would obviously answer Mr. Keynes's objection that a figure for "velocity" which would include the "velocity" of savings deposits would fail "to put us on the right track for discovering what sort of circumstances will change the velocity of circulation" (*Treatise,* I, 236). It is of some importance to emphasize again the fact that this does not mean that the global figure for velocity would not be a "true" velocity. Cf., in this connection, the argument on p. 394, above, in the light of which it must be obvious that Mr. Keynes's "Efficiency"—that is, the ratio of total payments to "total deposits"—is as truly a "velocity" as what he himself called "Velocity," as opposed to "Efficiency": namely, the ratio of the so-called "cash-deposits" to the payments against which "cash" deposits are held (see the *Treatise,* II, 22). This fact was obscured

the particular cash balances which are represented by "savings deposits" are more sensitive to certain elements, including the rate of interest and other factors, than are other types of cash balance, and should therefore be segregated from other types of deposit for analytical purposes, as well as for the purpose of computing significant statistical measures of "velocity."

It should be observed, however, that the argument for segregating "savings deposits" from other types of deposit is the same as that which Mr. Keynes himself advanced on behalf of segregating "business deposits" from "income deposits"—namely, that the various types of "deposit" are likely to be subject to different controlling forces and are therefore likely to be associated with differing "velocities." [29] This, obviously, is a vastly different thing from arguing that savings deposits have a "velocity of zero." It must be equally obvious, moreover, that the characterization of deposits other than savings deposits as representing "the active circulation," should not imply anything more than that cash balances of the type included in "the active circulation" have a *more* "active" circulation—that is, have a higher velocity of circulation—than do savings deposits.[30]

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by Keynes's practice (see, for example, the *Treatise,* II, 34), of excluding from the algebraic formula for the "total of payments" which represents the numerator of his formula for "Efficiency," the total of debits to savings accounts.

[29] Cf. the *Treatise,* I, 48 f., 245 f., II, 34, n. 2.

[30] Contrast, in this connection, Kahn, "Dr. Neisser on Secondary Employment," *loc. cit.,* 146: "The basic trouble arises from overlooking the fact that *M* is made up not only of the *active* circulation, to which a definite velocity of circulation, determined by people's habits, can be ascribed, but also of the *inactive* circulation, which serves as a *store of value,* rather than as a *means of exchange* and has a zero velocity of circulation" (italics Kahn's). For an example of the use, by Keynes himself, of the phrase "the active circulation," see his "Reply to Dr. Hayek," *loc. cit.,* 6; and cf. his use of the expression "active deposits" in his "Rejoinder" to Mr. Robertson, *loc. cit.,* 412 ff. The equivalent of the phrase "the active circulation" is to be found in N. Johannsen, who, in his *A Neglected Point in Connection with Crises,* 34 ff., spoke of "active money" in a context very similar to that in which it was used by Keynes. It is highly probable that Johannsen, in turn, got the phrase from Adolf Wagner. See, for example, the use of the expression "the active circulation" in Wagner's *Die russische Papierwährung,* 99. As it happens, a distinction of the type indicated goes back even further in the literature. See, for example, the use of the expression "the *effective* circulation," in Thomas Attwood, *The Remedy; or, Thoughts on the Present Distresses* (1816), 17; also the use of the concept of an "active" portion of the "circulation" in Mathias Attwood, *A*

It should *not* imply that savings deposits, because they serve as a "store of value," do not act "as money," and have a velocity of circulation of "zero." [31]

III

Investment, Borrowing, and "Cash Facilities"

The argument that money which serves as a "store of value" does not cease thereby to serve "as money" is a simple corollary from the proposition that if no particular significance attached to the "storing" of value in the form of a stock of unspecialized purchasing power, such as is represented by savings deposits, as well as by other deposits, "value" would be "stored" in some other form than that of unspecialized purchasing power, or "money." It might be "stored" in claims to shares in specialized forms of wealth, such as stocks and bonds; it might be "stored" in the form of holdings of specialized wealth itself, such as buildings and machinery. It is the task of "cash-balance analysis" to explain *why* wealth is "stored" in the form of unspecialized purchasing power rather than in any other form; and it has been our contention that this applies quite as much to the administration of those "stocks of unspecialized purchasing power" which are represented by savings deposits as by any other type of "deposit."

Letter to Lord Archibald Hamilton, on Alterations in the Value of Money (1823), 56. It is worth noting, however, that the usage of neither of the Attwoods—whose position on the matter of the effect of monetary expansion and contraction upon output will concern us at considerable length in Volume II of this study—was such as to imply agreement with the argument of Mr. Keynes and his followers to the effect that a difference in *kind*, rather than *degree*, was involved in the distinction between the "effective," or "active" circulation, on the one hand, and the rest of the "circulation," on the other.

[31] The reader is reminded that the discussion of the relation between "savings deposits" and the so-called "cash-deposits" presented in this section is in no sense intended as an exhaustive treatment of the issues involved. Such a treatment would, for example, have not only to deal, in much greater detail than was possible on pp. 466 f., above, with the problem as to whether savings deposits are to be regarded as part of the stock of "bank money," but to deal also with such problems as that of the "creation" of bank money, as affected by the operations of commercial banks and savings banks, respectively. My intention in this section has been solely to relate the problem of "savings deposits" to the problem of "velocity," in the light of the considerations suggested by the "cash-balance approach."

From the proposition, moreover, that, for the purposes of "cash-balance analysis," it is of the first importance to concentrate attention upon the differential advantages of holding wealth in the form of a stock of unspecialized purchasing power, on the one hand, and in the form of specialized wealth or claims to specialized wealth, on the other hand, further corollaries are to be drawn. Nothing, obviously, is to be included in the stock of cash balances unless it is actually in the form of cash—that is, unspecialized purchasing power. A stock or bond is not "cash" in this sense; on the contrary, as we have seen, it is precisely the task of "cash-balance analysis" to explain why individuals choose to hold unspecialized purchasing power, in the form of notes or bank deposits, rather than such things as stocks and bonds. The thing to be said is that the possession of certain forms of wealth other than "cash," in the sense of unspecialized purchasing power, makes it possible, by virtue of the fact that these forms of wealth have a greater prospect of conversion *into* cash without loss, to hold smaller amounts of "cash" than would be the case if all forms of wealth other than cash were very poor "bearers of options." No one would wish to deny that these "liquid" forms of nonmonetary wealth *affect* the size of cash balances relative to outlay.[32] All that is asserted here is that it is inconsistent with precision in analysis to regard these "liquid" forms of nonmonetary wealth as *part* of the cash balances which are held relative to outlay.

By a simple extension of the argument with respect to the effect of the possession of liquid forms of nonmonetary wealth upon the size of cash balances held relatively to outlay, it is easy to show that the possession of *one* type of cash balance, such as a savings deposit, may be expected to affect the size of other types of cash balance held by the same individual.[33] It does not follow, however, that simply because the individual in question may regard the particular cash which is tied up in a savings deposit as less immediately available than a noninterest bearing cash balance—as the result of a desire, for example, to avoid a loss in interest by withdrawal before the interest date—we are justified in re-

[32] Nor would it be denied, of course, that, conversely, the administration of cash balances will have some effect upon the prices of these "liquid" forms of wealth, by virtue of the relative degrees of liquidity which may be held to attach to them.

[33] See, on this matter, Keynes's *Treatise*, II, 28 f.

garding savings deposits as "not differing essentially" from such things as government securities.[34] The fact that savings deposits are assured with respect to the integrity of *principal,* under any but the most abnormal circumstances, in the degree in which only holdings of "money" are assured, would, when taken in conjunction with other factors such as those mentioned earlier in this chapter, warrant the drawing of a much sharper distinction between savings deposits and other forms of "liquid wealth" than can be drawn between savings deposits, on the one hand, and demand deposits, on the other.[35]

Fortunately, Mr. Keynes himself did not adopt the practice of regarding liquid wealth as making part of cash balances; on the contrary, a crucial part of the argument of the *Treatise* was concerned precisely with the fact that much depended upon whether individuals chose to keep wealth in the form of a "savings deposit" or in the form of an investment in "securities." [36] Unfortunately, however, the same degree of approval cannot be accorded to Mr. Keynes's treatment of the issues which are raised when we recognize that the existence of *investment* opportunities which provide assets of varying degrees of liquidity is by no means the only type of institutional factor which affects the size of cash balances held relative to outlay. There is also the matter of *borrowing* opportunities. For it is obvious that if the administrators of cash balances know that they can obtain cash by borrowing when they need it, there is no reason why they should heap up cash in anticipation of these needs. The extent of "cash-facilities," therefore, in the sense of opportunities to borrow cash, is definitely a factor affecting the size of cash balances held relatively to outlay.

Yet this is a very different thing from arguing, as Mr. Keynes did, that "cash-facilities," in the sense indicated, are to be regarded as *part* of a cash balance, so that we may even speak of the "velocity of circulation of cash-facilities." [37]

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[34] Cf. above, p. 467.

[35] The "other factors" referred to are those suggested on p. 467, above.

[36] Cf., for example, the famous discussion of "the choice between 'bank-deposits' and 'securities,'" and the related argument as to the connection between the "bearishness" of the public and the choice in question, in the *Treatise,* I, 141 ff., 249 ff., 267, II, 195 f.

[37] See the *Treatise,* I, 42 f., 236 f., II, 35. The usage has been taken over by other writers, including Professor Pigou. See, for example, the latter's *Theory of Unemployment,* 194, and cf. J. S. Robertson, *The Income Theory of Prices,* 15.

Suggestions similar to this can, to be sure, be found in the older literature. As early as 1822, for example, it was argued that "the power of procuring money, if wanted, is tantamount to the actual possession"; so that, "to use a scholastic phrase," we may say that "money *in posse* is equivalent for all purposes of trade to money *in esse*." [38] The proposition, indeed, that it was not money *"in esse"* alone, but such money in combination with money *"in posse,"* which determines the magnitude of "the money demand" for commodities over a given period, was explicitly defended by John Stuart Mill.[39] It would, however, require an extraordinary degree of indulgence to regard as satisfactory Mill's treatment of the problem, with its failure to distinguish sharply between the money-spending power of an *individual,* on the one hand, and the money-spending power of the individuals making up a community, on the other, as well as its disastrously loose treatment of "credit" as an element determining prices—a treatment exemplified by the suggestion that the differences in the *form* which this "credit" takes are of no great importance for the dimensions of the total "money demand for a commodity." [40] What is

[38] See the *Quarterly Review,* XXVII, 254. The passage was quoted by Tooke in his *Thoughts and Details on the High and Low Prices of the Last Thirty Years* (1823), I, 52; but his discussion of it (see also pp. 54 f. of the work cited) was confined essentially to a question of fact: namely, whether the magnitude of the volume of "money *in posse*" was as great during the period of the "Bank restriction" as the author of the article in the *Quarterly Review* contended.

[39] See the *Westminster Review,* XLI (1844), 590.

[40] On the first point, see the quotation from Viner on p. 467, n. 22, above. For examples of Mill's position with respect to the essential lack of importance attaching to such questions as whether the "credit" which is held to affect prices does or does not give rise to a "transferable acknowledgment of debt," see the *Westminster Review,* XLI, 590 ff., and also Mill's *Principles,* Book III, Chap. XII. The case of Mill is, of course, by no means the only one that could be cited by way of illustrating the dangers inherent in operating with concepts such as "money *in posse,*" which Tooke paraphrased as "the confidence which the merchant, tradesman, or farmer may have of being able, at the shortest notice, to raise money or obtain credit on undoubted security, whether of bills or of trading or farming stock" (*Thoughts and Details,* I, 55). The most extreme instance in our own day of the treatment of "credit" as equivalent to "confidence" in connection with problems calling for the utmost precision in analysis is probably that of L. A. Hahn, who, in his *Volkswirtschaftliche Theorie des Bankkredits* (1st ed., 1920), advanced the proposition that "the money- and capital-markets are the markets on which credit, in the sense of 'confidence,' in the most literal meaning of the term, is dealt in" (*op. cit.,* 51 f.).

provided, in other words, by the precedents for the concept of "cash-facilities"—as illustrated by concepts such as that of "money *in posse*"—is a warning against, rather than encouragement to, the use of similar concepts by later writers.

There are, to be sure, aspects of Mr. Keynes's proposal with respect to the substitution of "cash-facilities" for "cash-deposits" which make it less completely objectionable than Mill's treatment of "credit" as the element controlling the magnitude of the "money demand" for commodities. The fact, for example, that Mr. Keynes limited his supplementary "cash-facilities" to the single item of "unused overdraft facilities" removed much of the vagueness which necessarily attached to Mill's all-inclusive category of "credit." [41] Mr. Keynes's proposal was superior to that of Mill also by virtue of the further fact that the particular form assumed by an individual's stock of what Mill called "money due to him and payable on demand" which is involved in the case of Keynes's "cash-facilities" is money that is "due" from an agency which may be able to *create, ad hoc,* a net addition to the total stock of "money," instead of being a form of "money" which could be used by the individual to whom it was "due" only at the cost of a diminution

[41] The protean character of Mill's category of "credit" may be judged from his proposition that the issuance of bank notes, for example, is to be regarded as making "no addition whatever to the total amount" of money-spending power, since such issuance "merely converts so much credit from an unwritten into a written, and from a cumbrous into a convenient form." "The person to whom the notes are advanced is proved by that very fact to have credit. . . . Is it supposed that having credit, and intending to buy goods by means of it, he will be disabled from doing so because a banker is prohibited from one particular mode of giving him *credit?*" (*Westminster Review,* XLI, 591 f.; italics Mill's). A similar character has been imputed to Mr. Keynes's concept of "cash facilities" by some of his critics, who have argued, as a kind of *reductio ad absurdum,* that if the reasoning underlying the inclusion of "unused overdraft facilities" is sound, there is no reason why we should not include also "customers' securities in safe-keeping accounts, which could be pledged automatically for new loans," and even "the buildings and lands on which one can borrow money" (see, for example, J. H. Williams, "The Monetary Doctrines of J. M. Keynes," *loc. cit.,* 549). That there is some force in this criticism cannot be denied. The criticism itself, however, is best regarded as a criticism of Mr. Keynes for a sin of omission, rather than one of commission—in the sense that it really chides Mr. Keynes for having failed to provide a detailed argument designed to show in just what respects "unused overdraft facilities" differ from other forms of what the older writers called "money *in posse,*" so as to justify only *their* inclusion in the final formulation.

in some other individual's stock of money-spending power. Yet there remain cogent reasons for insisting that Mr. Keynes's proposal suffers from defects which argue strongly against its widespread adoption.

The nature of these reasons should be obvious from what was said above concerning the danger of regarding liquid assets—such as certain types of securities—which are "almost" cash, as the equivalent of cash.[42] The first of these reasons, it will be recalled, was that, from the standpoint of monetary policy, it is of the greatest importance to distinguish between the volume of liquid "assets" in the form of cash, such as bank deposits, the magnitude of which is limited by the fact that the amount of such "assets" cannot increase beyond a given ratio to the supporting bank reserves, and the volume of those liquid assets against which no such reserves need be held.[43]

It would, indeed, be going too far to suggest, as one of Mr. Keynes's critics did, that whereas "demand deposits [and savings deposits too, for that matter] are limited by reserves," "unused overdraft facilities are limited only by the wealth of the community and the prudence of the bankers." [44] It is at least conceivable, for example, that one of the elements involved in the "prudence of the bankers" is their anticipations with respect to the strain upon their reserves which would be set up if the "unused overdraft facilities" came to be "used"—that is, if loans were made to the amount of these "unused overdraft facilities." It is, however, of the first importance to recognize that the total of loan commitments cannot be used directly as a basis for

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[42] See above, pp. 467, 470 ff.

[43] It should hardly be necessary to emphasize the fact that there is no intention here of denying that the volume of truly liquid assets in any economic system will affect the extent to which banks must keep themselves "liquid," and therefore the ratio of bank reserves to bank liabilities. The argument in the text is directed, not against the suggestion that there is *some* kind of connection between the volume of truly liquid assets and reserve requirements, but against the specific suggestion that the connection is of the simple kind which is involved whenever it is implied that, for the purposes of monetary policy, no genuine significance attaches to such institutional elements as the fact that there are formal reserve requirements against bank-deposits, but none against assets which are "almost" as liquid as such deposits.

[44] So Currie, *The Supply and Control of Money in the United States*, 21.

estimating the magnitude of the strain on reserves set up as the result of banking operations.

The issues involved have, to be sure, often been ignored by those writers who have regarded it as a matter of indifference, in measuring the amount of "credit" which is held to operate upon prices, whether we take the volume of *loans* or of *deposits*.[45] It must be remembered, however, that the only result of banking operations which is directly relevant to the question of the effect of lending operations upon bank reserves is the ratio of those reserves to the *deposits* which result from such lending operations. That a given amount of loans may, under differing institutional and conjunctural conditions, result in a different amount of deposits, even when we have in mind the operation of the banking system as a whole, should be regarded as a commonplace.[46] The possibilities are still more varied, moreover, when it is remembered that the amount of deposits which may be expected to result from a given volume of loan *commitments* will depend upon such factors as the degree and timing of expansion by individual banks within the banking system, as well as upon factors affecting the banking system as a whole.[47] In the light of these considerations, surely, to re-

[45] See the examples of such a usage given by Currie, *The Supply and Control of Money*, 51 ff. The list could, however, be greatly extended; and the list of absurdities to which the usage in question has led would grow with it. See, for example, Roos, *Dynamic Economics*, 238, where, in what purports to be an elaboration of the Fisherine equation of exchange, the volume of "brokers' loans" is included in an omnibus item along with "all money other than that already mentioned"—that is, the "average amount of currency (gold, silver, paper notes)" and "the average amount of bank credits subject to check"—and is provided with a special "average velocity of circulation."

[46] It is hardly necessary to do more, in this connection, than to cite the effect of the internal and external drains. See, for example, the *Tenth Annual Report* of the Federal Reserve Board, pp. 23-29; and cf. E. A. Goldenweiser, *The Federal Reserve System in Operation* (1925), 21 n., 66 f., 92 f.

[47] The difference between loan *commitments* and actual *loans*, in this context, becomes obvious as soon as it is recognized that, whatever may be said against the use of *loans* as a measure of the amount of money-spending power created through banking operations—and, as we have seen, much can be said against it—it is at least true that the use of *loans* for the purpose in hand allows for the effect of a tendency to differential expansion on the part of individual banks; for if one bank tries to expand its loans more rapidly than its competitors, it will be forced, through adverse clearings, to contract almost as soon as it begins to expand. This contraction, in turn, will be reflected in the total of loans within the banking

gard the volume of loan commitments—"unused overdraft facilities"—as the equivalent of an exactly equal volume of deposits is to proceed on the basis of assumptions which are arbitrary in the highest degree.

The decisive argument, however, against merging the volume of "unused overdraft facilities" with the volume of "cash-deposits" in the computation of the total of cash balances to which the factor of "velocity" is to be applied is one which is analogous to the second of the reasons advanced above against the inclusion of liquid assets other than cash—such as securities—in the total of cash balances. It was argued above that the essence of the cash-balance approach is that it poses the problem of determining why individuals choose to keep assets in the form of cash rather than in some other form; and it was pointed out that if no particular significance attached to the "storing" of value in the form of cash, "value" would be "stored" in some other form.[48] Surely the same type of argument applies when we consider the wisdom of putting "cash" and "cash-facilities" on a par in computing the total volume of cash balances. It is certainly true that those to whom "cash-facilities" are available usually choose to keep on hand a certain amount of "cash" also; and it is surely a part of the task of "cash-balance analysis" to determine just why these persons insist upon holding cash rather than trusting entirely to the availability of "cash-facilities." It is equally certain that the forces determining variations in the volume of "unused overdraft facilities" which are available to different groups in the community, or to the same groups in the community at different times, are by no means necessarily identical with the forces determining the amount of *cash* in the possession of these groups; and it is surely a part of the task of "cash-

system as a whole. In the nature of the case, however, any estimate of the extent to which other banks will expand their loans—and such an estimate is obviously involved in the making of loan *commitments*—is in the highest degree tentative. It is even possible, indeed, that these commitments may never result in the making of actual loans. They could not, therefore, result in the creation of "cash" in the form of deposits. Yet we are told that the "cash-facilities" represented by "unused overdraft facilities" are to be regarded as being in all respects "truly cash for the purposes of the Theory of the Value of Money" (*Treatise*, I, 43).

[48] See above, pp. 470 f.

balance analysis" to establish the nature of the difference between the two sets of forces.

If, however, the argument of the preceding chapter with respect to the relation between "cash-balance analysis" and the concept of "velocity" is sound, this is equivalent to saying that no generally satisfactory treatment of the factor of "velocity" can be obtained if we insist upon including the volume of "unused cash-facilities" in the total of cash balances to which the "velocity" factor is to be applied. It follows, therefore, that Mr. Keynes's contention that "from the standpoint of qualitative analysis," a refusal to merge "unused cash-facilities" with cash balances in the literal sense of the term "fails to put us on the right track for discovering what sort of circumstances will change the velocity of circulation," is the very opposite of the truth.[49]

IV

Forces Determining the Relative Size of Cash Balances

The considerations advanced in the preceding sections of this chapter are all reducible to the proposition that it is only by the careful development and rigorous use of a body of principles designed to explain why cash balances—in the literal sense of sums of unspecialized purchasing power actually held—have a given size relative to outlay, that we are able to protect ourselves from the construction of categories for the purpose of dealing with the problem of "velocity" which can be shown to be treacherous in the extreme. It remains for us to ask how far the writings of Mr. Keynes may be taken as typical of the best that economic literature has to offer in the way of a careful and comprehensive statement of the principles in question.

It is certain, in the first place, that the analysis of the forces determining the size of cash balances which Mr. Keynes presented in his *Monetary Reform*—in which, as we have seen, he was an avowed protagonist of a variant of the "cash-balance approach"—can hardly be regarded as being

[49] Cf. the *Treatise*, I, 236.

other than primitive in the extreme. The size of the cash balances which it suits people "to hold or to carry about," we were told, "depends partly on their wealth, partly on their habits." [50] Yet to say, for example, that the size of cash balances depends upon the "wealth" of individuals is extremely ambiguous. Are we to understand this proposition to mean no more than that wealthier individuals keep balances which are *absolutely* larger than those kept by less wealthy individuals? In that case, the statement tells us nothing with respect to the amount which they keep relatively to outlay—the magnitude which, if the argument of the preceding chapter is sound, it is the real function of "cash-balance" analysis to explain. If, on the other hand, we are to understand the statement that wealthier individuals keep cash balances of a different magnitude relative to outlay than do less wealthy individuals, it is surely of some importance to know whether they keep *larger* or *smaller* balances relatively to outlay than do less wealthy individuals. And when this is decided, we have still to learn what specific circumstances lead them to do so.

Nor can much more be said for the proposition that the size of cash balance which it suits people "to hold or carry about" depends on their "habits." [51] If this were all that cash-balance analysis had to offer by way of explaining why cash balances are of the size they are relative to outlay, it would be true to say, as a recent critic has said, that all cash-balance analysis has to offer us is to tell us that the size of cash balances is what it is.[52] Surely it is the function of economics as an analytical discipline to *explain* why eco-

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[50] Keynes, *Monetary Reform*, 83.

[51] See *Monetary Reform*, 85 f. Cf. also the *Treatise*, II, 44, where "the considerations of *convenience*" which are regarded as determining the size of cash balances held relatively to outlay are dismissed with a reference to "social and business habits." It is something of a commentary upon the present state of discussion of the central problems of monetary theory that those, among Mr. Keynes's followers, who have adopted the position, expressed with such vigor in the *Treatise*, that equations of the form $MV = PT$ are not "enlightening," have also shown their unfamiliarity with the body of doctrine *lying behind* the variables of these equations by regarding as a satisfactory statement with respect to the forces controlling movements in "velocity," that these movements are "determined by people's habits." See Kahn, "Dr. Neisser on Secondary Employment," *loc. cit.*, 146.

[52] Cf. the reference to Angell on p. 419, n. 13, above.

nomic "habits" are what they are—not to accept them as data incapable in their entirety of explanation.[53]

Nor can it be said, finally, that an explanation of these "habits" is provided by the statement that the "habits" of the community with respect to the holding of cash balances "are fixed by its [members'] estimation of the extra convenience of having more cash in hand as compared with the advantages to be got from spending the cash or investing it," the "point of equilibrium" being reached "where the estimated advantages of keeping more cash in hand compared with those of spending or investing it about balance."[54] This, of course, is merely a paraphrase of the not uncommon statement to the effect that the size of cash balances relative to outlay will be determined as the result of a weighing of the relative "utilities" offered by the holding of cash, on the one hand, and of other forms of wealth, on the other. In both cases, what we have is little more than a statement of the problem, rather than a solution of it.

In the case of commodities other than money, to be sure, it is usually difficult, if not impossible, to explain why a given commodity has more or less "utility," without passing at once into realms—such as those of psychology and physiology, to say nothing of a Veblenian sociology concerned with leisure class tastes and "honorific" distinctions—which are in a fundamental sense beyond our competence as economists. It is, however, anything but clear that we can go no further, as economists, in unraveling the factors which induce individuals to keep cash balances of a given size relative to outlay than we can in attempting to ascertain why individuals consume potatoes in a given quantity and caviar

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[53] There would thus be considerable justification, if the analysis presented by Mr. Keynes in his *Monetary Reform* could be taken as typical of the analysis of "Marshall and his school," for characterizing this analysis as offering an explanation which, being referred to "habits," is not an "economic" explanation at all (*"una spiegazione consuetudinaria e non economica,"* as it is described by G. Del Vecchio, "La moneta nella teoria dell'equilibrio economico," *Giornale degli economisti*, LXIX [1929], p. 135). Cf. also the comment on the reference of the actions of cash-balance administrators to the element of "habits," by Greidanus, *The Value of Money*, 155.

[54] So Keynes, *Monetary Reform*, 85 f.

in a different quantity.[55] On the contrary, there is every evidence that the material for the construction of a satisfactory account of the forces determining the size of cash balances which individuals choose to hold relative to outlay lies ready at hand, awaiting only systematic survey and articulate statement in order to serve the powerful purposes of which a satisfactory organon should prove capable.

That materials for the construction of such an organon can be found in the writings of Mr. Keynes, as they can be found in the writings of economists from Petty and Locke down to our own day, it would be idle to deny.[56] Strangely enough, there is very little of such material in Mr. Keynes's *Monetary Reform*, the one work in which specific allegiance was given to a variant of the cash-balance approach—hardly more, in fact, than a suggestion, which is at least as old as Say, with regard to the effect, upon the holding of cash balances, of anticipations concerning drastic price rise under extreme paper money inflation.[57] There is much more,

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[55] The direct contrary seems to be implied by Mr. J. R. Hicks when he suggests ("A Suggestion for Simplifying the Theory of Money," *loc. cit.*, 13) that the "purely theoretical study of money can never hope to reach results so tangible and precise as those which value theory in its more limited field can hope to attain." On the contrary, there can hardly be any doubt that if we regard the purpose of "value theory" as being that of explaining why the "values" of all types of commodity are what they are, and the purpose of "monetary theory" to explain, among other things, why the value of money is what it is, the advantage, from the standpoint of the possibility of an economist's obtaining results which are both "tangible" and "precise" are certainly with the latter, rather than with the former. Hicks's comment, to be sure, is based on the rôle assigned to "subjective factors like anticipations" in the theory of money, rather than on the difference to which attention is called in the text. The latter difference, however, is surely relevant. It will be seen, moreover, from what is said in Volume II with respect to the rôle of "anticipations" in economic theory generally, that it is anything but clear that their rôle in "general value theory" has been in any significant respect different from their rôle in the theory of money and prices.

[56] The statement, therefore, by Angell ("The Components of the Circular Velocity of Money," *loc. cit.*, 232), to the effect that "despite its importance, the problem of payment habits has been ignored by most students of monetary questions" is certainly not to be taken to mean that no literature exists on the subject. The literature is a vast one, for all its uneven quality and lack of sharp articulation.

[57] See, for example, *Monetary Reform*, 89 ff. The older literature on this point, which received its most critical discussion in the German literature under the designation "The Black Peter Theory," cannot be summarized or be cited here in detail. For the passage in Say, however, see Holtrop,

however, in the *Treatise*; and there are further additions, particularly in the way of a redistribution of emphasis, in the General Theory.[58] What we do not find, in either the *Treatise* or the *General Theory,* is a systematic account of the forces determining the size of cash balances relative to outlay which, starting from such attempts at a systematic presentation as are now available, would, by both simplifying and amplifying, have provided a statement that would be more nearly satisfactory than any that are now available.[59]

It is obvious that a treatment of this problem which would present, in some detail, both the reasons for the inclusion of certain elements and the reasons for the elimination of others, would have to be of monographic dimensions.[60] No attempt, however, to restate the substance of received doctrine on the Theory of Prices could be regarded as satisfactory if it evaded entirely the task of stating, if only in very small compass, what, in a more protracted treatment, could be regarded as the end result of an extended process of examination, rejection, and final acceptance. In all diffidence, and with apologies for the omission of the supporting detail, the following is presented as a tentative list of the forces determining the size of cash balances held relatively to outlay:

1. The *time-shape* of individual income streams, both actual and anticipated,[61] in relation to

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"Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *loc. cit.*, 519.

[58] Most of the passages in the *Treatise* which are relevant to the matter under discussion are to be found in Chapters III, XXIV, and XXVI of that work. For more detailed references to the *Treatise,* see below, p. 483, n. 62. The passages in the *General Theory* which are relevant in this connection will be discussed in Volume II of this study.

[59] It is to be hoped that the suggestion that the statements of results which are now available can be improved upon will not be taken as representing a desire to minimize the value of these attempts. All further work in the field must start by building upon the attempts thus far made—for example, that of Holtrop, in his *De Omloopssnelheid van het Geld,* 116 ff. (cf. the version given by the same author, under the title "Die Umlaufsgeschwindigkeit des Geldes," in *Beiträge zur Geldtheorie,* 133 ff.). There can be no question, however, of attempting to deal here in detail with Holtrop's account, or others of great value which could be cited.

[60] Cf. above, p. 290, n. 1.

[61] The element of "uncertainty" is of course included under the heading of "anticipations." It should be added also that by "income stream" is meant the stream of money *receipts,* and not merely "income" in any of the narrower technical senses of the term.

2. The *time-shape* of individual expenditure streams, both actual and anticipated, the anticipated expenditure stream being affected, in particular, by anticipations with respect to the future course of prices.

3. The *size* of individual income streams in relation to the *size* of the corresponding expenditure streams.

4. The opportunities, both institutional and conjunctural, which are open to individual administrators of cash balances, for borrowing in anticipation of receipts.

5. The opportunities, both institutional and conjunctural, for the investment of "surplus" cash balances.

6. The inducements, both institutional and conjunctural, to such investment, including the effect of the rate of interest.

7. Institutional devices which force the holding of cash balances in an amount, relative to outlay, different from that which would be expected on the basis of the factors listed under items 1-6: for example, the forcing of the holding of minimum balances as a part of banks' administration of customers' accounts, or the introduction of special taxes on money balances.[62]

The list thus presented, as has been indicated, is highly tentative; and it can hardly be denied that its cogency and full significance can be established only as the result of the kind of examination, involving selection, rejection, and the distribution of emphasis, which was desiderated above. The purpose in submitting it has been merely to indicate the nature of the results that may be hoped for when once there is full acceptance of the cash-balance approach as an analytical device for explaining why the movements in "velocity" are what they are, and a determination to assemble and evaluate all specific suggestions with respect to the forces determining the magnitude of "velocity" which satisfy the methodological requirements of the "cash-balance approach."

[62] The interested reader may wish to examine the following passages in Keynes's *Treatise,* in which may be found suggestions that could be included under each of the headings listed above, the passages being grouped under the numerals corresponding to each of the headings: (1) I, 34 f., 44 f., 148, 310; II, 25, 79; (2) I, 45 f., 77, 239, 246 f.; II, 29, 45 ff., 79; (3) I, 45, 246, 299, 307; II, 30; (4) I, 246, 249; II, 38, 46; (5) I, 36; (6) I, 36, 185, 218; II, 44 f.; (7) I, 37, 39; II, 33.

CHAPTER SEVENTEEN

The "Volume of Transactions" (*PT*); the "Plurality" of Price Levels

I

KEYNES ON THE FISHERINE *T*

NO ONE really conversant with the uses to which the so-called "Fisherine" equation has been put would be prepared to deny that, of all the variables in that equation, the one which has been subjected to the loosest treatment has been the element *T*. It is a commonplace, for example, that the statistical measures for *T* have in most cases been little more than witches' caldrons—the best that could be said of them being that they give a very rough indication of the movements in certain variables, an increase in whose magnitude would tend, other things being equal, to depress general prices, and a decrease in whose magnitude would tend, other things being equal, to raise general prices.[1] The difficulty, however, is not merely one of statistical measurement; if it were, the Theory of Prices could be regarded as having reached a much more nearly definitive state than can honestly be assigned to it. The difficulty is rather that the looseness of statistical procedure is, in this case as in few others, a symptom of shoddy analysis which has served to darken counsel oftener than it has added to our understanding.

It is, therefore, of considerable interest that Mr. Keynes should have been critical of the *T* of the Fisherine equation to the point not only of refusing, in his *Treatise,* to grant it even an implicit place, along with "the quantity of bank-

[1] See, in this connection, the comments on the available indexes for *T*, in my article "The Statistical Measurement of the 'Velocity of Circulation of Goods,'" *Quarterly Journal of Economics,* XLVII (1932), 9 ff., and the references there given.

money" and "the velocities of circulation," in his Fundamental Equations, but also of insisting that it was precisely the presence of T in the Fisherine equation that made the equation useless both for the purpose of analyzing the problems, within the field of the Theory of Prices, in which "we are likely to be interested," and for checking the results obtained by the use of his own equations.[2] It is, however, one thing to be critical; it is quite another to present one's criticism in the form of a systematic and cogently reasoned argument which leaves no doubt either as to the validity of the criticism itself or as to its bearing upon the question concerning the relation of the received Quantity Equations to the apparatus presented as an alternative to these equations. In what follows, an attempt is made to establish clarity on both these heads.

II

"Consumers' Goods" Equations and the Plurality of Price Levels

The first of Mr. Keynes's criticisms of the Fisherine equation with which we are here concerned was that, by virtue of its inclusion of T in its second member, it did not provide a satisfactory "guide to the Purchasing Power of Money."[3] There was no mystery as to Mr. Keynes's meaning on this head, despite the fact that Fisher's *The Purchasing Power of Money,* which presented the best-known of all Quantity Equations involving the use of a T, undertook to show that it is precisely the "Purchasing Power of Money" which is best explained by the use of the equation in question. For

[2] For an example of the type of passage in the *Treatise* in which, although a place in the Fundamental Equations was assigned implicitly to the "quantity of bank-money" and the "velocities of circulation," none was assigned to the "volume of transactions," see the *Treatise,* I, 185. For the assertion that the fundamental "weakness" of the "Fisher equation" is its inclusion of T, for the reason that the latter does not represent a quantity "in which we are likely to be interested," see *ibid.,* I, 235. For the argument that the concern of the "Fisher equation" with the "volume of transactions" makes it irrelevant for the purposes of testing the results obtained by the use of the Fundamental Equations of the *Treatise,* see Keynes's "Rejoinder" to Robertson, *Economic Journal,* 1931, p. 419, and cf. above, p. 18, and below, pp. 514 ff.

[3] *Treatise,* I, 236; cf. also I, 221.

Mr. Keynes, with the emphatic assurance that "we need not hesitate over the answer" to the question as to "what we mean by the Purchasing Power of Money," had defined this "Purchasing Power of Money" to mean "the power of money to buy the goods and services on the purchase of which for purposes of consumption a given community of individuals expend their money income: . . . that is to say, it is measured by the quantity of such goods and services, weighted according to their importance as objects of consumption." [4]

As we have seen, this suggestion, far from being a new one, or one advanced so rarely as to warrant reference to no writers other than Marshall, has appeared in economic literature repeatedly during the last century, usually in association with some form of what has come to be called the "income approach" to the Theory of Prices, of which, as we have also seen, the apparatus presented in the *Treatise* has been regarded as a variant.[5] Tooke, for example, had declared explicitly that it is the prices at which commodities go "into consumption" which "may be considered with greater propriety than any other description as general prices"; Wicksell had argued that "the ideally correct procedure for observing and measuring the general price-level" would be "to confine the calculation to objects of direct consumption"; Wieser had made it clear that the "commodities" in whose prices he was interested were those which made up "the total quantity of consumers' goods, which are paid for out of income"; Schumpeter had in this respect followed Wieser literally; and other examples could be cited of an essentially identical procedure.[6]

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[4] *Treatise,* I, 54; cf. also I, 57 ff.

