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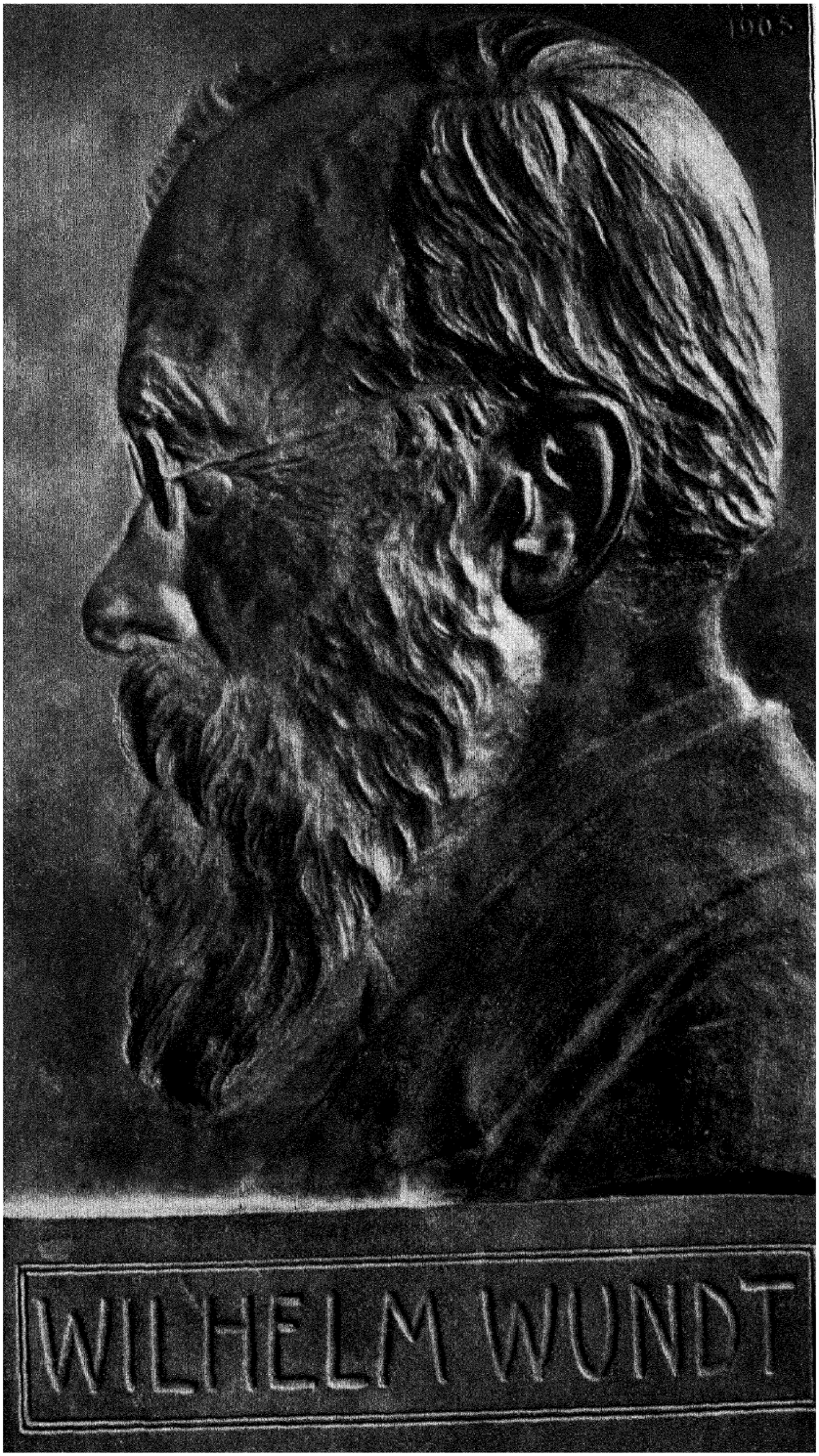
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A HISTORY OF EXPERIMENTAL PSYCHOLOGY

BY EDWIN G. BORING

PROFESSOR OF PSYCHOLOGY IN
HARVARD UNIVERSITY

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TO

EDWARD BRADFORD TITCHENER

PREFACE

That "psychology has a long past, but only a short history" was remarked by Ebbinghaus many years ago, but in general the histories of psychology have emphasized its long past at the expense of its short scientific history. It is now more than five years since I began to write this book, and there was at that time no historian who had written a book on the history of what was called in the '90's the 'new' psychology, or who had presented the experimental movement as anything other than the termination of the long development of philosophical thought about mind. The modern history of psychology cannot, however, be written merely by adding chapters to the older history. Strange as it may seem, the present changes the past; and, as the focus and range of psychology shift in the present, new parts of the past enter into its history and other parts drop out. Experimental psychology of to-day has its own history, even though that history is not all an account of experimentation. The systematic problems persistently enter into it, but they appear in a different way. Moreover, the degree to which they enter is itself a matter of history, which is independent of the will of the historian.

Of the purpose that has held me to this undertaking in the face of endless academic distractions, I need say only a word. The experimental psychologist, so it has always seemed to me, needs historical sophistication within his own sphere of expertness. Without such knowledge he sees the present in distorted perspective, he mistakes old facts and old views for new, and he remains unable to evaluate the significance of new movements and methods. In this matter I can hardly state my faith too strongly. A psychological sophistication that contains no component of historical orientation seems to me to be no sophistication at all.

When I began writing, I supposed that the history of experimental psychology could very well begin with Fechner's *Elemente* of 1860 and Wundt's *Beiträge* of 1862. In this sense experimental psychology is only seventy years old. However, the genetic account

requires the explanation of the new movement in terms of its ancestry. Hence my picture shows the lines of descent debouching from Descartes, Leibnitz, and Locke on the philosophical side, and developing within the new experimental physiology of the early nineteenth century on the physiological side. It was of the union of these two movements that experimental psychology was born.

At the other temporal extreme, it is plain that there is as yet no sound historical perspective. I have thought it unsafe to say very much about psychology since 1910. Nevertheless, I have made exceptions in the case of *Gestalt* psychology and of behaviorism, both as yet undignified by the least trace of antiquity, because of the light that each casts backward upon the past. The genetic account is reversible, and history can be understood by its effects as well as by its causes.

In a word, then, my book deals with the psychology of a half-century from 1860 to 1910, with its preceding development and its consequences—a spindle-shaped history, as it were. Naturally the words "experimental psychology" must mean, in my title, what they meant to Wundt and what they meant to nearly all psychologists for fifty or sixty years—that is to say, the psychology of the generalized, human, normal, adult mind as revealed in the psychological laboratory. In making this choice I have had no doctrinaire's thesis to defend. Animal psychology is of the laboratory; the mental tests are in a way experimental; abnormal psychology may be experimental. The first two of these subjects I have brought into my exposition in so far as their development was interpenetrated with the growth of 'experimental psychology'; but I do not, of course, pretend to have written an adequate history of either movement.

Perhaps I should say also why there is so much biographical material in this book, why I have centered the exposition more upon the personalities of men than upon the genesis of the traditional chapters of psychology. My reason is that the history of experimental psychology seems to me to have been so intensely personal. Men have mattered much. Authority has again and again carried the day. What Johannes Müller or Wundt said was nearly always important, quite independently of the weight of experimental evidence for the view of either. Moreover, personalities have been reflected in schools, and the systematic traditions of the schools have colored the research. Then, too, with personalities

playing such important rôles, it is inevitable that the psychologist should become interested in the psychology of history. Thus there is always the further question: if personalities lie, in part, back of psychology, what lies back of the personalities? I trust that I have been cautious in drawing such inferences; however, I have never been able to get this question out of my mind.

The exigencies of publication have made it impossible for me to take serious account of books published during the current year. Murphy's *Historical Introduction to Modern Psychology* appeared two weeks before I wrote the last word of my last chapter. I have referred to him in my Notes, but I did not have the advantage of his book while I was writing. Köhler's *Gestalt Psychology* came too late to affect the chapter on that topic. Pillsbury's *History of Psychology* finds mention only by additions in proof to the Notes. The publication of the *Psychological Register* is announced for next week. It should prove a support to this book, and might have made one or two of my selected bibliographies unnecessary.

It has been my good fortune to discover how very many persons were ready to give generously of their time and counsel when I sought aid concerning portions of the text. My gratitude to them all is much greater than these formal acknowledgments can express. It need scarcely be said, however, that the reader must not hold my friendly critics responsible for any portions of this book, for I have more than once insisted on having my own say in spite of advice. Professor H. S. Langfeld read the section on Stumpf and helped me greatly with it. Professor Kurt Koffka supplied some of the information about Stumpf's students. The section on G. E. Müller benefited from the unpublished paper of Dr. W. D. Turner, and some biographical obscurities were cleared up by letters from Professor Müller himself. Professor R. M. Ogden lent me his aid in the sections on Külpe, and the text shows the effect of some of his criticisms. He also transcribed some letters of Külpe's for me. Mr. David Shakow's unpublished biographical study of Ebbinghaus formed the starting-point for another section of the text. British experimental psychology on the formal academic side is obscure, and Professor William McDougall, Professor C. Spearman, Dr. C. S. Myers, and Mr. F. C. Bartlett have all supplied me with important information. Professor McDougall also read the entire chapter on British psychology; and

I have made a number of additions to it at his advice. Dr. J. McK. Cattell has been kind enough to answer some of my questions about early American psychology. I owe a real debt, even though it be indeterminate, to my seminary at Clark University in 1921-1922 and to my seminary at Harvard in 1928. My wife has labored untiringly on the manuscript, the proofs, and the indices. Mr. K. W. Oberlin has read most of the galley proofs, and Mr. P. E. Huston all of the page proofs. Professor R. M. Elliott, the editor of this series, has in all the final stages of work stood by, ready with wise suggestions, and my publishers have shown all the coöperation that an author could ask. And there are still others, who might be added to this list, and who realize my gratitude though I make no formal acknowledgment.

The frontispiece is a reproduction of Dr. Felix Pfeifer's bronze plaque of Wundt. The plaque was made in 1905, the year of the golden jubilee of Wundt's doctorate. I am grateful to Dr. Pfeifer for permitting this reproduction, and to Professor K. M. Dallenbach for photographing the bronze at Cornell for me.