[5] For Keynes's citation of Marshall, see the *Treatise,* I, 54. Cf. also, in addition to the references to Marshall's *Money, Credit, and Commerce* there given, Marshall's paper on "Remedies for Fluctuations in General Prices" (1887; *Memorials of Alfred Marshall,* 207 ff.).

[6] See the references to Tooke, on p. 314, n. 34; to Wicksell, on p. 326, n. 72; to Wieser, on p. 339, n. 111; to Schumpeter, p. 339, n. 111; and to Lindahl, on p. 328, n. 78, above. Cf. also E. von Philippovich, *Grundriss der politischen Oekonomie,* I, 311 ff.—a passage obviously written under the influence of Wieser—in which it was argued that what is "decisive for the objective exchange value of money" is the "relation of all money-incomes in an economic society to the objects of consumption, and therefore to real income"—"real income" being further understood (*op. cit.,* 312), to "consist only of consumers' goods . . . and services designed to satisfy personal

For all its long history, however, the suggestion that the center of interest in the Theory of Prices must be the prices of *consumers' goods,* and that therefore the shorthand formulation designed to summarize the Theory of Prices must be one which explains how these prices are determined, had never been accompanied by a supporting argument of such detail and cogency as to establish the case for the procedure in question beyond any possible doubt. It was, therefore, something of a disappointment that Mr. Keynes himself should have made virtually no attempt to provide such an argument.[7] The statement that the price level corresponding *"par excellence"* to what we mean by the Purchasing Power of Money was the price level of those goods on which "for purposes of consumption a given community of individuals expend their money income," is the statement of a conclusion, not an argument supporting that conclusion. The most that can be said for Mr. Keynes's exposition on this head is that, in avoiding an explicit statement of his reasons for choosing the price level of consumers' goods as that which represents the "Purchasing Power of Money" *par excellence,* Mr. Keynes at least avoided some of the fallacious arguments which had misled certain of his predecessors.

Tooke, for example, based his choice of "the prices at which the commodities have gone into consumption" as the prices which "may be considered with greater propriety than any other description as general prices" avowedly on the celebrated proposition of Adam Smith to the effect that "the value of the goods circulated between the different dealers never can exceed the value of those circulated between the dealers

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wants." In fact, of course, the suggestion that we are interested in "the Price of Goods to the *Consumers*" is much older than any of the writers cited. See, for example, Jacob Vanderlint's *Money Answers All Things* (1734), 110 ff. It is, however, fair to say that it is only in writers such as those cited above that we find some indication of an emphatic *preference* for a "consumers' goods" formulation as opposed to available alternatives.

[7] From the passage on p. 65 of the first volume of the *Treatise,* in which Keynes identified Edgeworth's "Welfare Standard" as corresponding to a variant of his own "Consumption Standard," which in turn was identified with his "Purchasing Power of Money" (*ibid.,* I, 57 ff.), one might be permitted to conclude that Keynes's preference for the "Consumption Standard" was based on the feeling that it is the one most directly concerned with "welfare." The passage in question, however, can hardly be regarded as containing an articulate argument to this effect. On the contention itself, see below, p. 490.

and the consumers; whatever is bought by the dealers being ultimately destined to be sold to the consumers."[8] In the context in which the original passage appeared in the *Wealth of Nations*, the proposition was at best extremely ambiguous, and at the worst of highly doubtful validity.[9] Used as a support for Tooke's argument that the prices which "may be considered with greater propriety than any other description as general prices" are the "prices at which the commodities have gone into consumption," it seemed to imply that, apart from such things as the "miscalculation of employers of capital and labour, in the distribution and the manufacture through the intermediate stages," an adequate description of the forces determining the prices of consumers' goods would automatically account for variations in the prices of producers' goods or of goods sold at wholesale.[10] Despite the fact that Tooke's argument was essentially that which was later presented, along with certain embellishments which were supposed to be provided by "subjective value theory," by Wieser, it remains one of extraordinary naïveté, and can hardly be regarded as the detailed and cogent statement, of which we are in search, of the reasons for selecting the price level of consumers' goods as the price level *par excellence*, in all discussions of the Purchasing Power of Money.[11]

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[8] Cf. Tooke, *Inquiry into the Currency Principle*, p. 71.

[9] The ambiguity of the passage, as it appears in the *Wealth of Nations* (Book II, Chap. II), derives from the fact that it seems itself to have been a mere *obiter dictum* incidental to Smith's use of the distinction between "the circulation of the dealers with one another, and the circulation of the dealers and the consumers," for a surmise as to the relative amounts "absorbed" in each circulation when account is taken of the probable differences in the "velocity" of each type of "circulation." It is not entirely clear, therefore, that Smith would have been prepared to defend his proposition against the criticisms to which it is obviously open when the passage is taken literally (see, for example, Hayek, *Prices and Production*, 43). There can be little doubt, in any case, that the proposition itself was a most unfortunate one.

[10] For Tooke's comment on the possible "miscalculation of employers of capital and labor," see his *Inquiry*, 74. It may be observed, in justice to Mr. Keynes, that although he himself was prepared to admit that the "prices of unfinished goods (i. e., of working capital) will . . . reflect the prices of finished goods" *in conditions of equilibrium* (*Treatise*, I, 245), he was quite emphatic in denying the validity of the proposition suggested in the text. See, for example, the *Treatise*, I, 57 ff.; and for an example of a protest, prior to the appearance of the *Treatise*, against the "erroneous theoretic position" according to which certain prices—for example, the "prices of the elements of consumption"—could be regarded as "determining all other prices," see G. Del Vecchio, "Un capitolo di teoria monetaria," *Rivista bancaria*, VI (1925), fourth paragraph from the end of the article.

[11] The following may be taken as typical of the position adopted by Wieser in his "Der Geldwert und seine Veränderungen" (*loc. cit.*, 516): "The value of money is in the last analysis established only in those acts of exchange which are concerned with consumers' goods. If producers' goods or income-producing properties [*Erwerbsmittel*] of any sort are also exchanged, the prices thus established are not independent prices; on the

Wicksell's argument in support of his contention that "the ideally correct procedure for observing and measuring the general price level is to confine the calculation to objects of (direct) consumption" covered a much wider range of considerations; indeed, the difficulty with his argument was precisely that it covered so much ground that one was left in doubt as to which of the several arguments involved was to be regarded as representing his central contention. There can be little doubt, in any case, that these several arguments differed greatly in the degree of cogency which could be said to attach to them. The appeal, for example, to "ordinary usage," even if there were general agreement, as there is not, on what "ordinary usage" involves with respect to the meaning of "the purchasing power of money," is certainly not an argument which is decisive, even if it could be shown in all cases to be safe.[12] Nor can much more be said, in the present context, for the contention that to use any index number, as a measure of the "purchasing power of money," other than an index of the prices of goods which "enter *directly* into consumption" can result only "in quite useless double counting."[13] *If* it be granted that the central problem of the Theory of Prices "is to know whether 'living'—ordinary consumption—has become cheaper or dearer," Wicksell's argument with respect to "double counting" (which was also the argument of Edgeworth, Marshall, and others), of course follows as an axiom.[14] It is, however, precisely the former proposition which has to be demonstrated.

Wicksell's chief arguments in support of this proposition seem to have been chiefly two. Of these, the first was the suggestion that the direction of primary attention to the price level of consumers' goods

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contrary, their prices are adjusted to those which exist for consumers' goods, and which establish the value of money. A 'functional theory' of money must therefore hold fast above all to those quantitative relationships which are effective in the exchange of consumers' goods." Just how far Wieser was influenced by Tooke in this connection, it is very difficult to say. It is true that Wieser, in his earlier paper "Der Geldwert und seine geschichtliche Veränderungen" (*Zeitschrift für Volkswirtschaft, Sozialpolitik und Verwaltung,* XIII [1904], 47) had spoken in very laudatory terms of Tooke's "contribution" to the problem of the relation between "the theory of the value of money" and "general value theory." He gave no specific citation to Tooke, however; and the context provides no clear warrant for assuming that Wieser had in mind the argument of Tooke which is here under discussion. It is noteworthy, moreover, that in the earlier article, which, interestingly enough, was concerned, in contrast to the later article (of which see 517 f.) hardly at all with formal considerations regarding "marginal utility," there was no comparable insistence on the necessity of confining the analysis to the prices of consumers' goods. On the contrary, the "value of money" was defined simply as "the numerical relation . . . , as measured by the money-prices of *all things,* in which money stands to *all things which get into trade*" (*op. cit.,* 47; italics mine).

[12] For Wicksell's appeal to the authority of "ordinary usage," see *Interest and Prices,* 16.

[13] Wicksell, *Interest and Prices,* 15.

[14] Cf. Edgeworth, *Papers Relating to Political Economy,* I, 214 n., and *Memorials of Alfred Marshall,* 208.

was indicated by considerations with respect to *"welfare."* [15] Yet it must be obvious, even if one granted that considerations with respect to "welfare" were the decisive ones, that it does not follow that "cost-of-living" price levels are the only ones which concern such "welfare." Surely the degree of *employment* which prevails in a community is a matter which may affect "welfare" quite as much as does the "cost of living": and for the former problem there is every reason for refusing to confine ourselves to the prices of those goods which enter "directly into consumption." [16]

The second of Wicksell's contentions is more difficult to evaluate, because of the fact that it was implied, rather than stated explicitly. From his reference to Pareto's relation of the problem of the "purchasing power of money" to considerations with respect to "marginal utility" as providing a procedure which is "the least open to theoretical objection," one is led to suppose that we have here a case in which Wick-

[15] See, for example, the reference in the footnote to p. 16 of *Interest and Prices,* on the relation of "increasing national welfare" to the possibility of "a falling purchasing power of money, even though the prices of all commodities remain perfectly constant"—a possibility which, though "scarcely in accordance with ordinary usage," Wicksell was prepared to accept as following from a "definition of the value of money" which is "the least open to theoretical objection." It would not be unfair to conclude, therefore, that whenever considerations of "welfare" contradicted the results obtained as the result of other considerations, Wicksell was prepared—at least "theoretically"—to let considerations of "welfare" decide. It may be pointed out, also, that Edgeworth, to whom Wicksell referred in this connection (*Interest and Prices,* p. xxix n.; cf. also the *Lectures,* II, 131 f.), had used the term "Welfare Standard" in a sense which led Keynes to identify it as a variant of his own "Consumption Standard," or the "Purchasing Power of Money." Cf. the reference to Keynes's *Treatise,* on p. 487, n. 7, above, and the citation to Edgeworth given by Keynes in the passage in question.

[16] In the light of this fact, Wicksell's comment (*Interest and Prices,* 14) on the reason assigned by a critic of Fisher's *Appreciation and Interest* for stressing the importance of fluctuations in *wholesale* prices—namely, that "on these it entirely depends whether entrepreneurs and other large users of credit have been working at a profit or a loss" is surely somewhat curious. Instead of denying this contention, Wicksell argued merely that "the interests of the entrepreneur are by no means the only interests which are affected by an alteration in the purchasing power of money." To this it may be retorted (1) that neither is it true that the "consumer is . . . everyone" (cf. the position of Foxwell, as summarized by Edgeworth, *Papers Relating to Political Economy,* I, 261); (2) that the particular "interests of the entrepreneur" which are involved have, under an economic system such as ours, a very important connection with the interests of the "community" by virtue of their effect upon the volume of *employment*; and (3) that what Wicksell was ostensibly undertaking to prove was not that some "interest" attached to the movements in the "cost of living" *along with movements in other types of price index,* but that there were cogent reasons —presumably connected with considerations of "welfare"—for "confining the calculation" of measurements in "the general price-level" to "objects of (direct) consumption." Cf., however, what is said concerning Wicksell's final position with respect to the desirability of working with a "plurality of price-levels," on p. 496, n. 28, below.

sell, who, as we have seen, was otherwise unsympathetic to the idea that the theory of money must at all points be forced into a mold suggested by the terms of "value theory" in general, and by "utility analysis" in particular, was impressed by the suggestion that the kind of theory of money and prices which would lead to the most satisfactory "theoretical" results would be one which, so far as the special problem of the definition of the "purchasing power of money" is concerned, would run in terms suggested by the supposed requirements of "utility analysis." [17] Yet this is a proposition which must be interpreted with the greatest care if it is not to lead to results which are nothing short of absurd.

There can, of course, be no thought of denying the proposition that, in dealing with the relations among the prices of consumers' goods, and indeed with the general level of prices of consumers' goods, whenever this general level can be shown to be affected by the choices of consumers as between consuming and saving, the whole body of so-called "utility analysis" is immediately and always relevant.[18] To argue otherwise would be to transform the elementary methodological device of stating "general pricing theory" on the assumption that there is no change in the general "purchasing power of money"—a device adopted for purposes of simplifying the analysis—into the absurd proposition that the whole of "utility analysis" ceases to apply whenever there is a change in the general "purchasing power of money," in the sense of a change, greater or smaller, in all money prices, upward or downward. The fact that a change in the general "purchasing power of money," in this sense, may be taking place does not invalidate the proposition, for example, that consumers will at all times tend so to distribute their income as to equalize the "marginal utility" of each dollar's purchase. To have emphasized this fact—to have emphasized, in the words of one writer, that an adequate theory of money and prices must be related to

[17] See *Interest and Prices,* 16 n.; cf. also *ibid.,* xxix n., and *Lectures,* II, 131 f. On Wicksell's position generally with respect to the application of "utility analysis" to the problem of the value of money, see above, p. 442, and especially n. 80 thereto. It may be noted that Mr. Keynes was much less sympathetic to the idea that "by the purchasing power of money ought really to be meant the *abstract marginal utility* that can be procured with one extra unit of money" than was Wicksell (cf. *Interest and Prices,* 16 n.). See, for example, Keynes's *Treatise,* I, 96 ff.

[18] It is of some importance to stress the fact that the uses of "utility analysis" which are here in question, and which have to do with the "utility" of the various possible applications of money *income,* are to be sharply differentiated from its uses when what is involved is the "utility" of a cash *balance* as opposed to that attaching to other forms of wealth. The latter type of use is relevant in the choice between the holding of a cash balance and *investing* (or *spending*) it; the former is relevant to the question of the distribution of expenditure over a given range of commodities either at any given time, or as between two periods, as in the case of a choice between expenditure on current consumers' goods and expenditure on producers' goods or titles thereto. The failure to keep the two types of problem separate has been so fertile a source of confusion in discussions of the rôle of "utility analysis" in monetary theory that any attempt even to cite instances of such confusion is out of the question here.

the apparatus of individual "demand and supply curves" which is represented by "modern value theory"—must be regarded as a merit of those forms of the "income approach" for which the assertion of the continuing association of the two bodies of theory is a proposition of cardinal importance.[19] The only part of the argument, as thus far stated, to which one could possibly take exception, is the suggestion, by certain of these writers, that an emphasis of this nature requires a break with received tradition on the subject of theory of money and prices—as represented, for example, by emphasis on the importance of changes in the quantity of money for the determination of general prices—instead of being what it really is: namely, the making explicit of a proposition which should always have been regarded as being implicit in these older formulations.[20]

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[19] For a particularly explicit insistence on the relation between "the income-theory" and the individual "demand" and "supply" curves of "modern value theory," see Aftalion, *Monnaie, Prix et Change,* 153, 160 ff., 223 f., 226 ff.; and cf. the same author's "Die Einkommenstheorie des Geldes, etc.," *loc. cit.,* 378, 386, 389. It is easy to cite other examples, from the writings of those who have been characterized by historians of doctrine as "income theorists," or who have claims to such a characterization, of an insistence upon the point that, instead of relying upon a "mechanical" impact of money against goods, we must be prepared, in analyzing the course of events during periods of general price change, to make use of the apparatus of general pricing theory, with its concern with the prices of *particular* goods and the "psychological" motives which lead individuals to "demand" more or less of one type of good than another. See, for example, Launhardt, *Wesen des Geldes,* 42 ff.; Wagner, *Sozialökonomische Theorie des Geldes,* 184; Zwiedineck, "Die Einkommengestaltung als Geldwertbestimmungsgrund," *loc. cit.,* 135 ff.; Liefmann, *Geld und Gold,* 59 f., 133 f., *et passim.* Cf. the following, from Hawtrey's chapter on "The Relation between Money, Prices, and Incomes," in his *Good and Bad Trade,* 7: "The relative prices of the various commodities will be determined by the demand and supply of each." See also p. 205 of the same work: "When the consumer finds his purchasing power diminished he will not economise his expenditure equally in all directions. Here the distinction between 'elastic' and 'inelastic' demand will apply; etc." These passages from Hawtrey should be sufficient to show that it is perfectly possible to accept the proposition that the principles of "general value theory" must be borne continually in mind in describing the processes of price change, without allowing these "principles" to become something of an obsession. Cf., in this connection, the remarks by Hawtrey on Aftalion's use of "the theory of marginal utility," in the *Weltwirtschaftliches Archiv,* XXVIII (1928), 100** f. It is obvious that these citations, as well as others which could be given, will be found to be relevant for a judgment as to the fairness of Mr. Keynes's statements in the *General Theory* regarding the extent to which the principles of "general value theory" have been borne in mind in discussions of the relation between money and prices. The matter will be discussed again in Volume II of this study.

[20] For examples of the suggestion that emphasis upon the "psychological" element lying behind the individual choices which determine prices is in conflict with the emphasis involved in "the Quantity Theory," see the references to Aftalion and Liefmann, in the preceding note. That the criticism is valid as against *some* expositions of "the Quantity Theory" goes without saying. The first type of emphasis, however, is, by the testi-

All this should always have been regarded as clear beyond question. What is anything but clear, however, is the nature of the argument by which one passes from the contention that the principles of "utility analysis," as incorporated in the "general theory of value," must be borne continually in mind in any attempt to explain why prices are what they are, to the contention that the "prices" with which alone we need concern ourselves are the prices of consumers' goods.[21] We have already had occasion to see what harm can be done by an unintelligent application of the supposed "principles" of "general value theory" to the theory of money and prices; and the present instance provides a further case in point.[22] "Utility analysis," in the strict sense of the

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mony of virtually all parties, implicit in all forms of the Theory of Prices in which a central rôle is assigned to *income*; and since, as we saw on pp. 348 ff., above, there is no difficulty whatever in establishing a *modus vivendi* between intelligent forms of both the "income-theory" and the "quantity theory," it follows that there is no fundamental conflict between "the quantity theory" and an emphasis of the kind desiderated by the writers in question. Similarly, a demonstration, of the kind presented on pp. 409 f., above, that the use of a "quantity equation" of the general Fisherine form is perfectly consistent with a satisfactory "income equation" should, in the light of the generally recognized relation between an emphasis on "income" and the "subjective" factors affecting individual choices, be sufficient to invalidate the suggestion that "any attempt to express in mathematical equations the law according to which the value of money is determined" must necessarily ignore "the only factors that are decisive in causing variations of the exchange ratios [between money and commodities or services], that is, variations in the subjective valuations of individuals" (so Mises, *The Theory of Money and Credit*, 143 f.). For an example which may be regarded as contradicting this latter proposition, see Kemmerer, *Money and Credit Instruments in their Relation to General Prices*, 3 ff.

[21] The best example of a fairly explicit argument to this effect is provided by Wieser (cf. above, p. 488, n. 11). It is, however, characteristic of the looseness with which the argument under discussion has been constructed, that, as often as not, the reader has been forced to supply on his own account the steps in the reasoning by which one passes from propositions with respect to the rôle of "utility" in pricing theory generally to the contention that the center of our interest, if not our exclusive concern, must be with the prices of consumers' goods.

[22] Cf. what is said with respect to the mistaken formalism engendered by an over-enthusiastic desire to tie up the theory of money and prices with the "principles of general value theory," on pp. 441 ff., above. A further comment on the type of application of the supposed "principles" of utility analysis to the theory of money and prices which is here under discussion is provided by the fact that Wieser was not dissuaded from taking the step to which objection is here taken by his own emphatic and repeated warnings against too literal a "carrying over" of the concepts of "general value theory" to the problem of the "value of money." See, for example, "Der Geldwert und seine geschichtliche Veränderungen," *loc. cit.*, 47; *Social Economics*, 265, 285. Actually, the net effect of Wieser's own concern with the problem as to the relation between the two bodies of theory seems to have been, on the one hand, a wrong application—as in the present instance —of the proposition that the principles of "general value theory" must continue to apply in the Theory of Money and Prices, and, on the other hand, a blindness to the possibilities of *useful* application of certain ele-

term, should, indeed, be confined to the prices of consumers' goods.[23] Unless, however, we are to fall back upon over-simplified propositions with respect to the way in which the decisions of consumers are reflected immediately and with unerring accuracy in the prices of producers' goods and indeed in all sectors of the economic process, we must be prepared also to admit to the confines of "general value theory" a body of analysis designed to deal with all types of "price," in the explanation of many of which "utility analysis" will play hardly any rôle at all.[24] The "methodological principle" of "modern value theory" which really matters is that the phenomena of market price must, if they are to be explained at all, be explained in terms of the decisions of *individuals*.[25]

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ments of "general value theory" to the Theory of Money and Prices, as in the case of the application of the concept of "cash-balances" to the statement of the "demand" for money.

[23] A possible exception to this generalization may seem to be provided by the fact that, if considerations of "utility" can be said to determine the distribution of expenditures *in time*, it would follow that the *relative levels* of the prices of consumers' goods, on the one hand, and of producers' goods, on the other, must be regarded as being affected by such considerations, for the same type of reason as that which has induced some writers to attempt to state certain of the elements of interest theory in terms of "utility analysis." See, for example, Zwiedineck, "Die Einkommengestaltung, etc.," *loc. cit.*, 167, on the relation of "capital formation" to the "scale of wants"; and cf. also the reference to Hayek's paper on "Utility Analysis and Interest," on p. 177, n. 55, above. Quite apart, however, from the dangers of over-simplification of interest theory which have sometimes been exemplified in such cases (see, for example, the reference to Fisher in the following note), the point made here is that "utility analysis," if it appears at all in the pricing of *one type* of producers' good *relative to other types* of producers' good, plays a far more remote rôle than it does in the pricing of one type of consumers' good *relative to other types of consumers' good*. It was the latter type of consideration, of course, which Wieser had in mind in the passage quoted on p. 488, n. 11, above.

[24] For examples of the type of "over-simplified" proposition to which reference is here made, see again the references to Tooke and Wieser on p. 488, nn. 10 and 11, above. Nor is it difficult to cite examples, outside the immediate range of problems with which we are here concerned, in which an insistence upon the primacy, if not the exclusive importance, of decisions by "consumers," has led to the premature dismissal of problems which are in fact of the most vital importance for problems within the field of "general" economic theory. See, for example, the remarks on the relevant parts of Irving Fisher's discussion of the determination of the rate of interest, in my article "Irving Fishers Theorie des Zinses," *Zeitschrift für Nationalökonomie*, II (1931), 677 f. See also, and more generally, the remarks by F. H. Knight (in his essay "Statics and Dynamics" [*The Ethics of Competition*, 170 f.]) on the reasons for contending that a scheme such as that of Marshall "does not take sufficient account of the fact that in the actual fixation of the prices of commodities which have a highly organized market and a definite price at a moment, the market is made and the price at any moment fixed, not by owners of supply and prospective consumers . . . but by a class of professional traders who come in between these primary groups."

[25] It is worth noting that among the writers who have stressed the necessity for associating the theory of money and prices with "general value theory," there are some who have put most emphasis precisely upon this

That the individuals involved do not in all, or even in most, cases make their decisions as consumers, is a self-evident proposition. With the acceptance of this proposition, however, there ceases to be any clear basis for the further proposition that we are forced, by the very assumptions underlying the "general theory of value," to a definition of the "purchasing power of money" as the power of money to purchase those goods on which "for purposes of consumption a given community of individuals expend their money income."

The conclusion, indeed, which emerges from an examination of the attempts that have been made to construct a case for the proposition that the price level which corresponds *par excellence* to what we mean by the Purchasing Power of Money is the price level of consumers' goods is, in fact, quite different from that which seems to have been originally intended by certain of its sponsors, including Mr. Keynes at the time he wrote the *Treatise*. At best, what is established is the case for constructing an apparatus in which *a separate place is assigned to the price level of consumers' goods*, as distinct from the "price-levels" of other types of goods. What is not established is that a Theory of Prices which lays any claim to comprehensiveness can confine itself to the study of the forces determining this price level, and this price level alone.

As it happens, more than one of the writers who have been understood as having argued (and have been praised or blamed for having argued) that the central problem of the Theory of Prices is the determination of the prices of consumers' goods, and of the prices of these goods alone, can be shown to have left room in their exposition for the construction of what Mr. Keynes himself, who in this respect fol-

necessity for referring market actions to the choices of *individuals*, even if their emphasis on the "subjective character" of the "value of money" and their continued reference to the "scale of wants" underlying the choices of these individuals would lead one to suppose that they had in mind primarily the choices of "individuals" as *consumers*. See, for example, Zwiedineck, "Die Einkommengestaltung, etc.," *loc. cit.*, 138 (though cf. also 135, 141 ff.); also Aftalion, *Monnaie, Prix et Change*, 163, 171. It is particularly noteworthy that some of these writers, for all their emphasis upon the importance of considerations with respect to "marginal utility," have also shown, through illustrations in which the "psychological motivation" involved has to do with the speculative "psychology" of traders, that they would not have wished to be understood as regarding as adequate, analysis dealing solely with the market action of *consumers*. See, for example, Aftalion, *op. cit.*, 216.

lowed strictly, although apparently unwittingly, the model set by his predecessors, called a "plurality" of price levels.[26] There was nothing, for example, in the proposition of Tooke to the effect that the prices which "may be considered with greater propriety than any other description as general prices" were the prices of "objects of immediate consumption"—at any rate, as that proposition was interpreted by Newmarch—to prevent the authors of the *History of Prices* from devoting close attention to the differences in the degree of price change evidenced by different "Groups of Commodities," including such commodities as "the Raw Materials most extensively used in Manufactures," during the process of monetary expansion.[27] Similarly, by the time that Wicksell came to publish his *Lectures,* his emphasis upon the desirability of concentrating upon the prices of "objects of (direct) consumption" had become, as we have seen, much less marked.[28] So true was this, indeed, that

[26] See the *Treatise,* I, 55, and especially Chapter V, entitled "The Plurality of Secondary Price-Levels." Important in this connection also is Chapter VII, on "The Diffusion of Price-Levels" (I, 89 ff.), since the chief burden of the latter chapter is a protest against the idea that the various types of price level, "while doubtless theoretically distinct, all come to much the same thing in practice," and also against the idea that since "in stable conditions different price-levels stand in defined relationships to one another, and . . . if these relationships are temporarily disturbed, nevertheless forces will be set up tending to restore the former relationships rapidly," we may use "any respectable Index Number" for our purposes, whatever those purposes may be.

[27] Cf. the references to Volume VI of the *History,* given on p. 503, n. 50, below.

[28] Cf. p. 326, n. 72, above, and the reference to the *Lectures* there given. Relevant also, in this connection, is the passage, in the preface to the first Swedish edition, in which Wicksell listed, among the "changes, and, as I believe, improvements" in the argument of the *Lectures,* as compared with *Interest and Prices,* his more articulate emphasis on "the character of saving as [involving] diminished consumption in the present and thereby diminished demand for present goods, which should normally go hand in hand with the diminished supply thereof which results from the circumstance that a certain quantity of labor and natural resources is drawn away from the production of present goods and is directed, instead, toward the future." (See p. xxii of the 3d [1929] edition of the *Föreläsningar.* For some reason the nature of which is not clear, Wicksell did not include the passage in question among those which were reproduced in the preface to the German [1922] version of the *Lectures.*) Wicksell implied that this new emphasis was a result of his desire to "place the problem of changes in the general price level" more explicitly than he had previously done "under the simple and easily comprehensible formulas of supply and demand for commodities or services" (*Föreläsningar, loc. cit.*). As far as the present problem is concerned, however, it may be pointed out that even in

some of his most eminent disciples saw in his analysis not primarily an emphasis upon the central position of the prices of consumers' goods, but rather an emphasis upon a "race between different 'price-levels,'" of which the price level for "consumers' goods" was only one.[29]

The matter is even clearer in the case of Schumpeter. The latter has on occasion been criticized for having implied —so it is alleged—that any equation or set of equations other than the one representing the expenditure of income for *consumers'* goods "can be omitted from a formal theory of purchasing power without serious consequence."[30] In fact, however, Schumpeter was explicit in stating that, along with his equation for the prices of consumers' goods, there should be set up an equation for the prices of producers' goods.[31] The best proof, moreover, that one could not regard this "introduction of the subject" as "very casual," as it has on occasion been characterized, is Schumpeter's still more explicit insistence upon the necessity for taking account of the *inequality* of price change, not only as between the prices of different types of consumers' goods, but also as

Interest and Prices, the actual details of Wicksell's argument—as opposed to his formal statements with respect to the "general price-level" as being confined to the prices of "objects of (direct) consumption"—really implied the use of a "plurality of price-levels" and the application of the concept of "moneyed demand" to other than consumers' goods. See the following note.

[29] See, for example, Myrdal, "Der Gleichgewichtsbegriff, etc.," *loc. cit.*, 381; also Ohlin's Introduction to "Interest and Prices," p. xiv, where Wicksell's argument is summarized as involving not merely a sharp distinction between "consumers' goods" and "capital goods," but also as being concerned precisely with the changes in the "relative" prices of the two types of good. To what is said in the preceding note with respect to the basis, in Wicksell's writings, for such an interpretation, it is necessary to add only that the emphasis upon a change in "relative" prices as related to the process of monetary expansion was really implied also in those parts of the argument of *Interest and Prices* which were concerned with the supposedly "direct" effects of changes in the rate of interest upon prices—whether the rate of interest is regarded as a "cost-factor" or as a "capitalization" factor. In view of the fact that the argument in question really implied a "bidding-up" process which was regarded—implicitly if not explicitly—as the mechanism whereby differentials in prices and profits were brought about in different lines of industry, it is obvious that this argument also implied the application of the concept of "moneyed demand"—and shifts therein—to other than consumers' goods. For a restatement of Wicksell's argument with respect to "relative prices" and the "bidding-up process," see pp. 248 ff., above.

[30] See Ellis, *German Monetary Theory*, 126.

[31] Cf. Schumpeter, "Das Sozialprodukt, etc.," *loc. cit.*, 675.

between the prices of consumers' goods, on the one hand, and of producers' goods, on the other—the latter, indeed, because of its association with the phenomenon of "forced saving," being nothing less than an essential element in Schumpeter's apparatus for dealing with the Theory of Prices.[32]

It is, indeed, only in the light of this part of Schumpeter's analysis that one can evaluate his alleged "presupposition of perfect adjustment between [the prices of] consumers' and [the prices of] producers' goods"—on the lines, say, of the argument of Tooke and Wieser which we have found it necessary to reject so emphatically.[33] The difference between Schumpeter and a writer such as Wieser is that the former, like Keynes in his *Treatise,* was careful to confine the proposition that "the sum of the prices of all consumers' goods must be equal to the sum of the prices of all producers' goods and both must be equal to the sum of all money incomes" to a *"condition of stationary equilibrium."*[34] The difference, surely, is categorical; for in this difference lies precisely the refutation of both the suggestion that a special concern with the prices of consumers' goods must necessarily mean a lack of concern with a "plurality" of price levels, and

[32] See Schumpeter, "Das Sozialprodukt," 687 ff. For an example of the characterization of Schumpeter's treatment of the "producers' goods" equation in the terms suggested in the text, see Ellis, *German Monetary Theory,* 126.

[33] Cf. above, p. 488. The quotation with respect to Schumpeter's "presupposition" is again from Ellis, *German Monetary Theory,* 138.

[34] Cf. Schumpeter, "Das Sozialprodukt," 634 f. (for the reference to Keynes, see above, p. 488, n. 10). The fact, to be sure, that Schumpeter himself characterized his own consumers' goods equation as being essentially the "fundamental equation connecting the sum of incomes with the social product which was first sharply emphasized by Wieser" (Schumpeter, *op. cit.,* 635), makes it understandable that his position with respect to the desirability of working with a "plurality of price-levels" was interpreted by some critics as being essentially that of Wieser. It is true, also, that, in his *Social Economics,* it was in a context containing numerous references to a "static economy" that Wieser repeated his proposition that the reason why "of all the prices paid in the market, those payments which are made for consumption goods are decisive for the exchange value of money" is that "it is from these [that is, the prices of consumers' goods] that the prices of the means of production are derived" (Wieser, *Social Economics,* 264). He did not, however, specifically limit the usefulness of the particular proposition under discussion to a "static economy"; and there is nothing in his writings on money, so far as I am aware, which corresponds to the positive concern with the causes and effects of differential changes in the prices of consumers' and producers' goods to which attention has been called above, in connection with Schumpeter ("Das Sozialprodukt, etc.," 687 ff.).

the suggestion that the fate of the "income approach," as such, is necessarily tied up with an unwillingness to concern one's self with the determination of any prices but those of consumers' goods.[35] The latter suggestion is, in fact, itself refuted by the circumstance that other sponsors of the "income-approach" than Tooke, Schumpeter, and Wicksell have, in their final formulations, left room for a "plurality" of price levels side by side with whatever special emphasis may have been placed upon the price level of consumers' goods.

That this was so in the case of Zwiedineck, for example, is clearly apparent from his discussion of the "dynamics" of price change, in which particular attention was paid to the process of price determination on the "market for *producers' goods*," even though it was held that the goods whose rise in price is "decisive" for the question whether money has "depreciated" in value are consumers' goods, or, at best, those objects dealt with on the "market for producers' goods," such as labor, which could be shown to be directly associated with the prices of consumers' goods through the effect of an increase in labor incomes upon the latter.[36] It must be remembered, moreover, that by no means all sponsors of the "income-approach" have been prepared to agree even that the primary place, among all possible "price-levels," is to be assigned to the price level of consumers' goods. The "income-equation" of Aftalion, for example, unlike the "income-equations" of Schumpeter and Lindahl, was concerned, not with the prices of consumers' goods, but with the price of the "articles" included in "national production," which, by definition, included the production not only of consumers' goods, but of producers' goods as well.[37] Similarly, Hawtrey was explicit in insisting that his "consumers' outlay" must be thought of as being expended not only upon "consumption," but also upon "investment"—a fact which must be borne in mind in interpreting those parts of his algebraic formulation in which consumers' outlay is made equal to the volume of "production *and consumption* of wealth," multiplied by the prices at which the goods thus produced and "consumed" are sold.[38]

It would, however, be a serious mistake to suppose that the emphasis upon the desirability of dealing with a "plu-

[35] Cf., in this connection, the remarks by Ellis on the "income theory" in general (*German Monetary Theory*, 187), and on Schumpeter's variant thereof in particular (*ibid.*, 184).

[36] See Zwiedineck, "Die Einkommengestaltung, etc.," *loc. cit.*, 155 ff., 160.

[37] See, for example, Aftalion, *Monnaie, Prix et Change*, 157.

[38] See, for example, *Currency and Credit*, 46 f., 61. (The references are to the 3d [1928] edition.)

rality of price-levels" is an exclusive, though incidental, contribution of those writers who have insisted that the "Purchasing Power of Money" must be identified with the power of money to purchase consumers' goods. On the contrary, it is easy to show that the former emphasis is much older than the latter.[39]

Mr. Keynes, as we have seen, did not present a detailed argument in support of his contention that the price level which deserved, *par excellence,* to be regarded as the inverse of the "Purchasing Power of Money" is the price level of consumers' goods. He did present, however, a fairly detailed argument on behalf of recognition of the necessity for dealing with a "plurality of price-levels," and his discussion may be taken as providing a scheme for presenting an account of earlier ideas on the subject.

Mr. Keynes's arguments, under this head, were simply a series of reasons for refusing to accept what he regarded as a cardinal contention of "current economic theory"—namely, that, given an "initial impulse . . . 'on the side of money,' such as an inflation of the currency, . . . all individual prices tend to be affected *equally.*"[40] That the attribution of this latter position to "current economic theory" constitutes as gross a libel as can be conceived upon the only part of "current economic theory" which is here relevant—namely, the theory of money and prices—can be demonstrated by abundant references to "current" textbooks.[41] It can be shown, moreover, not only that Mr. Keynes's denial of the proposition which he attributed to "current economic theory" has been a commonplace in economic literature since at least the eighteenth century, as well as in "current" textbooks, but also that the specific reasons advanced by Mr. Keynes for denying the proposition, and therefore for insisting upon keeping "our minds alive to the

[39] The reader is reminded that, in dating the beginnings of an emphasis on the proposition that the "Purchasing Power of Money" must be identified with the power of money to purchase consumers' goods, reference is made to an articulate insistence that this procedure is to be preferred to available alternatives. Cf. the comment on Vanderlint, in this connection, on p. 487, n. 6, above.

[40] *Treatise,* I, 89 f.

[41] The reader can easily demonstrate this for himself by consulting the index of any of the widely used textbooks under the heading "Price changes, consequences of." I take two examples at random: "If price movements affected all social classes and all business interests uniformly, they would work relatively small injury. . . . It is this extraordinary upset [in price relations] which is inseparable from a major rise or fall of prices" (Edie, *Money, Bank-Credit and Prices,* 75). "The peculiar effects of price-fluctuations are due to the inequality of these changes in prices" (R. D. Kilborne, *Principles of Money and Banking,* 134 of the 3d [1932] ed.). For an example of the position taken even by those who have been regarded as supporters of a more rigid variant of "the quantity theory," see Kemmerer, "Zur Theorie des Geld und Kreditwesens," in *Die Wirtschaftstheorie der Gegenwart,* II, 363.

Plurality of . . . Price Levels and the separate influences which determine their movements," have likewise been commonplaces in economic literature for years.[42]

Consider, for example, the first of Mr. Keynes's reasons for the procedure in question. "When increased or decreased purchasing power in the form of money," said Mr. Keynes, "seeking to realise itself in actual purchases, comes into, or is withdrawn from, the market, the increase or decrease (as the case may be) is not spread evenly and proportionately over the various buyers. It will, in general, be concentrated in the hands of particular classes of purchasers: in the case of war inflation, for example, in the hands of the Government; in the case of a credit boom, probably in the hands of those who borrow from banks; and so on. Thus the immediate effect is on the goods in which the purchasers primarily affected are most interested. . . . Since . . . a change in the quantity of money generally involves a changed distribution of purchasing power, it follows that relative prices can be affected, not only by a change on the side of things, but also by a change on the side of money." [43] One has, however, only to read this passage to recall that it was precisely such considerations which underlay those parts of the argument of the writers of the eighteenth century who were concerned with the consequences of an increase in the quantity of money which have since been characterized as contributions to "monetary dynamics." [44]

In our examination of the suggestion that it was a peculiar "advantage" of the "income theory of prices" to have stressed the fact that "the increase in prices will be different, depending upon who the individuals are whose money income is increased," we saw, for example, that Cantillon, as early as 1755, had advanced a proposition virtually identical with that advanced in Keynes's *Treatise*.[45] Nor must it be supposed that the case of Cantillon was an isolated one.[46] Hume, for example, was not only aware of, but drew important consequences from the fact that "when any quantity of money is imported into a nation, it is not at first dispersed into many hands; but is confined to the coffers

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[42] The quotation is from the *Treatise*, I, 93. The list of references to older economic literature which follow in the text is in no sense intended to be exhaustive (cf. below, note 46). We shall, moreover, return to the matter in Volume II of this study, in connection with the rôle of differential price change in the theory of the effect of money upon output.

[43] *Treatise*, I, 92 f.

[44] See the references given on p. 84, n. 30, and p. 307, n. 13, above.

[45] Cf. above, p. 307.

[46] It must again be emphasized that the few instances here cited, in addition to that of Cantillon, cannot be regarded as a substitute for the detailed history of doctrine on the point in question of which we are still sorely in need. There can be little doubt, for example, that a careful combing of eighteenth century literature would yield an abundance of illustrations in addition to those provided by Cantillon and Hume. See, for example, the citations to J. G. Büsch's *Abhandlung von dem Geldumlauf* (1780) given by Hoffmann, *Kritische Dogmengeschichte*, 76 f.

of a few persons . . ." [47] Even Ricardo, whose analytical habits were indisputably such as to make him sympathetic to the conclusion that "the alteration in the value of money arising from [the] scarcity or abundance [of money] will operate in an equal proportion on the prices of all commodities," was perfectly prepared to recognize, and to provide an explanation for, "the different effects which . . . were produced on the prices of commodities, from the altered value of money during the Bank-restriction," and thus implicitly recognized the validity of the case for working with a "plurality of price-levels," even if his own explanation of the "different effects" on different prices did not run in the terms suggested by the first of Mr. Keynes's reasons for expecting such "different effects." [48] On the other hand, Sir John Lubbock, writing in 1840, advanced an explanation in precisely Mr. Keynes's terms when, in protesting against the suggestion "that for the public interest it is immaterial *how* bank notes are issued," he pointed out that "the dealers in any article who are the first to receive issues [of bank notes] must always be benefited." [49] No one, moreover, could have been more explicit than J. E. Cairnes in insisting upon the fact that the "enlarged money demand" which would be expected to follow from "an increased production of gold" would evidence a "direction of expenditure" which would "naturally be determined by the habits and tastes of the persons into whose possession the new money comes," and that "the commodi-

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[47] Hume, *Of Money* (I, 304 of the 1777 edition of the *Essays and Treatises on Several Subjects*). The consequences which Hume himself drew from this fact for the theory of the effect of money upon output will be discussed in Volume II.

[48] See Ricardo's *Principles,* 190 ff. of the Gonner edition, and cf. Ricardo's *Evidence before the Committee of the House of Commons on the Expediency of the Bank resuming Cash Payments* (1819), 137. Ricardo's reason, in these instances, for not expecting an equal degree of price change in all cases, was the "inequality of taxation" between different types of commodity. As we shall see in Volume II of this study, the case discussed by the older writers under the head of the effect of "inequality of taxation" upon relative prices is by no means as devoid of interest for an adequate Theory of Prices as might be supposed. On the other hand, there can be no pretence that Ricardo did sufficient justice to either the reasons for, or the consequences of, inequality of price change. It is, indeed, clear that one result of the shortcomings of his position in this respect was his generally unsatisfactory analysis of the effects of money upon output, which will concern us in some detail in Volume II. The point of the citations given above is merely that Ricardo was perfectly prepared to admit the *fact* that prices had changed in unequal degrees during the Restriction period (see, for example, the *Principles,* 192). Cf. also what is said on p. 503 below, with respect to the rôle of "sectional" price levels in the "classical" theory of international trade.

[49] See [Lubbock], *On Currency,* 10. Cf. also *ibid.,* 40: "It is by no means however pretended that all articles or all localities are equally interested in [read: affected by changes in] the quantity of money or of deposits. Those who derive their resources more immediately from the banks of issue will of course be chiefly affected."

ties which will be more affected . . . will be those which fall most largely within their consumption." [50]

The same thing may be said of Adolf Wagner, whose emphasis, in varying contexts, on the necessity for taking account of a "plurality of price-levels," as we have seen, has seemed to some historians of doctrine to provide a further reason for regarding him as at least a forerunner of modern "income-theories" of prices.[51] There can be no doubt whatever, in any case, that a number of the more self-conscious "income-theorists" have regarded an emphasis upon differential price change—which is, after all, the one phenomenon that justifies and makes unavoidable the use of a "plurality of price-levels"—as a central reason for preferring the "income-approach" to the assumption of a uniformity of price rise which they regarded as inherent in all forms of approach to the Theory of Prices other than the "income-approach." [52] In truth, of course, the emphasis upon the necessity for dealing with a "plurality of price-levels," instead of being a contribution of the "income-approach" exclusively, lies at the heart of generally accepted analysis of a number of problems in which the distribution of "purchasing power" and the resulting difference in impact at various points in the price structure is one of the crucial elements in the problem. If, for example, one were to go beyond the limits of the present study, one would think immediately of the rôle played in the theory of international trade—which Mr. Keynes himself rightly regarded as requiring particularly careful attention to the idea of a "plurality of price-levels"—by the concept of "sectional" price levels, such as the "price-levels" of "domestic" as opposed to "international" commodities, in the manner in which, although the roots of the argument go far back into the literature, the theory of the subject has been developed in our own day particularly by Professor Taussig and his followers.[53] Within the field

[50] See Cairnes, "Essays Toward a Solution of the Gold Question," II ("The Course of Depreciation" [1858]), in *Essays in Political Economy, Theoretical and Applied* (1873), 57, 60 f.; also pp. 64 f., 83, 114. Cf. also the discussion by Newmarch, published in the preceding year (1857), of the "order, extent, and character of the changes . . . produced . . . by the new supplies of gold from California and Australia" (*History of Prices,* VI, 135 ff.), with its emphasis on the differential price change as between different "Groups of Commodities" (VI, 158 ff., 170; cf. also VI, 811), which was held to characterize "the whole of the complicated processes by which the New Gold has become distributed"—this distribution, in turn, taking place as "the extended demand for commodities, originally proceeding from the earliest Labourers who picked up the Gold . . . goes on increasing" in a "circle of rapidly expanding area" (VI, 189, 191).

[51] Cf. above, p. 319, and especially notes 49 and 50 thereto.

[52] See, for example, Liefmann, *Geld und Gold,* 59 ff.

[53] For Keynes's comment on the necessity for taking account, in the Theory of International Trade, of "the fact that monetary changes do not affect all prices in the same way, in the same degree, or at the same time," see the *Treatise,* I, 94. On the rôle played by the distinction between "domestic" and "international" commodities in the history of international

covered by this study, on the other hand, the most notable example of a current use of the concept of a plurality of price levels as determined by the distribution and successive impact of new "money," is that which is represented by the theory of the effect of money upon the *structure* of production—a theory which, as Professor Hayek and others have shown, goes back at least as far as the beginning of the nineteenth century, but to which, as we shall see, Mr. Keynes himself, paradoxically enough, devoted in the *Treatise* anything but an adequate amount of attention.[54]

The second reason advanced by Mr. Keynes for insisting upon keeping "our minds alive to the Plurality of . . . Price-Levels and the separate influences which determine their movements" is that "there are many kinds of money-contracts, money-customs and money-understandings fixed over periods of time."[55] Mr. Keynes referred to this type of consideration as a "familiar fact." It has indeed been familiar, over a very long period, not only to the business world, but to writers on monetary theory, who were as aware as was Mr. Keynes that to assume "that all classes of prices are affected more or less in the same way by a change 'on the side of money,' is . . . to assume away the very phenomenon which we are out to investigate."[56] To state this

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trade theory, see especially Viner, *Studies*, 323 ff. The relevance of the type of consideration involved even to the study of the process of price change within a "closed" system becomes obvious when account is taken of the fact—which, as Viner so admirably demonstrates (*op. cit.*, 84, 293, 595 f.), was anything but ignored by the older writers—that the theory of "international" trade "was not concerned solely [with], or was not applicable solely to trade between sovereign nations," but was also a theory of "interregional" trade even when such trade was "intranational." The consequences of this fact are manifold. That, for example, it may be necessary, under certain conditions, to take account of differences in "price-levels" as between different parts of the same country, is a fact which, as Viner shows (*op. cit.*, 155 ff., 235, n. 1), was recognized by the older writers. For the purposes of "international" trade theory, however, it is of course the relative price changes of commodities *within the same area* which are most important (cf. Viner, 319 ff.). The fact that this was an integral element of the so-called "orthodox" theory of international trade in itself provides a commentary on the suggestion that the "orthodox" theory was concerned with nothing but movements in "general" or "average" price-levels—a suggestion the wide acceptance of which in turn provides a commentary on the accuracy of the translations of "orthodox" doctrines which have been made either for purposes of popular "education" or for the purpose of providing an easy target for those to whom anything "orthodox" is anathema (cf. the comment by Schumpeter, in the *Journal of the American Statistical Association*, XXXI [1936], 792, n. 2). On the treatment of "prices" and "price-levels" in the "classical" theory of international trade generally, see the authoritative comments by Viner, 314, 379 ff.

[54] The range of problems thus indicated will concern us in more detail in Volume II of this study.

[55] *Treatise*, I, 93.

[56] *Treatise*, I, 94; see also I, 75, 91. Cf. in this connection, Tooke and Newmarch's *History of Prices*, VI, 195: ". . . it is precisely these omitted elements [namely, those associated with the "interval which will elapse" between the injection of new money and definitive price rise, and "the magnitude of the changes which will take place in connection with the process"]

fact, however, and others which might also be stated—such as, for example, the fact that there have always been writers to remind us of the pitfalls involved in the use of global averages for purposes of dealing with the more intricate problems of monetary theory—is to state that the necessity for dealing with a "plurality of price-levels," so far from being a discovery of our own day, has been a commonplace of monetary theory for generations.[57]

The results of our discussion of the concept of a "plurality of price-levels" are easily summarized. In the first place, the concept is an old one. It follows, therefore, that

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which constitute the essence of the question." The passage beginning on VI, 194 of the *History* bears further resemblance to Mr. Keynes's utterances on the subject under discussion by virtue of its sweeping condemnation of "perhaps all the authorities who have discussed these questions on abstract and theoretical grounds" (cf. Keynes's comment on the assumptions of "current economic theory," quoted on p. 500, above), and who were alleged to have assumed "that the doubling of the quantity of money leads hastily to the doubling of the prices of *all* commodities" (italics in the original). The basis for such a sweeping generalization with respect to what "authorities" on the question had assumed was shaky even in Tooke's day—*vide* Cantillon and Hume, for example. There was, however, much less justification for Keynes's similar generalization at the time the *Treatise* was published. See, in addition to the references given in this chapter to Keynes's predecessors on the subject of a "plurality of price-levels," the following note.

[57] A complete list of instances in which the writers concerned have associated their argument for the use of a "plurality of price-levels" with a general distrust of "averages" would be too long, even if it were to confine itself solely to contemporary writers, to be included here. For an example, however, see, in addition to the well-known remarks by Hayek (*Prices and Production,* 4 ff.), those by G. Del Vecchio, "Un capitolo di teoria monetaria," *loc. cit.,* on the reasons for preferring, "in place of an *average* of the quantities and velocities of commodities and of money, on the one hand, and a general average of prices, on the other, the specialized averages involved in certain partial systems of circulation, and then passing on to an investigation of the relation existing between the different levels of prices resulting from these separate studies of individual systems of exchange, the latter being taken in their constituent elements of quantities, velocities, and prices." It is a question, Professor Del Vecchio argued, of "substituting for the consideration of *one* mass of commodities, *one* mass of money and *one* level of prices, etc., *several* heterogeneous masses of commodities, *several* heterogeneous masses of money, *several* levels of prices, etc., and of then constructing the general theory of money as a complex made up of the relations existing among these various elements . . ." (italics Del Vecchio's). It may be added that no account of the suggestions leading to the "series of *equations of exchange*" (so, Del Vecchio; italics in the original) which such a statement desiderates would be complete if it failed to mention the possibilities that may be regarded as inherent in a system such as that of Walras. The potentialities of the Walrasian system for the future development of monetary theory in the direction indicated have as yet, as I hope to demonstrate on another occasion, hardly begun to be exploited. Cf., in the meantime, what is said concerning these aspects of the Walrasian "system," by C. Bresciani-Turroni, "The Theory of Saving," *Economica,* N.S., III (1936), 3 ff.

when Mr. Keynes charged "current economic theory" with holding that "all individual prices tend to be affected *equally*," he was, at the very least, attributing to all of "current economic theory" a position that, if it was held at all, can have been held only by those who were unaware of the substance of that received doctrine on the subject of the Theory of Prices which, in so far as it can be shown to possess continuing validity, should be regarded as part of "current" economic theory. It follows, also, that when Mr. Keynes characterized as "the traditional method," the method of "setting out from the total quantity of money irrespective of the purposes on which it is employed," he was presenting what can only be regarded as a caricature of "traditional methods" for dealing with the Theory of Prices.[58]

It should be obvious, in the second place, from our brief survey of the diversity of setting in which the necessity for dealing with a "plurality of price-levels" has been recognized, that, just as it cannot be said that a clear case has been developed for regarding any one "price-level" as the price level *par excellence* for measuring changes in the purchasing power of money, it cannot be said that any one set of "plural" price levels can be regarded, on *a priori* grounds, as having unique validity. On the contrary, there are as many valid sets of "plural" price levels as there are economic problems in which given sets of "price-levels" can be shown to have economic significance.[59] In so far as the particular set of "plural" price levels presented by Mr. Keynes in his *Treatise* can be regarded as both significant and unambiguous, it may be regarded as valid. It cannot, however, be assigned a greater degree of validity than any other set of

[58] For the statement quoted, see the *Treatise*, I, 134.

[59] Cf. Del Vecchio, "Un capitolo di teoria monetaria," *loc. cit.*: "Each person may write these [equations making up the "system" of interdependent equations each leading to its own price level] to suit himself." It should be added, in justice to Mr. Keynes, that he himself implied as much when he urged multiplication of "the number and variety of the specialized secondary and sub-Indexes . . . so as to render it as easy as possible to build up by various combinations of these sub-indexes more complex indexes appropriate to the *particular purpose or inquiry on hand*" (*Treatise*, I, 75; italics mine).

"plural" price levels which can likewise be shown to be both significant and unambiguous.

As it happens, although one commentator on Keynes's *Treatise* characterized its treatment of "the problem of . . . the plurality of price-levels" as "brilliantly successful," most of the other critics of the *Treatise* showed a striking degree of unanimity in insisting upon the ambiguity of the most important of Mr. Keynes's "secondary" price levels: namely, P', described as the "price-level of new investment goods." [60] The first result of this ambiguity was, of course, to raise the question whether the *Treatise* had done more than to point again to the desirability of operating with a "plurality of price-levels," instead of presenting a particular set of "plural" price levels which could be used by subsequent writers for the analysis of problems identical with, or similar to, those with which the *Treatise* was concerned. The fact, however, that Mr. Keynes was anything but clear as to the precise meaning of P', the most important of his "secondary" price levels is not the most important consideration for our present purpose. Much more important is the fact that a number of the uncertainties surrounding P' to which the

[60] For the favorable comment on Keynes's treatment referred to in the text, see Williams, "The Monetary Doctrines of J. M. Keynes," *loc. cit.*, p. 548 (though cf. also p. 508, n. 63, below); and for references to criticisms based on the ambiguity which was held to surround P', see below, pp. 508 f., notes 61-67. For the formal definition of P' as the "price-level of new investment goods," see the *Treatise*, I, 137, 268 f. The ambiguity surrounding P' was, of course, not the only ground upon which criticisms were directed against Mr. Keynes's own system of "plural price-levels." Similar—although less flagrant—ambiguity was held to attach, for example, to his P—that is, the price of R—by virtue of the vacillating treatment accorded to "consumption" (see, for example, Hart, "An Examination of Mr. Keynes's Price-Level Concepts," *loc. cit.*, 625 n.). It was likewise held that since II—the price of output as a whole—included P', to which so much objection was found, the former suffered also from the shortcomings of the latter (*ibid.*, 637). (It may be noted, however, that this follows only if one insists upon taking seriously all the details of Mr. Keynes's treatment of P'. It would be perfectly possible to consider Mr. Keynes's treatment of the price level of "output as a whole" quite independently of the shortcomings of his treatment of P'.) It was argued, also, that Mr. Keynes's system of price levels, being confined, in effect, to "finished goods," did not leave adequate room for the price level of "intermediate products," which, being at once more sensitive than the prices of "finished goods" and subject to a different type of market process, may be of very great importance for the theory of output (see, for example, Hawtrey, *The Art of Central Banking*, pp. 340 f.).

critics pointed—for example, its relation to the price of "securities" and of "old goods"—raised, by implication, the question whether, after all, something could not be said for the use of a "quantity equation" in which a specific place was given to something called the "volume of transactions." The other difficulties in Mr. Keynes's treatment were, after all, matters that could have been repaired by the exercise of greater care in definition and exposition. The issues involved in the use of an equation containing a term for the "volume of transactions," on the other hand, bring us at once back to Mr. Keynes's strictures upon equations of the general Fisherine form, and to the question whether these equations can or cannot be used as checks upon the results obtained by the use of equations of the form presented in the *Treatise.*

The fact that Mr. Keynes's treatment of P' was almost universally held by his critics to have been unsatisfactory makes it unnecessary to do more here than to summarize the principal charges leveled against this aspect of the argument of the *Treatise*, leaving the interested reader who is not satisfied with the incidental comments made in the present chapter and the two following chapters to pursue the matter further with the help of the references given.

The charges advanced were:

1. P' was characterized as applying to goods not "homogeneous in character," and Mr. Keynes himself was accused of having failed to bear in mind at all points the "composite nature" of the goods to which P' was supposed to apply.[61]

2. The fact that C, of which P' was supposed to be the "price-level," was defined in such a way as to refer not primarily to certain types of goods sold, but rather to the "net increment of Investment," meant that under certain conditions it would have to be regarded as a negative quantity, with results so paradoxical as to make the meaning of P' extremely difficult to interpret.[62]

3. Keynes was charged with having introduced "considerable obscurity and contradiction with regard to the relation between the price-level of 'new investment goods' [P'] and the price-level of 'securities.' " [63]

[61] Cf. Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 398 ff.; Bernstein, *Money and the Economic System,* 268 f.; and, more moderately, Hawtrey, *The Art of Central Banking,* 338 f.

[62] For the definition of C as the "net increment of Investment," see, for example, the *Treatise,* I, 135; and for the criticism itself, see Hawtrey, *The Art of Central Banking,* 340, and Hart, "An Examination," 636 f.

[63] Hayek, "Reflections," II, *loc. cit.*, 36 n. Cf. also Robertson, "Mr. Keynes' Theory of Money," 400, and Williams, "The Monetary Doctrines

4. Particular ambiguity was held to attach to that component of P', designated as p', which was defined as the "price of working capital"; and the precise nature of the relation of p' to the other components of P' was likewise characterized as obscure.[64]

5. Keynes was accused of a tendency "to identify the price-level of investment goods [P'] with the price-level of capital goods or instrumental goods." [65]

6. He was charged with having failed to deal adequately with the relation between changes in the price level of *old* "investment goods" (the "value of existing capital") and changes in the price level of "new" investment goods [P'].[66]

7. The fact that "Mr. Keynes . . . nowhere thought it necessary to reduce the forces determining P' to an equational form" was characterized as "the main source of weakness in the whole structure." [67]

It will be seen, from what follows, that propositions 2, 3, and 6 are those which come closest to raising the problems involved in a choice, for a generalized "equation of exchange," between a term representing the "volume of transactions," on the one hand, and one representing *solely* "output" or some special variety of "output," on the other.

III

"Consumers' Goods" Equations, Plural Price Levels, and Fisherine Equations

We saw, in the preceding section of this chapter, that although the case for designating the price level of consumers' goods as the price level *par excellence* for measuring changes

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of J. M. Keynes," *loc. cit.*, 567. On Keynes's attempt to answer the criticisms made against him on this head (cf. his "Rejoinder" to Robertson, *loc. cit.*, 421), see below, p. 546, n. 58, and pp. 595 ff.

[64] For the definition of p', see the *Treatise*, I, 314; and for a criticism of the type indicated, see Robertson, "Mr. Keynes' Theory of Money," 398 f.

[65] See, for example, Hawtrey, *The Art of Central Banking*, 339. Cf. also what is said with respect to the difference between "non-available output" and "investment" on pp. 345 f. of the same work.

[66] See Hayek, "Reflections," I, *loc. cit.*, 275 f. Cf. Keynes's "Reply to Dr. Hayek," *loc. cit.*, 12, and what is said on this matter on pp. 545 ff., below.

[67] So Robertson, "Mr. Keynes' Theory of Money," 398. For an example of an attempt to write a special equation, of the general form of the Fundamental Equations of the *Treatise*, for P', see Hayek, "Reflections," I, 283 (also Hayek's "Rejoinder to Mr. Keynes," *loc. cit.*, 15); and cf. Hart, "An Examination," *loc. cit.*, 632. These attempts, which are concerned only with *separate* equations for the determination of P', are, of course, to be distinguished from those rewritings of the equation for the "price-level of output as a whole" (Π), in which ΠO is merely broken up into PR and $P'C$. See, for example, Williams, "The Monetary Doctrines of J. M. Keynes," *loc. cit.*, 551, n. 5, and the reference to Hawtrey on p. 302, n. 1, above.

in the "Purchasing Power of Money" was of doubtful validity, there was certainly a clear case for constructing an apparatus which would assign a separate place to the price level of consumers' goods.[68] We saw, also, that this suggestion was anything but a novelty in economic literature. The first question to be decided, therefore, is what bearing this conclusion has upon the usefulness of equations of the general Fisherine form.

It should be clear, upon very slight reflection, that, in arguing that equations of the *general Fisherine form* could provide no "guide to the Purchasing Power of Money," in the sense assigned to the latter expression in the *Treatise,* Mr. Keynes was certainly throwing the baby out with the bath water.[69] All that the "general Fisherine form" requires is, in the words of Mr. Robertson, "the concept of a certain flow of money in a given time-interval meeting a certain flow of goods in the same time-interval," the "flow of money" being, one should add, in its turn resolved into the quantity of "money" and its "rate of flow," or "velocity."[70] To those who accept the conclusions stated in

[68] Cf. especially p. 495, above.

[69] It is striking that Mr. Keynes should, at no point in the *Treatise,* even have experimented seriously with the possibility of providing a "consumers' goods" equation of the general Fisherine form. The nearest he seems to have come to it is represented by his equation $P \cdot O = M_1V_1$, which he himself characterized as bearing a "family relationship to Professor Irving Fisher's familiar equation" (*Treatise,* I, 150). It will be observed, however, that, despite the inclusion of P, which was elsewhere defined as the price level of R (that is, the price level of "the volume of liquid Consumption goods and Services flowing on to the market and purchased by consumers" [*Treatise,* I, 135]) the goods involved in the equation quoted above are those included in "output as a whole" (O), the price level of which (II) was assumed to be equal to P under the conditions $I = I' = S$, an expression which was in turn regarded as a summary of the conditions of "equilibrium," the latter being otherwise undefined (*Treatise,* I, 149). Above all, however, it is to be noted that Mr. Keynes by no means regarded the equation $P \cdot O = M_1V_1$ as being true under all conditions, as would be the case with a really satisfactory "consumers' goods" equation of the Fisherine form. The equation in question would hold, said Mr. Keynes, only when $I = I' = S$.

[70] Cf. Robertson, "Mr. Keynes' Theory of Money," *loc. cit.,* 401. Mr. Robertson's suggestion that Keynes had, in his "first fundamental equation," applied the "rigorous Fisherine concept . . . without question to P [the price of consumers' goods]" should, of course, be accepted only in a very special sense. Cf. what is said on this matter on p. 125, n. 59, and on p. 136, n. 81, above. For an example of the suggestion that a "consumers' goods" equation, or any "partial system" of equations, would naturally require the decomposition of the "flow of money" involved into the "con-

Chapter Three of this study, it is a matter of extreme simplicity to conceive of an application, to the problem in hand, of the method which, in the context provided by that earlier chapter, was characterized as the second method for correcting an alleged asymmetry in a given Fisherine "equation": the method, namely, of so defining the terms in the two members of the expression in question that they are necessarily equal by definition.[71] In the present instance, our problem is posed for us by the condition that the P of our equation shall represent the inverse of what Mr. Keynes called the "Purchasing Power of Money"—that is, the price level of consumers' goods. The T of this equation thus of necessity becomes, when provided with a suitable subscript, the volume of consumers' goods sold in the period under examination. Similarly, the "flow of money," in this case, is of necessity the flow of money going to purchase consumers' goods during the period under examination, and is resolvable either into the total "quantity of money" times "income-" or "circuit-velocity," in one of the senses of the latter term, or, very preferably, into the total of cash balances held specifically against outlay on consumers' goods, multiplied by the rate of turnover, or "velocity of circulation," of these balances.

Nor have instances been lacking of the actual formulation of such an equation. A crude variant, in nonalgebraic form, was presented, as we have seen, by Wieser; and it was this crude "income-equation" of Wieser which Schumpeter used as the basis for his own algebraic variant, in which the concept of "income-" or "circuit-velocity" was introduced in the guise of the "efficiency" of money.[72] A similar equation was implicit in the exposition of Foster and Catchings, who were referred to by Lindahl, in 1929, as having suggested his own more careful formulation, in which appropriate subscripts were provided for the familiar variables of the Fisherine equation.[73] If, finally, we recall the argument of Chap-

stituent elements" represented by "quantities" and "velocities," see the quotation from Del Vecchio on p. 505, n. 57, above; and see also the concrete examples presented by the various writers cited on this page.

[71] Cf. above, pp. 64 ff.; also 99 ff.

[72] Cf. above, p. 339, and n. 111 thereto.

[73] Cf. above, p. 328, n. 78.

ter Fifteen, which presented a "consumers' goods" equation that avoids the concept of "income-" or "circuit-velocity," the case against the suggestion that the use of an equation of the general Fisherine *form* is inconsistent with any attempt to represent the forces determining the prices of consumers' goods becomes complete.[74]

Precisely the same thing must be said of the use of the Fisherine equation for the representation of a system of "plural" price levels. For any one of these price levels, an equation of the general Fisherine form may be constructed along the lines suggested above for the construction of a "consumers' goods" equation.[75] The procedure thereafter should be perfectly obvious to anyone familiar with the manner in which Fisher himself, as well as writers who have come after him, passed from a series of "equations of exchange" for the "individual transactions" of "individual persons" to an "equation of exchange" for a given community.[76] The "individual" equations of exchange are simply *summed*; and all that is necessary, for the representation of a system of "plural" price levels, is that the P of our equation be subdivided, by the use of appropriate subscripts, into the particular "price-levels" which are held to be significant for the problem in hand.[77]

Again it must not be supposed that instances are lacking of actual formulations of this type.[78] Fisher himself, as we

[74] For the equation referred to, see above, p. 428.

[75] Cf., in this connection, the procedure of Schumpeter, referred to on p. 497, above, according to which, after the setting up of a "consumers' goods" equation which, as Schumpeter himself put it, was "externally quite identical with the Newcomb-Fisher equation," it was then stated that "a similar equation would apply to the market for productive services," the only difference being that "the equation for the market for means of production refers at each moment to a different concrete social product than the simultaneously applicable equation for the market of consumers' goods."

[76] See Fisher, *The Purchasing Power of Money*, 16, 26, 355-362, 364; and cf. G. C. Evans, *Mathematical Introduction to Economics*, 94 ff.

[77] For an example of this type of formulation, see above, p. 428.

[78] Reference is here made, of course, to actual formulations, rather than to general ground sketches for these formulations, such as those of Schumpeter and Del Vecchio, or to formulations such as that of Walras, which, although it covers a much broader range of phenomena than those ordinarily regarded as relevant to "monetary theory," can nevertheless be shown to be capable, with comparatively slight, though crucial, modifications, of providing the type of formulation suggested in the text. On Del Vecchio and Walras, cf. above, p. 505, n. 57.

have seen, provided what amounted to a variant of such an equation, involving a differentiation both between "purchases at home" and payments "outward" (that is, for imports), and between the prices of goods sold at home and those sold abroad, as an incident to his discussion of the "modification" of the equation of exchange which may be said to be "required by international trade."[79] In 1928, moreover, L. V. Birck, elaborating upon a formulation which he himself had presented at least twenty-five years before, presented a "monetary equation" in which separate notation was assigned to the prices and quantities of (1) consumers' goods, which were in turn subdivided into (a) necessities and (b) luxuries; (2) producers' goods; and (3) "capital claims" (such as stocks, bonds, and the like).[80]

Similarly, Professor Fanno, likewise building upon a formulation which he had presented in 1913 as part of his utilization of the Fisherine equation for purposes of dealing with the "demand for loans" in the money market, proposed the subdivision of the second member of this equation into price groups corresponding to (1) the "quantity of producers' goods and personal services which landlords, industrialists, and mine-owners purchase in a given period"; (2) the quantity of goods purchased by "traders"; and (3) the quantity of goods purchased by "consumers" during the same period; and he also provided an algebraic formulation for the particular set of "plural" price levels involved.[81]

[79] Cf. above, pp. 56 f.

[80] See Birck, *Den økonomiske Virksomhed* (Copenhagen, 1928), 344 ff. (The material there presented is not included in the abridged version of the same work published in 1934.) Birck himself, in expressing a preference for his own formulation over that of Fisher (see Birck, *op. cit.*, 350), referred to an equation which he had adduced in 1900. He did not indicate, however, in which of his earlier publications the equation was to be found; and the only one of those publications known to me in which there appears an equation similar to that published in the work of 1928 is his *Bidrag til en teori om de økonomiske perioder* ("Contribution to a Theory of Economic Periods"), Copenhagen, 1903, 13, 16.

[81] Cf. Fanno, "Die reine Theorie des Geldmarktes," *loc. cit.*, 26, where $Q_p P_p$, representing the total volume of goods sold times their prices, was made equal to $Q_{p_1} P_{p_1} + Q_{p_2} P_{p_2} + Q_{p_3} P_{p_3}$, in which the numerical subscripts refer to the three categories of "goods" indicated in the text. In the version presented in *Le banche, etc.* (p. 214), Q_1 and Q_2 refer respectively to the quantity of instrumental goods acquired by the group of "industrialists and agriculturists," on the one hand, and to "the quantity of products exchanged by traders," on the other. In both versions, however,

There have been examples since.[82] One wonders, therefore, that it can still be regarded as inherent in the use of equations of the general Fisherine form that they should lead to a "hotch-potch" price level, with no differentiation of separate price groups which can be shown to be significant for the intricate problems involved in the Theory of Prices.[83]

IV

Fisherine Equations and the Price Level of Output

The conclusion just reached with respect to the use of equations of the general Fisherine form for the representation of a "plurality" of price levels brings us, indeed, to grips with what was in many respects the most important of Keynes's criticisms of equations of this type: namely, that since the T of Fisher's variant of the "equation of exchange" was not the "volume of output," equations of the Fisherine form are "incapable of leading us to the price-level of output." It was "impossible," to Mr. Keynes's mind, that any one should "suppose that the Fisher Equation purports to tell us the price-level of output."[84] It was, in fact, for precisely this reason that Mr. Keynes refused to admit that the

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a separate notation was provided for the price of "products," on the one hand (P_p), and the price of "securities" (P_t), on the other. See *Le banche, etc.*, 224 ff., and "Die reine Theorie, etc.," 30 ff.

[82] See, for example, J. M. Thompson, "Mathematical Theory of Production Stages in Economics," *Econometrica,* IV (1936), 82 ff.; also Roos, *Dynamic Economics,* 238, where M_cV_c, representing the total of payments made by bank checks, is "split up" into one "fraction" (y_c) representing the amount "used for the purchase of consumer goods and all services," and another "fraction" ($1-y_c$) representing the amount "available for . . . purchase of stocks, bonds, mortgages and capital goods," and where separate place is given, in the second member of the equation, to the sums of the prices of "raw materials," "consumption goods," "capital goods," "capital services," and "services" rendered for wages and salaries, respectively.

[83] Cf. Keynes, *Treatise,* I, 221, 236. It is curious that on this particular point Keynes should have seemed to find support in Hayek. See, for example, the latter's *Prices and Production,* 3 ff., with its implication that a formulation such as Fisher's "equation of exchange" is to be regarded as constructed solely in order "to admit of statistical verification" (cf. Keynes's *Treatise,* I, 235), and its further implication that "mathematical formulae" of this type cannot be used for anything but attempts "to establish *direct* causal connections between the *total* quantity of money, the *general level* of all prices, and, perhaps, also the *total* amount of production" (italics Hayek's).

[84] See Keynes's "Rejoinder" to Robertson, *loc. cit.,* 419 f., and cf. the *Treatise,* I, 235.