Acknowledgment must also be made for permission to quote Torrey's translation of Descartes, Duncan's translation of Leibnitz, James's *Principles,* and the English translation of Helmholtz's *Optics,* although I preferred to deviate from the last-named translation in minor ways. I am also grateful for permission to reproduce from my own drawings Figures 1, 2, and 4.

In dedicating this book to Edward Bradford Titchener I am acknowledging my greatest intellectual debt. Whatever of merit in care, thoroughness, or perspective the book may have derives originally from him. Especially was it due to his influence that I gained the conviction that the gift of professional maturity comes only to the psychologist who knows the history of his science. In experimental psychology Titchener was the historian *par excellence.* He should have written this book, and it is with great diffidence that I offer a poor substitute.

E. G. B.

August 25, 1929
Cambridge, Massachusetts

PREFACE TO THE SECOND PRINTING

The necessity for a second printing of this book and the generosity of my publishers have provided me with an opportunity to correct the plates. I have corrected numerous typographical errors and about a score of minor errors of fact, like the *1885* for the *1886* of Mach's *Analyse*. I have changed a few sentences which critics have found ambiguous. I have considered every criticism or query that has been brought to my attention, and I have in consequence made fourteen changes—for the most part within a single sentence—where I have been convinced of my own error (pp. 47, 90, 160f., 165, 233, 407, 415, 443, 446, 479f., 568, 586, 617). These changes constitute my replies to criticisms of details. Altogether twenty-two persons have furnished me with the information or criticism which has led to these corrections, and I am very grateful to them for their pains in pointing out mistakes. It has, of course, not been possible to bring the references up to date nor to undertake any extensive revision.

The book has on the whole seemed to me to be well received and I have been correspondingly gratified. To several critics I may remark that I meant and still mean what is printed at the end of Chapter 24, although this degree of candor is perhaps not politic. To Professor R. S. Woodworth I say that I, too, wish that the book included, in accordance with my original plan, the detailed history of the experimentation in the various fields of experimental psychology. We ought to have such a book, and yet I wonder who is to write it. To Professor Édouard Claparède I must admit some of my neglect of experimental psychology on the European continent outside of Germany. Still he has not convinced me that Americans have set up the German psychologists as "sky-scrapers" simply because they love to have the gigantic to admire. To Professor H. P. Weld I want to reaffirm my belief in the continuity of thought in the history of science, and yet to add that I think that both the appearance of historical continuity and of personal originality can easily become

artefacts of an historical point of view. I fear the two horns of this dilemma are about equally dangerous. To Professor K. Koffka I have to say that I am convinced that Ewald Hering has received inadequate treatment in my book as to his great influence upon experimental psychology in general and especially upon those in Brentano's phenomenological camp. I regret more than any other sin which my critics have mentioned this injustice to his memory. To all my critics and reviewers I am deeply grateful: they have done me the honor to disagree seriously as well as to approve, and I hope I have profited by the one as much as I have been encouraged by the other.

August 27, 1931 E. G. B.

Cambridge, Massachusetts

EDITOR'S INTRODUCTION

Psychologists have been thought to be professionally self-conscious and jealous proprietors over the domain of their achievements. If the charge be true, the reason may well be thought to lie in unique and esoteric problems of methodology, though schools of psychology have been founded wholly on the denial, as well as on the affirmation, of this supposed particularity of method. That the charge of undue self-concern cannot be unreservedly sustained, however, would seem to follow from the relative scarcity of historians among psychologists. The history of a science is the record of its achievements. No matter how much the accomplishment of psychology is disparaged—and, as a corollary, the significance of its history belittled—there is no gainsaying that the notorious variety of psychologies challenges every resource of historical scholarship. And yet—or is it because of this?—until the appearance of this volume there has been no unqualifiedly historical treatment of the antecedents and entire history of experimental psychology. There have been several histories of psychology and, recently, an historical introduction to modern psychology, but any one of these books could have borne the title of the present work only as a misnomer.

While Professor Boring will want the merits of his history to be their own spokesman, there can be no impropriety in observing that few of the many past and current misconceptions, undervaluations, and defeatist notions against which the earnest psychologist must struggle will survive to harass or mislead the reader who is persuaded to pattern his judgments after the fact-mindedness which this volume exemplifies. Remarkably, this is true even where the biographical warp of its weave is most apparent. No generalization is stronger than the details upon which it rests, warned William James. Because psychologists have not been historically minded, they have strayed far from objectivity in appraisal, and in claims and counterclaims unsubstantiated by detailed documentation. The addition to psycho-

logical literature of a definitive history of experimental psychology rich in biographical material supplies the corrective.

It remains to point out the appropriateness of introducing The Century Psychology Series with this history and to record the gratification of the editor and publishers at Professor Boring's confidence in the project.

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INTRODUCTION

Chapter 1

THE EVOLUTION OF THE SCIENTIFIC METHOD AND POINT OF VIEW

THE history of psychology is inextricably bound up with the history of philosophy, whereas the rise and development of experimental psychology is explicitly a phase of the history of scientific endeavor. Nevertheless, philosophy and science are not to be regarded as inharmonious parents of scientific psychology, for the beginning of theoretical science itself is also the beginning of philosophy.

The Beginnings of the Scientific Attitude

Early Greek philosophy was objective: it sought primarily an understanding of nature. Thales (*ca.* 624-550 B.C.), the first philosopher of whom we have any record, asserted that water is the ultimate reality in nature. Water is abundant, is essential to plant and animal life, may exist as vapor, liquid, or solid, and, by the cycle of evaporation, rain, and drainage, interpenetrates the natural world. Thales's view was reasonable at his time, and its truth or falsity is of no concern to us now. His conception marks the beginning of science because it is a first attempt to find an explanation of nature within nature itself. Previously the human urge for explanation had been satisfied by mythology, by an account of how natural objects had come into being. Thales turned the attention from mythical events to the organization of nature itself, and this is the fundamental principle of science, that nature can be explained internally without reference to an external agency. The modern abhorrence of the admission of a *deus ex machina* into the scientific system has its origin in this conception.

Here also we find at the very start of things the emergence of the persistent problem of scientific method, which nowadays takes the form of the question of analysis. If water is the ulti-

mate substance of which all things consist, how then can there be anything else than water? This is the problem of the One and the Many, as it was called. It is the problem of analysis, because it is also the problem of elements. Its solution requires a theory of the nature of change, for if water be the only substance, it must somehow change and yet remain itself. This question still concerns modern physics and, as we shall eventually see, modern analytical psychology.

The 300 years after Thales (*ca.* 600-300 B.C.) witnessed the rise of Greek philosophy to its zenith in Socrates, Plato, and Aristotle. Not only did this period have an astonishingly great influence in determining the course and nature of subsequent and modern philosophy, but it also marked the emergence of the fundamental scientific concepts which are prevalent to-day. Heraclitus (*ca.* 536-470 B.C.) sought to meet the problem of the One and the Many by denying the reality of permanence and asserting that change, as represented by fire, is the one reality. On the other hand, Parmenides (*ca.* 550 B.C.) maintained that the only reality is Being. This position is equivalent to denying both change and the Many in the assertion of the reality of the One. To deny difference in the objective world seems a palpable absurdity; but, in the first place, Parmenides's argument served by its paradoxical nature to emphasize the importance of the problem; and, in the second place, it gave notice that the scientific understanding of nature, even though it sought to explain nature within itself, would not be able to ignore reason. Thus modern science, though it may distinguish itself from philosophy by stressing its empirical basis, succeeds only by an effective use of logical tools.

The solution of the dilemma of change and permanence belongs to Empedocles (*ca.* 490-435 B.C.) and Anaxagoras (*ca.* 499-428 B.C.). Empedocles held that there are four elements, fire, air, water, and earth, and that different objects consist of these elements in varying combinations. Change can occur by an alteration of the spatial relations of portions of the elements. The doctrine is essentially the modern chemical doctrine of compounds. Anaxagoras extended the theory by positing the existence of a vast number of elements, one for every sensible quality. The combination of atoms of these elements would account directly for the various combinations of attributes that constitute the vari-

ous natures of different natural objects. This atomic theory transcends the modern chemical theory: hydrogen and oxygen combine to make water, but it is not true that one, for example, contributes wetness and the other density. The theory is more like the modern structural psychology of Wundt except for one important difference: for Anaxagoras, synthesis was effected by movement of the atoms in space; in a psychology of mental elements, it has never been clear what takes the place of space in the process of mental integration.

Not only does this early period provide us with the current theory of atomic analysis; it also marks the beginning of other scientific principles that are still of major importance. The Pythagoreans (Pythagoras, *ca.* 580-497 B.C.), with their fantastic beliefs in the significance of numbers, did much to pave the way for the rôle that mathematics was to play in science. Democritus (*ca.* 460-370 B.C.) developed the quantitative aspect of the new atomic science and emphasized the importance of the mechanics of impact in the displacement of one atom by another.

The problem of scientific law had arisen. Heraclitus had noted that the world, though naught but change, consisted nevertheless of lawful change. Anaxagoras had sought to explain change by referring it to mind: the elements are essentially inert; movement among them is dependent upon the action of an intelligent, self-moved mind. In this view there was a return to a *deus ex machina* which in a crude form had been avoided by the abandonment of mythological explanation. Nevertheless, the attitude toward nature remained essentially objective. Democritus dealt with the problem of perception, but he conceived the mind materialistically as effluxes of atoms, entering the body at the sense-organs and giving rise to motions in the atoms of the soul.

Greek Philosophy and Greek Science

Greek philosophy culminated in the teachings and writings of Socrates (469-399 B.C.), Plato (428-348 B.C.), and Aristotle (384-322 B.C.). From the birth of Socrates to the death of Aristotle is a century and a half, but there is nevertheless a personal relationship among the three. Socrates as a philosopher is known almost entirely through the writings of Plato, who, still a young man when Socrates died, was more concerned to develop the

philosophy of his master than to refute it. Aristotle was originally a disciple of Plato's, but Plato did not die until Aristotle was about thirty-eight, and by that time a difference of point of view had become definitely established between them. With Socrates and Plato, interest shifted from the natural world to the world of man. Ethics and society, knowledge and ideas, became the primary philosophical problems. The problems of man, as modern psychology demonstrates, can be dealt with scientifically, but the method of Socrates and Plato was rationalistic. They were more interested in reasoning about nature and in hypothesizing its fundamental principles than in observing it; and indeed, when any distinction can be drawn between philosophy and science, it is presumably this difference between the focusing of interest upon reason and its focalization upon observation. Actually, the difference is never more than a difference of emphasis, for the philosopher cannot ignore experience, nor can the scientist proceed except rationally by way of observed relationship and hypothesis. The difference is real, however, and in some men and in some historical periods it has become very great. Socrates and Plato were in temperament philosophers, not scientists.