"Fisher equation" could be used as a check on the results obtained by the use of the Fundamental Equations of the *Treatise.*[85]

One answer to this proposition—and, for all its simplicity, it is a valid answer as far as it goes—of course lies at hand, if we choose to answer it in the way in which we attempted to answer Mr. Keynes's contention that equations of the general Fisherine form cannot lead to the "Purchasing Power of Money"—that is, to the price level of consumers' goods. It was argued above that one ought to be prepared, if the baby is not to be thrown out with the bath water, to distinguish between a *particular variant* of the Fisherine equation and the *general form* of that equation; and that, so far as the latter is concerned, it is perfectly possible to construct an equation of the general Fisherine form which would lead to any particular price level in which we may happen to be interested.[86] If this is true with respect to the price level of consumers' goods (the "Purchasing Power of Money"), it is also true with respect to any "price-level" we may wish to consider—including the price level of "output as a whole," or of any particular variety of output. This was, indeed, recognized, although incompletely, by Mr. Keynes himself, when, at more than one point in the *Treatise,* he presented equations purporting to represent the forces determining the price level of "output" which, as he put it, bore "a family resemblance" to the "Fisher equation." [87]

We may go a step further. It was argued above that, after individual equations of this type had been formulated, it would be possible to construct a system involving a "plu-

[85] Keynes, "Rejoinder," *loc. cit.,* 419. Cf. above, pp. 68 ff.

[86] Cf. above, pp. 510 f.

[87] See the *Treatise,* I, 150, and II, 5. It should be remembered, to be sure, that so far as the first of these "equations" is concerned, Keynes did not regard it as a true equation under all circumstances (cf. above, p. 510, n. 69); and that the second loses a large part of its "family relationship" to the Fisherine equation as soon as it is recognized that the V' was not "the Velocity of Circulation" of the Fisherine equation, but a "complex notion" which included, along with an element "of a similar character to the traditional velocities of circulation," another element "dependent on the balance between Saving and Investment." Cf., on this matter, p. 15, above. Both "equations" show, however, that Mr. Keynes was at least prepared to consider the *possibility* of constructing an equation of the general Fisherine form for the "price-level of output"; and this is all that is necessary for our present purpose.

rality" of price levels by the simple device of *summing* these separate equations, the various "price-levels" being distinguished by appropriate subscripts in the final formulation.[88] It is necessary to point out here only that it follows, from this argument, that *any summation of a series of equations for individual "price-levels" simultaneously provides a summary of the forces determining the individual price levels themselves.*

Let us suppose, for example, that we have written the following equations, representing the forces determining (1) the "Purchasing Power of Money"—that is, the price level of consumers' goods—which we may represent by P_r; and (2) the price level of "new investment goods," which, following the notation of the *Treatise,* we may represent by P':

$$M_rV_r=P_rR \tag{1}$$

$$M'V'=P'C, \tag{2}$$

R and C being the volume of consumers' goods and of "new investment goods" sold in the period under consideration, M_r the volume of cash balances held against expenditure on consumers' goods, M' the volume of cash balances held against expenditure on "new investment goods," and V_r and V' the respective "velocities of circulation" of these balances.[89] Let it be desired to construct an equation for the price level of "output as a whole." According to the *Treatise,* "the price-level of output as a whole during any period is made up of two components—the price-level of the goods coming forward for consumption and the price-level of the goods added to the stock of capital."[90] That is,

$$\Pi O=P_rR+P'C.[91] \tag{3}$$

To obtain an equation of the general Fisherine form for the "price-level of output as a whole," obviously we have only to add equations (1) and (2), obtaining

$$M_rV_r+M'V'=P_rR+P'C=\Pi O. \tag{4}$$

[88] Cf. above, p. 512.

[89] Attention should be called to the fact that R is here understood in a sense analogous to that assigned to it by Keynes on I, 135 of the *Treatise* —namely, as "the volume of . . . Consumption-goods . . . flowing on to the market and *purchased by consumers*"—and not in the sense involved in the expression $O=R+C$ (cf. *ibid.*), which would make R not the volume of consumers' goods *sold,* but the volume of consumers' goods *produced.* The same thing must be said with respect to C. (On the issues involved in the distinction between "goods produced" and "goods sold," see below, pp. 539 ff.) It should be noted also that the terms M_r, M', and V' are here given meanings other than those assigned to them elsewhere in this study or in Keynes's *Treatise.*

[90] See the *Treatise,* I, 179. This statement, together with the formulation cited in the following note, of course provides the basis for the rewriting of the equation for the "price-level of output as a whole" by Hawtrey and Williams, mentioned on p. 509, n. 67, above.

[91] Cf. the *Treatise,* I, 137.

It is thus seen that our "Fisherine" equation representing the forces determining the price level of "output as a whole" must be regarded as *including* a formulation of the forces determining the "price-levels" of *any segment of output* included in "output as a whole." For the "price-level of consumers' goods," for example, we have

$$P_rR=\Pi O-P'C=\Pi O-M'V'=M_rV_r, \tag{5}$$

and for the "price-level of new investment goods," we have

$$P'C=\Pi O-P_rR=\Pi O-M_rV_r=M'V'. \tag{6}$$

The application of this simple reasoning to an evaluation of Mr. Keynes's contention that equations of the Fisherine form "are incapable of leading us to the price-level of output," simply by virtue of the fact that they are concerned with "the volume of transactions" and the "price-level of transactions," should be immediately obvious.[92] Mr. Keynes himself made it clear that by the "volume of transactions" he understood the volume of "articles traded," and by the "price-level of transactions" he meant the prices at which these articles are "traded." [93] It is of course obvious that *among* these "articles traded" during the period covered by the equation are the "articles" which go to make up the "output" of that period. We may therefore write $PT=\Pi O+P_xT_x$, in which T_x represents "articles" sold other than current output, much as Mr. Keynes himself wrote $\Pi O=PR+P'C$.[94]

[92] For Keynes's statement of the contention in question, see again his "Rejoinder" to Robertson, *loc. cit.*, 419.

[93] See the *Treatise*, I, 234. It may be remarked, in passing, that the definition of the P of the Fisher equation as the price of the "articles traded" is a vastly more sensible way of describing what is involved than is its definition as the "price-level of *transactions*" (cf. Keynes's "Rejoinder" to Robertson, *loc. cit.*, 419). A "transaction" may *involve* a "price"; it is certainly confusing, however, to speak of a "transaction" as being "priced." What is "priced" is not the "transaction," but the "article" which is "sold" or "traded" at the price *involved* in the "transaction." The matter is more than one of mere terminology; for it is difficult to believe that anyone who was in the habit of speaking of the price of "articles traded," instead of the price of "transactions," could long be blind to the fact that among these "articles traded" were "articles" which made part of the "output" of the period under consideration.

[94] It is of some importance to stress the fact that the formulation $PT=\Pi O+P_xT_x$ itself involves a considerable amount of oversimplification, by virtue of the issues involved in (1) the difference between the volume of "current output," and the volume of "current output" which is *sold during the period under discussion*, and (2) the fact that the (PT) of our Quantity Equation includes certain "transactions" which are not resolvable into a volume of "articles traded" times a "specifiable price." On the first of these sets of issues, see, in addition to what is said on pp. 133 ff., above, the

If we write, in the manner suggested earlier,

$$M_oV_o=\Pi O \tag{7}$$

and

$$M_xV_x=P_xT_x, \tag{8}$$

in which M_o and M_x represent the volume of cash balances held against expenditure on O and T_x, respectively, and V_o and V_x the velocities of circulation of those balances, it follows that we may also write

$$M_oV_o+M_xV_x=\Pi O+P_xT_x=PT. \tag{9}$$

It follows that

$$\Pi O=PT-P_xT_x=PT-M_xV_x=M_oV_o.^{95} \tag{10}$$

No further argument, surely, is necessary to demonstrate the absurdity of suggesting that it is "impossible" for any one to "suppose that the Fisher equation purports to tell us the price-level of output." The "Fisher equation" tells us at least as much about "the price level of output" as does any equation of the Fisherine form which is concerned solely with the "price-level of output," by virtue of the simple fact that it *must always be understood as* including *an equation which represents solely a stream of "output" offered for money going against a stream of money offered for "output."*

V

The Case for a "Total Transactions" Equation

It is of the very first importance, however, to recognize that the "Fisher equation," which represents a stream of money going against the total "volume of transactions," tells us not only as much as do the less inclusive equations of the

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discussion of the distinction between "goods produced" and "goods sold" on pp. 540 ff., below; and on the second, see pp. 57 f., above, and also pp. 572 ff., below. It is only in order to confine the discussion to the specific issues involved in Mr. Keynes's contention that a "total transactions" equation cannot lead to the "price-level of output," that the oversimplification is indulged in here.

[95] It is obvious that, by the same reasoning, a "total transactions" equation can be shown to lead also to the price level of that part of "output" which is represented by the output of consumers' goods. For, since $\Pi O = P_rR+P'C$, and $M_oV_o = M_rV_r + M'V'$, it follows that we may write

$$M_rV_r+M'V'+M_xV_x=P_rR+P'C+P_xT_x=PT; \tag{11}$$

whence

$$P_rR=PT-P_xT_x-P'C=PT-M_xV_x-M'V'=M_rV_r. \tag{12}$$

general Fisherine form, but tells us more; and it is equally important to understand precisely why this "more" lies at the heart of the question whether any useful purpose is served by equations of the general Fisherine form which purport to represent the consummation of a total volume of "transactions" as well as by equations of the Fisherine form which purport to represent the consummation of some particular subgroup of "transactions."

The central issue involved in this question is not a new one. It may, indeed, be said to have been implicit in all discussions of the significance of "intermediate transactions" for the determination of the "value of money," from the early part of the nineteenth century to the present day.[96] An articulate discussion of this question, however, was forced to wait until a direct challenge had been provided by those forms of the "income-approach" to the Theory of Prices which argued, or implied, not only that the central "equation" for the "value of money" must be an equation leading to the price level of consumers' goods, but also that such an equation was in itself sufficient for the purposes of general monetary theory.[97]

[96] For an example of early discussions in which these issues were involved, though they were certainly not sharply articulated, see the references to Tooke on p. 314, nn. 36 and 37, above; and cf. also the reference to Wicksell on p. 326, n. 73. Actually, of course, the whole of the discussion with respect to the rôle in the Theory of Prices of the concept which has been discussed under the head of the "number of middlemen's sales" (on which see below, pp. 554 ff.), is likewise strictly relevant—as is also the discussion with respect to the rôle of "security transactions" in the determination of the "price-level of output" (cf. below, pp. 576 ff.). Mention should be made, finally, of the implications of the question, raised by Foxwell and Edgeworth (cf. the latter's *Papers Relating to Political Economy*, I, 261 ff.) as to the justification for "weighting" an index number in accordance with the respective influence of its components upon the "demand" for money—though it must be added that the virtual identification of the problem of "measuring the demand for money" with the measurement of the "general exchange value of money" which was thus involved can hardly be regarded as having had fortunate consequences. See, for example, the unsympathetic comments by C. M. Walsh, *The Measurement of General Exchange Value* (1901), 85; and cf. Keynes's *Treatise*, I, 78.

[97] It may be pointed out again (cf. above, pp. 495 ff.) that most of the writers who have been understood as having held the latter position have, in fact, given evidence, in one way or another, that they did not really believe that a "consumers' equation" was in itself "sufficient for the purposes of general monetary theory." There can be little doubt, on the other hand, that (1) the particular issues involved in the problem of the treatment of "intermediate transactions" were either slighted or erroneously treated by some of these writers; and (2) that, in any case, it was the *interpretation* of these writers as having held that a "consumers' equation"

One answer to this latter suggestion has already been adduced, in the form of an argument designed to show the necessity for an apparatus dealing with a "plurality" of price levels, if we are to deal with such problems as changes in the price structure, the sequence of these changes, and all the problems, including that of the effect of money upon output, to which these changes may give rise.[98] This, however, is not the argument directly involved in the suggestion that a formulation which purports to deal with the *total* "volume of transactions" must remain an essential part of the Theory of Prices. The latter argument is the one which was adduced by Professor Mises in his critical discussion of Wieser's contention that "in an investigation of the value of money we are not concerned with the total demand for money"; that, as Wieser argued, "the demand for money to pay taxes with," the demand for money to meet "capital and interest payments" and for money to accomplish similar forms of "transaction," are irrelevant for the central purposes of the Theory of Prices.[99]

Mises's argument, in its essence, was simply that the "demand" for money for the purposes excluded by Wieser was

was in itself sufficient that led to the most articulate statements, in recent years, of the arguments against such a position.

[98] Cf. especially pp. 490 and 498, above. The significance of the phenomena in question for the theory of the effect of money upon output will be discussed in Volume II of this study.

[99] See Mises, *The Theory of Money and Credit*, 136 f. It is unfortunate that Mises should have suggested that the essence of his argument is not aided by, if indeed it is even consistent with, the reduction of the "amount of the demand for money" to its "elements" in the form of the expression T/V (Mises, *op. cit.*, p. 135). The purpose of "reducing" the "amount of the demand for money" to its "elements" T and V is, as we have seen, that it makes it possible to distinguish what has been called the "absolute" demand for money, on the one hand, which is concerned with the forces summarized by T, as well as by V, from the "relative" demand for money, which is concerned only with the forces summarized by V. Having thus distinguished the forces behind T from those behind V, it is then possible to deal, as Mises does, with the forces behind T which affect the "demand" for money. In the light, moreover, of what is said on p. 447, above, with respect to the relation of the choices by individuals to the "absolute" demand for money, one could agree with Mises's statement that the "theories" which make use of the T and V of the equation of exchange in dealing with the "demand" for money, "break down at the crucial point," only in one sense: namely, that some formulations have not gone beyond the point reached by the association of certain factors affecting "demand" with the T of their equation, to bring in the relation of these factors to individual choices. It remains true, nevertheless, that Mises's answer to Wieser is the clearest statement of the central point under discussion known to me.

just as effective, as a demand for "reserves of purchasing power," as is the "demand" for "reserves" against the special types of payment regarded by Wieser as the ones which are alone important for the determination of the "value of money." ". . . Those . . . who participate in exchange transactions," wrote Mises, "and consequently desire to acquire or dispose of money . . . value the monetary unit . . . also because they require money to pay taxes, to transfer borrowed capital and pay interest, and to make presents. They consider the level of their purchasing-power reserves with a view to the necessity of having money ready for all these purposes, and their judgment as to the extent of their requirements for money is what decides the demand for money with which they enter the market." [100] "No theory of the value of money with pretensions to completeness," therefore, "dare omit an explanation of the influence on the value of money exerted by processes" such as those omitted by the users of what amounts to an equation representing only a *part* of the demand for money; such a theory "cannot be compared," from the standpoint of adequacy, "with the point of view which opposes the total stock of money to the total demand for it (*i.e.*, to the total demand of economic agents for reserves)." [101]

The matter can be put in still other terms. No one would suggest that, in dealing with a commodity which is subject to a "composite" demand—in the Marshallian sense of the term—it would be safe to consider only one of the demands for the commodity in question, in relation to the price realized by the commodity in the particular use which underlies this one demand.[102] It would immediately be granted,

[100] Mises, *The Theory of Money and Credit*, 137.

[101] Cf. Mises, *The Theory of Money and Credit*, 424. Mises was here discussing Schumpeter rather than Wieser, who, it is true, had not drawn Schumpeter's distinction between "circulating" and "non-circulating" money which was discussed in an earlier chapter of this study (cf. above, pp. 459 ff.), and which was also selected for attack by Mises in his discussion of Schumpeter. So far as the argument for excluding transfers in the sphere of "capital" is concerned, however, the two writers were on a par, and Mises's argument applies against both.

[102] A still different way of putting the matter is represented by the comparison of the issues involved in this problem with those involved in a comparison between "partial equilibrium" and "general equilibrium analysis"—the concept of "composite demand," like that of "joint demand," being of

surely, that such a procedure makes a tacit assumption which will conform to the facts only by the merest accident: namely, that the other "demands" for the product in question do not change in magnitude. Nor is it difficult to find problems in monetary theory, other than that under discussion, in which this fact has been clearly recognized. No one would think, for example, of dealing with the "demand" for gold in terms of either the monetary demand or the arts demand taken alone. Even if we are interested solely in the question of the "adequacy" of the supply of gold for *monetary* purposes, it is necessary to deal with the forces affecting the nonmonetary demand for gold; for, obviously, any change in the arts demand must affect the amount of gold that is available for monetary purposes.

It is obvious, therefore, that an apparatus for dealing with the Theory of Prices which confined itself solely to the demand for cash balances held against expenditure on consumers' goods involves the tacit assumption—which there is no reason whatever for supposing will conform to the facts—that the demand for cash balances *for other purposes* will not change in magnitude relative to the demand for cash balances held against expenditure on consumers' goods.[103] This is a matter which, as we have seen, entered—somewhat blindly, to be sure—into Mr. Robertson's discussion of the possibility that the cash balances which were designated by Mr. Keynes as "income-deposits" might be either augmented or diminished by the flow of money out of and into cash balances of other types.[104]

It should also be evident, however, that the same reasoning must apply to the question whether there is any point in including, in a formulation designed to account for changes in the "price-level of output," a term covering transactions other than those in "output." This, indeed, is the crux of

course one of the devices used by Marshall, along with the device of holding the "marginal utility of money" constant, for keeping his conclusions consistent with those demanded by "general equilibrium" analysis. For a more specific application of the "partial equilibrium-general equilibrium" analogy to the point in question, see below, pp. 532 f.

[103] Cf. the comment, in similar terms, by Williams, on the implications of the expression $P = (M_1V_1)/O$, in "The Monetary Doctrines of J. M. Keynes," *loc. cit.*, 560, n. 9.

[104] Cf. above, pp. 401 ff.

the problem as to the advantages of a formulation which includes the total volume of "transactions" of all types, as compared with a formulation which would include only a term for "output." Surely there can be only one answer to this question. No quarrel can be raised with an equation of the form $M_oV_o=\Pi O$ on the grounds of accuracy, so far as the issues under discussion are concerned.[105] It is accurate enough; the question which concerns us here, however, is whether it is possible to develop, as part of the analysis "lying behind" this type of equation, an adequate account of the forces determining M_o, without taking cognizance of the fact that the amount of cash available for the alimentation of M_o will be determined in part by the amount demanded as cash balances to be held against transactions other than those in Output—that is, for those transactions which are represented by T_x. The essential point of the argument is that—in the words of one writer—the holders of the various types of cash balance "compete for purchasing power" with the holders of other types; or, to paraphrase Edgeworth, that each type of transaction against which cash balances are held "absorbs, or exercises a pull upon, the currency," and thus affects the amount that is available for use in other types of transaction.[106] If there should be a change in the magnitude of T_x, there must, unless there is to be a change in some part of the price structure, also be a change in M_o. By the terms of the equation $M_oV_o=\Pi O$, a change in M_o must, other things being equal, lead to a decline in Π. It is

[105] It must be remembered, on the other hand, that difficulties—of a different nature, to be sure—arise when by O is understood not "output *sold*" during the period under discussion, but "output" pure and simple—that is, "goods produced." Cf. above, p. 516, n. 89 and p. 517, n. 94, and the forward references there given.

[106] Cf. Edgeworth, *Papers*, I, 264; and for an example of the use of the idea of a "competition for purchasing power" in a context similar to the present one, see Roos, *Dynamic Economics*, 238. The discussion of the "competition for purchasing power" on p. 243 of the same work, unfortunately, runs, not in terms of a demand for *reserves* of purchasing power—that is, cash balances—but in terms of an allocation of differing proportions of the *stream* of purchasing power to various "competitive" uses. The "streams" themselves, however, are of course alimented by, as they themselves aliment, *stocks* of "purchasing power." The algebraic formulation presented by Roos on p. 238 (cf. above, p. 514, n. 82), is, moreover, perfectly reconcilable with an analysis running in terms of a "competition" for "reserves" of "purchasing power."

obvious, therefore, that any formulation designed to account for changes in the level of Π—the "price-level of output as a whole"—must be prepared to deal, either explicitly or implicitly, with the forces determining the magnitude of T_x. This, however, is merely another way of saying that an equation which includes not only a term for "output," but also a term for "transactions" other than those in "output," is, *even from the standpoint of one interested in the determination of the "price-level of output,"* a more nearly adequate formulation than is one which would concern itself solely with dealings in "Output." It follows also, as a matter of course, that there was no basis whatever for Mr. Keynes's contention that "not only does the price-level of output not occur explicitly in the old [that is, Fisher's] Quantity Equation, but it is not even a function of those variables which do occur in it." [107]

[107] See Keynes's "Rejoinder" to Robertson, *loc. cit.*, 420.

CHAPTER EIGHTEEN

The "Volume of Transactions" (*Continued*)

I

KEYNES'S *Treatise* AND "NON-OUTPUT" TRANSACTIONS

THE conclusion reached in the preceding chapter with respect to the bearing of nonoutput transactions (T_x) upon the price level of "output"—either "as a whole" or in any of its parts—may seem to be concerned with matters that are, after all, of minor importance. This is a view which I cannot share; but it is not necessary at this point to reiterate the central contentions thus far developed, or to anticipate the substance of this chapter, and of the one which follows, with a rehearsal of the arguments for the opposing view. For the point which must now be made is that, despite Mr. Keynes's repeated rejection of the "Fisher equation" precisely on the ground that, instead of being concerned solely with the volume of "output," it is concerned with the "volume of transactions," which Mr. Keynes regarded as of no importance for the central problems of monetary theory, it is precisely these "non-output transactions" which implicitly constituted the heart of the argument that he seems often to have regarded as the essential contribution of his *Treatise* to the Theory of Prices.

That this is so will become immediately obvious if we consider again the details of Keynes's reply to Robertson's comments on what the latter regarded as "the crucial defect" in Keynes's analysis.[1] This "crucial defect," according to Robertson, was that Keynes had failed to establish his contention that, despite the equation $\Pi O = PR + P'C'$, "if P [the price level of consumers' goods] declines . . . then, even though *there is no increase in the disposition to hoard*

[1] See Robertson, "Mr. Keynes' Theory of Money," *loc. cit.*, 400.

money unspent, there need be no counterbalancing rise in P', and there will therefore be a fall in Π, the price of output as a whole." [2] In particular, Robertson refused to regard as established the contention that either of the two factors adduced by Keynes in his argument on the point under discussion—namely, the "excess savings factor" and the "excess-bearish factor"—could "produce the result in question" even if it did not "resolve itself into an increased desire to 'hoard' "; and he argued that Keynes could never have reached "his paradox that P can fall, P' remain unchanged, and yet no new hoarding take place" if, in dealing, for example, with the forces determining P', he had adhered to "that rigorous Fisherine concept of a certain flow of money in a given time-interval meeting a certain flow of goods in the same time-interval." [3]

In his reply, Mr. Keynes accepted the challenge directly, by agreeing that the "central difference of opinion" between Robertson and himself, "off which most of the other fragments of contentious matter are splinters," did in fact turn on the validity of his contention "that if P, the price-level of consumption-goods, declines owing to an excess of saving over the cost of new investment, then there *need* be no counterbalancing rise in P', the price-level of investment goods, 'even though there is no increase in the disposition to hoard money unspent.' " [4] He went on to agree, moreover, that it was "absolutely fundamental" to his analysis with respect to the forcing down of the price level of "output as a whole," to "distinguish *two* factors at work," instead of laying stress on "hoarding" alone "as a dominant feature of trade depression." [5] This, he insisted, was precisely "the vital matter" in the dispute.[6]

To one who accepts the "Fisher equation" as true—and it will be remembered that Keynes himself, in his reply to Robertson, emphatically disclaimed any intention of casting doubt on the "truth" of the equation—it follows, as a matter

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[2] *Ibid.* (Italics Robertson's.)
[3] Robertson, "Mr. Keynes' Theory of Money," 401 f.
[4] See Keynes's "Rejoinder" to Robertson, *loc. cit.,* 412.
[5] *Ibid.,* 412, 423.
[6] *Ibid.,* 412.

of course, that the difference between the two disputants with respect to the forces determining the price level of "output as a whole" or of that part of "output" which is represented by "new investment goods"—variables which, despite Mr. Keynes's denial, were implicitly included in the T of the "Fisher equation"—must be capable of translation into the terms of the "Fisher equation." It was Mr. Robertson's contention that when the type of situation envisaged by Mr. Keynes was actually translated into the terms of the Fisher equation, the conclusions supported the Robertsonian contention that there could be no change in Π under the conditions laid down by Keynes, unless there is an "increase in the disposition to hoard money unspent." [7] Mr. Keynes, as we have seen, refused to accept the "Fisher equation" as relevant; so that on this issue—which was, indeed, the "vital" one in the dispute—there was no meeting of minds. Our problem, therefore, now that we have demonstrated the continuing relevance of the "Fisher equation" for the problem in hand, and have called attention to the crucial importance, for this problem, of "non-output transactions" (T_x), is to establish, in terms of the "Fisher equation," the precise nature of the dispute in question.

The nature of this dispute, whatever may have been said of it at the time it was being carried on, can hardly be obscure to anyone who will read again the passages in which Mr. Keynes undertook to describe the types of action in which "excess-bearishness," for example, would manifest itself.[8] "Excess bearishness" was associated with "an in-

[7] Robertson, "Mr. Keynes' Theory of Money," 402 f.

[8] The reason for selecting the "excess bearish" factor for discussion in the present context, rather than the "excess savings factor," is that the former was confined to a smaller range of issues than was the "excess savings factor" and therefore requires less digression into matters which are treated elsewhere in this study. By "excess savings," Mr. Keynes of course meant, at the time of writing the *Treatise,* an "excess" of savings *over "investment."* The latter type of excess was, in turn, associated not only with phenomena ordinarily summarized by the concept of an "excess of savings over investment," but also with a specific proposition regarding the relation between the value and the "cost" of output—that is, profits and losses. The argument presented in Chapter Five above was designed to show that the *Treatise* failed in its attempt to provide a set of equations which would perform simultaneously the twofold task of (1) explaining the determination of "prices" in terms of a "mutual impact of relevant flows" and (2) the *emergence of profits and losses.* The matter will be discussed again

creased preference for savings-deposits as against other forms of wealth." [9] There can be little doubt, despite superficial appearances to the contrary, that Mr. Keynes would have been prepared to admit that *one* result of this preference would probably be what, in more traditional terms, would have been described as a decline in the "velocity of circulation of money." [10] What he really protested against was the suggestion that an increase in "bearishness" need take *only* this form.

Our central problem, therefore, is to ascertain what form these phenomena would assume other than a decrease in the velocity of circulation of money—that is, "hoarding," in one of its most common senses.[11] Whatever may have been the case in 1930, there can no longer be any doubt as to the nature of Mr. Keynes's answer. ". . . The decision as to holding bank-deposits or securities," said Mr. Keynes, "relates not only to the current increment to the wealth of individuals, but also to the *whole block of their existing capital.* Indeed, since the current increment is but a trifling proportion of the *block of existing wealth,* it is but a minor element in the matter." [12] Involved, therefore, in the "decision as to holding bank deposits or securities," are not only decisions with respect to the proportion of cash receipts which will be added to the cash balances held against outlay—and which will therefore affect the "velocity of circulation" of these cash balances—but also decisions with respect to the *sale* of *"capital assets,"* in the form, say, of securities, the amount of which offered for sale may be expected to depend, among other things, upon the "price-level" of these assets.[13]

in Volume II; here it is necessary only to emphasize the desirability of leaving out of consideration, for the purpose of dealing with the problem in hand, those aspects of an "excess of saving over investment" which are aspects of the relation between costs and selling prices.

[9] Cf. the *Treatise,* I, 142.

[10] For examples of what might have seemed to be a refusal of Mr. Keynes to make any such admission, see his "Rejoinder" to Robertson, pp. 416, 423. This matter, however, will be discussed in more detail in Volume II.

[11] One of the difficulties in the interpretation of the argument of the *Treatise* was, of course, precisely that Mr. Keynes himself used the term "hoarding" in several different senses. See, for example, Keynes's own comments in the footnotes to I, 141, and I, 144, of the *Treatise.*

[12] *Treatise,* I, 141. (Italics mine.)

[13] See the *Treatise,* I, 142, and cf. Keynes's "Rejoinder" to Robertson, *loc. cit.,* 413. It should be added that Mr. Keynes himself must be held partly

The trouble with Robertson's argument, Keynes contended, was that it suggested that the whole volume of funds provided by savers, "in the absence of increased hoarding, will have to be directed to the purchase of the *newly produced* non-liquid assets already in the market, and that no other assets will come to the market whatever price may be offered." "This," said Mr. Keynes, "is to mistake entirely the nature of the capital market." For we must, if we are to do justice to the facts, take account of what happens to the price and the volume of market offers—the two are obviously interconnected—"of investment-goods, *old and new alike.*" [14]

What did this argument amount to, if not that it was through an increase in the volume of nonoutput transactions (T_x) that Mr. Keynes conceived of his "two money-streams"—that is, the stream of money directed against P and that directed against P'—as getting "out of step with one another"? This was, indeed, the suggestion that Mr. Robertson himself tentatively advanced.[15] In so arguing,

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responsible for the failure of his critics to assign a proper amount of weight to the effect of the "price-level" of these assets upon the amount of them sold. The reason for this is that his argument also envisaged the "sale of assets" by others—namely, entrepreneurs making losses—who, in view of the circumstance that we were asked to think of these entrepreneurial sales of assets as being virtually *forced* in order to "finance" or "make good" entrepreneurial losses (see, for example, the *Treatise,* I, 145), can hardly be thought of as adjusting their sales of assets with any degree of sensitiveness to the "price-level" of these assets, though the *purchasers* of these assets might be expected to adjust their purchases with reference to the "price-level" of the assets in question. Mr. Keynes's answer, as it turned out, seems to have been that we had to deal with *two* types of motivation for the "sale" (and the purchase) of assets—the controversy then turning on the question of time-sequence as between the two types of transaction. See, for example, Keynes's "Rejoinder" to Robertson, p. 417.

[14] See Keynes's "Rejoinder," 417 f. (italics in the original). Cf. also the *Treatise,* I, 145: ". . . The total value of the investment-goods (new and old) coming on to the market for purchase out of current savings is *always* exactly equal to the amount of such savings and is irrespective of the current output of *new* investment goods."

[15] See Robertson, "Mr. Keynes' Theory of Money," 403, and cf. what is said on this matter on pp. 537 f., below. Robertson's reason for hesitating to adopt an interpretation of Keynes's argument which was virtually equivalent to that suggested in the text—and which he characterized as "an entirely acceptable, if not very startling conclusion"—was that the "alleged inadequacy [of the old "quantity equations" to register the type of operation in question] extends also to periods of boom, when there is no counterpart to the increase of T through the forced sales of securities in the slump." It must be remembered, however, that an essential part of the

he obviously advanced a challenge which Mr. Keynes could hardly have been expected to avoid.

He did not avoid it; but his answer was such as to provide as clear a case as one could wish for the contentions advanced in the preceding chapter with respect to the relation of an equation of the "total transactions" type to those equations which include only terms for "output" or the various parts of that "output." Mr. Keynes simply did not "understand the relevance of the quantity equation"—of the Fisherine form—which Mr. Robertson had used in his argument. "We are discussing," Mr. Keynes insisted, in a passage which we have already had occasion to cite, "the relation between the prices of consumption-goods and of investment-goods—whether, asssuming no change in the propensity to hoard, the one must go down when the other goes up, like buckets in a well—which he [Robertson] affirms and I deny. But neither of these price-levels occurs in his equations, which are concerned with the price-level of output as a whole and the price-level of transactions." [16]

As we have seen, *both* of these "price-levels" which, according to Mr. Keynes, did not appear in Mr. Robertson's version of the "Fisher equation," are necessarily included in that equation by implication. We have, therefore, only to make these implications explicit. If we write

$$(PT) = P_r R + P'C + (PT)_x,$$

it becomes immediately obvious that the reason why, "assuming no change in the propensity to hoard"—which we may associate with a change in the V of our quantity equation—it is possible to have the "prices of consumption goods" go down without the "prices of investment goods" going up, "like buckets in a well," is simply that an increasing proportion of the stream of money is found to be directed against a third element which is included in an equation of the "total transactions" type: namely, T_x.

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argument of the *Treatise*—namely, that which had to do with the increased requirements of the "Financial Circulation" during times of boom (cf. below, pp. 598 f.)—likewise represented an increase in "non-output" transactions (T_x), even though these "non-output" transactions were of a different kind from those which were supposed to characterize the slump.

[16] Keynes, "Rejoinder," 419. Cf. above, p. 264.

Thus the great mystery of the *Treatise,* the matter which, by mutual agreement between Keynes and Robertson, represented "the central difference of opinion . . . off which most of the other fragments of contentious matter are splinters," becomes a matter of extreme simplicity when handled by a simple Fisherine equation of the "total transactions" type for which Mr. Keynes had so little use. It is extremely difficult to believe that if an adequate variant of the rejected form of equation had been used from first to last in the exposition of the *Treatise,* the difficulties which bulked so large in an evaluation of its argument could ever have arisen. The fact remains, in any case, that, in the one instance in the *Treatise* in which a formulation of the type in question was suggested, it was not applied to the problem in hand.[17] The result was confusion worse confounded—not only on the issue which was regarded as involving "the central difference of opinion," but also on the subsidiary issues which Mr. Keynes regarded as "splinters" off this central issue.

It is easy to demonstrate, indeed, that "the other fragments of contentious matter" involved in the dispute between Robertson and Keynes were in fact "splinters" off the central point at issue, when the latter is stated in terms of the relation of the volume of non-output transactions to the price level of output and of its various subdivisions. Consider, for example, Mr. Robertson's objection to Keynes's statement that "the price-level of consumption goods is entirely independent of

[17] The formulation in question appears on II, 83 of the *Treatise,* where the "volume of bank clearings" (the MV of an equation of the Fisherine type) was equated to $Q_1R_1 + Q_2R_2$, in which R_1 represented "the volume of Wages and current production of goods (finished and unfinished) traded"; R_2 the "volume of Bonds, Shares, Real Estate and other financial obligations changing hands"—"each weighted in proportion to its cash-using importance"; and Q_1 and Q_2 represented the "price-levels of each of these, weighted on the same system." It will thus be seen that (Q_2R_2) is of the same general nature as the $(PT)_x$ of the formulation suggested above. Instead, however, of using this formulation for the handling of the theoretical issues raised in Volume I of the *Treatise,* Mr. Keynes used it principally in connection with the question of the interpretation of the available statistics of bank clearings. On the last two pages of the chapter in question (II, 90 f.), moreover, Mr. Keynes showed how little he was aware of the bearing of the formulation presented on p. 83 on the fitness of the older quantity equations for obtaining "certain useful results" of the kind under discussion here, by advancing an argument which, as Mr. Robertson was quick to point out (cf. above, p. 68, and n. 80 thereto), amounted to a retraction of his disclaimer of any intention to charge the older equations with leading to "*wrong* results."

the price-level of investment-goods."[18] This was, indeed, an astonishing statement, even if one went no further than Keynes's own equation $\Pi O = PR + P'C$; for it would seem obvious that, so long as this equation holds true, it is impossible, *in all cases in which* ΠO *remains constant,* for $P'C$ to change without PR changing. In all these cases, surely, it is impossible to argue that "the price-level of consumption goods is entirely independent of the price-level of investment goods." If Mr. Keynes meant anything by the latter statement, therefore, he must have meant that it is possible to conceive of cases in which changes in one price level may occur without a change in the other price level. That this might happen whenever ΠO itself changes is, of course, self-evident; and it is impossible to believe that Mr. Robertson or any other among Keynes's critics would have had any trouble on this score.

If there was trouble, it was because there was not a clear understanding between the two disputants as to the *nature of the forces which might be expected to bring about a change in* ΠO. Specifically, the "central difference of opinion" on this head turned upon whether, assuming no change in the dimensions of the total money stream intended for purchases of all kinds, such as might come about from a change in "hoarding"—and assuming also no initial change in the "volume of output" (O)—ΠO could change, and therefore make it possible to have a change in PT without having a change in $P'C$. The answer, on the basis of the analysis presented above, is simplicity itself: with no change in either the dimensions of the money stream or the volume of output, Π might change simply as the result of a change in T_x, which would change the dimensions of the stream of money going against ΠO, even though there is no necessary change in the dimensions of the total money stream intended for purchases of all kinds.

We are, in short, dealing with a case which is in many respects analogous to the case of "partial equilibrium" analysis versus what Mr. Keynes himself called "multiple equilibrium" analysis, but which, in the opinion of some of his critics, he had not borne sufficiently in mind in the course of his argument.[19] The central point in debate between the

[18] See Robertson, "Mr. Keynes' Theory of Money," 398, and the reference to the *Treatise* there given. The other passages from the *Treatise* (I, 143, 152) which Robertson cited by way of support for the suggestion that "we need not take the statement on p. 136 too much to heart" unfortunately did not provide any true solace for those who wished to believe that Mr. Keynes could not have argued for an "independence" of P and P' on the grounds which are here under discussion, since in neither case was anything said concerning the possibility of a change in P and P' in the same direction *in the absence of hoarding or dishoarding, or a change in the volume of money or output.*

[19] For Keynes's reference to "multiple equilibrium," see the *Treatise,* I, 143, where the reader was warned against "forgetting that we are dealing with a case of multiple equilibrium in which each element affects every other element more or less." For a criticism of the general argument of the *Treatise* on the ground that it did not bear in mind sufficiently the teachings of the "general theory of equilibrium," see G. Del Vecchio, in the *Giornale degli economisti,* LXXII (1932), 608, (reprinted in the same

partisans of "partial equilibrium" analysis and partisans of "general" or "multiple" equilibrium analysis is familiar *ad nauseam*. All "partial equilibrium" analysis involves, implicitly or explicitly, the assumption of a constancy in magnitudes which, in "general" equilibrium analysis, are given an explicit place and are allowed to vary freely; whether this fact vitiates the results obtained by means of "partial equilibrium" analysis turns entirely upon whether it can be shown that, at a crucial point in the argument, magnitudes which were held constant in the "partial equilibrium" analysis can be shown in fact to vary.

In the case under discussion, the magnitude which was implicitly held constant in an equation of the type $MV = \Pi O$, in which MV represents the stream of money expenditure of all types, is the factor T_x, which is in fact only a special case of the $k_2 = T_{mv}/T$ of the generalized formulation, presented in Chapter Three of this study, of the reasons for an alleged asymmetry as between the two members of a given "quantity equation."[20] It is a commonplace, in discussions of the relative merits of "partial" and "general" equilibrium analysis, that the latter is always the safer, even though the former has in many cases shown itself more fruitful from the heuristic standpoint; and it is equally a commonplace, for all except the invincible die-hards of either "school," that there is in this fact an argument for using both types of analysis if we wish both to advance and to consolidate our positions as we advance. Translated into the terms provided by the controversy under discussion, what this amounts to is that all results obtained with respect to the "dependence" or "independence" of the price levels involved in Mr. Keynes's set of "plural" price levels are bound to be uncertain in the extreme unless, at some point in the argument, a formulation is presented which is analo-

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author's *Progressi della teoria economica* [1936], 330). Del Vecchio's criticism, on this occasion, was applied, not to the point under discussion, but to Keynes's "explanation of crises, which points to one among the many disequilibria which constitute the crisis [that is, to the disequilibrium between "savings" and "investment"] as the cause of all the others." See, however, the earlier (1925) remarks by Del Vecchio, in his "Un capitolo di teoria monetaria" (*loc. cit.*), where, having desiderated a "series of equations of exchange" (cf. above, p. 505, n. 57), he went on to contend that "the true economic problem arises in determining how the relation between the prices [involved in the separate "equations of exchange"] is established, in view of the fact that these prices, so far from being independent elements, are rigorously determined with respect to each other by the general conditions of economic equilibrium and by the conditions of supply affecting the various instruments of circulation." The reader who is interested in curious adumbrations of what has come by now to be generally accepted in economic analysis would do well to consult Samuel Gale, *An Essay on the Nature and Principles of Public Credit*, London, 1784, 146 ff., where, although of course the analysis was not applied directly to the problem under discussion, a description is given of the process by which "a *par* or *equilibrium* in the comparative value of money," in its various "channels," is "preserved." The passage is particularly interesting because of its use of an analogy strongly suggestive of Professor Irving Fisher's celebrated mechanical contrivance for the representation of general economic equilibrium.

[20] Cf. above, pp. 50 ff. and 70 ff.

gous to that provided by "general" equilibrium analysis in that it includes all the factors which might possibly affect the particular variables selected for special study.[21] The contention here advanced is simply that a "total transactions" equation, while it certainly includes, explicitly or implicitly, a term for the "volume of output," is also more "general," by virtue of the fact that it includes more than this in its second member, and is, by that very fact, a safer instrument for purposes of the general Theory of Prices.

A second "fragment of contentious matter" which was involved in the dispute between Keynes and Robertson, and which can likewise be shown to be a "splinter" off the "central difference of opinion" between the two writers as that "central difference" is now defined, even though it remained in the background throughout, has to do with the issues discussed in Chapter Fifteen above, in connection with the concept of "real balances" and the "demand for money." [22] It will be recalled that our argument, in connection with the concept of "real balances," was not that—apart from the controverted question of "realism"—it was a wrong concept in itself. Our argument was simply that the concept of "real balances," especially in the form in which it appeared in the writings of certain monetary theorists, including Keynes himself, was particularly likely to lead to erroneous conclusions unless a determined effort was made to disentangle the various components of "real balances," and unless, in particular, a sharp distinction was drawn between the concept of a "relative" demand for cash balances, and the "absolute" demand for such balances. Concretely, it was argued that it is of extreme importance to keep separate the factors determining the size of cash balances held *relative to outlay*, on the one hand, and those determining the absolute size of cash balances held against such outlay, on the other.

It is not difficult to demonstrate the relevance of these contentions for the problem in hand. The "central difference of opinion" between Keynes and Robertson, as we have seen, turned upon whether certain results could be obtained in the absence of "hoarding." Now, for those who are *not* in the habit of thinking in terms of "real balances," "hoarding" can mean only one thing: namely, an increase in the *relative* demand for cash balances—or, alternatively, an increase in the size of cash balances held relatively to outlay, which is the same thing as a decline

[21] It is, of course, a not uncommon objection to this procedure that the factors which "might possibly" operate in the real world are so numerous and complicated as to make their inclusion impossible as a practical matter. To this, however, it is to be retorted (1) that if it is possible, "as a practical matter," to enumerate and classify the factors the omission of which invalidates a result obtained in abstraction from them, it is possible to include these factors in our final formulation; and (2) that, "as a practical matter," it will usually be found that the factors which are likely to have an appreciable influence in *any concrete case* are comparatively few in number. On the last point, cf. my remarks in the *Journal of Political Economy*, XXXVII (1929), 323 f., 331 f., 338 f.

[22] See above, pp. 436 ff. and 444 ff.

in the velocity of circulation of money.[23] To those accustomed to thinking in terms of "real balances," on the other hand, a decline in the velocity of circulation of money is to be thought of as a factor increasing the "size" of "real balances." This is correct as far as it goes; what is incorrect is the further suggestion that a decrease in the velocity of circulation of money, or an increase in "hoarding," is the only thing, apart from an increase in the volume of output, which can affect the "size" of "real balances." On the contrary, "real balances" may be affected also by an increase in what we have called T_x. To identify an increase in the "size" of "real balances" with "hoarding" is not incorrect if "hoarding" is not identified, in turn, with a decrease in the velocity of circulation of money—or, in Mr. Keynes's terms, with a change in the relative volume of "inactive" deposits.[24] With the use of a cash-balance equation which differs from an equation of the "Fisherine" type only in the substitution of the term $1/K$ for V, the answer to the question involved in the controversy under discussion becomes crystal clear: the result indicated may be obtained, not only by an increase in K (a decline in V), but also by an increase in T, as a result of the increase in the T_x which that T includes. Alternatively, we may say that without any change in the "relative" demand for cash balances, we may have an increase in the "absolute" demand for cash balances, represented by the increased quantity of cash balances which, although their "velocity of circulation," or the relative volume of "inactive" deposits, may remain as before, are required to be held against the increased volume of "transactions" represented by the increase in T_x, unless the price level of output is to fall. It is, as we have seen, precisely the vice of "real balance" equations of the form $n=pk$, that the k fails to distinguish sharply between the factors affecting the "relative" demand for cash balances from

[23] The rôle played by "hoarding" in effecting a discrepancy between "Saving" and "Investment" will be discussed in some detail in Volume II of this study.

[24] See Keynes's "Rejoinder" to Robertson, *loc. cit.*, 416. Unfortunately, the bearing of an increase in the relative volume of "inactive deposits" upon a change in "velocity" and an increase in "hoarding" was more than a little obscured by at least three features of Mr. Keynes's exposition. The first of these was his suggestion in the *Treatise*, which we have already discussed, to the effect that a change in the relative volume of the various types of "deposit," each with a different velocity, was not to be regarded as a change in "true" velocities. Cf., however, what is said on this matter on p. 394, above. The second troublesome element was his insistence that "inactive" deposits had a velocity of circulation of "zero," and that the velocity factor ought to apply only to the volume of "active" deposits. Again, however, cf. what is said on pp. 459 ff., above. The third was his statement that "the forces determining the quantity of hoards" were an "affair of the bankers" and were quite different from "the forces determining the propensity to hoard," which were "the affair of the public" (see, e.g., Keynes's "Rejoinder," 419). This matter has already been discussed in connection with Keynes's distinction, in the *Treatise*, between the forces determining the "volume of cash balances," on the one hand, and the "volume of real balances," on the other. Cf. above, pp. 437 ff. and see also below, p. 536, n. 26.

those affecting the "absolute" demand—to say nothing of its failure to distinguish, among the factors affecting the "absolute" demand, between those which affect the volume of output and those which affect the volume of non-output "transactions."[25] The only comment to be made, therefore, on Mr. Keynes's insistence that what, in so far as it is connected with the demand for cash balances at all, amounts to the distinction between the "absolute" demand for cash balances and the "relative" demand for such balances, had not been "revealed" to him by "the old quantity equations," is that he was particularly unfortunate in having confined his own attention to precisely that one, among "the old quantity equations," which was indeed unlikely to "reveal" the elements that really matter for the problem in hand.[26]

With this analysis in mind, we may return to the argument of Robertson. The "state of affairs" described by Keynes, he insisted, "cannot come about except as the result of an act of 'hoarding,' *i.e.*, of holding back unspent part of the stream of money which is normally spent *on the part of some one*."[27] It is, however, of very great importance to observe what Mr. Robertson, at this stage, regarded as "hoarding."[28] He not only admitted the possibility that a "hitch-up of the money stream" may come about as the result of an increased holding of cash by "dealers in securities," but insisted that it was "of the utmost importance" to recognize that such a thing "frequently" happens.[29] The

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[25] Cf. above, pp. 444 ff.

[26] For Keynes's reference to the inadequacy of "the old quantity equations" for dealing with the problem in hand, see his "Rejoinder" to Robertson, 419. In actual fact, of course, it would be difficult to find a better argument for the "old quantity equations" than that which is suggested by Mr. Keynes's summary of the problem of "hoarding" in paragraph 12 of his "Rejoinder" to Robertson (p. 423). This paragraph, surely, can remain obscure only so long as the phenomena there summarized are not translated into the variables of the "old quantity equations," and a sharp distinction is not drawn between the absolute volume of cash balances, on the one hand, and their volume relative to outlay on the other—a distinction which is in turn associated with the distinction between the "absolute" and the "relative" demand for cash balances. The translation into the terms of "the old quantity equations" is itself, apart from the further ambiguities introduced by the omnibus nature of the concept of "an excess of saving over investment" (which will concern us in Volume II of this study), extremely simple. The *absolute* volume of "money," including the volume of "money" held as "hoards," is of course affected by the actions of the "bankers." To say this, however, is to indicate merely that this part of the problem is associated with the M' of our Quantity Equation. *How much* of a given absolute volume of cash-balances will be *treated as "hoards" by the owners of the balances*, on the other hand, is a matter which concerns the size of cash balances relative to outlay, and is therefore associated principally with the V of our equation—T being affected only in so far as the process of adjustment of cash balances involves some sale of assets (cf. above, pp. 455 f.).

[27] "Mr. Keynes' Theory of Money," *loc. cit.*, 400.

[28] Robertson's later position, which identified "hoarding" specifically with a change in "velocity," will be touched upon in Volume II.

[29] Robertson, "Mr. Keynes' Theory of Money," 401.

difficulty with his exposition, however, was that, in arguing that the "one thing" that can make such a "hitch-up" occur was "an increased desire on the part of somebody to 'hoard,' that is, to keep resources idle in the form of bank-deposits," he did not make it clear that the keeping of resources "idle" might take the form not only of increased balances held relatively to outlay—Mr. Keynes's increase in the relative volume of "inactive deposits"—but also of increased balances which could be said to be "idle" only in the remote sense that they were allocated to some use other than that performed by balances held against transactions in current output.

It would not be fair to Mr. Robertson to suggest that he was not aware of the fact that involved somehow in the controversy was an "increase in the stream of transactions." On the contrary, as we have pointed out, he called attention explicitly to just this fact.[30] The most that can be said against his argument concerns the matter of exposition, which again brings us back to issues that were discussed in an earlier chapter. "Does the alleged inadequacy of the quantity concept [read: quantity equations] to reveal the truth," asked Mr. Robertson, "amount only to this—that we must be careful not to expect the price-level of *one set of things* to vary with the velocity of circulation of money against *another* set of things?"[31] To those familiar with the type of concept represented by "income-velocity," the meaning of Mr. Robertson's question is sufficiently clear. "Income velocity," in one of its meanings, is represented by the ratio of expenditure on "real income" (or "output") to the stock of cash balances of all kinds.[32] This ratio, however, as we have seen, will be affected not only by a change in "velocity," in the more conventional sense of the term, but also by an increase in T. The reason for this, of course, is that such an increase will, by requiring the devotion of an increased absolute volume of cash balances to the work of financing non-income transactions, change the ratio of the total of cash balances of all kinds to "income"—that is, will change the extent to which a given volume of cash balances will support a given level of money income. To say, therefore, that there is a change in "income velocity" *for the reason here specified* is, indeed, the same thing as saying that there is a "hitch-up" of funds that might otherwise have been spent upon "output"; but this "hitch-up" is not due to the keeping of "resources idle in the form of bank-deposits" in any sense of the proposition which would relate it to a change in "velocity" in its ordinary meaning—that is, in the sense of a change in the size of cash balances held relatively to outlay. In short, we have here a further example of the dangers of misunderstanding which may be said to be inherent in the use of the concept of "income-velocity," as well as in the use of the concept of "real balances." In both cases, there is every reason for holding that a statement of the argument which

[30] Cf. above, p. 529, and especially note 15 thereto.
[31] "Mr. Keynes' Theory of Money," 403.
[32] Cf. above, p. 380, and also n. 33 to p. 360, above.

would have avoided both the "hybrid" conception of "income-velocity" and the equally "hybrid" conception of "real balances," would have made the issues clearer than they were actually made. This, however, is merely another way of saying that much would have been gained if Mr. Robertson—whose recognition of the fact that "an increase in the stream of transactions" was involved in the dispute must certainly be regarded as a virtual, if not sharply articulated, solution of the problem in debate—had carried on his argument throughout in terms of concepts strictly appropriate to the implications of a "total transactions" equation of the Fisherine type, which he, unlike Mr. Keynes, had recognized as providing a final court of appeal for the issue in hand.

II

Output versus Transactions in Output

Recognition of the fact that, in any adequate formulation of the Theory of Prices, a specific place must be given to what we have loosely characterized as "non-output" transactions is, then, the first step in any evaluation of the body of doctrine which may be said to "lie behind" the T of our quantity equation which does not content itself with regarding T merely as a vague and unsatisfactory symbol for the volume of "output." Unless, however, our T is to be something more than the cesspool of statistical computation that it has often been, such recognition is only the first step. For it is clearly the task of an adequate Theory of Prices to go behind what has been loosely designated as the volume of "non-output" transactions, and to state with some precision the nature of the components that make up this volume of "non-output" transactions. Alternatively, we may say that our task is to describe with accuracy the nature of the forces determining the magnitude which Mr. Keynes himself designated, though with virtually no supporting analysis, as the ratio T/O, in which T is the "total volume of transactions," as it appears in the "Fisher equation," and O is the "volume of output." [33]

[33] The expression T/O appears, as far as I am aware, only once in the *Treatise*—namely, in the brief section (I, 239 f.) entitled "The Relationship between the 'Fisher' Equation and the Fundamental Equations of Chapter 10." It is noteworthy, moreover, that Keynes did not at this point comment on the significance of the expression T/O, nor indeed upon any other aspect of his curious formula expressing the "relationship" in question, beyond expressing doubt as to whether the whole expression was

This statement of the problem is, in fact, more inclusive than its statement in terms which would suggest that the only issues involved are those which arise when we are concerned with the forces affecting the magnitude of "non-output" transactions. That the latter statement of the problem represents, indeed, a gross oversimplification of the difficulties involved becomes apparent as soon as it is recognized, for example, that the "volume of output" is by no means necessarily the same thing as the volume of *transactions in output*. We may, therefore, begin with an examination of the issues involved in any attempt to establish the precise nature of the relation between the latter two magnitudes.

It is of the greatest importance to recognize, in the first place, that, despite implications to the contrary which are found very commonly in current usage, it is anything but clear that justice is done to the problems raised by the inclusion of a term for "output" in a formulation designed to summarize the forces affecting general prices, if such a practice is accompanied by the suggestion that the *whole* of "output" is to be included without correction in the "volume of transactions"—or, as Mr. Keynes himself put it, the "volume of articles traded." Recognition of this fact is at least as old as Hume; and the point has been made by subsequent writers at intervals from his day to our own.[34]

"worth much" (cf. above, p. 14). Further indication of the lack of any genuine interest on the part of Mr. Keynes in establishing the nature of the economic forces affecting the magnitude of the expression T/O, may be found in the fact that, at a later point in the *Treatise* (II, 82), he spoke of "an index of the volume of trade or output" as if "trade" were the same thing as "output," though a few pages later (II, 87), he expressed mild approval of Carl Snyder's reasons for preferring "an Index of the Volume of Trade . . . to indexes based on statistics of production and output." (On the inadequacy of the treatment by Snyder and others of the distinction in question, cf. my article "The Statistical Measurement of the 'Velocity of Circulation of Goods,' " *Quarterly Journal of Economics*, XLVII [1932], 15 ff.) The one point involved in the relation between the T of the "Fisher equation" and "output" in which Keynes was genuinely interested was represented by his distinction between the volume of "transactions" and the volume of "trade," the former including "financial and Stock Exchange transactions" as well as "the volume of *trade* transactions arising out of current production and consumption" (*Treatise*, II, 82, 84; italics mine). Cf. also p. 531, n. 17, above.

[34] See, for example, the references given in my article "The Definition of the Concept of a 'Velocity of Circulation of Goods,' " I, *Economica*, Novem-

The central argument in support of the point itself, moreover, is so simple as to be virtually self-evident to all those who would accept the proposition of Professor Pigou that a formulation such as the Fisherine "equation of exchange" is, in a fundamental sense, merely a formulation designed to summarize the forces affecting the "demand" for money.[35] The "demand for money" with which we are here concerned is obviously what we have called the "absolute" demand for cash balances to be held against transactions in output. It is clear, however, that a given "volume of output" may give rise to a greatly differing volume of "transactions" in output, in the sense of a given volume of sales of output for money, and therefore to a greatly differing "demand" for money.[36]

A first set of problems is therefore provided by the fact that, of a given "volume of output," the proportion of this "output" which is *intended, or offered, for sale* during a given period may differ greatly under different sets of condition.[37] Briefly summarized, the principal factors which affect the ratio of output, or "goods produced," to the volume of *goods produced for sale* in the period under examination, are as follows:[38]

1. The greater or smaller proportion of "goods produced" (or "output"), which are intended, not for sale, but for direct consumption, in the literal sense of the term, by the producer, as in the case of agriculture.

2. The greater or smaller proportion of "goods produced" (or "output"), which are disposed of by barter in-

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ber, 1932, 450 ff. In the pages which follow, I have drawn freely on this article and the companion article in *Economica* for August, 1933, as well as on other articles that I have published on related matters.

[35] Cf. Pigou, *Essays in Applied Economics*, 177.

[36] It will be seen that the discussion which follows bears directly on issues upon which we have already found it necessary to touch briefly—for example, the suggestion that no great amount of importance attaches to the question whether we regard "money income" as income actually *received in the form of money*, or as income "measured" in money. Cf. what is said on this matter on pp. 377 ff., above, and note 71 thereto.

[37] On the relative advantages of the terms "intended for sale" and "offered for sale"—the two are used here in an identical sense—see my article "The Definition, etc.," *loc. cit.*, 440, n. 20.

[38] For the supporting argument, as well as for a treatment of the literature on the point in question, see "The Definition, etc.," I, *loc. cit.*, 450 ff.

stead of being sold for money—a factor which has been of great importance over longer periods which have witnessed the expansion of the "money economy," and has taken on renewed importance, in our own day, in the more advanced stages of extreme paper-money inflation.

3. The greater or smaller proportion of "output" represented by production which takes place within the framework of an integrated business unit, as compared with production which takes place in a series of separately organized business units.[39]

4. In any Theory of Prices which does not confine itself, as does that which is presented in this study, to a "closed system," but deals with the complications introduced by international trade, account must be taken of changes in the proportion of total "output" which is intended for sale within the borders of the particular country subjected to investigation.

This matter of distinguishing between "goods produced" (or "output"), and "goods produced for sale" may seem to some a trivial matter. It has not seemed so, however, to writers from Hume to Wicksell. The latter, indeed, insisted that "a satisfactory solution" of the problem of constructing index numbers for the measurement of the purchasing power of money "is possible only if regard is paid to the quantities of goods actually exchanged," and that "if this is not done the whole question of average prices becomes vague and uncertain, and the method ordinarily employed may under certain circumstances lead to contradictory results." [40] Nor would it be difficult to cite examples from writers of the highest standing in our own day who have been led to conclusions with respect to the forces determining the relation of "goods bought" to "goods produced" which can be explained only through a failure to consider the type of distinction here in question, as well as

[39] It has been very common to regard this type of factor as affecting directly, not the ratio with which we are here concerned—namely, the ratio of the volume of "goods intended, or offered, for sale" to the volume of goods produced—but rather the "velocity of circulation of goods." On the objections to this practice, see "The Definition, etc.," I, 451 ff.

[40] Wicksell, *Interest and Prices*, 7. For references to Hume and others, see "The Definition, etc.," I, 451 ff.

others with which we shall deal presently.[41] Indeed, it is not necessary to go beyond Keynes's *Treatise* itself for an example of the confusion and even definite error to which a failure to draw a sharp distinction of the kind here indicated may lead.

The degree of confusion which was introduced into the argument of the *Treatise* by a failure to grapple directly with the distinction in question will be sufficiently indicated by the inadequacy of those distinctions employed in the *Treatise* which may be regarded as even remotely related to the problem in hand. It is, for example, obvious that no adequate coefficient of correction for the term for Output in the Fundamental Equations is provided, for the purpose in hand, by the distinction between "available" and "non-available" output. The distinction between "available" and "non-available" output turned, not upon whether the goods involved were offered for *sale during the period under examination*, but solely upon whether these goods were or were not "in a form available for immediate consumption."[42] Of the "non-available output," for example, as thus defined, the proportions which would be intended for sale during the period in question might vary greatly.[43] Still less help, for the purpose in hand, was provided by the further classification of goods into "Finished Goods" and "Unfinished Goods."[44] Among the Finished Goods, for example, were included the "Instrumental Goods" included in "Fixed Capital," a large part of

[41] See, for example, the comment by Hawtrey, in the *Weltwirtschaftliches Archiv*, XXVIII, 100**: "Goods bought can only differ from goods produced by the amount by which *stocks of goods* vary." It may be noted that this proposition is invalidated, in the first place, by the distinction between "goods produced" and "goods produced for sale"—unless we understand by "stocks" something much more inclusive than what is ordinarily meant by the term. It is invalidated also by the fact that since, in a given period, the same goods may be "bought" and sold several times after having been "produced," the volume of goods "bought" may be much greater than the volume of goods "produced." This matter is dealt with on pp. 554 ff., below, in connection with the concept of "the number of middlemen's sales." Mr. Hawtrey's proposition is invalidated, in the third place, by the fact that "goods sold" may include "old goods" as well as goods currently produced. See, on this matter, pp. 545 f., below. The list of quotations involving a similar type of over-hasty generalization would be a very long one.

[42] See the *Treatise*, I, 127.

[43] Cf. the comment by A. G. Hart, "An Examination of Mr. Keynes's Price-Level Concepts," *loc. cit.*, 637. It should be added that the difficulties which would have been involved in any case, in an attempt to relate "output" to "goods sold," were aggravated by Keynes's identification of one part of "output"—namely, C, or "new investment goods"—with the "net increment of investment," with the result that nonavailable output was regarded as being capable of having a "negative" value (cf. the *Treatise*, I, 127)—a fact which had curious consequences for the interpretation of P', the "price-level" of C. Cf., on this matter, p. 508, above.

[44] Cf. the *Treatise*, I, 129.

which, by virtue of the very fact that "Fixed Capital" was thought of as comprising "goods in use," is not likely to be offered for sale in any given period taken for examination.[45] There is nothing, on the other hand, in the category of "Unfinished Goods" which would prevent their being offered for sale during the period in question.[46] In any case, neither category was related in any articulate way to the Output of the Fundamental Equations, which, if it is to be set against "the flow of money available for purchases," must be corrected by a coefficient measuring the varying proportion of "Output" which is offered for sale during the period in question. Nor, finally, can much be said for the distinction which Mr. Keynes adduced in a passage in his "Reply to Dr. Hayek" in which he confessed frankly to the "verbal confusion" of which Hayek had complained in the course of the latter's argument with respect to the relation of output to the "value of existing capital": namely, the distinction between "production" and "output."[47] This was a distinction which certainly had not appeared articulately in the *Treatise,* though it was used subsequently by commentators thereon.[48] Even, moreover, if one could accept the distinction between "production" and "output" as being concerned with those goods which actually "come on the market" ("output") as opposed to those goods on which "work" is being done during the period under discussion ("production")—and it is extremely difficult to apply the distinction to the usage of the *Treatise,* particularly so far as the "output" of "investment goods" is concerned—we should still be left in confusion as to the relation between the goods which "come on the market" and those which are actually "purchased" during the period in question.[49] The only frank procedure to have followed, surely, would have been to admit openly that the issues involved in the distinction between "output," or "goods produced," and goods produced *for sale during the period under investigation* were completely absent from Mr. Keynes's mind at the time the *Treatise* was written.

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[45] On "Finished Goods" as including "Instrumental Goods," and on "Fixed Capital" as equivalent to "goods in use," see the *Treatise,* I, 128, 129.

[46] See, for example, the *Treatise,* II, 83, where Keynes spoke of "unfinished" as well as "finished" goods being "traded."

[47] See Keynes's "Reply to Dr. Hayek," *loc. cit.,* 13.

[48] See for example, Hart, "An Examination," *loc. cit.,* 627, 630, 632, n. 15.

[49] It is not clear, from the passage on I, 135 of the *Treatise,* whether Keynes, in defining R as the "volume of liquid Consumption-goods and Services flowing on to the market and purchased by consumers," meant to imply that "flowing on to the market" and "purchased by consumers" are or are not identical. There is, of course, a vital difference between the two if, by "goods flowing on the market" is meant what has been called "goods offered, or intended for sale" (cf. above, p. 540, and n. 37 thereto) —the difference being precisely that with which the concept of a "velocity of circulation of goods" was designed to deal (cf. "The Definition, etc.," I, *loc. cit.,* 442 ff.). Still unresolved, moreover, was the contradiction involved in defining R as the volume of goods "purchased by consumers" (that is, "goods sold"), and, on the very same page, identifying R as part of the volume of *output,* or "goods produced," as in the equation $O = R + C$.

It is of the first importance to recognize, moreover, that these issues, which might have been insignificant in the case of other writers, happened to be of the very greatest importance for the argument of the *Treatise;* for nothing less was at stake than the *formal validity, under all conditions,* of the Fundamental Equations of the *Treatise* itself. It is obvious, for example, that the use of the term Output in the way Keynes used it in his Fundamental Equations permits the use of these equations as the representation of "a mutual impact of the relevant flow of money and the relevant flow of goods," and therefore as a substitute for the older "quantity equations" which they were alleged to supersede, *only on the condition that there is no change in the proportion of output that is intended for sale during the period in question.*[50] This, however, is by no means all. As we saw in Chapter Five of this study, it is absolutely essential, if the Fundamental Equations are to perform the twofold task their fitness for which was regarded as their greatest claim to novelty—that is, the task of representing "a mutual impact of relevant flows" simultaneously with the representation of the relation between costs and selling prices—that there should be no change in the proportion between "goods produced" and "goods sold" during the period under discussion.[51] One of the factors which may cause a change in this proportion, obviously, is the relation between goods produced and goods produced for sale during the period under examination.[52] It is, indeed, impossible to believe that if Mr. Keynes had wrestled with the problems to which a change in this relation may give rise, he would ever have advanced the "Fundamental Equations" of the *Treatise.* Once again, therefore, the "volume of transactions" of the "Fisher equation"—as opposed to the "volume of output," to which it is related, but with which it is by no means identical—comes into its own as an element to which we must hold fast if we are to avoid errors that are at once among the simplest and the most far-reaching of those which have appeared in the Theory of Prices.

III

Goods Produced versus Goods Intended for Sale

The argument advanced in the preceding section of this chapter may be summarized as amounting to the contention

[50] The only alternative to making such an assumption is, of course, to regard any change in the proportion in question as being somehow registered in the term $(I—S)$, which would thus have become even more of a catch-all than it was intended to be. It should be noted, moreover, that even on this interpretation of the function of the term $(I—S)$, the failure to distinguish between "goods produced" and "goods sold" was still disastrous for the claim that the equations of the *Treatise* were capable of performing the twofold function indicated above.

[51] See especially pp. 133 f., above.

[52] It is of some importance to stress the point that the factor in question is *only* one of those involved in the relation between "goods produced" and "goods sold." See sections iii and iv of this chapter.

that a term for "output," or "goods produced," fails to register accurately the movements in the volume of "goods offered for sale" by virtue of being *too inclusive* in character, since it includes not only goods produced for *sale* during the period under discussion, but also goods which are not intended for sale during that period. We now come to a second reason for insisting that any term for "output" which is included in a formulation supposed to represent an "impact of relevant flows" must be corrected by a coefficient designed to register the relation between "goods produced" and "goods intended for sale": namely, that the magnitude "goods *produced* for sale" is not inclusive enough.

Of the reasons for so arguing, one may be passed over lightly here: namely, the fact that, if our analysis goes beyond the framework of a "closed system" and attempts to deal with the complications introduced by international trade, we must *add,* to the volume of "goods produced for sale" within a country's own borders, the volume of *imports* intended for sale within that country.[53] Nor is it necessary to pause long over a second source of discrepancy between the volume of goods "produced for sale" within a given time period and the volume of goods "intended for sale" during that period: namely, that the volume of goods "produced for sale" must be supplemented, in order to arrive at the volume of goods "intended for sale" in a given period, by the volume of what has been called the "carryover"—that is, the volume of stocks of commodities produced for sale within a given period, but not removed from the market in that particular year.[54]

The element, on the other hand, to which, in the light of the argument of the *Treatise,* especial attention must be called in this connection, is the volume of what might be called "old goods"—that is, goods *owned* for purposes of use, which are the result of the "production" or "output" of past periods, but which are now brought forward for sale, and are thus to be added to "goods produced for sale" in order to arrive at a measure of "goods intended for sale."[55] The

[53] On this matter, see my "The Definition, etc.," II, *Economica,* November, 1933, 275 ff.

[54] See *ibid.,* 277 ff.

[55] See *ibid.,* 279; and cf. also "The Definition, etc.," I, 438 f.

relation between these "old goods" thus offered for sale and the total volume of goods offered currently for sale is a matter which, as I have attempted to show elsewhere, has implicitly constituted a problem for the Theory of Prices ever since the days of Davanzati, who argued that the magnitude on the "goods" side of the equation of exchange in which we must be interested is the total quantity of goods *in existence*.[56] It remained, however, for Mr. Keynes to drag the issues into the open by those aspects of his argument which we had occasion to consider in the first section of the present chapter.

That argument, as we have seen, when translated into the terms of equations of the familiar Fisherine type, amounted simply to the contention that, if we are to have a complete understanding of the forces determining the price level of "output as a whole," or of any of its subdivisions, we must take cognizance of the fact that among the things which may be sold in a given period, in addition to the volume of current output during that period, are segments of the "existing wealth" of the individuals whose actions determine the relative magnitude of the streams of money and of goods the "mutual impact" of which gives us the prices whose determination it is our task to explain.[57] Mr. Keynes's argument on this head, as we have seen, was obscured by a failure, among other things, to draw a sharp distinction between the volume of "old capital goods" thus offered for sale, and the volume of existing "securities" offered for sale.[58] This is a matter to which we shall return presently.[59] It is sufficient here to emphasize only two things.

The first is that it was precisely this argument of Keynes which gave rise to the greatest difficulty in understanding his analysis, and called forth the most elaborate attempt to

[56] See "The Definition, etc.," I, 436 ff.

[57] Cf. above, pp. 528 ff., and the references to Keynes there given.

[58] Cf. above, p. 508, and especially n. 63 thereto. Particularly important, for our present purpose, is Mr. Keynes's insistence that the "two conceptions" which he had "intended" to involve in the argument were not "existing Stock Exchange securities" and "new machines," but "the existing stock of capital" and "the currently produced capital." See Keynes's "Rejoinder" to Robertson, *loc. cit.*, 421.

[59] See below, pp. 596 ff.

paraphrase the argument of the *Treatise* for the benefit of "simple-minded readers" in the form of a "Parable of Savings and Investment," in which "old capital goods" appeared as "gold" and current output appeared as "green peas." [60]

The second is that it is precisely this type of consideration which shows the essential absurdity of arguing that a formulation which makes use of the concept of a "total volume of transactions" against which cash balances must be held, is inferior to a formulation in which the "volume of output" is regarded, not as an element *related to* the "total volume of transactions," but as an element which is to be substituted for the latter.

Bearing these points in mind, the reader may himself decide (1) as to the relative advantages of stating the point in question in terms, on the one hand, of a coefficient relating "goods produced" to "goods intended for sale," which in turn is to be regarded as part of the analysis that may be said to "lie behind" the T of our Quantity Equation, and of stating them, on the other hand, in terms of a discrepancy between "Savings" and "Investment," in the peculiarly heterogeneous context of meanings which the latter dichotomy was given in the *Treatise;* and (2) as to the degree of responsibility for the confusion existing on this point in the years immediately following the publication of the *Treatise* which may be attributed to Mr. Keynes's insistence that the "Quantity Equations" on which we were all "brought up," and in particular "total transactions" equations of the "Fisherine" type, were altogether "incapable . . . of leading us to certain useful results," such as the "result" which is here under discussion.[61]

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[60] See Joan Robinson, "A Parable of Savings and Investment," *Economica,* XIII (1933), 76 ff.

[61] Involved also in this matter of the "degree of responsibility" for the confusion in question is Mr. Keynes's persistent implication, examined in detail in Chapter Two of this study, that the fate of the Quantity *Equations* was associated indissolubly with the fate of the "Quantity *Theory*" in its crudest forms. See, for example, Joan Robinson, "A Parable," *loc. cit.,* 77, where, instead of discussing the fitness of the Quantity *Equations* for dealing with the problem in hand—and it must be continually borne in mind that Mr. Keynes had called into question their fitness for just this purpose —the "Quantity *Theory* of Money" is blamed for having led "the simple minded reader" to "concentrate too much on Demand"—that is, on the dimensions of the total stream of money—so that he "had been apt to for-

IV

The Velocity of Circulation of Goods

The argument thus far developed has led us to the conclusion that, if a term for "output" is to be included in a formulation designed to summarize the forces determining money prices—and no one, least of all those who would insist also upon the use of a formulation of the "total transactions" type, would deny that a term for "output" must be included—this term for "output" must be corrected by a coefficient representing the ratio of "goods produced," or "output," to "goods intended for sale." [62] This, however, is not the end. For the magnitude at which we must finally arrive, even in a formulation in which transactions in "goods" are segregated from transactions in other "articles," is not "goods intended for sale," but *goods sold.* And for this purpose, we must introduce the concept which, ever since the early part of the nineteenth century, has been called "the velocity of circulation of goods." [63]

It would be idle to pretend that, despite the relative antiquity of the latter concept, it has received the amount and kind of attention which might have been expected to lead to a consensus of agreement as to the place which it is to occupy in an adequate Theory of Prices.[64] Yet this circumstance provides no justification for treating the concept as if it were without roots of any kind in the received body of doc-

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get Supply": that is, the dimensions of the stream of articles sold for money!