Aristotle, who is said to have been in intellect the most remarkable of men, was both a philosopher and a scientist. He wrote extensively on all the sciences of his time, and contributed many original observations. For a thousand years, his word in matters concerning nature was accepted as final. As a scientist, however, he was a compiler and reasoner, even more than an observer. Nevertheless, he described a very large number of animal forms, and laid down a zoölogical classification of the animal kingdom into two main groups with five subdivisions each. Some of his distinctions, like the differentiation of whales from fishes, show considerable insight. By accepting the doctrine of the sphericity of the earth, a doctrine then coming into scientific vogue, he served to establish that view in science. His rejection of the Pythagorean notion of the motion of the earth about the sun fixed this view also for a long period, in spite of the discovery of Aristarchus a very little later that the sun is from 5,000 to 8,000 times as large as the moon, therefore presumably much larger than the earth, and hence more likely to be the center of

the system. Aristotle is best known for his logic and metaphysics, his ethics and politics. In spite of his numerous observations and his many dicta upon natural phenomena, his dominant emphasis was not quite that of the scientist. After all, observational science had hardly yet begun.

In noting in Greek culture the beginnings of a differentiation between science and philosophy, we must not, however, make the mistake of assuming that the difference was marked or even conscious at that time. The history of science and the history of philosophy for this period mention the same names, but with different emphasis. Thales's view that water is the element of all things was as arbitrary a dictum as any of Aristotle's statements. Thales appears as almost the first known scientist because, in the face of the fixed habits of mythological explanation in his time—habits that must have been almost as old as human thought itself—he nevertheless conceived the notion of looking for another kind of explanation of nature within nature itself. In Aristotle's time the exact meaning of an experiment had not been realized, but there had already, especially in astronomy, been a great deal of careful observation for purely theoretical purposes, and there had been some selection of conditions of observation that foreshadowed the experiment. The primacy of observation, however, never fully captured Aristotle's mind.

On the whole, Greek philosophy remained essentially objective, and a sharp distinction between philosophy and science did not appear until a subjective philosophy emerged with John Locke (1632-1704 A.D.).

Thus psychology, as it appears in Greek thought, was neither science nor philosophy in the distinctions of the time. Actually, however, the history of psychology among the Greeks belongs to the history of philosophy, because the psychological method was principally rationalistic. Science and philosophy were both objective in the broadest sense of the word, and this point of view did not lend itself to an observational psychology.

The decline of philosophy after Aristotle is quite naturally paralleled by a decline of science, because of the interpenetration of the two. Since, however, the history of the two is not identical, we must now turn from the account of early science as it appears within philosophy to the early history of the observation of nature.

The Emergence of Observation as Method in Science

Observation is the key to the method of science. Yet observation is not all of science, nor is all observation scientific. Science and philosophy share a common interest in the interpretation of nature in terms of nature itself, and in this they both proceed rationally. Ideally, however, science founds the rational process upon observation; its premises are given externally to itself, are found in nature. Still, the mere observing and recording of natural phenomena do not make science; observation must be selectively crucial in the interests of rational generalization. This statement means that science requires a particular kind of observation. Nowadays we try to express this limitation by saying that scientific observation is 'controlled' or 'experimental,' but the modern notion of the experimental method was not yet clear to the ancient world.

Astronomy is the oldest science. It began in the practical need for the measurement of time and the prediction of events that depend upon time. Day and night are of fundamental importance in human activity, and the time of day is indicated by the position of the sun in the heavens. A longer unit of time is provided by the phases of the moon, and the lunar month was established quite early. The seasonal changes of the year are of the utmost importance in human affairs, and the year was originally marked by the seasons. In Egypt, before the calendar was fixed, it began with the inundation of the Nile. The position of the sun is, of course, the cause of seasonal differences, but it was some time before the exact duration of the seasonal year was worked out in relation to the course of the sun.

One gets certainly the beginnings of scientific observation in the attempts to work out the exact relationships of the day, the lunar month, and the seasonal year—three units of time which do not have exact multiple relations. Both the Egyptians and the Assyrians had established a 360-day year of twelve thirty-day months many centuries before the time of Thales. Quite early they intercalated extra days—five for the Egyptians—in order to obtain greater accuracy. A year of 365 days is, however, about a quarter of a day too short; the seasons would become exactly reversed in about 750 years. It would appear that the ancient Egyptians never succeeded in taking account of this

fact, and that the relation of the seasons to the year must have been quite different in different centuries. However, even this approximate adjustment of the calendar required observation, and the time of the summer solstice, first known by the approximation of the sun to Sirius, was taken into account by the Egyptians in predicting the annual inundation of the Nile. The Assyrians knew the approximate date of the equinox. They also attempted to adjust the lunar month, which is actually about twenty-nine and one-half days, by alternating months of twenty-nine and thirty days. Presumably they then added six more intercalary days in order to keep the year unchanged.

In all this one sees the working-out of general laws by way of observation where knowledge is important in the prediction of human affairs. Science began as technology. There was, however, as yet no interest in the constitution of nature for its own sake. The heavenly bodies were explained mythologically. Mythology was good enough for man when there was no obvious way of improving his welfare by its abandonment. The Assyrian astrologers observed the stars, yet their purpose was not an understanding of the heavens, but the deduction of omens for human affairs. Their practice continued because there was no technique of controlled observation to demonstrate the falsity of their conclusions.

It was much the same with cosmology. With no method for the solution of the problem and no problem of human weal demanding a method, one guess was as good as another. The Egyptians believed that the earth was a flat or slightly concave surface, somewhat elongated, supporting the sky upon pillars at the extremes.

We have seen that it was Thales who turned attention from the accounts of mythology and from the problems of human weal to the theoretical problem of the constitution of nature itself. Thales was an astronomer, and, according to legend, his contemporary fame was based upon his prediction of an eclipse of the sun which, occurring during a battle, brought peace between the Medes and the Lydians (585 B.C.). It has always been thus. Science progresses most when it can disregard practical ends, but the practical values are nevertheless its ultimate social sanction. Thales could not have predicted the eclipse accurately; the point is rather that he was famous because he was believed to have predicted it. Thales, however, did not contribute to

observational method. He accepted the world as flat. He rejected mythology, and guessed instead that we should get somewhere if we conceived of everything as composed ultimately of water. One of his pupils constructed a sun-dial; another guessed (with some truth!) that the earth was evolved from a chaos and man from fish.

Pythagoras was the first person to hold that the earth is a sphere and to suggest that it might not be fixed, but might move about the sun. Parmenides also believed in a spherical earth. The sphericity of the earth might have been inferred from eclipses, but the Pythagorean doctrine was presumably merely a correct belief based on the insufficient ground that the circle is the perfect figure. We have not yet come to scientific observation.

A little later, with Anaxagoras (*ca.* 499-429 B.C.), we get a hint of the coming scientific attitude. Although he believed that the earth is flat, he offered nevertheless a correct explanation of eclipses. He was thus forced to a belief that the moon's light comes from the sun, and he saw that the moon's phases would be explained if the moon itself were spherical. The sun, the source of light, he described as a great mass of burning iron, and he may have fixed upon iron, not only because when white-hot it is similar to the sun, but also because iron is a prominent constituent of meteors. Anaxagoras is known to have been much interested in a fallen meteor. This is the stuff—reasoning based on observation—of which science is made.

Anaxagoras might, of course, have inferred the earth's sphericity from lunar eclipses. He did not, and it remained for Aristotle to accept the notion that the earth is a sphere, to point to eclipses as supporting the view, and thus by his authority to fix for some time the notion that the earth is round.

At this time astronomy must have been advancing, even more than we have records to show. It is not until about a hundred years after Aristotle (384-322 B.C.), however, that we meet with important discoveries that are properly to be called experiments. In the third century B.C., Aristarchus measured the relative distances between the earth, sun, and moon, and Eratosthenes measured the size of the earth.

Aristarchus (*ca.* 280 B.C.) planned an experiment. He observed the angle between the sun and the moon when the phase of the moon is exactly at half. This measurement was, however, some-

thing more than a mere observation: it was a planned, crucial observation. If one considers the triangle formed by the sun, the moon, and the observer on the earth when the moon's phase is at half, it is plain that the angle at the moon must be a right angle. If, as seen from the earth, the sun illumines one half of the moon and not the other half, the line from the moon to the

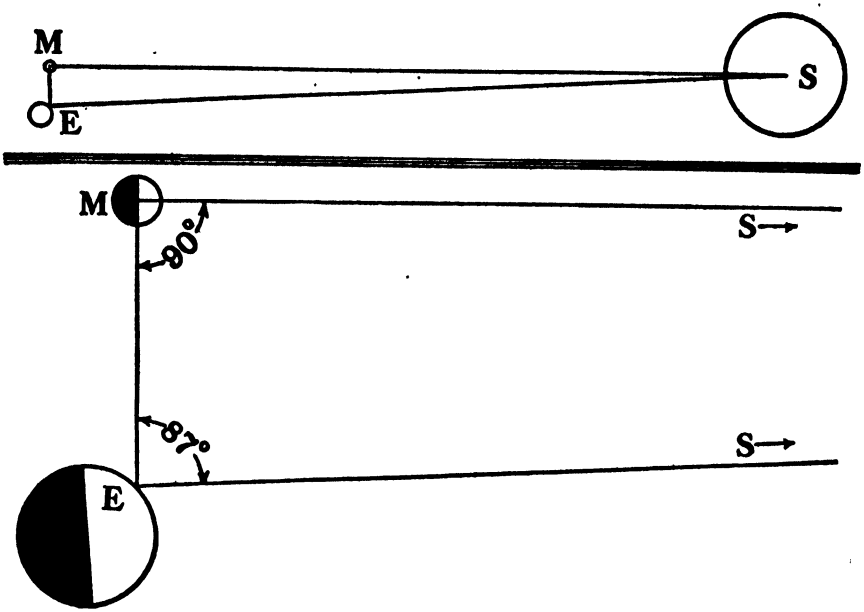

FIG. 1. EXPERIMENT OF ARISTARCHUS. S = sun; M = moon, seen from the earth at half phase; E = earth. The upper half of the figure shows the relationships approximately to the scale of Aristarchus's proportions for the distances and diameters of the three bodies. The lower half of the figure, drawn to a larger scale, shows why the angle EMS would be a right angle when the moon is at half phase. Aristarchus on the earth measured the angle MES as 87°. The relationships of the upper diagram are such as to suggest a heliocentric, rather than a geocentric, theory. The diagram is suggested by the figure in H. S. Williams, *History of Science*, I, 218, which is not, however, in the proper proportions.

sun must be perpendicular to the line from the moon to the earth. With this angle a right angle and the angle at the earth observed, the remaining angle is determined, and thus the form, though not the size, of the triangle becomes known. Aristarchus measured this crucial angle as 87° and concluded that the distance to the sun is between eighteen and twenty times the distance to the moon. Actually the ratio is nearer 400 than twenty, for Aristarchus was in error in his measurement almost a whole degree. The point to note is that the measurement was an experiment and that it

gave the first information of this sort that mankind had ever had.