[62] In practice, of course, the passage from "goods produced" to "goods intended for sale" would require the use, not of one coefficient, but of at least two, the first of which would establish the ratio of "goods produced" to "goods produced for sale," and the second of which would establish the ratio of "goods *produced* for sale" to "goods *intended* for sale." Cf. my article "The Statistical Measurement of the 'Velocity of Circulation of Goods,' " *loc. cit.*, 22 ff.

[63] Cf. above, p. 97, and especially the reference to my article in the *Zeitschrift für Nationalökonomie* given in n. 58 thereto. It should be added that the article in question should not be taken as providing a complete history of the concept of a "velocity of circulation of goods," as that concept has appeared in economic literature.

[64] So clearly is this the case, that writers of standing in our own day have actually declared that the fact that there is such a thing as the "velocity of circulation of goods" is "usually completely overlooked." See, for example, the references to A. Amonn and E. Cannan given in my "Léon Walras and the 'Cash-Balance Approach,' " *loc. cit.*, 575, n. 13.

trine on the subject with which we are here concerned. If there are writers of the highest standing—Wicksell and Schumpeter are examples—who have on occasion seemed to deny any validity whatever to the concept, there are writers of equally high standing—of whom Marshall, Edgeworth, and A. A. Young may be taken as typical—who were quite explicit on the point that place must be left for the concept, in one or another of its aspects, in our apparatus for dealing with the determination of money prices.[65]

It is, moreover, of the first importance to emphasize the fact that it is by no means true, in all cases in which a writer of high standing has rejected the formal concept of a "velocity of circulation of goods," that the writer in question has meant to deny the importance, for price determination, of the specific phenomena which the concept itself was intended to summarize.[66] This, after all, is the important

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[65] For references to Marshall, Edgeworth, Young and other writers, see my article on "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " II, *loc. cit.*, 497. (To the reference to Marshall there given may be added a further reference: namely, his answer to question 11760 in his Evidence before the Committee on Indian Currency [1898]. See Marshall's *Official Papers*, 268.) For references to Wicksell, Schumpeter, and other writers who have adopted a similar position on the point in question, see the article cited at the beginning of this note, pp. 478, n. 2, and 480, n. 9.

[66] Wicksell—for whose rejection of the "formal concept" of a velocity of circulation of goods see the reference given in the preceding note—may be taken as a case in point. It is clear, for example, that Wicksell's argument with respect to the desirability, in "observing and measuring the general price-level," of "confining the calculation to objects of (direct) consumption" (cf. above, p. 326, and notes 72 and 73 thereto), was not intended to be the kind of argument for confining one's attention to events on the market for consumers' goods which has often been imputed to writers holding the ideas which Wicksell advanced with respect to the meaning of the "general price-level" at the time he wrote *Interest and Prices*. See, in this connection, the passage, on pp. 15 f. of the same work, in which he included, along with other considerations, the fact that "the same commodity changes hands several times before entering into consumption," among the circumstances that may "increase the need for money and to this extent may occasion a change in the value of money." It is hardly surprising, moreover, that the references to what is here called the "number of middlemen's sales," as well as to the elements discussed in sections ii to iv of this chapter, as factors affecting the value of money should have been more numerous in the *Lectures*; for, as we have seen (cf. above, p. 326, and n. 72 thereto), the emphasis upon price determination on the market for consumers' goods was much less pronounced in the latter work. See, for example, the *Lectures*, II, 71, 145, 156. In both *Interest and Prices* and the *Lectures*, moreover, as well as in others among Wicksell's writings, there are references to what is here called changes in the "rate of sale"

matter. For even if it should turn out that the weight of responsible opinion will incline toward the position of those who have rejected the concept of a "velocity of circulation of goods" on the ground that it "attempts to deal with components which are quite heterogeneous in their nature"—in other words, that it is the same sort of "hybrid" conception which Mr. Keynes charged the concept of "income-velocity" with being—this would merely mean that, with general agreement as to the necessity for dealing with the phenomena in question, the difference of opinion turns only on the question as to which of the available sets of apparatus is best fitted for the analysis of these phenomena.[67]

For the purposes of the present study, which is concerned more to establish the points on which there already exists a very large measure of agreement than to defend a particular position within fields in which the work may be said to be very much "under construction," it is not necessary to do more than to indicate the reasons for believing that the concept of a "velocity of circulation of goods," *when properly defined,* includes fewer "heterogeneous components" than do the concepts which have thus far been suggested as alternatives.

It is obvious, for example, that a proper definition of the "velocity of circulation of goods" would make it clear that it is incorrect to attribute heterogeneity to the factors which it is designed to summarize on the ground that some of these factors "have no reference to time."[68] The definition for the "velocity of circulation of goods" which I myself have proposed, for example, would regard this "velocity" as established by the ratio

$$\frac{\text{Physical volume of goods sold}}{\text{Physical volume of goods intended for sale}}$$ [69].

By its very nature, the term "volume of goods sold" must be understood as meaning the volume of goods sold *within a given time period.*[70]

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of goods as a factor affecting prices. See the references given on p. 187, n. 80, above, and also the *Lectures,* II, 160.

[67] For an example of the rejection of the concept of a "velocity of circulation of goods" on the ground suggested in the text, see Neisser, "Der Kreislauf des Geldes," *loc. cit.,* 404.

[68] See Neisser, "Der Kreislauf des Geldes," 404. Cf. also the same author's *Der Tauschwert des Geldes,* 24 n.

[69] For the detailed argument in support of this definition, including a statement of the reasons for rejecting proposed alternatives thereto, see the two articles in *Economica* on "The Definition of the Concept of a 'Velocity of Circulation of Goods,' " cited on p. 539, n. 34, above.

[70] The failure to recognize this fact is, of course, as I have indicated elsewhere (cf. "The Definition, etc.," II, *loc. cit.,* 290, n. 85), the source of the erroneous suggestion—often attributed, not entirely without justice, to

Similarly, the denominator of the ratio in question must be understood as referring to the volume of goods intended for sale *within the same time period.*[71] If there is something in other concepts which have been associated with a variable called the "velocity of circulation of goods"—for example, the concept that has been called "capital-intensity"—which has no "reference to time," this may be any argument against the association of such concepts with the concept of a "velocity of circulation of goods," but it is not an argument which bears upon the question whether a proper definition of the "velocity of circulation of goods" itself attempts to summarize factors which are alleged to be "heterogeneous in nature," on the ground that some of them have "reference to time" and others have no "reference to time."[72] The point to be emphasized here is simply that both of the magnitudes involved in the proposed definition for the "velocity of circulation of goods" have "reference to time," and that both of them have reference to the same period of time in all cases.

Nor is it easy to see what element of heterogeneity is introduced by the fact that the "velocity of circulation of goods" is held to be related to what have been called "temporary hoards"—that is, cash balances held "because of non-continuous sale and purchase and because of transitory accumulation of sums of money for the purpose of later capital-investment."[73] The argument which the critics in question have in mind is presumably the one asserting that the "velocity of circulation of goods" is a factor affecting the velocity of circulation of money; and the further argument, presumably, is that matters such as "the technical conditions of production and consumption" are matters which should be kept separate from the concept of a "velocity of circulation" of goods.[74] An answer to this type of objection is, however, again provided by a proper definition and use of the concept of a "velocity of circulation" of *goods.* I myself, for example, have argued at length against the practice of using the expression "velocity of circulation of goods" to describe forces affecting the "velocity of circulation of money" when in fact what is meant by the "velocity of circulation of goods" is nothing more nor less than the size and time-shape of the streams of money receipts and money expenditures, actual and anticipated, relative to each other.[75] The mere fact, in any case, that the

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the two Mills as well as to other writers—that the velocity of circulation of *money* has no "reference to time." A detailed discussion of the writers concerned cannot, however, be undertaken here.

[71] See, in this connection, my remarks in "The Definition, etc.," *loc. cit.*, I, 445, 453 f., II, 277 ff., and "The Statistical Measurement of the 'Velocity of Circulation of Goods,' " *loc. cit.*, 22 ff.

[72] For the charge that the concept of a "velocity of circulation of goods" includes elements heterogeneous in nature by virtue of its inclusion of the factor of "capital intensity," which is alleged to have reference to time "only in part," see Neisser, "Der Kreislauf des Geldes," *loc. cit.*, 404.

[73] Cf. Neisser, "Der Kreislauf des Geldes," 403 f.

[74] Cf. Neisser, "Der Kreislauf des Geldes," 404.

[75] Cf. my articles, "The Relation between the Velocity of Circulation of Money and the 'Velocity of Circulation of Goods,' " I, *loc. cit.*, 291, n. 7,

term "velocity of circulation of goods" has been used as a loose and confused metaphor for certain factors which may be held to affect the velocity of circulation of money constitutes no ground for asserting that a *proper* definition of the "velocity of circulation of goods" involves the jumbling of heterogeneous elements that had best be kept separate.

Precisely the same thing is to be said with respect to the suggestion that the concept of a "velocity of circulation of goods," if it means anything, should mean something different from factors associated with the "*technical* conditions of production and consumption." [76] Again, I myself have argued against certain other proposed definitions of the concept of a "velocity of circulation of goods" precisely on the ground that they involve a confusion between what I have ventured to characterize as the "technological" rate of flow of goods, on the one hand, and the "marketing" rate of flow of goods, on the other; and I have argued on behalf of the particular definition which I favor precisely on the ground that it is free from such confusion.[77] Until the latter definition can be shown to suffer from the same defects as those definitions against which the charge under discussion can properly be leveled, it is surely as unfair to charge the vice in question against the concept of a "velocity of circulation of goods," as such, as it would be to charge against the concept of a velocity of circulation of money, as such, all the vices which may be said to inhere, for example, in the concept of "virtual velocity." [78]

The second part of a defense of the concept of a "velocity of circulation of goods" against the charge that it contains elements which are too "heterogeneous" in their nature to make it possible to interpret satisfactorily the statistical movements, say, in a figure for the "velocity of circulation of goods" computed in accordance with the formula suggested above, does not involve a denial that the effect of more than one type of factor is included.[79] It involves merely the contention that *fewer* "heterogeneous" factors are included in the proposed concept than are included in the alternatives favored by other writers. These alternatives are, for practical purposes, two: namely, Professor Hayek's "coefficient of monetary transactions," and the much more widely

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307, n. 40; "The Definition, etc.," II, *loc. cit.*, 286, n. 74; and especially the article "Zur Dogmengeschichte des Begriffes einer 'Umlaufsgeschwindigkeit der Güter' und seines Verhältnisses zur Umlaufsgeschwindigkeit des Geldes," *loc. cit.*, 200 ff. On the rôle played in the theory of the forces determining the size of cash balances relative to outlay by the size and time-shape of the streams of money receipts and money expenditures—and, one may add, by the opportunities for and inducements to investment of "surplus" balances, since Neisser includes this element in his criticism—cf. above, pp. 482 f.

[76] Cf. Neisser, "Der Kreislauf des Geldes," 404 (italics mine).

[77] Cf. "The Definition, etc.," I, 449, n. 39, and especially II, 279 ff., 284 ff.

[78] On the latter concept, cf. what is said on pp. 366 ff., above.

[79] For a discussion of the fact that at least two types of factor may be expected to affect the ratio representing the definition of a "velocity of circulation of goods" which I happen to favor, see "The Definition, etc.," II, 288 ff.

favored concept of "income-velocity."[80] I have discussed elsewhere the relation between the concept of a "velocity of circulation of goods," when properly defined, and Professor Hayek's concept—which, it may be added, has received much less attention than is its due.[81] Without repeating the details of the argument here, therefore, I need point merely to such facts as that certain of the elements involved in Hayek's "coefficient"—for example, the distinction between goods produced and goods produced for sale—are, in the procedure which I favor, kept sharply distinct from the elements involved in the "velocity of circulation of goods," which is reached only after a series of separate steps have been taken, the net result of which is to prevent the "velocity of circulation of goods" from registering the effect of factors quite different in their nature from those covered by the successive steps in question.

The same thing may be said, with even more assurance, with respect to the concept of "income-velocity." In the words of one writer, this concept may be regarded as attempting to "combine in one factor" the elements "velocity of circulation of money" and "velocity of sale of goods," in the sense that it may be regarded as "including within itself [the effect of] all changes which [also] affect the so-called velocity of sale of goods."[82] That there is more than a little basis for this statement, even though it is possible to decribe the function assigned to the concept of "income velocity" in terms which are more illuminating for other types of problem in which the concept has been involved, there can be little doubt.[83] To the extent that this is the case, nothing more need be added here to what was said, in Chapter Fourteen of this study, with respect to the reasons for preferring, for purposes of close analysis, the alternative to "income-velocity" there suggested, which involves, among other things, a sharp differentiation between factors affecting the

[80] It is noteworthy, for example, that Neisser, who has rejected the concept of a "velocity of circulation of goods" on the ground that it attempts to include "components quite heterogeneous in their nature," specifically proposed the use of the concept of "income-velocity" for the purpose of dealing with the issues involved. See "Der Kreislauf des Geldes," *loc. cit.*, 404.

[81] See "The Definition, etc.," II, 293 ff., especially 294, n. 95, 296 f.

[82] See the references to Neisser in my "The Relation, etc.," II, 489 f. Cf. also Ellis, *German Monetary Theory*, 199, where one of the reasons given for regarding "circuit velocity" as an "impure concept" is that "it carries over into the index of rapidity of money circulation a variable which pertains only to the rapidity of circulation of goods."

[83] I should like to take this occasion to apologize for the fact that, in the article cited in the preceding note, I characterized Neisser's statement with respect to Schumpeter's "efficiency" as "hardly illuminating," without taking pains to indicate that it is at most "hardly illuminating" for the problem with which the article in question was concerned: namely, the alleged indifference, for the problem of the value of money, of changes in the "number of middlemen's sales." Actually, Neisser's statement is the better one when the concept of "efficiency," or "income-velocity" is dissociated, as it should be, though it unfortunately has not always been, from the particular fallacy with which the article in question was concerned. See, on this latter point, p. 384, above, and especially pp. **561 f.**, below.

velocity of circulation of *money,* in the sense of a given ratio of cash balances held against outlay *from these balances,* and factors which, in all strictness, affect the T rather than the V of our Quantity Equation.[84] It will be seen, therefore, that a case is not made against the continued use of the concept of a "velocity of circulation of goods" by a demonstration that the concept in question can be shown to register the effect of more than one type of factor. On the contrary, the making of such a case involves, in addition, a demonstration that the alternatives proposed for the handling of the problems for which the concept of a "velocity of circulation of goods" was intended to deal are themselves not open to this objection in greater degree than the concept of a "velocity of circulation of goods" itself.

As to the nature of the two central phenomena with which the concept of a "velocity of circulation of goods" itself was designed to deal, there need, in any case, be no uncertainty. The first of these phenomena has to do with what has been called the "number of middlemen's sales." [85] That a change in the number of the sales to which a given commodity is subjected before it leaves the market may with propriety be regarded as a phenomenon of "velocity" is an idea which will commend itself at least to those accustomed to think of "velocity" in terms of what Holtrop has called the "motion-theory" of velocity of circulation—that is, in terms of the "number of times during a given period money passes from hand to hand." The matter of terminology, however, is of quite subordinate importance; the real issue is whether or not changes in the "number of middlemen's sales" represent a factor to which specifie place must be given in an adequate Theory of Prices.

The reason for arguing that they do represent such a factor is, of course, simply that an increase in the "number of middlemen's sales," by increasing the "volume of transactions" against the consummation of which cash balances must be held, increases, *other things being equal,* the "absolute" demand for such cash balances, and therefore affects the value of money. It happens, to be sure, that an argu-

[84] I hope, on a later ocasion, to be able to emphasize the point by a close consideration of the tortuous analysis which has on occasion been indulged in as part of an attempt to do justice, through the use of the concept of "income-" or "circular" velocity, to the type of phenomenon with which we are here concerned.

[85] See "The Relation, etc.," II, 479 ff., 497 ff.; "The Definition, etc.," I, 448, 453; II, 281 ff., 288 f.

ment has been developed, by writers of unquestioned standing in the field, which is designed to show not only that one factor, in particular, cannot, in the nature of the case, "remain equal," but also that it will vary in precisely such a way as to offset the change in the "velocity of circulation of goods" which is brought about by a change in the "number of middlemen's sales." [86] This compensating factor is an alleged change in the "transactions velocity" of money. If the argument in question were sound, it would, of course, follow that a change in the "number of middlemen's sales" could not possibly affect the "absolute" demand for cash balances. This "absolute" demand, as we have seen, is affected not only by such factors as are included in the T of our Quantity Equation, but also by those which are associated with what we have called the "relative" demand for cash balances, which is a part of the "absolute" demand: it is affected, that is to say, not only by T, but also by V. If, in other words, it could be shown, with respect to the expression T/V, which may be taken as a shorthand summary of the forces determining the "absolute" demand for cash balances, that the special factor making for a change in T with which we are here concerned—namely, the "number of middlemen's sales"—is always compensated for by a corresponding movement in V, it would be literally true that a change in the number of middlemen's sales could not possibly affect the "absolute" demand for cash balances, and may therefore be ignored in any attempt to construct a satisfactory Theory of Prices.

This is a question of analysis, to which only one answer can be given. I have elsewhere attempted to provide an answer, the gist of which is that the argument in question is found wanting when it is subjected to the one test which, if the argument presented in Chapter Fifteen of this study is sound, must always be invoked whenever our argument involves some contention with respect to changes in the velocity of circulation of money: the change in the "velocity" of

[86] For references, see my "The Relation, etc.," II, 486 ff. I hope to be able, in the near future, to deal at length with the literature on the subject which has appeared since this article was published. See, in the meantime, what is said in this connection on pp. 556 ff., below.

money must be shown to be capable of explanation strictly in terms of the "cash-balance approach." [87] It is my contention that the sponsors of the argument in question have failed to show that there is anything in a change in the "number of middlemen's sales" which *necessarily,* in and of itself, may be expected to bring about a change in the ratio of cash balances held *relatively to outlay* by the "middlemen" whose actions have undoubtedly increased the *T* of our Quantity Equation.[88] As long as this is so, it remains true that a change in the "number of middlemen's sales" must be regarded as a factor which is *potentially* capable of affecting the "absolute" demand for cash balances, and therefore must be included in our list of the variables which combine to make prices what they are.

It has already been noted that the issues raised in one of my earlier articles have been dealt with at some length in publications which have appeared since that earlier article was written.[89] There can be no question of summarizing here the arguments presented in this later literature, to say nothing of dealing in detail with them. Since, however, some of these later writers have either questioned the interpretation of their own earlier writings which they understood my article of 1932 to impute to them, or have expressed dissatisfaction, on one ground or another, with my treatment of the issues involved, I may be pardoned for stressing the following points, which seem to me to underlie whatever disagreement remains between the writers in question and myself:

1. I have at no time put my own argument in the form of a "categorical" or unqualified statement to the effect that with a change in the degree of "differentiation" in industry—that is, with a change in the magnitude which has been called the "number of middlemen's sales"—the velocity of circulation of money "does not change." [90] I was, on the contrary, careful to say that the velocity of circulation of money does not *necessarily* change in such a degree as to offset the change in *T* which is, *per contra, necessarily* involved in an increase in the "number of middlemen's sales." [91] My contention was, and is, that there is

[87] See especially "The Relation, etc.," II, 501 f.

[88] It is of some importance to stress the word *necessarily*. Cf. what is said in the text below, under point 1.

[89] Cf. above, p. 555, n. 86.

[90] For an attribution to me of such a position—which it is the more necessary to disavow since the commentator in question believes such a position to be the "right" one, so far as it goes (though cf. also the reference to p. 152 of Ellis's book in the following note)—see Ellis, *German Monetary Theory,* 136, 153.

[91] Cf., for example, the last sentence of the first paragraph on p. 502 of "The Relation, etc.," II; also *ibid.,* 499, n. 43. I may add that Ellis comes

nothing in the process of "differentiation," as such, which tells us anything with certainty with respect to changes in the velocity of circulation of *money*, which, from first to last, must be approached by means of the "cash-balance" apparatus.[92] My further contention was, and is, that I have not yet seen any analysis, along "cash-balance" lines, to show that a change in the degree of differentiation would necessarily, in and of itself, permit such a reduction in the size of cash balances held relatively to outlay as would permit us to assume that a change in the degree of "differentiation," with its inevitable effect upon the T of our Quantity Equation, would always and inevitably cause this latter change to be offset by an equivalent change in the velocity of circulation of money.

So far as I am aware, only two serious attempts have been made to apply "cash-balance analysis" to the problem under discussion—both of these attempts being made in articles published since the appearance of my article to which reference has been made. It is of some interest, therefore, to observe that the authors concerned have either accompanied their contention as to the inevitability or "automatic" nature of offsetting movements in the velocity of circulation of money, in the case of a change in the degree of "differentiation," with the statement that their contention would hold only on certain "presuppositions" which may or may not be realized in practice, or have stated that the "automatic" and compensatory movement in the velocity of circulation of money will not correspond "exactly" to the increase in T.[93] Surely

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much closer to my own position when he states that, so far as the problem under discussion is concerned, "if rapidity of commodity turnover . . . influences the person-turnover of money, it is an incidental and special connection, in no wise inhering in the necessities of the case" (*German Monetary Theory*, 152).

[92] Concretely, of course, this would mean that any argument with respect to a "necessary" change in velocity as a result of a change in the "degree of differentiation" would have to demonstrate either that (1) such a change would inevitably affect one of the elements included in the list of factors affecting the size of cash balances held relatively to outlay, presented on pp. 482 f., above; or (2) that the list there given is not complete as a summary of the forces affecting the size of cash balances relative to outlay. It was precisely because a positive discussion of the effect of a change in the "degree of differentiation" presupposes a careful description of the elements involved in "cash-balance analysis" that I specifically referred the positive aspect of the problem to a later publication (cf. "The Relation, etc.," II, 499, n. 43)—contenting myself, in the article in question, with the refutation of the analytical error with which the article in question was alone concerned. I was aware, of course, that some readers of the article, forgetting its avowed purpose, would, by virtue of its avoidance of an answer to the question as to how "differentiation" in business *might* affect the velocity of circulation of money, find that my "treatment of the problem" gave something less than "full satisfaction" (cf., for example, F. Lutz, "Über die Umlaufsgeschwindigkeit des Geldes," *loc. cit.*, 399).

[93] See Marschak, "Volksvermögen und Kassenbedarf," *Archiv für Soz.-Wiss. u. Soz.-Politik*, LXVIII (1933), 398 ff.; and Lutz, "Über die Umlaufsgeschwindigkeit," *loc. cit.*, 401 n.

admissions of this kind are sufficiently crucial for the problem in hand to make unnecessary here a detailed consideration of the cases in which an "inevitable" or "automatic" compensation, albeit an incomplete one, is held to be involved.[94] For it must be obvious that to concede that a change in the degree of "differentiation" does not lead *in all cases* to corresponding and offsetting changes in the velocity of circulation of money is to concede that the number of middlemen's sales is potentially a factor affecting general prices, and that the *extent* of its effect upon general prices must be left for investigation in the individual concrete case.[95]

2. The conclusion that I drew from the fact that it is not possible to demonstrate that changes in the "degree of differentiation," or the "number of middlemen's sales," are necessarily accompanied by compensating changes in the velocity of circulation of money is that it is extremely dangerous to operate with such concepts as Holtrop's "coefficient of differentiation," and the equivalents of this concept, which involve, as their characteristic feature, the insertion of an equivalent magnitude on *both* sides of the equation of exchange, this magnitude then canceling out.[96] To this, one need add only that the use of the expression "coefficient of differentiation" to mean merely the "number of middlemen's sales," *without its simultaneous insertion in the "money" side of the equation of exchange,* changes its character completely, and makes it, as far as I am concerned, unexceptionable for the purpose in hand. It is the association of the concept with the idea of "price-neutrality" by virtue of *its insertion in both sides of the equation* that makes it objectionable, and also makes it difficult to understand the continued adherence to such a practice by those who assert that they have not meant to say more than that a change in the "degree of differentiation" *may*—not *must*—affect the velocity of circulation of money.[97]

At best, the combination of these two usages can mean only that the "coefficient of differentiation" is a symbol for *those particular cases* of

[94] Once again I must ask to be allowed to defer a discussion of these cases to the later publication to which reference has been made.

[95] Cf., in this connection, the comment by Marschak, ("Volksvermögen und Kassenbedarf," *loc. cit.*, 399) that "the exact degree of dependence [of the velocity of circulation of money, regarded as a price-making factor, upon the degree of differentiation] can be established only empirically."

[96] For a summary of Holtrop's treatment of the concept of a "coefficient of differentiation," including a translation of what is obviously the crucial passage in that treatment, see "The Relation, etc.," II, 494 ff. (Appendix C to the article in question, on the algebraic aspects of Holtrop's formulation, should be read only in the light of my "Further Note on Holtrop's Formula for the 'Coefficient of Differentiation' and Related Concepts," *Journal of Political Economy,* XLI [1933], 237 ff.) For the use, by Marschak and Pigou, of a concept similar to the "coefficient of differentiation," see Appendix A and Appendix B to the article first cited.

[97] Cf. Marschak, "Volksvermögen und Kassenbedarf," *loc. cit.*, 409; and for an example of Marschak's continued adherence to the practice in question, see *ibid.*, pp. 399 f.

changes in T associated with changes in the "degree of differentiation," or "number of middlemen's sales," which happen to be accompanied by offsetting changes in the velocity of circulation of money.[98] With respect to this type of usage, only two comments seem to be called for. The first is that this new type of usage amounts to a virtual withdrawal of the contention criticized in my earlier article, in so far as that contention involved the suggestion that the "coefficient of differentiation" was to be taken literally as a measure of the degree of "differentiation" in business—that is, as the equivalent of the "number of middlemen's sales."[99] To continue to use the concept of a "coefficient of differentiation" when the latter is no longer supposed to measure the degree of differentiation in business—or the number of middlemen's sales—seems to me misleading in the extreme, in view of the past history of the concept. My second comment is that I see no reason why we should apply to the factor of "differentiation" in business a treatment which we do not apply to other factors that can conceivably be shown to affect more than one element in our Quantity Equation. On certain "presuppositions," for example—to use the expression more recently used to qualify the concept of a "coefficient of differentiation"—we can imagine that the same type of factor may affect in a similar, and therefore "compensating," direction the M' and T of our equation. We do not, on that account, introduce into both sides of our equation a special "coefficient" which is then alleged to cancel out. The reason we do not do so, of course, is that we cannot be sure that the "compensation" will be inevitable and complete in all cases; and we regard it as safer to deal with M' and T separately, allowing the element of "cancellation" to

[98] It may be observed that in the case of Marschak, for example, some generosity is required to support this interpretation, in view of the fact that the element which Marschak would include "on both sides of the equation" and then "cancel," is not a "coefficient of differentiation" having the meaning indicated in the text, but the "degree of differentiation," pure and simple. It is hardly surprising, therefore, that some commentators on Marschak's later article have found his utterances in this respect "rather Delphic" (cf. Ellis, *German Monetary Theory*, 151). Nor is it surprising that Marschak himself finds it difficult to understand why I attribute the idea of "*a priori*" price neutrality of "velocity" to "the theory of the degree of differentiation" (Marschak, "Volksvermögen und Kassenbedarf," *loc. cit.*, 409). Where the "degree of differentiation" is sharply distinguished from a "coefficient of differentiation" which appears on both sides of the equation of exchange and "cancels out," I have not imputed the idea of a "price-neutrality" of "velocity" to the "theory of the degree of differentiation." The confusion is obviously to be traced to the fact that, in some contexts, Marschak uses the expression "the degree of differentiation" in the literal sense of the equivalent of "the number of middlemen's sales," which he himself argues, as I do, is not necessarily "price-neutral," and in other contexts (as on p. 399 of the article cited), he uses it in the sense of a special "coefficient of differentiation" of the kind indicated in the text.

[99] For Holtrop's usage in this respect, see the passage reproduced on pp. 494 f. of my "The Relation, etc.," II, and also 503, n. 47 of the latter article. For Marschak's usage in his earlier paper, see *ibid.*, 505 f. (cf. however, Marschak's later "Volksvermögen und Kassenbedarf," *loc. cit.*, 391, n. 6).

take place by virtue of a simultaneous change in M' and T if and when it can be shown, in the concrete case, that the special conditions under which such a simultaneous change is inevitable, are present. Since the present position of the defenders of the concept of a "coefficient of differentiation" is precisely that the "compensation" as between the change in the number of middlemen's sales and the change in the velocity of circulation of money will not under all conditions be inevitable or complete, I see no reason why we should not abandon, once and for all, devices of the type represented by the "coefficient of differentiation."

3. It should be a source of gratification, rather than the opposite, that some of the writers whose analysis involved most explicitly the use of the concept of a "coefficient of differentiation," or its equivalent, should have hastened to disavow their allegiance to the central idea associated with that concept which my article was primarily intended to combat. This central idea was that there is something in a change in the degree of "differentiation" in business which, in and of itself, "automatically" increases the velocity of circulation of money in a degree sufficient to counteract the change in the "number of middlemen's sales," and therefore in the T of our Quantity Equation, in such wise that a change in the "degree of differentiation," or the "number of middlemen's sales," may be regarded as a phenomenon that is "neutral" as far as the effect on general prices is concerned. The effect of such a disavowal, obviously, is to establish the one fact which it is important to establish: namely, that that element in the "velocity of circulation of goods" which we have called the "number of middlemen's sales" must remain an important weapon in the arsenal of monetary theorists.[100]

Given this fact, what remains of the controversy is largely of interest to those concerned with curiosities in the development of economic doctrine. When, for example, Holtrop points to the passages in his work on *The Velocity of Circulation of Money* in which he had argued that an increase in the number of "middlemen" might be expected, under certain conditions, to result in a *decrease* in the velocity of circulation of money, the only question that remains is the question as to how it is possible to reconcile such passages with others in which (1) the "coefficient of differentiation" was explicitly identified with the "degree of differentiation"—that is, with the "number of middlemen's sales"—and (2) the "coefficient of differentiation" was then, both algebraically and in words, declared to represent those transactions which were definitely without "influence on the price-level" by virtue of the fact that the "additional transfer of goods" involved in an increase in the number of middlemen's sales would be accompanied by "an equally great increase in the money-stream." [101]

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[100] Cf. my comment at the end of "The Relation, etc.," II, 504 f.

[101] For references in support of (1), see above, p. 559, n. 99; and in support of (2) see "The Relation, etc.," II, 494 ff., 511 (equation 10); and "A Further Note, etc.," *loc. cit.*, 240 f. For Holtrop's later defense, see his "Die

4. I have been particularly careful to avoid any suggestion that the fallacy with which the article in question was concerned was inherent in the concept of the "efficiency" or "income-velocity" of money.[102] That any such suggestion would be unfounded is obvious from the simple fact that it is possible to state the objections to the fallacy with which my earlier article was concerned in terms of "income-velocity" quite as well as in terms of the "velocity" of the Fisher equation.[103] It has been argued in an earlier chapter of this study that the use of concepts of the type of "income-velocity" *unaided by detailed analysis of the kind which is provided by the use of velocity concepts of the "transactions" type* is particularly *likely* to lead to error, and in any case must be regarded

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Umlaufsgeschwindigkeit des Geldes," in *Beiträge zur Geldtheorie,* 130, n. 1, and the references to his *De Omloopssnelheid van het Geld* there given. I may point out that I myself had called attention to certain remarks by Holtrop, in the Dutch version of his book, with respect to the relation of "cash-balance analysis" to the problem in hand (see "The Relation, etc.," II, 502, n. 46). My argument, however, was that these remarks should have led to a treatment of the "coefficient of differentiation" different from that which actually appeared in the book in question. Cf. also the brief comment in my review of the German version of Holtrop's monograph, in the *Journal of Political Economy,* XLII (1934), 128 f.

[102] Cf. what is said on this matter on p. 385, above.

[103] I should add, however, that I am unable to see why an exposure of the fallacy in question in terms of the concept of "efficiency," or "income-velocity," is to be regarded as more nearly "correct" than one which would point out simply that with the T of the Fisher equation increasing as a result of the increase in the number of middlemen's sales, prices will fall, unless it can be shown that the increase in the number of middlemen's sales will necessarily bring with it an exactly compensating movement in the "velocity" of the Fisher equation (cf. Ellis, *German Monetary Theory,* 136). If "efficiency" falls, in the case under discussion—and of course a demonstration of the possibility of a fall in "efficiency" would destroy the argument as to the "price-neutrality" of a change in the number of middlemen's sales—it is only *because* there is a change in the T of the Fisher equation which is unaccompanied by an equivalent increase in the V of that equation. A demonstration, therefore, that there is no inherent necessity for a change of the desiderated magnitude in the Fisherine V is simultaneously a demonstration that, in the absence of such a change, "income-" or "circuit-velocity" must fall. I cannot, therefore, see the justification for the charge that in my earlier article I failed to "apprehend" that "the whole matter turns on what sort of velocity remains the same" (Ellis, p. 152). It should be clear that the error with the exposure of which alone the article in question was concerned means *either* the assumption that the V of the Fisher equation will, instead of "remaining the same," necessarily vary in precisely the same degree as the number of middlmen's sales, *or* the assumption that "income-velocity" will necessarily "remain the same" because the increase in the number of middlemen's sales will automatically be accompanied by an increase in the "transactions velocity" of money. What is involved, if anything, is merely a preference for stating the same result in one way rather than another. One need add only that Ellis's suggestion that the statement of the result in terms of what happens to "income-velocity" is "correct" and other methods of stating it are "incorrect," rings strangely when coming from one who, unlike myself, has gone so far as to propose the "elimination" of the concept of "income-velocity" from "scientific discourse" (Ellis, p. 199).

as an avoidance, rather than a solution, of the problems associated with the concept of a "velocity of circulation of goods."[104] Neither in that chapter nor in my earlier publications, however, have I argued that the use of the concept of "income-velocity" must necessarily lead to error, or to *results* which are inconsistent with those obtained by the use of an apparatus which is at every point strictly consistent with the implications of the cash-balance approach.[105] When, therefore, the sponsors of the argument against which the article under discussion was directed, state their position in terms which make it clear that they are prepared to admit the effect of a change in the number of middlemen's sales upon general prices whenever such a change can be shown to affect income velocity, and when they go on to make it clear that they do not regard "income-velocity," and therefore general prices, as "independent" of changes in the "degree of differentiation," or the "number of middlemen's sales," agreement must be held to obtain on the main issue which is here under discussion.[106] What remains is the comparatively unimportant question of the reconciliation of such a position with the aspects of the exposition of these writers which, taken literally, must be regarded as having implied the contrary, and the broader, and really important question, as to the relative advantages of the type of analytical device represented by "income-velocity," on the one hand, and the much more complicated, but also much safer and more nearly adequate apparatus summarized in Chapter Fourteen of this study.

Changes in the "number of middlemen's sales" are, however, not the only type of change with which the concept of

[104] See again p. 385, above.

[105] I have italicized the word "results" by way of distinguishing the proposition stated in the text from the proposition defended in Chapter Thirteen of this study, with respect to the inconsistency of the concept of "income velocity," as ordinarily defined, with the *methodological implications* of the cash-balance approach (cf. above, pp. 368 ff.). Contrast the position of Ellis, *German Monetary Theory*, 199, where it is argued that while "the cash balance approach is . . . only apparently in conflict with the Fisher velocity analysis," there is "a real and irremediable conflict" between "Schumpeter's concept of 'efficiency,' [or] the circuit velocity of money," on the one hand, and both the "Fisher velocity analysis" and "the cash balance approach," on the other. The context would suggest that at least part of the "conflict" lies in a failure to recognize that "differentiation necessarily produces a fall in circuit velocity"—in other words, that the very use of the concept of "income-velocity" implies of necessity the commission of the error which is here under discussion. It will be observed, in passing, that Ellis takes a more extreme position with respect to the "necessary" consequences of "differentiation" than I do. Cf., on this matter, p. 556, n. 90, above.

[106] For examples of an adoption of the position in question, see Marschak, "Volksvermögen und Kassenbedarf," *loc. cit.*, 399 (where it is stated that the "degree of differentiation" may be regarded as "independent" of "efficiency" only if we are prepared to neglect certain definite conditions which make the latter dependent upon the former), and p. 400. Cf. also Holtrop, "Die Umlaufsgeschwindigkeit des Geldes," *loc. cit.*, 130, n. 1. From a letter to me by Professor Pigou, I interpret him as holding the same position.

a "velocity of circulation of goods" was designed to deal. There is also the matter of changes in what has been called the "rate of sale" of goods—that is, the rate at which a given volume of goods "intended for sale" will actually be sold within a given period of time.[107] Here, again, there should be no difficulty in conceiving of the "rate of sale" of goods as a factor associated with "velocity"—for those, at any rate, who are accustomed to think in terms of the "cash-balance approach" to the problem of velocity of circulation, and in particular of that special application of this approach which involves the idea of a "rate of exhaustion" of a stock of money.[108] Again, however, the matter of terminology is of quite subordinate importance. What matters is whether or not the concrete phenomenon that is described under the heading of a change in the "rate of sale" of goods is or is not a factor of importance in the determination of money prices.

That the importance of changes in the "rate of sale" of goods—or, what comes to the same thing, changes in the levels of stocks of commodities held for the purpose of sale in their present form—has been minimized by some writers, cannot be denied.[109] It is also true, however, that other writers of high standing—of whom Mr. Hawtrey is the most outstanding example at the present day—have been equally insistent that these movements in the levels of commodity stocks are of the greatest importance for the central problems of monetary theory.[110] The point to be made here is

[107] See "The Relation, etc.," II, 482 ff., 503 ff.; "The Definition, etc.," I, 448, 453; II, 285 ff., 288 f.

[108] Cf. "The Definition, etc.," II, 290.

[109] See, for example, the references to Schumpeter and Holtrop in "The Relation, etc.," II, 504, n. 48, and cf. also Neisser, *Der Tauschwert des Geldes,* 68 n. It is worth noting that all three of the writers cited admit, as a "temporary" or "transitional" phenomenon, the importance of changes in what is here designated as the "rate of sale"; so that the real question would seem to turn upon the claims of short-term or "transitional" phenomena to close attention on the part of economists.

[110] This part of Hawtrey's argument will concern us again in Volume II, in connection with the rôle played by variations in stocks of commodities on hand in Hawtrey's discussion of the effect of monetary expansion and contraction upon output. Here it should be sufficient to call attention to the fact that the problem as to the importance and the timing of variations in the stocks of commodities on hand has been a continuing element of contention between Hawtrey and the leading representatives of the "Cambridge School." See, for example, Hawtrey's remarks on Robertson's treat-

simply that, in order to demonstrate the irrelevance of changes in the "rate of sale" of goods for the Theory of Prices, it is not sufficient to argue that such changes are not likely to be of great "practical" importance under most conditions. This is a matter on which the last word will be said, not by the Theory of Prices, but by inductive investigations covering specific periods chosen with special reference to the probable presence or absence of those factors which, on theoretical grounds, may be expected to affect the "rate of sale." [111] For the purposes of the Theory of Prices, it is sufficient to emphasize the conclusion that so long as no satisfactory argument has been developed to show that changes in the "rate of sale" of goods cannot possibly affect general prices, a specific place must be left for such changes —and therefore for changes in what has been called the "velocity of circulation of goods"—in any formulation with respect to the forces determining general prices which would lay claim to completeness.

V

Keynes's *Treatise* and the Velocity of Circulation of Goods

It should be not without interest, for an evaluation of the extent and the nature of the impact upon current monetary theory of Keynes's *Treatise,* to examine the treatment accorded in that work to the issues with which these last pages have been concerned. There was, of course, no specific term for "the velocity of circulation of goods" in the Fundamental Equations of the *Treatise*—nor, indeed, in any of the equa-

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ment of the problem, in the former's "Mr. Robertson on Banking Policy," *loc. cit.,* 423; and in his contribution to the symposium on "Saving and Hoarding," in the *Economic Journal,* XLIII (1933), 703 ff., 708; also his remarks on the treatment of variations in commodity stocks in Pigou's *Industrial Fluctuations,* in *Trade and Credit,* 159 ff. Mr. Hawtrey was also dissatisfied with the treatment accorded to the problem in Keynes's *Treatise* (on which see below, pp. 566 ff.). Cf., for example, Hawtrey's *Art of Central Banking,* 338.

[111] It should hardly be necessary to add that there is every reason to suppose that "inductive investigations," intelligently conducted, will themselves provide further leads for a further development of the *theory* of the forces affecting the "rate of sale." Cf. what is said on this matter in "The Definition, etc.," II, 291 f.

tions of the general Fisherine form which appeared at various points in that work.[112] Our first problem, however, is to discover whether any room was left, in the Theory of Prices presented in the *Treatise,* for the *phenomena* which the concept of a "velocity of circulation of goods" was designed to summarize.

One indication of an awareness of the existence of such phenomena would, of course, be provided by a recognition of the distinction between, say, an index of "production" and an index of the "volume of trade." There was, at one point in the *Treatise* at least, some indication of such an awareness.[113] The passage in question, however, can hardly be regarded as evidencing a deep concern with the analytical issues involved. The single reference to other writings on this question was to the work of Carl Snyder, whose own tentative essays in this direction must be regarded as disappointing from the standpoint of the analytical issues involved; and it can hardly be argued that Mr. Keynes himself pushed the analysis beyond that of the author in question in any important respect.[114] Nor can it be said that Mr. Keynes showed a sufficient degree of familiarity with the literature on the subject of the relation between the velocity of circulation of money and the "velocity of circulation of goods" to encourage hope that he might have dealt at some length with the element in the latter concept which is summed up by the concept of the "number of middlemen's sales." The one reference to the analytical literature on the subject was at second hand, and then to authors the

[112] The equations of the latter type which appear in the *Treatise* included, of course, either merely a term for "output" (O)—as on I, 149 f.; II, 5—or the Fisherine T (that is, the "volume of transactions")—as on I, 150, 233 ff. For examples, on the other hand, of earlier equations of the general Fisherine form in which a specific algebraic term for the "velocity of circulation of goods" appeared, see what is said on p. 575 of my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.,* and cf. also above, p. 97, n. 58.

[113] Cf. the *Treatise,* II, 87. The distinction made on II, 80 of the same work has to do not with the difference between "an Index of the Volume of Trade," on the one hand, and "indexes based on statistics of production and output" (cf. II, 87), on the other, but with the difference between "the volume of 'transactions' and that of 'trade.'"

[114] On what seem to me to be the chief shortcomings of the treatment by Snyder and other writers of the relation between "trade" and "output," see "The Statistical Measurement of the 'Velocity of Circulation of Goods,'" *loc. cit.,* 15 ff.

connection of some of whom with the argument discussed above is, to say the least, doubtful.[115] For the rest, the discussion was carried on entirely in connection with Snyder's well-known thesis regarding the existence of a close correlation between the cyclical movements in "velocity" and in the "volume of trade"—a thesis whose precise analytical relation to the rôle played by the "number of middlemen's sales" is again extremely loose.[116]

We are left, therefore, with the conclusion that as far as discussion in the *Treatise* of the "number of middlemen's sales" as a factor affecting general prices is concerned, it was virtually nonexistent. The question then arises whether it can be argued that the Fundamental Equations of the *Treatise*—and particularly the Fundamental Equation which was alleged to lead to the price level of "output as a whole"—were vitiated formally by this fact. The answer is no; for the "number of middlemen's sales" must be regarded as being included, by implication, among the factors which make for a discrepancy between "Savings" and "Investment." This is a matter to which we shall return in Volume II of this study. For the moment, the reader is asked to decide whether much would not have been gained in the way of a clear recognition of the issues involved in the concept of the "number of middlemen's sales" if, instead of being lost in the heterogeneous mass of phenomena which is concealed under the rubric of a "discrepancy between Savings and Investment," the concept had been formally introduced into the discussion as part of the analysis lying behind those versions of the Fisherine equation in which a specific place had been given to the "velocity of circulation of goods."

The component of the "velocity of circulation of goods" which does receive extended consideration in the *Treatise* is that which has been designated as the "rate of sale" of goods. The concept of "hoards," in one of the several senses

[115] See the *Treatise*, II, 80, where Keynes refers to Angell for authority for the citation of Wieser, Mourre, and Working; and cf. the comments in my article "Zur Dogmengeschichte des Begriffes einer 'Umlaufsgeschwindigkeit der Waren,' etc.," *loc. cit.*, 200, n. 1. On the position of Wieser especially, see "The Relation, etc.," I, 291 f.

[116] Cf., in this connection, my "Zur Dogmengeschichte, etc.," 199 f.

in which "hoards" are spoken of in the *Treatise*—namely, as "hoards" of commodities—was nothing more nor less than a specific recognition of the possibility that some significance may attach to changes in the level of stocks of commodities intended for sale.[117] At more than one point in the *Treatise*, moreover, Mr. Keynes was explicit in admitting that variation in the levels of these stocks, when they occurred, might have marked effects upon price movements over the short period.[118] It is true, to be sure, that at other points in the *Treatise* he presented reasons for supposing that the fluctuations in the level of these stocks themselves could not be expected to be considerable in amount.[119] It must be obvious, however, that this circumstance is of much less crucial importance for the Theory of Prices than was Mr. Keynes's failure to grapple directly with the component of the "velocity of circulation of goods" which has been called the "number of middlemen's sales." For at least the phenomenon itself was considered with sufficient articulateness to provide not only a starting point for further analysis—which would begin, for example, with a consideration of the merits of Keynes's attack upon Hawtrey's emphasis on the practical importance of variations in the levels of commodity stocks—but also, and more importantly, a starting point for further inductive investigation by way of determining, for given historical periods, which of the several situations that theoretical analysis can show to be possible were in fact realized during those periods.

A more important question, however, has to do with the relative merits of the Fundamental Equations of the *Treatise* and equations of the Fisherine type in which a specific place was accorded to the "velocity of circulation of goods,"

[117] For the use of the term "hoards" in the sense indicated, see the *Treatise*, I, 128 ff.; (cf. also I, 320 ff.; II, 47). It may be observed also that, even when, in the immortal "banana parable," the "plausible" supposition was made that the bananas "would not keep for more than a week or so" (*Treatise*, I, 176), a protective footnote was added (I, 177 n.), giving a forward reference to a treatment of "the case where the bananas will keep." (The forward reference should have been, one assumes, to Chapter XX [cf. the *Treatise*, I, 320 ff.], rather than to Chapter XIX.)

[118] Cf. the *Treatise*, I, 321; II, 140, 147.

[119] See, for example, the *Treatise*, II, 130, 133 ff., 145, 349. On the difficulty of evaluating some of these passages for the purposes of the problem in hand, cf. my "The Statistical Measurement, etc.," *loc. cit.*, 34, n. 3.

from the standpoint of the clarity with which they show the relation of the factor in question to movements in general prices. For equations of the Fisherine type, the matter is simplicity itself: a change in the "rate of sale" of goods will mean, other things being equal, that a given change in output or in the stream of money will have a greater or less effect upon prices than would have been the case had there been no change in the "rate of sale." For the Fundamental Equations, on the other hand, the only way of representing a change in the "rate of sale" of goods is to represent it as a change in the volume of "investment" relative to the volume of "saving." At more than one point in the *Treatise,* Mr. Keynes made it clear that it was by just such a translation of changes in the level of stocks of commodities intended for sale, into changes in the level of "investment" relative to that of "saving," that he proposed to deal with changes in what we have called the "rate of sale." [120] It is, once more, for the reader to decide which of the two methods of representation conduces to greater clarity, so far as the description of the actual process of price determination is concerned. The one thing to be emphasized here is that, whatever else can be said against Quantity Equations of the "transactions" type as compared with those which give specific place only to a term for "output," they cannot be charged with being "incapable of leading to the results" which, by the terms of Mr. Keynes's own argument, must be regarded as "useful" in the general Theory of Prices.

[120] See, for example, the *Treatise,* II, 146, 382 f.

CHAPTER NINETEEN

The "Volume of Transactions" (*Continued*)

I

The Price Level of "Services" and the Fisherine Equation

THUS far our concern with the components of "the volume of transactions" has been to establish the analytical steps by which we are able to pass from "output," or "goods produced," to *goods sold,* and to describe the analytical devices corresponding to each of these steps.[1] If we were justified in identifying the "volume of transactions" with the "volume of goods sold," our task of establishing the nature of the components that go to make up the "volume of transactions" could be regarded as completed. As it happens, some of the most eminent sponsors of the use of equations of the "Fisherine" type—and this would include both Newcomb and Fisher—have on occasion seemed to identify the T of those equations with the "quantity of *goods* which change hands" for money, or the "amount of *goods* bought by money."[2] Unless, however, we are prepared to use the term "goods" in a sense much broader than that in which it has usually been used, such a procedure

[1] It may be pointed out here that the term "goods sold" has itself been subjected by some writers to a treatment which, for one reason or another, must be rejected as unsatisfactory. Cf., in this connection, "The Definition, etc.," I, 441 ff.

[2] See Newcomb, *Principles of Political Economy,* 341, where, although the $K \times P$ of the "equation of societary circulation," corresponding to the PT of Fisher's equation, was regarded as representing "the entire exchange transactions of the community," K itself was defined as "the quantity of *goods* which change hands" (italics mine). See also Fisher, *The Purchasing Power of Money,* 14, where the "volume of trade" (T) was identified with the "amount of *goods* bought by money" (italics mine). On the meaning to be attributed to the word "goods" in the light of the practice of these two authors elsewhere, see the following note.

represents a premature avoidance of difficulties which must be faced directly if our analysis is to carry conviction.[3]

We cannot, for example, avoid the question whether any complications are introduced into our analysis by the fact that not only "goods," in the more common sense of "material commodities," but also "services," are sold for money. It must, of course, be obvious that since the volume of "services" sold in a given year represent "transactions" against which cash balances must be held, they represent a factor affecting the "absolute" demand for cash balances and therefore the value of money.[4] Nor can there be any doubt that this is the central point involved in any attempt to justify the inclusion of "services" in a formulation designed to summarize the forces determining "prices," including the prices of "output," or of any of the subdivisions of "output." Granted, however, that the volume of services sold for money in a given period makes part of the T of our Quantity Equation, it is necessary at this point to ask whether there are any reasons which would argue for segregating these services in a special term, in the manner, say, suggested by the expression $PT = P_gT_g + P_sT_s$, in which

[3] It should be noted that, in fact, both Newcomb and Fisher gave evidence that they did not intend that the "goods" they had in mind should be used in the common (and narrow) sense of the term. Newcomb, for example, intended to include in the "goods" of his "industrial circulation" not only articles of "wealth," but also "services" (see, for example, Newcomb, *Principles*, 326 ff.), though he also used the term "goods" in the narrower sense of something distinct from "services" (so, for example, on p. 328). Fisher, of course, was even more explicit, despite his loose identification of the term "amount of goods bought for money" with the "volume of trade," in making it clear that his "volume of trade" included not only "commodities," but also "securities and labor" (*The Purchasing Power of Money*, 291 f., 486 f.; see especially p. 185).

[4] It should be obvious that the "services," sold for money, which become "embodied" in material commodities, affect the absolute demand for cash balances during the period within which they are being thus "embodied" as much as do services rendered directly to consumers. It follows, therefore, that whatever might be said for drawing a distinction between the two types of "service" for the purpose of dealing with other problems within the field of monetary theory, no categorical distinction between the two can be made for the purpose of the problem with which we are here concerned. It follows, also, that for our present purpose, it is unnecessary to deal with the extended controversies with respect to the type of "services" that should be included "in measuring the exchange value of money," if they should be included at all. Cf., on this matter, C. M. Walsh, *The Measurement of General Exchange Value*, 121 ff., and the references to the literature there given.

T_g and T_s represent the volume of goods and services, respectively, sold during a given period, and P_g and P_s the respective prices at which the goods and services in question are sold.

It is, of course, evident that the general argument developed in Chapter Seventeen with respect to a "plurality" of price levels, the conclusion of which was that we are justified in constructing as many sets of "plural price-levels" as there are economic problems for which a differentiation between given groups of prices can be shown to be significant, must mean that a segregation of the kind suggested above as between the volume of "goods" and of "services" and the respective prices of these "goods" and "services" is called for whenever economic significance can be shown to attach to such segregation. That such significance does attach to the segregation of certain types of "service," such as wage labor, from most "goods," follows from that central pillar in the theory of the relation of money to output which is concerned with the differential rates of change likely to attach to wage rates, on the one hand, and the selling prices of the goods in the manufacture of which these wage rates represent a cost. There is, however, nothing in the nature of an equation of the "total transactions" type which would prevent such a segregation.[5] Nor is there anything in such an equation that would prevent the segregation of different groups of wage rates—say, the wage rates in capital goods industries as distinguished from wage rates in the consumers' goods industries—whenever such segregation can be shown to be important, as in the case of the theory of the effect of money upon the "structure" of output.

It must not be supposed, on the other hand, that the case for a segregation of "service" transactions from other

[5] Cf., in this connection, Roos, *Dynamic Economics*, 237 f., where a special term for "the total value of wages and salaries"—namely, $\sum_{i=1}^{S} p_{si} q_{si}$—is inserted in the second member of the equation of exchange, p_{si} representing "the price of the ith service (wage or salary)," q_{si} "the quantity of the service paid for" during the period covered by the equation, and S represents "the number of services required in the period." The rôle played in the theory of the effect of money on output by differential rates of change in wage rates and in the selling prices of the relevant goods will be dealt with at length in Volume II.

types of transaction included in the T of our Quantity Equation is to be based solely on grounds similar to those which would justify the segregation of ordinary "commodities" into significant price groups. There are other reasons for so doing, which derive specifically from the fact that "services" differ in some important respects from "commodities," in the more common use of the latter term.

If, for example, we are to include, as we should, a term for the "velocity of circulation of goods" among the components of the T of our equation, it is at least necessary to deal with the question whether there is a significant sense in which we may speak of a "velocity of circulation" of *services*.[6] A number of writers have argued that we may do so—at least in the sense of that component of the "velocity of circulation of goods" which is represented by the "number of middlemen's sales"—on the ground that services, like material "commodities," may be subjected to repeated sale. As I have attempted elsewhere to demonstrate, however, there are cogent reasons against accepting any such conclusion, quite apart from the fact that there is nothing in the sale of services which would permit the application to "services" of a concept strictly analogous to that of a "rate of sale" of goods.[7] Our conclusion, therefore, must be that the wisest procedure would be to conceive of a rewriting of the PT of our Quantity Equation in the form $PT = P_gT_g + P_sT_s$, as suggested above.

Still, however, our difficulties are not at an end. For it has been suggested, by M. A. Copeland for example, that certain *types* of "service," at any rate, differ from "commodities" in that the "transactions" to which they give rise cannot be resolved into the sale of a specifiable "volume" of services at a "specifiable price."[8] It was argued, in an

[6] On this matter, and for references to the earlier literature on the subject, see "The Definition, etc.," I, 447 ff.

[7] See "The Definition, etc.," I, 449. It may be added that to adduce, in support of the application to "services" of the concept of a "rate of sale," the fact that such services may in fact be sold more or less "rapidly," as when a workman provides more services during a day at one time than at another, is to involve the confusion between what has been called elsewhere the "technological" rate of flow, on the one hand, and the "marketing" rate of flow, on the other. On this last matter, see the references given on p. 552, n. 77, above.

[8] Cf. above, p. 57, and n. 50 thereto.

earlier chapter, that for many of the types of "service" specified by Copeland in this connection, this kind of difficulty is of more importance in actual statistical practice than it is for purposes of theoretical analysis.[9] The same thing must be said of such items as interest payments, which may be regarded as payments for the "service" of "capital provision." There is, for example, so far as this particular point is concerned, no difficulty in arguing that, if the amount of interest paid on a loan of $10,000 falls, as a result of the fall in the interest rate, from $600 to $400, a given quantity of "service" is being provided at a lower "price." [10] Precisely the same thing must be said, moreover, with respect to the payment of taxes in return for the provision of the "services" of government, which are certainly resolvable analytically, whatever may be thought of the difficulties involved in practical application, into either a given volume of services at a changing level of "prices" for these services, or a changing volume of services at a given level of "prices" for the services.[11]

The thing to be said of the types of service in question, indeed, is not so much that they are not resolvable analytically into a "volume of services" provided at a "specifiable price," but that the "services" in question are of so heterogeneous a nature that a price index including them all would be bound to be virtually meaningless. How, for example, are we to interpret movements in a price index which includes not only the prices of "commodities," in the ordinary sense of the term, but also "prices" such as interest rates, which may be taken as the "price" of the "service" of capital provision? [12] The very least that can be done is to segregate the various types of "price" involved into groups

[9] See above, p. 60, n. 58. It should hardly be necessary to add that I have no intention of minimizing the seriousness of the statistical difficulties involved. The point here is that the *theoretical* difficulties are of a different order, and that it is important not to confuse the two types of difficulty. Cf. what is said on this matter on p. 64, n. 65, above.

[10] Cf. Roos, *Dynamic Economics*, 237, where p_{rt} is made to represent the "average price of capital services," and q_{rt} "the quantity of such services demanded" in the period of time under examination.

[11] Cf. above, p. 60, n. 58.

[12] It is worth noting that Roos (cf. above, note 10), segregates the "price of capital services" from the other "prices" included in his equation of exchange.

sufficiently distinct from one another to prevent the drawing of conclusions that can be shown to be absurd in the extreme.

The fact, however, that some types of transaction which have been characterized as not being resolvable analytically into a volume of goods or services sold at a "specifiable price" are in fact so resolvable, does not mean that there are no transactions which would fall into the first of these categories.[13] The repayment of the principal of debts, for example, certainly represents this type of transaction. Yet the solution of this difficulty, as we have seen, has been available to us for almost a hundred years, in the form of the proposal made originally by Lubbock, and subsequently adopted by Newcomb, Fisher, and Copeland, though none of these later writers made reference to Lubbock, and each of them had different types of transaction in mind.[14] This proposal, as we have seen, is simply that a special term—E in the case of Lubbock, B in the case of Newcomb, E'' and E''' in the case of Fisher, and R in the case of Copeland—be inserted in the second member of our Quantity Equation, which then becomes $PT + E$, in which T would include only objects sold "at a specifiable price," and E would represent the money value of "transactions" which are not resolvable into such terms.

It must be said, therefore, that the PT of the Fisherine equation, when regarded as a symbol for "total transactions," is only a loose symbol for that purpose.[15] This, however, is a very different thing from arguing, as Mr. Keynes

[13] It may be not out of place to point out that since this is granted, the issues involved in the preceding paragraph concern mere matters of detail, which are of much less importance than the question, discussed in the paragraph which follows, with respect to the inclusion of the *total volume* of the types of payment in question in a formulation designed to summarize the forces determining general prices.

[14] Cf. above, pp. 11 and 56 f.

[15] The "looseness," it will be observed, derives not from any necessary looseness in analysis, but simply from the fact that the expression PT would lead one to suppose that all payments may be resolved into a "T" and a "P." Whether an error of *analysis* is involved in such a usage, depends upon the problem under investigation; and there is no reason why, as a matter of exposition, more need be done than to write PT in the form (PT), by way of indicating that the expression is intended to include payments that are not individually resolvable into a P and a T.

did, that the trouble lies in the very nature of equations of the general "Fisherine" form which purport to represent the "total volume of transactions." On the contrary, the amendment which is now proposed—namely, the insertion in our Quantity Equation of the equivalent of Lubbock's *E*—is intended to convert into an equation of the "total transactions" type an equation which, on a strict interpretation of *PT*, did not include all types of transaction.[16] What really matters, for our purpose, is the logic of such a procedure. This logic, once more, is that all "transactions" involving money payments, whether or not they are resolvable into a "volume" of goods sold at a specifiable price, require, in the absence of a fortunate combination of circumstances—the likelihood of whose occurrence it is for the opponents of the use of equations of the "total transactions" type to demonstrate—the holding of a certain volume of cash balances against their consummation, and thus, by affecting the "absolute" demand for cash balances, affect the value of money. To have argued the opposite—as Wieser did, for example, when he insisted that such things as the volume of transactions represented by tax payments could not possibly affect the value of money—is merely to demonstrate again the dangers inherent in the uncontrolled use of an equation, say, of the "income," or "consumers' " type, as opposed to an equation of the "total transactions" type, which Mr. Keynes had argued has no place in a satisfactory Theory of Prices.[17]

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[16] It may be pointed out that the less inclusive variant of the "transactions" type of equation (as opposed to the "output" type) is not necessarily an inaccurate expression *in itself*. It should be obvious, for example, on the basis of the argument presented in Chapter Three of this study, that it is permissible to include in the second member of the equation only "transactions" resolvable into a "volume" of articles traded, times the price of these articles traded, *on the condition* that we excise, from the first member, the payments involved in transactions which are not so resolvable. The point made here is merely that such a "transactions" equation would have to be *supplemented* by an equation which would deal solely with transactions of the "non-resolvable" type, the total absolute demand for cash balances being then derivable from a summation of the relevant elements in *both* equations.

[17] See Wieser, "Der Geldwert und seine Veränderungen," *loc. cit.*, 515; and cf. the comments of Mises, *Theory of Money and Credit*, 137.

II

The Rôle of Transactions in Securities

Of all the components of the "volume of transactions," other than the volume of "output," the element whose inclusion in a formulation designed to summarize the forces determining the "value of money" has been subject to the most intensive discussion, is the "volume of *securities*" sold in a given period. It is, therefore, of some importance that the issues involved in the inclusion of this element in the *T* of our Quantity Equation should be examined with some care.

The proposition that sales of securities must be included in a formulation of the forces determining the value of money was at least implied as early as the last quarter of the eighteenth century.[18] The earliest instance known to me of its use by an author of current reputation, however, is that provided by Sismondi, who, in presenting a non-algebraic version of what he called the "necessary equation between the mass of values which are sold for money, and the total amount of cash which is used to pay for them, multiplied by the rapidity of its circulation," added that, in reality, the equation should be "modified" so as to take account not only of what amounts to the "velocity of cir-

[18] See, for example, [Samuel Gale], *An Essay on the Nature and Principles of Public Credit* (1784), 7 f., where the author, in refusing to admit "as an *axiom*, 'that public stocks of transferable annuities shall produce the same *effects* as would be produced by an increase in the quantity of circulating money,' " pointed out that "the former is a commodity to be bought and sold; the latter, is a commodity wherewith to buy and sell," and that "it must therefore deserve a very serious consideration, whether the effects produced by public stocks may not be the very *reverse* of what is supposed" in the argument he attacked. Cf. also pp. 84 f. of the same work, where the author, having laid down the proposition "that so far as the increase of the articles to be bought and sold, shall be *internally* produced, so far the *efflux* of the money will also be provided against," went on to observe "that as the establishment of public stocks of transferable annuities, creates a new internal article to be bought and sold; such establishment must necessarily contribute to lessen the natural inconveniences that would be attendant on an increase in the quantity of circulating money; and must also operate towards preventing its efflux"; though he admitted that "these good effects produced by a public debt, in lessening that increase in the prices of commodities, which would be occasioned by an increase in the quantity of circulating money, must not however be considered as unlimited."

culation of goods," but also of bills of exchange when the latter are "bought and sold for money." [19] It can hardly be said, however, that Sismondi himself provided a detailed supporting argument for such a procedure. The whole matter obviously seemed to him to be in the nature of a self-evident proposition: "When bills of exchange are bought and sold for money . . . they then act as commodities, and must be counted as such."

It did not seem a self-evident proposition, however, to Tooke, whose own brief discussion of the matter made no mention of Sismondi's even briefer utterance. On the contrary, Tooke's concern with the question seems to have derived exclusively from his concern with a problem in connection with which we have already had occasion to cite him: namely, the "Connection between the Rate of Interest and Prices." [20] "There is one further remark before dismissing the question of the connection of the currency with prices which it occurs to me to make," said Tooke at the close of the chapter of his *Inquiry into the Currency Principle* to which reference has already been made, "and that is, that with the same laxity of language as is observable in all discussions of this topic, the term prices is often applied indiscriminately to commodities and to securities." [21] This, according to Tooke, was completely wrong, for the simple reason that there are logical grounds for expecting that variations in the rate of interest would affect the prices of commodities differently from the prices of securities. It was in connection with the prices of commodities alone that he maintained—except for the vacillation to which attention has been called elsewhere—that movements in the rate of interest should be expected to vary directly, rather than inversely, with "prices"; and he objected emphatically to the use of the movements in the "price of consols" as an

[19] See Sismondi's *Nouveaux principes d'économie politique*, II, 120 n. (of the 2d [1827] edition).

[20] See above, pp. 189 ff., 249 ff. Cf. also Gregory's Introduction to the *History of Prices*, 27, where, in the course of a discussion of Tooke's notions with respect to "The Rate of Interest," a passage in the *History* (III, 166) is cited in which Tooke declared that certain "effects" which he had been discussing of "variations in the circulation and in the rate of interest on the prices of securities have no analogy in the markets for commodities."

[21] Tooke, *Inquiry*, p. 86.

indication of the movements in the "value of money." [22] The "value of money" which was associated with the "price of consols," he insisted, was the "value of money in use or on hire," and only "confusion" could result from failing to draw a sharp distinction between the value of money in such a sense, and the value of money in the sense of "purchasing power or exchangeable value, as applied to commodities and labour." [23]

It is a pity that this argument of Tooke's was not seized upon for further discussion, despite the fact that it started off under the initial handicap of an erroneous conclusion with respect to the way in which variations in the rate of interest might be expected to affect the prices of commodities, and despite the further fact that by setting up the two senses of a change in "the value of money" he set an example which succeeded in distracting later writers from the problem in hand, without any compensating gains in positive analysis.[24] It is still more unfortunate that the issues involved were not discussed in connection with a proposition such as that of Sismondi, for which, as we have seen, no detailed supporting argument had been developed by Sismondi himself. The result, unhappily, was that when interest in the "equation of exchange" was brought to the forefront by the publication of Kemmerer's *Money and Credit Instruments in their Relation to General Prices* and Fisher's *Purchasing Power of Money,* both of which included "stock prices" and the volume of "sales on the New York Stock Exchange" in their measures of the P and T of the "equation of exchange," the analytical aspects of the problem were no further advanced than they were in the time of Sismondi.

One could, indeed, wish for no better evidence of this than is provided by the fact that even the most severe critics of Fisher's treatment of "security transactions," in

[22] Cf. the *History of Prices,* III, 121 ff.

[23] *History of Prices,* III, 121, 123.

[24] For an example of a concern with the "two senses" of the "value of money," see the well-known passage in John Stuart Mill's *Principles,* Book III, Chap. VIII, sec. 1 (489 f. of the Ashley edition); cf. also H. Sidgwick, *Principles of Political Economy,* 240, 250; and M. Pantaleoni, *Pure Economics,* 227

the years immediately following the publication of the *Purchasing Power of Money,* based their criticism, not upon a questioning of the logical justification for the inclusion of "security transactions" in a formulation designed to summarize the forces determining the purchasing power of money, but upon a questioning whether, in his index for T, Fisher had assigned a proper "weight" to security transactions—the argument being, as often as not, that he had grossly *under* weighted such transactions.[25] Both parties to the controversy, that is to say, implicitly accepted Sismondi's proposition that the mere fact that securities are "bought and sold for money" is enough reason for putting them on a par with "commodities," in the more conventional sense of the latter term.[26]

As far as I am aware, it was not, indeed, until the publication in 1928 of the third edition of Hawtrey's *Currency and Credit* that the justification for the inclusion of security transactions—or, in Hawtrey's words, "dealings in credit instruments and rights to receive money"—in an equation designed to summarize the forces determining the "purchasing power of money" was seriously questioned. It was, however, then questioned with a vigor that might be said to have made up adequately for lost time: for, according to Mr. Hawtrey, the inclusion of "security transactions" in the Fisherine formula constituted nothing less than a "fatal defect" in that formula.

[25] The literature on the subject of the weight to be assigned to "security transactions" is summarized by A. F. Burns, "The Quantity Theory and Price Stabilization," *loc. cit.,* 564 ff. It will be seen, from what follows (cf., for example, p. 593, n. 48, below) that the question of the "weight" to be accorded to "security transactions" is indeed of importance, *once a valid case is presented for the inclusion of such transactions in a formulation summarizing the forces determining general prices,* and once the place of these transactions in such a formulation is described with some precision. The point made here is merely that the discussion referred to dealt with neither of these crucial problems.

[26] The only instance of which I am aware in which critics of *The Purchasing Power of Money* prior to Hawtrey pointed to the peculiarities of "security transactions" as a reason for objecting to the fact that "for . . . the 'equation of exchange,' all exchanges stand on a par" was that of B. M. Anderson, Jr. (*The Value of Money,* 520 f.). His objection, however, was merely that certain other factors associated with "speculation" would more than counteract the fact that an increase in security transactions would increase the number of "exchanges." On this matter, cf. what is said on p. 585, below.

Hawtrey's central argument, as it turns out, is at bottom the one which was implied in Tooke's protest against the simultaneous use of the expression "the value of money" in the sense of the "value of money in use or on hire" and in the sense of "purchasing power or exchangeable value, as applied to commodities and labour." "When a purely *pecuniary* right, such as a bill of exchange, or a bond or debenture, is sold for money," said Mr. Hawtrey, "the transaction throws no light on the purchasing power of money; it merely expresses an equivalence between money at one time and on one set of conditions, and money at another time and on another set of conditions . . . Fees and salaries are . . . payments of *money for things*. The purchase of a pecuniary right is a payment of *money for money*." [27] "A payment of money for money," one might add, normally involves, under present conditions, the inclusion of an interest payment.[28] This was, in fact, pointed out by Tooke; a "high price of securities," he contended, is "almost synonymous with" a low rate of interest.[29]

So stated, the argument amounts essentially to the proposition that, in a formulation which is designed to represent the "mutual impact of relevant flows of money and of goods," and which carries the further implication that "prices" will be determined by the relative breadth of the two "flows,"

[27] Hawtrey, *Currency and Credit*, 3d ed., 37 (italics Hawtrey's). It is worth comparing with Hawtrey's statement a proposition advanced by Samuel Gale (*Essay*, 77 f.), in the course of an argument designed to show that "in considering the nature of the rate of interest for money, it is necessary to distinguish between the *quantity of money to be lent*, and the *quantity in circulation*." "Let it be supposed," wrote Gale, "that there should be *double* the present quantity of money in circulation; and only the same quantity of other commodities that there now is:—as also, that every man *individually* should have double the quantity of money that he now has, and the same quantity of other commodities that he now possesses.—In this case, money would bear only *half* the representative value that it now bears with respect to other commodities:—but this could produce no effect whatever on the comparison of *money* with *money*" (italics in the original). Unfortunately, however, the author did not go on to establish the relation between this proposition and his position elsewhere with respect to the effect of transactions in "public stocks of transferrable annuities"—which certainly involved "a comparison of *money* with *money*" —on the "price of commodities" (cf. above, p. 576, n. 18).

[28] Cf. the comment of Lindahl, in supporting Hawtrey's general contention, in the former's *Om förhållandet mellan penningmängd och prisnivå*, 5, n. 2.

[29] Cf. Tooke, *Inquiry*, 86.

it is absurd to include objects the "price" of which is the reflection, not of the relative breadth of the "flows" of money and of goods within a given period, but of an element which is in no sense *directly* related to the relative breadth of the two "flows": namely, changes in the rate of interest.[30] That there is an inescapable kernel of truth in this proposition, and that it presents a challenge which cannot be answered, in the manner of Sismondi and later writers, simply by being ignored, cannot be denied. Yet it is of extreme importance that the proposition itself should not be made to carry more of a burden than is proper to it.

The essential point involved will become immediately clear if we return briefly to the argument developed in Chapter Nine of this study, with respect to the rôle of interest as a "capitalization factor" in the determination of prices. With all recognition of the excesses of certain writers in attributing to the rate of interest, in this aspect, a degree of omnipresence in the pricing process which can hardly be justified from the standpoint of realism, it was there argued that, *for certain types of goods,* the rôle of interest as a "capitalization factor" must be granted as soon as it is recognized that, for the types of good in question, the capitalization of an expected series of income payments is a factor involved in the price offers of the potential purchasers of these goods.[31] It was also argued, however—and this point is strictly relevant to the present argument—that one cannot conclude, simply because a factor, such as the rate of interest, is an

[30] It is, of course, no answer to this contention to argue that the relative breadth of the two "flows" may be related *indirectly* to changes in, say, the rate of interest—for example, by virtue of the effect of such changes upon the size of cash balances held relative to outlay, or, conversely, the effect of changes in the size of either unlent or "absorbed" balances upon the rate of interest. It would be only on the basis of an uncritical acceptance of Mr. Keynes's proposition, in his *General Theory,* to the effect that the rate of interest is determined by the supply of and demand for "money," that even a remote case could be made for ignoring the differences between the two problems. Keynes's proposition, which he himself, so far as I am aware, has nowhere explicitly applied to the problem in hand, will be discussed briefly in Volume II. On the bearing, on the problem under discussion, of the fact that "both the expected yield [of securities] and the rate of interest are affected by causes which do not depend directly on the value of the monetary unit," see Hawtrey, "Money and Index Numbers," *Journal of the Royal Statistical Society,* XCIII (1930), 83 (*The Art of Central Banking,* p. 328).