With these relative distances known, Aristarchus could next compute from the measurements of shadows cast in eclipses the relative size of the earth, sun, and moon. In this way he came to the conclusion that the bulk of the earth is between fifteen and thirty times the bulk of the moon, and the bulk of the sun between 250 and 350 times the bulk of the earth. The ratios are much too small, largely because of the original error in the distances; to the minds of that time, which had always supposed that both the sun and the moon were relatively small bodies moving about the earth, the conclusion must have been startling. It led Aristarchus, at any rate, to a heliocentric view of the universe. It was absurd to suppose that a little body like the earth was the center about which the sun, a great body perhaps 300 times its size, rotated; it was much more likely that the earth revolved on its axis daily and encircled the sun yearly. Thus Aristarchus in modern times has been called the Copernicus of antiquity.

Had Aristarchus known the absolute size of the earth, he could have computed the sizes of the sun and moon. The first determination of the size of the earth was made, however, by a contemporary, and the fact was not known to Aristarchus. This man was Eratosthenes (*ca.* 273-192 B.C.), who also planned an experiment. Eratosthenes is said to have learned that the town of Syene was 5,000 stadia directly south of Alexandria, and that in Syene at noon on the day of the summer solstice a vertical rod cast no shadow and a deep well was illumined to the bottom. In other words, Syene lay on the tropic of Cancer. Eratosthenes's experiment consisted in choosing noon of the summer solstice to measure the angle of the sun at Alexandria. The measurement of the angle is very simple, since it depends on the relation of the length of the shadow cast by a vertical rod to the length of the rod itself. He thus knew the angle of the sun at Alexandria at the instant the sun was vertical at Syene, and he knew the distance between the two points. A very simple geometrical construction shows that the angle measured at Alexandria is also the angle at the center of the earth which 5,000 stadia subtend, and thus he computed the circumference as 252,000 stadia, which probably represents about 24,662 miles.

It seems to the author that these two experiments mark the actual achievement of the scientific attitude and the scientific method. They will serve later as examples of the business of science. It is worth while, then, to stop long enough to note just what they mean.

In the first place, we see that both experiments depended upon

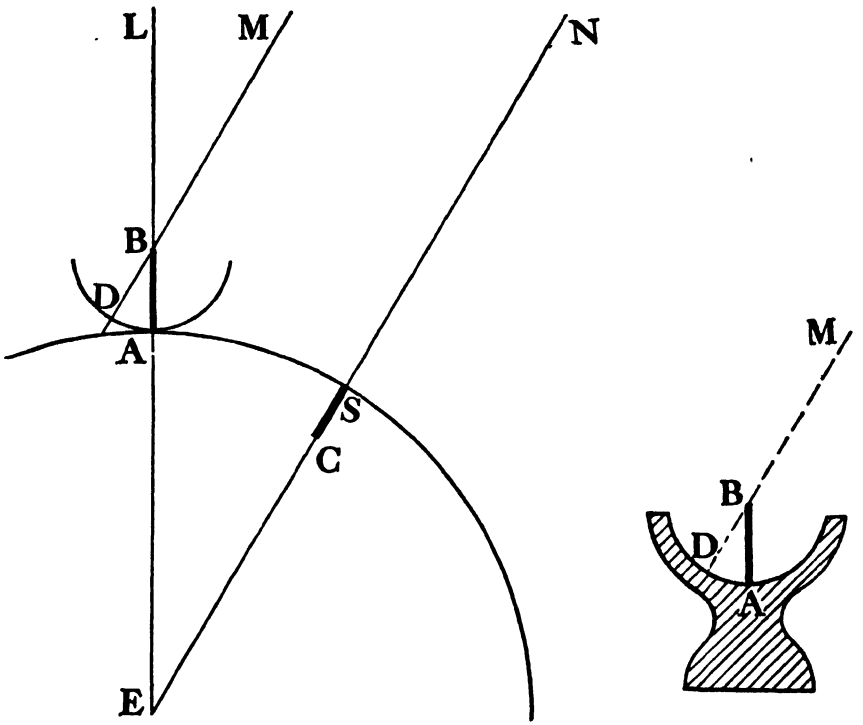

FIG. 2. EXPERIMENT OF ERATOSTHENES. E = center of the earth; A = point on the surface of the earth at Alexandria; S = point on the surface of the earth at Syene; MBD and NSC = sun's rays; AB = the gnomen which casts a shadow; SC = well, illumined when sun is directly overhead; LBAE and NSCE = verticals, i.e., extensions of the earth's radius. Eratosthenes measured the angle ABD, which is equal to the angle AES, i.e., the angle at the center of the earth subtending an arc of 5,000 stadia, since AS = 5,000 stadia. At the right is shown the gnomen which Aristarchus probably used. AB is vertical, and the crucial angle ABD is measured by the shadow AD which AB casts in the bowl. Modified from H. S. Williams, *History of Science*, I, 230.

the knowledge of the times. Both drew upon the geometry of Pythagoras and Euclid (323-285 B.C.). Thales is said to have measured the distance of a ship at sea by determining two sighting angles at the extremes of a base line on the shore. Eratosthenes could not well have done without a knowledge of the distance between Alexandria and Syene with which Ptolemy Soter's

surveying provided him. Like many great scientific discoveries, these experiments were little more than simple applications of available knowledge to a novel situation.

In the second place, we must observe that the experiments also depended upon that body of information that is half knowledge and half opinion. They assumed the sphericity of the moon, earth, and sun, the accepted doctrine of the time. The moon's phases indicated the sphericity of the moon; lunar eclipses signified the sphericity of the earth; if the moon and earth are spherical, there is raised a strong presumption for the sphericity of the sun. Nevertheless, one can hardly say that the sphericity of these bodies had been proved. It was current belief and it formed a premise in the experiments. Most scientific experiments thus depend upon some unproven premises, subsequent refutation of which would invalidate the experiment. These premises are frequently part of the current thought of the period or scientific school of the experimenter, and he is often unaware of the assumptions he makes. For this reason new discoveries always have a certain tentativeness. The method of science is not a sure, inevitable reasoning from observation alone.

Third, it is clear both that science is observational and that the observation is selective. Aristarchus and Eratosthenes were experimentalists. There was no blind compilation of facts, but a careful selection of crucial conditions of observation. It is this attention to the conditions of observation that marks off scientists from philosophers.

Fourth, we see that science is quite as rational as philosophy. While it depends for the initiation of its method upon controlled observation, it proceeds to its end by a rational process. It is not even correct to say that reasoning is secondary to observation. If these two experimenters had simply measured angles and done nothing more, they would have done nothing of significance.

Finally, we may not neglect the especial rôle of mathematics and measurement in these experiments. There is undoubtedly a qualitative science: Anaxagoras's observation, that the form of the moon's phases indicates that the moon is a sphere, was qualitative. Productive science, nevertheless, tends to become experimental, and experimental science to become quantitative. It is no mere accident that these first two important astronomical experiments made use of mathematics in the interest of measure-

ment. Measurement provides a precision of differentiation and definition in observation that can be had in no other way; mathematics provides the necessary means of carrying measurements through a logical development to their consequences without loss of their precision.

The development of astronomy under the Grecian influence after the time of Aristarchus and Eratosthenes need not concern us. Hipparchus (*ca.* 160-125 B.C.) was a great astronomer. He rejected, unfortunately, Aristarchus's heliocentric view of the relation of the sun and the earth, and developed the theory of cycles and epicycles to account for the sun's motion. He measured the solar year with an error of only seven minutes. Strabo (63 B.C.-24 A.D.) was a great geographer, assuming in his geography the sphericity of the earth. Pliny (23-79 A.D.) was a great expositor of science. Claudius Ptolemy (*ca.* 150 A.D.) was a great expositor and encyclopedist of astronomy who derived most of his views from Hipparchus. He is the Ptolemy who has lent his name to the geocentric view of the universe. Science, however, was already on the decline, and there is no event in the field of astronomy more significant to us than the experiments of Aristarchus and Eratosthenes.

Early Physics and Biology

The development of a scientific physics lagged behind the development of astronomy, and biology lagged behind physics.

There was no scientific physics in ancient Egypt or Assyria. Some practical mechanical principles were known. Presumably levers, simple pulleys, and inclined planes were used in the erection of the pyramids (*ca.* 4000 B.C.). We have seen that speculation as to the nature of matter began with Thales and Pythagoras. Anaxagoras practically formulated the atomic theory, but the view is ascribed to Democritus, who emphasized the nature of the atom more clearly and gave it its name. Archimedes (287-212 B.C.) is the first well-known experimental physicist. He was a contemporary of Aristarchus and Eratosthenes. The culture of the Alexandrian period was favorable to experimental work.

Archimedes's primary interest was in mathematics. He worked out many complex geometrical relations, including the relation of the sphere to the cylinder, the figure for which he asked to

have placed upon his tomb. He determined approximately the ratio of the circumference of a circle to its diameter, and he used a crude kind of logarithms in one of his elaborate computations. Archimedes is famous, however, not for his mathematics, but for his mechanics. He is called the father of mechanics. He discovered the law of the lever, and at the command of King Hiero of Syracuse utilized this principle, and possibly also the principle of the compound pulley, in engines of war that hurled great missiles upon the attacking fleet of Marcellus of Rome, and also, it would seem, in a mechanical contrivance by which Marcellus's galleys could be lifted prow in air with upright keel and then let fall to their destruction. The story of his discovery in his bath of the hydrostatic principle that bears his name is known to every one. It is not so generally known that the principle was applied to the problem that motivated its discovery, and that King Hiero's crown was found to have been adulterated by the goldsmith with silver. For all his practical work, Archimedes was not a technologist. The applications of his discoveries in mechanics were made at the command of his king and not because he was primarily interested in them. He even refused to describe them in a book. We do not know certainly that he worked by the experimental method. It seems impossible, however, that a man of his inventive genius could have resisted the experimental verification of his beliefs, or that he could have established them so accurately as laws without empirical test. At any rate, his practical inventions were experiments, demonstrating the truth of his contentions.