[31] Cf. above, pp. 258 ff.

important element affecting the prices of certain "goods," that the relative breadth of the total stream of money and of goods, respectively, is not important for the money prices of these goods. The reason for rejecting such a conclusion, as we saw, was that if, for example, we are confronted with a rise in the price of "capital goods" as the result of a fall in the rate of interest, the condition which will determine whether the money prices of *other* goods will remain constant or will fall will still be the *relative breadth of the total stream of "money," on the one hand, and the total stream of goods, on the other.*[32]

The application of this argument to the point under discussion is not difficult. Let us suppose that the price of "securities"—Mr. Hawtrey's "payments of money for money"—rises, solely as a result of a fall in the rate of interest. *So long as the securities in question are bought and sold for money,* it follows, from the argument developed above, that an increase in the amount of expenditure devoted to securities must mean that, *unless the total stream of money devoted to the purchase of articles traded for money is increased,* there must be less available for expenditure upon other things. Among these "other things," obviously, are the "things" which are admitted, by all parties to the present dispute, to be relevant to the "value" of money in the second of Tooke's senses: namely, its "purchasing power or exchangeable value, as applied to commodities and labour." It is, therefore, not strictly true to say, with Mr. Hawtrey, that "when a purely *pecuniary* right . . . is sold for money, the transaction throws no light on the purchasing power of money" over "things" such as the "services rendered" in return for "fees and salaries." The statement is certainly not accurate if, by "throwing no light," we mean to suggest that the volume of transactions in "pecuniary rights" has *no influence upon* the "purchasing power of money" in the sense indicated. For it must be obvious that an increase, for example, in the volume of transactions in "pecuniary rights" does have an influence upon the "purchasing power of money" whenever, assuming

[32] See above, pp. 251 ff.

no increase in the total stream of money devoted to the purchase of articles traded for money, such an increase involves the segregation of a part of the total stream of money expenditure that might otherwise have been used to purchase the commodities or services which are admitted by all parties to be relevant to the question of the "purchasing power of money."

The point involved may be stated in terms suggested by our earlier analysis if we examine in somewhat greater detail what is implied by the condition, stated above, that "the total stream of money devoted to the purchase of articles traded for money" does not increase. Of course it is obvious that if an increase, for example, in the value of "security transactions" brings with it an increase in the stream of money available for the financing of these transactions which does not trench on the previously existing "stream of money," the increase in the value of securities traded will leave the prices of "other things," such as the prices of "goods and services," unaffected. Yet one has only to ask how such an increase in the "stream of money" might be brought about, to conclude that, *in the absence of some increase, however small, in the total stock of money,* an "automatic" increase in the total stream of money, sufficient to leave unaffected the stream of money going against "goods and services" during the period under examination, is unlikely in the extreme. An increase in the total stream of money expenditures of the required amount, it will surely be granted, can come about only through either (1) an increase in the total volume of cash balances which is uncompensated by a decrease in the rate of turnover of existing balances, or (2) through an increase in the rate of turnover of existing balances sufficient to increase the dimensions of the total money stream in a degree adequate to finance the increase in the value of "security transactions" which is here assumed to have taken place. The first possibility is ruled out *ex hypothesi*; the second must be shown to be not only a probability, but a necessity. For if it is not a necessity, an increase in the value of security transactions *may* require, for the financing of these transactions, the devotion to such financing of cash balances which were pre-

viously required for the financing of transactions involving "other things" than securities.

We are dealing, in short, with another case of composite demand for cash balances. Given an increase in the value of one segment of the total volume of transactions involving the use of cash balances, it is possible to leave unaffected the prices of articles involved in other segments of the total volume of transactions only upon one of two conditions: namely, (1) if there is an increase in the total volume of cash balances to compensate for the increase in the "absolute" demand for cash balances which is represented by the increase in the value of the "segment" of total transactions covered by "security" transactions; or (2) if there is some force which *necessarily,* and in every instance, makes up for the seepage of cash balances into the segment in question by an increase of precisely the required degree in the rate of turnover of the cash balances remaining in other segments.

Once this is recognized, it is seen that Sismondi's proposition that the mere fact that securities are "bought and sold for money" means that they "act as commodities and must be counted as such," is by no means wholly wrong. "Securities" do indeed "act as commodities," for purposes of the Theory of Prices, in so far as transactions in securities require for their consummation the allocation of a part of the total stock of "money" which would otherwise have been available for other purposes. The fault of Sismondi's treatment, and of that of the writers who followed him, lay in assuming as self-evident what must be demonstrated. The required demonstration, however, as we have seen, is easily provided by the use of the apparatus represented by such devices as the concept of a composite demand for a limited stock of cash balances in the form of "money" which may be devoted indifferently to the financing of either "security" transactions or transactions of other types.

It is of some importance to observe that when the argument for an inclusion of security transactions (Mr. Hawtrey's transactions in "pecuniary rights") among the factors affecting the prices of "commodities" is put in terms of the share of such transactions in a composite demand for cash balances, it becomes unnecessary to develop a special argument to refute the contention of those who would argue that "security trans-

actions" do not "absorb" any "money-spending power," on the ground that since the seller of a security comes immediately into the possession of the money-spending power renounced by the purchaser of a security, all that is involved is a transfer of money-spending power from one potential buyer of commodities to another. There can be no question of the soundness of this contention as against the argument of those who insist upon assuming, for example, that all funds devoted to security-purchases are immediately "tied up" in the stock market, or somehow become "dissipated" in bidding up the prices of securities. It is not sound, however, as against the argument that in so far as the consummation of security transactions requires the holding, for this purpose, of cash balances which might otherwise have been available for expenditure upon "commodities," "security transactions" must necessarily "absorb" part of the stock of available money-spending power. The amount so "absorbed" may, under certain conditions, be small; and, given the particular set of institutional devices existing in the United States during the 1920's, the amount "absorbed" in stock-market speculation was in all probability very much smaller than was often assumed. A demonstration that the stock market absorbed *no* "money-spending power," however, would require proof that "security transactions," in the special circumstances prevailing at that time in the United States, required the holding of *no* cash balances whatever. No such demonstration, to my knowledge, has ever been provided. In any case, it is perfectly possible to imagine another set of institutional devices which would require a larger holding of cash balances for the financing of security transactions than was required in the special circumstances prevailing in the United States during the 1920's. It is self-evident, surely, that an apparatus, purporting to summarize the Theory of Prices, which could deal with such cases without sacrificing any of its fitness for dealing with cases in which "security transactions" absorbed only a small amount of cash balances, is to be preferred to one which was incapable of dealing with cases of the first type. This again, however, is merely another way of saying that an equation of the "total transactions" type, which includes, by definition, transactions in securities as well as in "commodities," must remain a necessary part of the apparatus represented by an adequate Theory of Prices, despite Mr. Keynes's argument to the contrary.[33]

With these considerations established, there remains only the problem of reconciling them with what was character-

[33] It should be obvious that there is no intention here of charging Mr. Keynes with having argued either that "security transactions" absorb no cash balances, or that such absorption is of no importance for the Theory of Prices generally and for the determination of the price level of "output" in particular. Cf., on the contrary, what is said on pp. 598 f., below. The point made here is merely that the same considerations which would argue for the conclusions advanced in the *Treatise* with respect to these issues would argue also for the continued use of an equation of the "total transactions" type.

ized above as the "inescapable kernel of truth" in Hawtrey's argument that a satisfactory summary of the forces determining general prices must do justice to the distinction between "payments of *money for money*" and "payments of *money for things.*" [34] Actually, the solution of the problem becomes simplicity itself as soon as it is recognized that Hawtrey's real objection, in this connection, was against the use of a *price "index"* which jumbles together the prices of securities and the prices of those goods in whose value, in terms of money, we are primarily interested. That price "indexes" of this type are something of an "absurdity" may be granted without question.[35] This, however, is a far cry from Hawtrey's argument that "it would be an improvement to exclude" from the *equation of exchange* "all dealings in stocks, shares and pecuniary rights and to restate the Equation of Exchange . . . in terms of transactions in goods and

[34] Cf. above, p. 580.

[35] Cf. Hawtrey, *Currency and Credit*, 3d ed., 37. R. F. Harrod, in his review of Hawtrey (*Economic Journal*, XXXIX [1929], 242), charged Hawtrey not only with having failed to find "a way out of the difficulties which beset Professor Fisher" in this connection, but also with having fallen "victim to his own condemnation" of the latter by virtue of his own usage elsewhere, according to which not only consumers' goods, but also capital goods must be included in the index number of goods "bought by consumers per unit of time" (cf. Hawtrey, *op. cit.*, 59; also the same author's essay "Money and Index Numbers" [pp. 325 f. of *The Art of Central Banking*]). With respect, however, to Harrod's contention that this amounted to an admission that "mere rights to receive money" should also be included in the index of "goods bought by consumers" (Hawtrey's formulation), the following comments are in order.

1. If it is true that there are similarities between the forces affecting the price level of "securities" and the price level of capital goods—for example, the fact that both may be affected by changes in the rate of interest (cf. above, pp. 581 f.)—there are also significant differences (cf. below, pp. 596 f.). The fact, therefore, that Hawtrey was prepared to include the prices of *capital goods* in his "index" does not prove that he fell "victim to his own condemnation" of the practice of including the prices of *securities*. See, for example, Hawtrey's "Money and Index Numbers" (*The Art of Central Banking*, 326).

2. The mere fact that "the consumer sometimes lays his money out by investing in War Loan" (Harrod, *loc. cit.*, 241) means merely that the amount expended upon "War Loan" should be included *somewhere* in the second member of the equation representing "consumers' outlay." It should be obvious, however, from the argument in the text, that this does not mean, despite Harrod's statement to the contrary, that "the price of War Loan must be included in his [Hawtrey's] 'quite definite index number,' " or that "mere rights to receive money must be included in the 'quantity of *goods* bought by consumers per unit of time' " (italics mine).

services only." [36] All that is necessary, in order to meet Hawtrey's objections with respect to the type of index number used, and *at the same time retain the advantages, indicated above, of an equation of the "total transactions" type,* is to invoke the concept of a "plurality of price-levels" and the argument developed in Chapter Seventeen of this study with respect to the relation of the concept of a "plurality of price-levels" to an equation of the "total transactions" type.

The essence of this relation, as we saw, was that the "transactions" involved in an equation of the "total tranactions" type represent simply the *summation* of the transactions associated with as many separate "price-levels" as it is found convenient to distinguish.[37] The mere fact that a total represents the sum of its parts, and that, for purposes of elementary exposition, we choose to write out only the total figure rather than the figures for the individual parts, does not mean that we must always refrain from representing the parts separately, even though, from first to last, our "equation of exchange" remains, by virtue of its inclusiveness, an equation of the "total transactions" type. What this amounts to, of course, is that the association of equations of the "total transactions" type with what Mr. Keynes called "hotch-potch" price levels is no more inevitable than the concept of a "hotch-potch" price level itself. The proof of this is again that we have had examples of the use of an equation of the "total transactions" type by writers who have also been careful to segregate the prices of "articles sold" into various groups, one of which has either actually been, or could easily be, the prices of "securities" sold, which would thus be segregated from the prices and the volume of those "commodities" which Mr. Hawtrey himself regards as alone strictly relevant to what he characterizes as the "purchasing power of money." [38] In the light of this central

[36] So Hawtrey, "Money and Index Numbers" (*The Art of Central Banking,* 328).

[37] Cf. above, pp. 512 ff.

[38] Cf. above, pp. 513 f., and especially notes 81 and 82 thereto. It may be noted that Fanno (*Le banche, etc.,* 226 ff.; "Die reine Theorie, etc.," 330 ff.) supplied a special notation—namely, P_t for the price of "securities," as

fact, the inclusion of "security transactions" in equations of the Fisherine type—as opposed to their inclusion in a single comprehensive price index—must be regarded as anything but the "fatal defect" which it seemed to be to Mr. Hawtrey.

III

The "Volume" and the "Value" of Transactions in Securities

It is possible to demonstrate, likewise, that the other "defects" which Hawtrey attributed to equations of the "total transactions" type by virtue of their inclusion of transactions in "pecuniary rights" are by no means so "fatal" as Hawtrey himself believed them to be. Consider, for example, his contention that it is impossible "to measure the 'volume,' as distinguished from the value of the transactions in bills or bonds." [39] "Suppose," said Mr. Hawtrey, "that there has been a big rise, say, 50 per cent., in the price level, and suppose that there has been no change in the volume of goods financed with bills of exchange. The total value of bills will have risen by 50 per cent. Professor Fisher would regard this as an increase in the *volume* of bills. . . ." In fact, however—so Hawtrey implied—there has been nothing but an increase in their "value." This argument, for all its apparent simplicity, is in reality sufficiently complex to warrant its being approached by successive stages.

It may be noted, in the first place, that if there is any difference between the "volume" and the "value" of security transactions it is that the "value" of such transactions includes both *"volume"* and *"price."* Hawtrey's contention, therefore, that it is impossible to distinguish "volume" from "value" reduces to the contention, which we have already

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opposed to the price of "goods" (P_p), the latter, in turn, as we have seen (cf. above, p. 513, n. 81), being subdivided into various categories. The treatment by Roos, on the other hand (*Dynamic Economics*, 237), was such as to imply that the "price of capital goods" could be taken as identical with the price of "equities"—although it is obvious that only the slightest of algebraic emendations would be required in order to obtain the desired differentiation.

[39] See *Currency and Credit*, 3d ed., 37.

had occasion to examine, that there are certain types of transaction which are not capable of being resolved into a "volume" of transactions times a "specifiable price."[40] We have, of course, freely admitted that there are such types of transaction; and we have admitted that certain types of transaction in "pecuniary rights" are of this nature.[41] It is, however, a long jump from this admission to the conclusion that transactions of the type indicated should therefore be excluded from a formulation designed to summarize the forces determining general prices. On the contrary, it is still true that these transactions will contribute to the composite demand for cash balances; they must, therefore, be included in our formulation. If this is granted, the fact that the transaction in question cannot be resolved into a "volume" of transactions times a "specifiable price" is a matter of comparative irrelevance. All that one has to do, in order to save the situation, is to adopt the equivalent of either Lubbock's or Fisher's E, or Copeland's R. This is the central answer to Hawtrey's contention; and it is an answer which must lead to the use of an "equation of exchange" of the "total transactions" type even if one should refuse to regard as cogent the reasons for believing that Mr. Hawtrey has not made a clear case for his contention that it is impossible to distinguish between the "value" and the "volume" of transactions in "pecuniary rights."

By way of indicating the nature of the reasons for believing that Mr. Hawtrey's contention is anything but a self-evident proposition—though it must be remembered that the case for the use of an equation of the "total transactions" type is independent of the truth or falsity of the particular contention under examination—the following points are worthy of consideration.

It is not clear, in the first place, that the difficulty of distinguishing between the "volume" and the "value" of transactions is as great in *all* types of "security" transactions as Mr. Hawtrey implies. That he was able to make as strong a case as he appears at first glance to have made follows from the fact that he chose as his example a case which is particularly favorable to his contention—namely, that of an increase

[40] Cf. above, pp. 572 ff., and the backward references given in note 8 thereto.

[41] See especially p. 574, above.

in the total value of *new* "securities" due solely to the fact of a rise in the price of commodities. He went on, however, to argue that his criticisms "apply equally to long-dated securities, such as company debentures, mortgages, and national and municipal debts." Here he is on much weaker ground, by virtue of the fact that we now have to deal also with *"old"* securities. An "old" security had, at the time of issue, a definite price—let us say, $1,000. *This very fact, however, means that the unit of "volume" is fixed in all future transactions in these securities,* since our problem now is to ascertain what has happened to the volume of "old" securities having an issuing price of $1,000 which are subsequently traded, and to ascertain the prices at which they are traded. It follows, obviously, that if there is an increase in the total "value" of "old" securities traded, there is not the slightest difficulty in distinguishing, for each individual security, whether the change in the "value" of such securities traded is due to a fall in the price of the security, or in the "number" of such securities traded. There is, therefore, something more in the suggestion of Fisher with respect to the fact that the *original* "price" of certain types of security does not change during periods of commodity price change—though of course the *market* price of such securities may change—than Mr. Hawtrey was prepared to admit.[42]

Even, moreover, in the case of "new" securities, the "value" of which is affected greatly by the rise in the price of the "goods financed" by these securities, it is by no means clear that we are unable to say that, in a significant sense, there has been an increase in the "volume," as distinguished from the "value" of such securities. Suppose, for example, that our problem is to discover whether there has or has not been a change in the "volume" of security transactions relative to the "volume" of transactions in commodities. Let it be admitted that the "value" of "security transactions" has been greatly affected by the rise in the price of the "goods financed" by these transactions. There is no analytical difficulty, surely, in the way of supposing that we may "deflate" the figure for the "value" of securities by a price index made up of the commodities which the securities "finance," in order to determine whether the increase in the "value" of these securities registers merely the rise in the price of the "underlying" commodities, or an increase in the volume of securities used to finance them. As a matter of statistical practice, this is precisely the way in which, because of the inadequacy of our original data with respect to "physical volume," many of our so-called "physical volume" series for *commodities* are obtained.[43] It is perfectly conceivable that, without any change in the

[42] For Hawtrey's comment on Fisher, in this connection, see *Currency and Credit,* 3d ed., 37.

[43] That the whole process of "deflating" dollar-value series is full of pitfalls, is freely admitted. Cf., for example, my article "The Statistical Measurement of the Velocity of Circulation of Goods," *loc. cit.,* 8 f. The point made here, however, is that, from the point of view of strict logic, the difficulties are no more conclusive against an attempt to obtain a meas-

physical volume or price of commodities, the volume of securities used to finance them may, as the result of changes in the methods of business finance, change greatly. In this case, surely, there is some significance in speaking of a change in the "volume," as opposed to the "value," of securities traded.

As it happens, a further examination of Mr. Hawtrey's argument against the inclusion of security transactions in a formulation designed to summarize the forces affecting the value of money provides some basis for concluding that his real objection to the practice had very little to do with the alleged impossibility of distinguishing between the "volume" of transactions in securities, and the "value" of such transactions. The true nature of his objection becomes clear, in fact, when one considers his rather surprising conclusion that *shares*—that is, common stocks—as opposed to "company debentures, mortgages, and national and municipal debts," "do have a significant price, and if the shares dealt in at two different dates could be made really comparable, they might legitimately enter into an index number of prices." [44] In the light of Mr. Hawtrey's earlier argument, this is surely a striking proposition: for it is difficult to see, at first glance, just why the same objections should not apply in equal degree to common stocks. "How," we may ask, in Mr. Hawtrey's own manner, "are we to measure the 'volume,' as distinguished from the value, of the transactions in common stocks?" The "value" of the common stocks needed to finance a given volume of goods at a higher level of commodity prices may be greater solely as the result of the rise in commodity prices. Where, precisely, is the difference?

The difference, by the terms of Mr. Hawtrey's argument, lies in one fact, and in one fact alone: namely, that there are reasons for supposing that the prices of common stocks, "old" as well as "new," unlike the prices of "old" "company debentures" and other fixed-interest bearing securities, will *vary with the price of commodities*; and the reason for this

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ure of the "volume," as distinguished from the "value," of securities than they are against an attempt to obtain a measure of the "volume" of commodities sold.

[44] *Currency and Credit*, 3d ed., 38.

was held to be that common stocks are "rights to participate in the *profits* derived from capital goods." [45] We need not pause long on the validity of the assumptions underlying the proposition that we may expect a concomitant variation in the prices of commodities and of common stocks, though the experience of the United States in the later 1920's should show the danger of assuming that these assumptions will always be realized in fact.[46] The important thing, for our present purpose, is that Mr. Hawtrey's argument with respect to the difference between common stocks and fixed-interest bearing securities has in reality nothing whatever to do with the supposed fact that the "price" of common stocks may be distinguished from the "volume" of such stocks traded, in a sense in which we could not say the same thing of transactions in fixed-interest bearing securities.

Mr. Hawtrey's real concern, in fact, was to establish the necessity for segregating from the index measuring the price of "commodities" all those prices which might reflect the influence of other factors than those which were directly relevant to the prices of such commodities. For reasons of expediency, he was willing to permit the inclusion of the prices of common stocks, on the ground that they would not seriously distort the practical result. From a logical standpoint, however, his complacency in this respect represented a serious retrogression from his original position that transactions in "pecuniary rights" are "payments of *money* for *money*," and therefore may be expected to reflect the influence of certain factors—such as the rate of interest—which may be expected to affect the price of "commodities"

[45] *Currency and Credit*, 3d ed., 38.

[46] It must be obvious, for example, that, with no rise in the prices of commodities, the price of common stocks may rise as the result of (1) an increase in the yield to be capitalized, deriving from a fall in unit costs while selling prices remain comparatively stable; and (2) a fall in the rate of interest used in capitalizing these yields. It is worth noting, indeed, that in "Money and Index Numbers" (*loc. cit.*, 83; *The Art of Central Banking*, 328), published two years after the third edition of *Currency and Credit*, Mr. Hawtrey pointed out that the "increase in yield" on investments underlying common stocks in the United States prior to 1929 was largely attributable, not to a change in the "commodity price level," but to "improved methods, improved organization and the increased scale of production"; and it is equally worth noting that he showed himself much less complaisant than he had shown himself to be two years earlier toward the idea of including the prices of common stocks in a formulation designed to

in quite different degrees.[47] Purchases of common stocks represent "payments of *money* for *money*" quite as much as do purchases of other types of security. The case, therefore, for the application of the concept of a "plurality of price-levels" to transactions in common stocks is quite as cogent as the case for applying it to transactions in other types of security, and should never have been allowed to become confused with other considerations, such as the central problem of the place of "securities" generally in a formulation designed to summarize the forces determining the prices of "commodities."

The confusion of issues thus involved had, in fact, at least one extremely serious consequence. For, not content with arguing that *security* transactions should be eliminated from such a formulation, Mr. Hawtrey went on to argue in terms that would certainly suggest to some readers that he proposed to eliminate from his formulation also what might be called the volume of "intermediate transactions," such as are associated with what has been called the "number of middlemen's sales."[48] As he stated the argument, it had to

summarize the forces affecting the "value of money"—on the ground, say, that "if the expected yield of a share doubles because the price of the product has doubled, and if this rise of price no more than corresponds to a general rise in the commodity price level, then the consequent increase in the price of the share is evidence of a fall in the value of the monetary unit."

[47] It may again be pointed out that Mr. Hawtrey seems to have regained his "original position" by 1930. In "Money and Index Numbers" (*loc. cit.*, 82 f.; *The Art of Central Banking*, 326 ff.) there is, for example, no sharp distinction between "shares," on the one hand, and "company debentures," and the like, on the other, of the kind that had appeared in the third edition of *Currency and Credit*. In the later publication, on the contrary, the fact that the price of shares would also be affected by the "rate of interest" was regarded as an argument against their inclusion in a price index purporting to measure changes in the "value of money."

[48] See, for example, Hawtrey's argument that an element of "overweighting" would be involved in the inclusion of commodities which are "dealt in in a very active market, changing hands often before being finally disposed of" (*Currency and Credit*, 3d ed., 38). It should be pointed out that there is no suggestion here that Hawtrey left no room for the effect of middlemen's transactions on the "value of money" in his own positive apparatus. Cf., on this matter, the following note, and the references there given. What is objected to here is the specific argument against equations of the "total transactions" type which he presented in 1928. It is, indeed, the more necessary to refute the latter argument because there was no specific disavowal of it in the later paper of 1930, which continued to regard as a "defect" in Fisher's formula the fact that the latter "formula" included "dealings in securities" in "the totality of transactions" which appeared in

do with the "weighting" of the commodities involved in a formulation which would eliminate security transactions; and his contention was that there was something inherently unreasonable in a system of weighting based upon the number of "transactions" to which a given commodity was subject. It should, however, require only slight reflection to reach the conclusion that the particular system of "weighting" to which Mr. Hawtrey objected was merely a crude device for taking account of the relative share to be attributed to various types of "transaction" in what has been called the "composite demand for cash-balances." If the argument developed in these chapters has been sound, it follows that it is of the first importance to include the various components of this "composite demand," and to take account of the relative magnitude of these components, in any formulation designed to summarize the factors affecting any group of "prices" in which we may happen to be interested. It can be shown, indeed, that Mr. Hawtrey was prepared, on other occasions, to recognize this fact.[49] It must, therefore, be obvious that his argument with respect to the supposedly bad "weighting" which he regarded as a "fatal defect" of a formulation of the "total transactions" type is without cogency. This, again, is a corollary from our conclusions that (1) an emphasis upon the desirability of working with a "plurality of price-levels" is in no sense inconsistent with the use of a formulation of the "total transactions" type; and that (2) the use of such a formulation is required, if we are to do justice to the factors involved in the concept of a "composite demand for cash-balances," and the relation of that demand to the determination of the particular prices in which we may happen to be interested.

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it, and continued also to imply that all that was involved in the inclusion of such "dealings" was the question of their inclusion in a *"price-index"* for which nothing could be said except that it might serve to "verify Professor Irving Fisher's Equation of Exchange."

[49] In Hawtrey's system, the phenomenon in question would be regarded as one of the elements which may lead to an "absorption of cash" by "traders." See, for example, *The Art of Central Banking,* Index (p. 452), under the entry "Cash—release or absorption of"; and *Capital and Employment,* Index (p. 331), under "Absorption of cash" and "Absorption or release of cash."

IV

Keynes's *Treatise* on Transactions in Securities and the Price Level of Output

The conclusions we have reached in our discussion of the place to be accorded to "security transactions" in a formulation designed to summarize the forces affecting general prices are, then, (1) that these transactions *must* be included in such a formulation; and (2) that if we are to avoid the construction of indexes of "hotch-potch" price levels which are of little economic significance, these transactions must be segregated in a special term or set of terms in our final formulation. It remains for us only to ask to what extent justice was done to both of these conclusions in the argument of Keynes's *Treatise*.

There can be not the slightest question, in the first place, that at more than one point in the *Treatise*, Mr. Keynes was emphatic in his insistence that, if we are to avoid the distortion of our price indexes for "output," or the various components thereof, we must segregate transactions in "securities" from other types of "transaction."[50] If this position had been consistently adhered to, one could have welcomed this aspect of the argument of the *Treatise* as indicating a willingness to push further the best thought on the subject, as it had begun to be developed by Fanno, Hawtrey, and others. Unfortunately, however, the gains thus made were virtually canceled by the confusion which, as Mr. Keynes's critics were quick to point out, surrounded that particular

[50] See again, for example, the *Treatise*, II, 83, where Keynes assigned separate terms—namely, Q_1 and Q_2—to the "price-levels" attaching to the "volume of Wages and current production of goods (finished and unfinished) traded," on the one hand, and the "volume of Bonds, Shares, Real Estate and other financial obligations changing hands." It is true that the putting of "Real Estate" on a par with "Bonds, Shares, and other financial obligations" was not entirely happy. It is worth noting, however, that in stating the reasons for expecting divergent movements in Q_1 and Q_2, respectively, Q_2 was discussed as if it included only such things as a "price-index of shares" and an "index of gilt-edged securities" (*Treatise*, II, 83 f.). Cf. also *ibid.*, I, 249, on the differences between the nature of the forces affecting the "price of existing securities" and the volume of trading in such securities, on the one hand, and those affecting the price and volume of output, on the other.

one of his "plurality of price-levels" which he designated as P'.

The characteristic of Keynes's treatment of P' with which we are here concerned was that, despite the formal definition of the latter as "the price-level of new investment goods," there was a continuing series of suggestions to the effect that it was a matter of indifference whether we substituted for "new investment goods," at any point in the argument at which it was felt desirable, the "price-level of securities." [51] That this was indeed a major retrogression from the standpoint of an adequate Theory of Prices becomes clear when one reviews the central arguments which can be advanced against any such suggestion:

1. The purchasers of "securities" may be, and usually are, entirely different from the purchasers of concrete "investment goods," from the standpoint of motivation, knowledge—including the ability to "anticipate" the subsequent course of market events—and resources.[52] There is every reason to suppose, therefore, that the determination of the prices of "securities" will be affected by factors which may

[51] See, for example, the *Treatise,* I, 142, where, although the section in which the passages in question appear was entitled "The Price Level of New Investment-Goods" (p. 140), much of the discussion was concerned with the "price-level of securities," in some instances the confusion being increased by the use of the expression "the price-level of *investments,*" in a sense undefined (I, 143). See also I, 255 ff., where the discussion of the forces determining "the value of securities" was carried on in terms that would suggest that there is no significant difference between these forces and those determining the value of the "goods represented by the securities" (see especially p. 257, where Keynes spoke of the case in which "the value of existing securities of a kind which are capable of being reproduced comes to differ from the current cost of production"). It may be noted that when these passages, and others of a similar nature, were pointed out by Mr. Keynes's critics (cf. above, p. 508, n. 63), he retorted in terms which amounted in effect to a reiteration of the position that no real significance attached to the distinction between "securities," and the concrete capital goods underlying them (cf. above, p. 546, n. 58). It is worth noting, also, that some of Mr. Keynes's popularizers followed his example in shifting without apology from "securities" to "old capital goods." See, for example, J. Robinson, "A Parable of Savings and Investment," *loc. cit.,* 76 ff., 81 ff.

[52] See especially, in this connection, the discussion by Fanno, "Die reine Theorie," *loc. cit.,* 28 ff. (Cf. also *Le banche, etc.,* 223, 226 ff.) Fanno refers also in this connection (*Le banche,* 228 n., "Die reine Theorie, etc.," 32 n.) to the brief discussion by T. E. Burton, of the different nature of the forces affecting the "prices of securities," on the one hand, and the "prices of commodities," on the other (Burton, *Financial Crises and Periods of Industrial and Commercial Depression* [1903], 232 ff.).

enter in very small degree, if at all, into the determination of the prices of concrete "investment goods," and vice versa.

2. A serious error in economic analysis is involved in identifying the yield on, and therefore the price of, concrete "investment goods" with the yield on, and therefore the price of, the *ownership shares in a conglomeration of entrepreneurial assets* which are represented by "securities." The extreme importance of this distinction can be appreciated only by those who are conversant with the problems raised for the theory of the forces determining the *rate of interest* when one analyzes closely the versions of this theory which run entirely in terms of "real capital."[53] The distinction itself, however, should need only mentioning to be appreciated.

3. It follows, from proposition 2, that the volume of transactions in concrete capital goods bears no necessary relation whatever to the volume of transactions in the ownership claims which are represented by "securities." Mr. Keynes himself was perfectly clear upon the point that there is no necessary relation between the volume of transactions in *current output* and the volume of transactions in securities.[54] Unfortunately, however, he gave no clear evidence that he was aware of the fact that there is no necessary relation between the volume of transactions in concrete "*old* investment goods" and the volume of transactions in securities.[55]

4. Even if the movements in the volume of transactions in concrete "investment goods" could be taken as an *index* of the movements of the volume of transactions in "securities," it does not follow that one could be *substituted* for the other. So long as there are transactions in *both,* these

[53] The issues here involved are at once so complicated and of such great importance that they cannot be dealt with in detail here. I intend, however, to return to the subject in the publication on *The Natural Rate of Interest* to which reference has several times been made.

[54] See, for example, the *Treatise,* I, 248.

[55] On the contrary, on the very same page cited in the preceding note, Keynes *identified* "the volume of *securities*" with "the existing stock of wealth"; and on the following page (*Treatise,* I, 249), he spoke of "existing securities" as "consisting" largely of "properties which cannot be quickly reproduced," and "of natural resources which cannot be reproduced at all." See especially his reply to Robertson on the point in question, cited on p. 546, n. 58, above.

transactions are, from the standpoint of the absolute demand for cash balances, *additive* in nature.

Proposition 4, it will be observed, brings us back to the first of the conclusions stated at the outset of this section: namely, that room must be left, in any adequate formulation of the Theory of Prices, for the volume of "security transactions" as a factor affecting the price of "output," or any of its subdivisions. On this point, however, Mr. Keynes cannot be charged with a positive error of analysis. Quite the contrary; for anyone who accepts—as I do—Mr. Keynes's discussion of the rôle played in the Theory of Prices by the demands of what he called the "Financial Circulation" as one of his positive contributions of emphasis, if not of novelty in substance, to that Theory, it is clear beyond any possible doubt that on this point Mr. Keynes was on the side of the angels.[56]

If Mr. Keynes is to be criticized at all in this connection, it is on grounds on which it has unfortunately been found necessary to criticize him so often in this study: namely, on the ground that he has insisted on regarding as antithetical to certain received doctrines on the Theory of Prices a proposition which is in fact an integral part of it. The essence of Mr. Keynes's argument with respect to the rôle of the "Financial Circulation," surely, was that the requirements of this "circulation" must be taken into account by a central banking authority in deciding upon the amount of cash balances which must be "created" for the purpose of financing transactions in "output" on a scale involving neither "inflation" nor "deflation." [57] Alternatively, we may say, in the terms suggested at earlier points in this study, that "security transactions" must be regarded as a component of the composite demand for cash balances. This, however, as we have seen, is merely another way of saying that an equation of the "total transactions" type must remain an essential part of the apparatus which is included under the head of the Theory of Prices. When, therefore, Mr. Keynes launched his attack upon equations of the "total transac-

[56] The *locus classicus* in the *Treatise* is, of course, I, 248 ff.
[57] See, for exampie, the *Treatise,* II, 370.

tions" type—such as that of Fisher—without making this clear, he can only be regarded as having contributed to the "haze" surrounding the subject which he was to deplore when he came to publish his *General Theory*.

V

The Rôle of Output in the Theory of Prices

One final word only need be added—and that by way of reminder. It cannot be too often insisted that the use of an equation of the "total transactions" type does not for a moment imply a denial of the proposition that, from many points of view, the center of our interest, so far as the components of the "volume of transactions" are concerned, is, and must remain, the volume of "output" and the prices attaching to the various segments of that output. So clearly is this the case that the relations between the Theory of Output and the Theory of Prices must be reserved for separate and detailed discussion in Volume II of this study.

The only point to be made here is that a concern with these relations is not ruled out by the use of an equation of the "total transactions" type. On the contrary, the theory of the relation between output and prices is part of the doctrine which may be said to *lie behind* the T of equations of the "total transactions" type. It is a matter of extreme simplicity to demonstrate this proposition by breaking up (PT) into its components.[58] If the simple Quantity Equation adopted as a starting point in Chapter Six of this study

[58] We may, for example, let $(PT) = PT + E$, in which E represents the value of transactions not reducible to a "volume" of articles traded multiplied by a "specifiable price" for such articles (Lubbock, Copeland). We may then write $PT = P_g T_g + P_{sr} T_{sr} + P_{sc} T_{sc}$, in which T_g, T_{sr}, and T_{sc} represent the volume of goods, services, and securities, respectively, sold for money "at a specifiable price" and P_g, P_{sr}, and P_{sc} the prices at which they are sold. If we write $T_g = G \cdot v$, in which G represents the volume of goods intended for sale during a given period, and v the "velocity of circulation" of these goods, it is obvious that we may also write $G = \omega \cdot O$, in which ω is a coefficient establishing the relation between output (O), and the volume of goods intended for sale (G), the magnitude of ω being of course determined by the factors indicated on pp. 540 ff., above. It should hardly be necessary to repeat that this formulation is capable of still further breakdown. For example, "output" (O) may be subdivided into $O_R + O_C$, representing the output of consumers' and producers' goods, respectively, and so on.

did not do so, it was only for the sake of avoiding, at an early stage in the exposition, a formulation so complex as to be inconvenient for the purposes of preliminary analysis. The proposition which it has been our aim to establish in the last three chapters is that a concern with "output" and with the prices of the elements included in output does not warrant neglect of the other components included in the T of an equation of the "total transactions" type, even from the standpoint of one interested primarily in the price of "output" and its several elements.

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