One sees the difference between an inventor and a scientist if one contrasts Archimedes with Heron (*ca.* 100 B.C.). Heron invented a device for opening the doors of a temple when the fire on the altar was lighted, a slot-machine for giving a squirt of sacred water when a five-drachma piece was inserted, a toy steam-engine, and many other unique and mysterious contrivances; but he made no notable contribution to science. These devices were not experiments because they bore no crucial relation to the nature of the fundamental principles involved; they were made simply to amuse and mystify.

The development of scientific biology was undoubtedly held back by the difficulties of scientific experimentation. In Egypt and Assyria, medicine was not divorced from magic. Surgery was practised in Assyria, but in both countries dissection of the human

body was strictly forbidden, and adequate knowledge was impossible. In the light of modern information, one notes that some of the biological guesses of Pythagoras, Empedocles, and Anaxagoras were extremely shrewd, but they were not scientific except in so far as they departed from magic and sought an understanding of the organism in natural terms. Nevertheless, medicine developed by trial and error, and the dissection of animals was allowed, although dissection of the human body was forbidden in the early Grecian colonies. Hippocrates (*ca.* 460-370 B.C.), called the father of medicine, knew a great deal of human anatomy for his time, and possessed some skill in surgery and great skill in prognosis and medicine. It was in the Alexandrian period, so favorable to science, that dissections of the human body were first allowed. It is even said that condemned criminals were then turned over to the scientists for experimentation. In this period, the work of Herophilus and Erasistratus (300-200 B.C.) is preeminent. They discovered that the nerves lead to the brain and spinal cord, and distinguished between sensory and motor impulses which they inferred the nerves carried. They learned a great deal of the anatomy of the eye. They described the ventricles of the brain, and suggested that one of them might be the seat of the soul. After their period human dissection was again forbidden, and there is no very famous name until Galen (*ca.* 129-199 A.D.). He performed experiments upon animals, and even demonstrated the function of motor nerves by sectioning them. His knowledge of anatomy was great, but his interpretation of the function of the organs was faulty.

It is hard to say why biology did not progress further and more surely. The answer to the question would undoubtedly be of importance to psychology. Experimental biology has always been behind experimental physics and astronomy, and experimental psychology (except so far as it is included in experimental biology) has lagged behind biology—was born much later, and so remained junior. One gets among the Greeks a hint of the trouble. Biology is more personal than physics or astronomy. It may require some courage to regard the stars objectively, divorcing them from their rôle as omens of human weal and woe, but it takes less courage than it does to regard the human organism, the stuff which seems to be our very selves, with complete objectivity. Only in the most enlightened period could men agree

to the dissection of dead human bodies. The appeal to mysticism is so strong because the body is so personal, and because it is the seat of that even more personal entity, the soul. It has been said that the tardy development of biology is the result of the tremendous complexity of the organism. Physical nature is, however, also very complex. It is more probable that the beliefs that permitted the measurement of the stars and prevented dissection of the human body are responsible for the retardation, first of scientific biology, and much later of experimental psychology.

The Rise of Modern Science

Greek science was at its height in the third and fourth centuries B.C. After that it declined, and the reëmergence in Western Europe of the scientific attitude with anything like effective productive power was not until almost 2,000 years later in the sixteenth century, the century of Copernicus and Galileo.

It is not to be supposed that in this long intervening period there was no science in the world. There was a great development in mathematics, astronomy, physics, and medicine in the Byzantine and Moslem civilizations (*ca.* 500-1300). Chinese science assumed importance in the third century and even earlier. All through the East there was progress. It was only in the West that the 'dark ages' (*ca.* 500-1200) were really dark, and that perhaps mostly in respect of science. There civilization paused to assimilate the victorious barbarian before it continued its natural line of development. Undoubtedly the introduction of new strains into the peoples of the West was of advantage in the long run, but meanwhile science remained dormant. The golden age of the East dropped out of history, only recently to be discovered in anything like its true value, because it does not belong in Western history. The West did not know about the East and could not found its new structure upon the Eastern base.

In the thirteenth century Western science began to reappear; nevertheless the medieval milieu (*ca.* 1200-1500) was not favorable to scientific progress. In these centuries the revival of learning was centered almost entirely in theology, and the resultant frame of mind was unfavorable to science. Science and religion are both founded upon faith. In science the fixed faith is in observation, and the temporary faiths are in the hypotheses

of the age. Religion requires a fundamental faith in God and many secondary beliefs that may belong to an age. The two appear to be similar. Actually, however, there is a sharp contrast between them. Both religion and science develop by way of change of belief or hypothesis, but religion has always, whether necessarily or no, been more resistant to change than science. Moreover, religion revises its beliefs by a rationalistic process, whereas science finds its revisions given in empirical observation and experiment. With change thus given in science, progress becomes the chief scientific characteristic. Religion centers about orthodoxy and is conservative. The dominance of the one can hardly promote the other.

The definite resurgence of science in the West occurred about the sixteenth century. Three centuries earlier, Roger Bacon (1214-1292) anticipated the coming age by his insistence upon experimental observation as the fundamental method for the investigation of truth, thus laying the ground for the formal recognition of experiment as the basal method of science. He was also an investigator of undisputed originality, as well as an alchemist. He lived, however, long before his time, and his espousal of free empirical inquiry was so out of harmony with the religious spirit of the times that he was forbidden to write, and even imprisoned. The new age actually began with Copernicus, although Leonardo da Vinci preceded him.

Da Vinci (1452-1519) is best known as an artist, but he combined the spirit of scientific inquiry with his art. He asserted before Copernicus, and by way of a mathematical calculation, the movement of the earth about the sun; he invented a 'steam-gun' that fired a heavy ball a considerable distance; he described the *camera obscura* and acoustic resonance; and, in a sense, he began geology by making the inference that fossil shells found on mountains indicate that the substance of the mountains must once have been at the bottom of the sea. He observed that objects near the eyes, when viewed binocularly, appeared to be "transparent," thus noting the facts of binocular parallax and only just missing the related fact of the disparity of the ocular images which provided the key to stereoscopic vision. Da Vinci is in no sense the founder of modern science, but the fact that his scientific genius could flourish unhampered shows that civilization

was at last ready, though at first a little reluctantly, to accept the scientific method and to assimilate the resultant truths.

Copernicus (1473-1543), as we all know, is responsible for the overthrow of the Ptolemaic cosmology and the establishment of the heliocentric view of the universe. The heliocentric view had been stated on experimental evidence by Aristarchus almost 2,000 years earlier; it had been stated recently by da Vinci; but it required more than incidental argument to establish it against accepted belief of theological doctrine. Copernicus dealt with the theory thoroughly and forcefully, with the entire weight of careful elaboration throughout a long period of years. His work was published only at the time of his death and it may be that he thus escaped persecution for it. For all that science is empirical, it is only gradually that strong scientific traditions can be radically altered, and Copernicus was not immediately successful. The great astronomer, Tycho Brahe (1546-1601), who immediately followed him, sought to confute him. Fortunately for scientific progress, Kepler (1571-1630) and Galileo (1564-1642), whose work was only a little later than Tycho Brahe's, accepted the Copernican view and within this system contributed notably to astronomical discovery. By the time of Newton (1642-1727), who was born in the year of Galileo's death, the Ptolemaic view had passed into history.

With Galileo the renaissance in physics also began. His dramatic experiment in dropping the weights from the tower at Pisa is known to every one. His important contributions all dealt with falling bodies; he experimented with falling bodies in a vacuum and determined the laws of the pendulum and of the projectile, which are special cases of falling bodies. After him, names known to every student of elementary physics follow in rapid succession: Torricelli (1608-1647), Boyle (1627-1691), Mariotte (1620-1684), Hooke (1635-1703), Huygens (1629-1695), Newton (1642-1727), and the ever-increasing list from Newton down to the present time. With Galileo the new physics came to stay, and it is hard, in modern civilization, to imagine a point of view in which the method and resultant facts of physics would be regarded as representing anything other than truth.

The same period witnessed the emergence of the biological sciences. Medicine, we have seen, flourished under Moslem and Byzantine culture. Paracelsus (1493-1541), an egoistic dogmatist,

was undoubtedly the greatest physician since Galen, and there were many other famous physicians of the sixteenth and seventeenth centuries. At this time experimental anatomy, divorced from the immediate interest in disease, appeared. Vesalius (1514-1564) was the author of the first comprehensive work on human anatomy; Harvey (1578-1657) is famous for his experimental demonstration of the circulation of the blood. These names are but two among several that typify the progress of the period. Von Leeuwenhoek (1632-1723) discovered and described bacteria —millions of microscopic animals "in the white matter between the teeth," "more than there are human beings in the united Netherlands." Linnæus (1707-1778), the author of the great classifications of plants and of animals, is in this sense the father of modern botany and zoölogy. It would be irrelevant to our present purpose to go into the details of this development; it is enough for us to see that, as physical science gradually tore itself loose from theological restrictions, the new freedom gained for the experimental method led at once to its fruitful application to biological problems, and to the rise of the biological sciences as independent of physics, on the one hand, and as relatively independent of therapeutics, on the other.

In this epitome of the epoch of Galileo, we have had nothing to say of philosophy. Science and philosophy, as we have seen, began together, but modern science is far from philosophy. "It is predominantly an anti-rationalistic movement, based upon a naïve faith. What reasoning it has wanted has been borrowed from mathematics which is a surviving relic of Greek rationalism following the deductive method. Science repudiates philosophy." Thus to speak is not to justify the fact. It may be that there are signs in the present century of a *rapprochement* between the two. But, if we take science as we find it, there is no escaping the fact that the scientific age has accomplished its results, influenced unconsciously by philosophy and accepting intuitively a philosophic position, but without any conscious reliance at all upon philosophy. It is for this reason that science seems to have progressed most among the Greeks as it became differentiated from philosophy; it is simply that this differentiation was the beginning of the path along which the centuries have led.

In the history of the scientific epoch of which we are speaking it is customary to include the names of some great philosophers:

Francis Bacon, Descartes, and Leibnitz. These men had a great influence upon scientific thought, but they were not important scientists, nor as important for science as the men who have been cited.

Bacon (1561-1626) emphasized the importance of observation, experiment, and induction as the scientific method of the *Novum Organum,* as opposed to the *antiquum organum* of Aristotle. Bacon was a contemporary of Galileo and thus furnished philosophical justification for the scientific reaction away from scholasticism. He was not an experimentalist, although he outlined some ingenious experiments.

Descartes (1596-1650) lived but a few years later. His influence upon science in general, and upon psychology in particular, was to establish them upon mechanistic principles. He was both a theorist and an experimenter in science. His most important contribution to physics was his formulation of the laws of motion, for Newton's first law of motion appears to have been anticipated by Descartes. He lacked, however, the experimental rigor of his contemporary Galileo, and the validity of his laws suffered accordingly. He undertook some physiological experimentation, and pictured the human body as a machine with the muscles as engines, the nerves as pipes through which flow the animal spirits from their fountain in the heart, and the brain as the central office of control. While the influence of such a view upon psychology is obvious, it hardly required Descartes to render physics mechanistic. In general he was too much given to the *a priori* method to represent the new science.

Leibnitz (1646-1716), a contemporary of Newton and John Locke, was not a scientist at all, although his theory of the constitution of the universe of monads affected scientific thought. Probably more important is his discovery of the calculus, destined to become a universal scientific tool.

No consideration of this period is complete without mention of astrology and alchemy, which have sometimes been called the 'pseudo-sciences.' Both are as old as science itself. The transmutation of mercury into gold by the aid of certain powders was seriously claimed as late as 1782 by James Price, an English physician, and a nineteenth century astrologer made much of the astrological predictions of the death of George IV in 1830. Not all the alchemists and astrologers were charlatans. Roger

Bacon was an alchemist, and Tycho Brahe an astrologer. Many scientific astronomers practised astrology quietly in order to obtain a living. In our day, the place of alchemy and astrology is taken by psychic research. The *a priori* belief that furnishes the compelling motive for all three kinds of research is external to observation itself. If it be said that the same is true of science, then it can only be replied that in the case of science the belief has been justified by its productivity, whereas the 'pseudo-sciences' have been sterile, yielding only occasionally a contribution to knowledge, and that rather as a by-product than in resolution of the original problem.

The Development of Biological Science

We must not follow the development of the physical sciences further, nor examine the great impetus given them by Newton, nor observe the separation of chemistry and geology from astronomy and physics. We have seen first the emergence of the empirical notion (Thales) and then the establishment of the appeal to observation, experiment, and induction, which, by a frequent resort to mathematics, makes up what has come to be considered the scientific method. At the beginning of the seventeenth century, the time of Newton, physical science was already well started on the remarkable course it was to pursue during the next two centuries. It had achieved its fundamental method and point of view, and could pursue its productive way, oblivious of theological opposition or of its own philosophical assumptions, on to such achievement as is now a matter of historical record.

Our own path leads to physiology and biological science. We have been concerned with physical science because it is there that the scientific point of view was achieved, and it is there that the type for science was determined. It may be objected that psychology, or even biology, has made a mistake in attempting to pattern itself upon physics, but such objection is not here in point. The actual fact is that science was what physics and astronomy were. Scientific physiology developed because physics had provided a method for it, and physiology was scientific because it held to this method. Later we shall see that physiology gave birth to experimental psychology, at first called 'physio-

logical psychology,' in much the same way that physical science gave birth to physiology.

It is, of course, experimental physics that set the stage for experimental physiology, for the problems of the organization and functions of the body are quite as old as the problem of nature. What biology needed was the method of observation, experiment, and induction. We have already seen how this began. First there was medicine, even in Egypt and Babylonia. In the Alexandrian period dissections and probably vivisections were allowed, and anatomy and physiology flourished. Hippocrates (*ca.* 460-370 B.C.) and Galen (*ca.* 129-199 A.D.) were the great physicians of antiquity, and Galen at least was an experimentalist. We begin to feel the need for distinguishing scientific anatomy from the art of medicine about the time of Vesalius (1514-1564) and experimental physiology certainly comes into its own with Harvey (1578-1657). Linnæus (1707-1778), the great classifier for botany and zoölogy, takes us past Newton's time.

A contemporary of Linnæus was Albrecht von Haller (1708-1777). He was a precocious youth, a brilliant and versatile man, and has sometimes been styled "The Great." He is best known for his doctrine of irritability, having laid down the principle that the substance of muscles is capable of contracting when stimulated independently of energy conducted to it by the nerves or other artificial stimulus. He demonstrated his thesis experimentally, performed one hundred and ninety experiments to determine which tissues of the body are irritable, and studied the problem further in the developing embryo of the chick. Since only living tissues are irritable, this discovery seemed to give a hint as to the nature of life.

Another item about the nature of life appeared a little later when it was discovered that digestion is a chemical process that affects dead, but not living tissue. Spallanzani (1729-1799) made this discovery experimentally about 1777. John Hunter (1728-1793), a brilliant surgeon and anatomist, a pioneer in research on the lymphatic system, the discoverer of the vicarious functioning of the collateral branches of arteries when the main trunk is stopped and thus of the surgical cure of aneurisms, at first opposed Spallanzani's view, holding to the older belief that digestion depends upon the mechanical triturating effect of the contractions of the stomach. Hunter, to his everlasting credit,

instead of sticking to his guns, like many famous men to whom personal consistency is more important than empirical proof, presently became convinced of the correctness of Spallanzani's demonstration, and thereafter contributed notably to the extension of the theory. Among other things he observed that the stomachic lesions, so often found in post-mortem examinations, may be due to the digestion of the stomach itself by its own juices, which act upon dead but not upon living tissue.

Another great advance came a little later in the knowledge of the nature and structure of tissue. Bichat (1771-1802), who first applied the word *tissue* to the bodily materials, formulated a classification of these types of living substance. He also noted that the body can be divided organically into two systems, a voluntary and an involuntary system. It was not, however, until about 1830 that the compound microscope was sufficiently perfected by the reduction of chromatic and spherical aberration to make possible the necessary definition under the high magnification required for the direct observation of cells and cell-structure. Lister (1786-1869), an amateur optician to whom is due the chief credit for the improvement of the microscope (not the Lister famous for antiseptic surgery), described the red blood corpuscles and the cells of animal and plant tissue in a paper in 1830. The new optical powers of the microscope were destined at once to yield important results. Robert Brown (1773-1858), the discoverer of 'Brownian movement,' was first to describe (1833) cell-nuclei in plant tissue. Schwann (1810-1882) showed in 1839 that all cells of animal and plant tissue have nuclei. Even then it was only gradually that interest in the nature of living substance shifted from the cell-wall and the cytoplasm to the nucleus.

We have now reached the period of Johannes Müller (1801-1858) and Claude Bernard (1815-1878), two great experimental physiologists of the first part of the nineteenth century, but again we must stop. The period from Haller to Claude Bernard carries us over from the establishment of the scientific method in physics to the active period of the experimental physiology of the nervous system, which we shall consider in the next chapter. Johannes Müller, whose pupil Schwann was, we shall meet there. But before passing on we must take note of the rise of the theory of evolution and other doctrines related to it.

The eighteenth century believed in the special creation of every species, that is to say of "every living creature after his kind"—a belief quite consistent with the Biblical account of the creation of animal life and its preservation in the ark at the time of the deluge. There were occasional men who toyed with the idea of the development of one species from another and of intermediate forms, but no real progress toward the solution of the problem of the origin of species was made during the century. Toward the end of the century the problem had become acute, for geographical exploration had so multiplied the number of known species that it was no longer possible to conceive that Noah had crowded a pair of each into the ark.

The first steps toward the solution came from Goethe, the poet, whose flair for scientific observation is not very common knowledge, and from Erasmus Darwin, the grandfather of Charles Darwin. Goethe (1744-1832) is responsible for the theory of the metamorphosis of parts (1790). As the result of a great deal of careful observation of plants, he came to the conclusion that, apparently, different forms of a plant can be created by the change of one part into another form: for example, in the double flower, stamens of the single flower may have been transformed into petals. Later Goethe participated in extending the theory to animal life, suggesting, for example, that the vertebrate skull is essentially a modified and developed vertebra. Erasmus Darwin (1721-1802) independently formulated the notion of the transmutation of species. The idea is similar to Goethe's and touches more directly upon the problem of the origin of species. Erasmus Darwin was, however, a lover of nature who rimed about it, and his views lost force when presented in verse. Nevertheless the idea was there at the end of the eighteenth century, when most thoughtful men took the special creation of species for granted.

The first great figure in the history of the theory of evolution is Lamarck (1744-1829), who belongs in the nineteenth century because he first published his views on this topic in 1809. A Lamarckian nowadays is a man who believes in the inheritance of acquired characteristics, but what Lamarck stressed was the modification of animal form through effort on the part of the animal further to adapt to his environment, and the inheritance of the modified characteristics thus acquired. If a short-legged

bird gains a living by fishing, standing in shallow water, it will, when the fish are scarce near shore, wade in just as far as the length of its legs allows. Lamarck's belief was that, by thus trying ever to go deeper, it would stretch its legs a tiny amount, perhaps strengthening them by straining them and thus causing them to grow a bit longer. If its progeny inherit this tiny increment and continue the same adaptive effort, the cumulative result would be a long-legged bird like a heron.

In this view Lamarck was vigorously combated by Cuvier (1769-1832), who argued for fixity of species. Cuvier had earlier enunciated the doctrine of the correlation of parts, the doctrine that the adaptation of a species to its environment and mode of living was reflected in certain relationships of use among its parts. Thus a flesh-eating animal must have not only sharp teeth, but also legs for the swift movement of attack. An animal that carries its prey in its mouth must have not only strong teeth, but also strong neck-muscles. Cuvier claimed that by a knowledge of these relationships the form of an entire animal could be reconstructed from a single fossil bone, but this extreme boast has never been made good. Cuvier thought, of course, of the adaptation of the species as given in its creation, and not as acquired through its own adaptive efforts. Although Lamarck seems to have made the more thoughtful argument, Cuvier's influence was so great as effectually to hinder a wide acceptance of the Lamarckian view.

To Charles Darwin (1809-1882) belongs the credit for the far-reaching and plausible theory of the evolutionary origin of species, which took the world by storm, raised immediately a tempest of protest, and is considered by many as the greatest scientific achievement of the century. Like many great ideas, it is extremely simple when viewed in retrospect; its greatness lies in its adequacy, its novelty, and, like Thales's idea of explaining nature by an appeal to nature itself, in the degree to which it ran counter to the accepted belief of the time. The fact of variability among individual animals of the same species is obvious. Darwin believed that this spontaneous variability was sometimes of such a nature as to be inheritable. By selecting in accordance with such 'chance' variation animal breeders can, cumulatively through many generations, produce great changes in a breed. Presumably, therefore, in nature we have a natural selection by way of the

survival of the animals best fitted for their environment and requirements of living. Thus ultimately, in æons of time, natural selection taken together with gradually changing environmental conditions may give rise to new species. The species are not sc discrete as was originally supposed; they form continuous orders; but their present relative discreteness means simply that, in a cross-section of time, the various animal forms exist as approximately adapted to the conditions under which they live. Darwin's theory differed radically from Lamarck's only (1) in that it assumed that inheritable variation occurs spontaneously or by chance rather than by the adaptive effort of the organism, (2) in that it recognized that selection is natural to the struggle for existence in which all animals engage (Darwin had been impressed by Malthus's theory of the limitation of population by natural conditions), and (3) in that the substitution of the chances of variation and of survival for definitely directed adaptive effort made it necessary to assume a much longer time for the emergence of a new species.

It is important to notice that there is no essential incompatibility between the theories of Darwin and Lamarck. To call the inheritable variation spontaneous or 'chance' is not to forbid its explanation by adaptive effort, and Herbert Spencer (1820-1903) championed this synthetic view. *Chance* is generally a term for ignorance of causes where a belief in the existence of determinative cause still persists. The Lamarckian view would, therefore, seem to complete the Darwinian theory. Weismann (1834-1914), however, in 1883 seriously challenged the belief in inheritance of any acquired characteristics, and the issue between Weismann and Lamarck is still to-day in dispute. The issue is not easy of experimental resolution, since even Lamarck supposed that a single generation would show only an almost infinitesimal increment of change.

Darwin's great work, *The Origin of Species,* was published in 1859. The compendious and elaborately revised notes upon which it was based he began to make in 1837 as a young man of twenty-eight in his trip around the world in the *Beagle.* He came rather quickly, as the result of his observations, to the theory in outline. In 1844 he made a manuscript abstract of the conception, but stored it away, with provision for its publication in event of his death, while he collected more observations and revised his notes

again and again. And then, just as he was preparing to publish, his friend Alfred Russell Wallace (1823-1913) sent him for publication a paper putting forth the same view in brief, though without the same full observational background. Darwin published Wallace's paper and his own simultaneously, and his book the following year. In the storm of criticism that followed, Darwin remained imperturbable, ably defended by many men of science, of whom Herbert Spencer and Thomas Henry Huxley (1825-1895) were perhaps the most notable.

Notes

The most complete and scholarly encyclopedia of the history of science is G. Sarton, *Introduction to the History of Science*, 1927, vol. I, which covers the period from early Greek and Hebrew knowledge to the end of the eleventh century. Later volumes are not yet published. This book is a work for reference and not for continuous reading. For more popular presentations, the reader may consult H. S. Williams, *History of Science*, 1904, I, chaps. 5-7; W. T. Sedgwick and H. W. Tyler, *Short History of Science*, 1921, chaps. 3, 4; W. Whewell, *History of the Inductive Sciences*, 1837, I, bk. i. The histories of philosophy give the full information about the period, but are less germane to the present purpose. The ten volumes of the *Science-History of the Universe*, edited by F. Rolt-Wheeler, 1909, should be mentioned, but they present science less from the historical point of view than from that of the elementary handbook.

The Emergence of Observation as Method in Science

In general, see Williams, *op. cit.*, I, chaps. 2, 3, 7-10; Sedgwick and Tyler, *op. cit.*, chaps. 5, 6; Whewell, *op. cit.*, I, bk. ii.

That Aristarchus held the heliocentric view with some assurance appears from the ascription of it to him by Archimedes in citing Aristarchus's "writing against the astronomers"; cf. Williams, *op. cit.*, I, 212ff; Sarton, *op. cit.*, 156. Archimedes, however, accepted the geocentric view of Aristotle; Hipparchus later accepted Aristotle; and finally Ptolemy fixed the view until the time of Copernicus, 1,500 years later.

The angle which Aristarchus sought to measure, 87°, is nearly a right angle, and is a difficult angle to observe accurately. The true angle is more nearly 87° 52′, and at this angle a slight deviation makes a tremendous difference in the distances. Aristarchus was aware of this fact, and in all cases stated his measures as lying between certain rather divergent limits—in a way, a statement of probable error. Nevertheless, he estimated the relative distance to the sun as much too small, and consequently the diameter of the sun also appeared too small. Had his measurement been more exact or his error in the other direction, the grounds for the heliocentric theory would have been strengthened.

In Fig. 1 Aristarchus's proportions are given approximately by the numbers: diameter of moon = 1; diameter of earth = 3; diameter of sun = 19; distance from earth to moon = 6; distance from moon to sun = 114.

Eratosthenes's measurement that 7° 12′ subtend 5,000 stadia gives exactly 250,000 stadia as the circumference of the earth, although Eratosthenes seems somehow to have

increased this value to 252,000 stadia. There are various stadia, and for a long time the exact meaning of this measurement in miles could not be stated. It now appears that the stadium used was equal to 516.73 feet, which gives a circumference of 24,467 or 24,662 miles, according as we use 250,000 or 252,000 stadia. If the polar circumference of the earth is 24,860 miles, then Eratosthenes's error was only 393 or 198 miles in decrement. His addition of 2,000 stadia to the first figure was in the direction of the truth, but he was lucky. Syene does not lie directly on the tropic, but is said to be twenty-five to fifty miles above it. A correction of 5,000 stadia by this amount gives a diameter of 700 or 2,000 miles in excess of the true diameter.

For Aristarchus, see Thomas Heath, *Aristarchus of Samos*, 1913, 299-336, 352-411; or the briefer account by the same author, *The Copernicus of Antiquity*, 1920, 39-54; also other sources given by Sarton, *op. cit.*, 156 f.

For Eratosthenes, see Heath, *op. cit.*, 1913, 339 f., 1920, 54 f.; and references in Sarton, *op. cit.*, 172 f. Some of the figures for the preceding computations were taken from J. L. E. Dreyer, *Observatory*, 37, 1914, 352 f.; cf. also H. Payn, *ibid.*, 287 f. Payn gives a photograph of the old well upon which Eratosthenes is said to have based his experiment; Dreyer shows that there is no evidence that Eratosthenes actually used the data of a well, and shows that he doubtless depended upon the statement that there was no shadow of the gnomen at noon on the day of the solstice in Syene. In either case, Fig. 2 shows the principle. It gives the sun's rays parallel, which is practically correct for such a measurement; it is, however, schematic in that it shows an angle of about 30° instead of 7° 12′.

On the fundamental rôle of mathematics in science, cf., e.g., A. N. Whitehead, *Science and the Modern World*, 1925, chap. 2.

Early Physics and Biology

See Williams, *op. cit.*, chaps. 7, 9; Sedgwick and Tyler, *op. cit.*, chaps. 5, 6; Whewell, *op. cit.*, I, bk. ii. On the early history of biology, see E. Nordenskiöld, *History of Biology*, 1928, 3-81.

The Rise of Modern Science

The sudden emergence and rapid development of science in the sixteenth and seventeenth centuries is more easily recorded than it is explained; yet no few paragraphs can even indicate the miracle of the change in the times. The student must read elsewhere to understand the period. The author has largely followed vol. II of Williams, *op. cit.*, which gives a complete and interesting account of the period from the point of view of the scientist. Chaps. 1 and 2 of Whitehead, *op. cit.*, present a less detailed summary and evaluation of the period from the standpoint of an author who knows both philosophy and science. See also Sedgwick and Tyler, *op. cit.*, chaps. 10-13; Whewell, *op. cit.*, I, bk. v; II, bk. vi.

Sarton, *op. cit.*, exposes almost for the first time the full extent of Eastern science in the first half of the Christian era. On Western medieval science and the 'dark ages,' see C. H. Haskins, *Studies in the History of Mediaeval Science*, 1921; also Sedgwick and Tyler, chaps. 7-9; Whewell, I, bk. iv.

On the history of physiology in the sixteenth, seventeenth, and eighteenth centuries, see Sir Michael Foster, *Lectures on the History of Physiology*, 1901.

On the relation of science to philosophy, see Whitehead, *loc. cit.* The quotation of the text is from Whitehead (pp. 22 f.), but does not represent Whitehead's view. He writes: "The truth is that science started its modern career by taking over ideas derived from the weakest side of the philosophies of Aristotle's successors.

In some respects it was a happy choice. It enabled the knowledge of the seventeenth century to be formularised so far as physics and chemistry were concerned, with a completeness which lasted to the present time. But the progress of biology and psychology has probably been checked by the uncritical assumption of half-truths. If science is not to degenerate into a medley of *ad hoc* hypotheses, it must become philosophical and enter upon a thorough criticism of its own foundations" (pp. 23 f.). That science is already beginning to enter upon this criticism of its foundations and that a new epoch in the relation of science to philosophy has even now begun, Whitehead seems to detect in the effect of the theory of relativity upon recent scientific thought.

That, from one point of view at least, philosophy has never played more than a secondary and indirect rôle in science is indicated by the fact that it is possible to write a coherent history of modern science with but little reference to philosophers: cf. Williams, *op. cit.* Whewell, *op. cit.*, writing, it is true, in a more materialistic age than the present, ignores Leibnitz, neglects Bacon, and protests the *a priori* dogmatism of Descartes (II, 141, 479).

The Development of Biological Science

The history of physiology in the century of Haller and Spallanzani is dealt with by Foster, *op. cit.* For a general account of the eighteenth and early nineteenth centuries, see Williams, *op. cit.*, IV, chaps. 4, 5; Sedgwick and Tyler, *op. cit.*, chaps. 14, 17. However, there is now (since the first writing of this text) the excellent history of biology by Nordenskiöld, *op. cit.*, to which the reader can be referred without reservation. On the biology of the sixteenth to eighteenth centuries, see 82-298; on the first half of the nineteenth century (Lamarck, Cuvier, microscopy, etc.), see 301-452; on Darwinism and neo-Darwinism, see 453-616.

The reader will readily find a large literature on organic evolution. The views of Buffon, Erasmus Darwin, and Lamarck, especially as related to the views of Charles Darwin, are considered in a readable and interesting manner by S. Butler, *Evolution, Old and New*, 1879, 3d ed., 1911. The modern theory is presented by T. H. Morgan, *Critique of the Theory of Evolution*, 1916, in which the relation of the present view to its early history is indicated (pp. 27-39). A very thorough work, dealing with cosmological as well as organic evolution, is H. Smidt, *Geschichte der Entwicklungslehre*, 1918, 549 pp.

The history of science is largely the history of its geniuses, and in this connection it is interesting to note what has been said of the 'intelligence' of these men. C. M. Cox (*Early Mental Traits of Three Hundred Geniuses* [*Genetic Studies of Genius*, II], 1926) has computed the intelligence quotients (I Q) of fifteen of the more recent great men mentioned in the chapter. Her results are approximate; the biographical data affect them. The values for early manhood are nearly always higher than for childhood, due to the fact that the indicated 'intelligence' is less when the data are scanty. Thus many of the values are minimal, and the low ones, which have in general a low statistical reliability, would presumably be higher if more data were available. Cox's list for these fifteen men, from least to greatest taken without regard to reliability, runs as follows: Copernicus, John Hunter, Harvey, Linnæus, Charles Darwin, Leonardo da Vinci, Bichat, Francis Bacon, Newton, Gallileo, Descartes, Cuvier, Haller, Leibnitz, and Goethe. There is good ground to believe that the first men in the list were much more intelligent, as the tests test intelligence.

than computed quotients show. There can be no doubt that the last named were very brilliant indeed, perhaps of the order of one man in 100,000,000, or even, if such a computation can be trusted, one man in 100,000,000,000. It is obvious that this 'intelligence' is a normal aspect of greatness. It is less clear that all men with the intelligence of these fifteen are great, for the world ought to contain more men of this level than get into history as great. For one thing, it would seem that intellectual greatness depends in part on the relation of original thought or discovery to the trend of the times in which the originator lived.

PHYSIOLOGICAL PSYCHOLOGY IN THE FIRST HALF OF THE NINETEENTH CENTURY

thing like the full expansion of knowledge. Especially during the preceding two and a half centuries, a vast amount of patient investigation had been accomplished. If the reader will turn to Johannes Müller's *Handbuch,* he will see that in the fourth decade of the nineteenth century there was a tremendous amount of physiological knowledge available, most of it identical in its gross facts with the accepted physiology of to-day. He will also gain some impression from the foot-notes of the large amount of competent physiological investigation that was going on in this first third of the century. In these pages we can only indicate the occurrence of a few crucial events in the whole vast range.

Müller's *Handbuch* was written on broad and exhaustive lines. It is divided into eight books, the subject-matter of which gives some notion of the breadth of physiological knowledge at that period. The first book deals with the circulation of the blood and the lymph (288 pages). The second considers chemical matters of respiration, nutrition, growth, reproduction, secretion, digestion, chylification, and excretion (308 pages). The third is on the physiology of the nerves (270 pages). Books iv, v, and vi constitute what might be regarded as the experimental psychology of that period, and book iii might be added to this group. The fourth book is on muscular movement in general and voice and speech in particular (248 pages), adding to the third book's consideration of reflex action, a new conception. The fifth book is on the five senses and begins with the formulation of the famous doctrine of the specific energies of nerve substances (256 pages). The sixth book, entitled "Of the Mind," deals with association, memory, imagination, thought, feeling, passion, the problem of mind and body, phantasms, action, temperament, and sleep—a truly psychological chapter (82 pages)! The last two books are on reproduction and development, embryonic and postnatal (179 pages).

Historically, the psychological portions of the *Handbuch* (books iv to vi) are the most important, but to Müller's treatment of sensation and sensory physiology we are to return later. Here we must mention particularly the matter of reflex movement.

The gross phenomenon of reflex movement had long been known. Even Galen had described what we now call the pupillary reflex. The word *reflex* was first used by Astruc in 1736, and the better translation is "reflected" or "reflection." He thought

of the animal spirits of sensation as reflected by the spinal cord or brain through the other nerves (the nerves were believed to be tubes) to produce movement. "As with light, angles of incidence and reflection are equal, so that a sensation produced by a concussion of the animal spirits against the fibrous columns is reflected and causes motion in those nerve tubes which happen to be placed exactly in the line of reflection." Whytt in 1751 had published important observations on this kind of movement. The essential distinction between voluntary action and reflex movement was not, however, clearly seen until Marshall Hall (1790-1857) pointed it out in 1832. Hall was a Scotchman and a brilliant physician, working in London, where he contributed to the Royal Society a great deal of valuable physiological research. He was working on circulation in the lungs and experimenting for this purpose with a decapitated newt, when he discovered that the brainless newt responded to sensory stimulation of the skin. He then tried an experiment upon a snake, dividing the spinal cord between the second and third vertebræ. "From the moment of the division of the spinal marrow it [the snake] lay perfectly tranquil and motionless, with the exception of occasional gaspings and slight movements of the head. It became quite evident that this state of quiescence would continue indefinitely were the animal secured from all external impressions. Being now stimulated, the body began to move with great activity, and continued to do so for a considerable time, each change of position or situation bringing some fresh part of the surface of the animal into contact with the table or other objects and renewing the application of stimulants. At length the animal became again quiescent; and being carefully protected from all external impressions it moved no more, but died in the precise position and form which it had last assumed."

From such observations Hall concluded that there are four kinds of bodily movement: (1) voluntary movement, dependent upon consciousness and the action of the cerebrum; (2) respiratory movement, dependent upon the important vital center known to exist in the medulla oblongata; (3) involuntary movement, dependent upon muscular irritability under direct stimulation (cf. Haller, *supra*); and (4) reflex movement, dependent only upon the spinal cord and independent of the brain and consciousness.

In the first volume of the *Handbuch* Johannes Müller dealt ex-

tensively with reflex or 'reflected' movement, and he amplified the discussion in his section on the voice in the second volume. Müller himself had undertaken experiments along this line, but he yielded the priority to Hall, who reported on his study in 1832 and published in 1833. The first *Abtheilung* of the first volume of the *Handbuch* appeared in the spring of 1833 and mentioned reflex action, but the second *Abtheilung* of the first volume, which discussed the reflex fully, did not appear until 1834. Müller differed from Hall in believing that the brain was frequently, although not necessarily, involved in spinal reflexes, but the two views are essentially the same. Müller gave corroboration and systematic organization to the doctrine, and established it in systematic physiology by his authority and the influence of the *Handbuch.*

Another great physiologist whose early research comes within the period we are discussing was Claude Bernard (1813-1878). Bernard studied medicine at the Collège de France and there came under Magendie's influence. He remained working as an assistant to Magendie in his inadequate laboratory, and also working in other better laboratories whenever possible until Magendie's death in 1855, when he succeeded to his chair. Until about 1860, he was incessantly occupied with research in many domains of physiology: it was only after 1860 that he undertook to collect his work in published lectures. His most important discovery, based upon a long series of carefully planned experiments, was the glycogenic function of the liver. The hepatic tissues possess a substance, which Bernard called glycogen, capable of creating sugar, which is stored in the liver. This physiological fact has very recently come to have meaning in the psychology of emotion, but in general Bernard's work on the chemical functions of the organism, while tremendously important to physiology, has little obvious meaning for psychology. In an era when so many were working upon the nervous system, he was primarily engaged with other functions.

It is interesting to note that Bernard expressed the conscious effort of physiology to become scientific after the model of physical science which one finds stressed bv the more rigorous experimental psychologists of his time and also fifty years later. In 1878 he wrote: "To-day physiology has become an exact science; it ought to separate itself from the philosophical and theological ideas which have been mixed with it for a long time. We no longer

ask a physiologist whether he is a spiritualist or a materialist, any more than we ask it of a mathematician, a physicist or a chemist. . . . The tendency, which appears to be reviving in our day, of desiring to introduce theological and philosophical questions into physiology, in order to pursue their pretended conciliation, is in my view a tendency sterile and dead, because it mixes sentiment and reason, and confounds what we recognize and accept without physical demonstration, with other things, which we ought to admit only after experimentation and complete demonstration."

The Velocity of the Nervous Impulse

An event of great interest in the physiology of the nervous system occurred in 1850, when Helmholtz (1821-1894) first measured the rate of transmission of the nervous impulse. It had been supposed that transmission was extremely rapid and practically unmeasurable. Johannes Müller in the *Handbuch* mentions three values that had been given. "Haller calculated that the nervous fluid moves with a velocity of 9000 feet in a minute; Sauvages estimated the rate of its motion at 32,400, and another physiologist at 57,600 million feet in a second." It will be seen that Haller's estimate is really 150 feet per second, which is not very far wrong, since a recent determination for man is about 400 feet per second (123 meters per second); but the last figure cited by Müller is almost sixty times the velocity of light. This high figure had been arrived at by assuming that the rate of flow of animal spirits in the nervous tubes and of the blood in the arteries would be the same for vessels of the same size, and would vary inversely with the size of the vessel. Müller did not give credence to this logic, but he did accept the general belief that the rate of transmission was extremely rapid, perhaps of the order of the velocity of light. He wrote: "We shall probably never attain the power of measuring the velocity of nervous action; for we have not the opportunity of comparing its propagation through immense space, as we have in the case of light." It was not many years, however, before his erstwhile pupil Helmholtz measured the rate and found it to be much slower even than sound, in fact, only about ninety feet per second in a frog's motor nerve. He performed this experiment while he was professor of physiology at Königsberg, measuring the delay of the muscle twitch

for different lengths of nerve on the myograph, which he had newly invented. To determine the time in sensory nerves—for, since the establishment of Bell's law, it was not safe to assume that sensory nerves would have the same properties as motor nerves—he instituted reaction experiments, then already coming into use in astronomy for the determination of the personal equation (*vide infra*). He stimulated a man upon the toe and upon the thigh, noting the difference in the reaction time. By this method he placed the rate of transmission for sensory impulses at between fifty and 100 meters per second. These times were measured more accurately later and corrected by Du Bois-Reymond (1818-1896), another famous physiologist, who was a friend of Helmholtz and had also been a pupil of Johannes Müller, and who devoted his life to a study of the electrical phenomena of the human body.

The importance for scientific psychology of the discovery that the transmission of the nervous impulse is not practically instantaneous, but relatively slow, is not to be underestimated. In the period under consideration, the mind had come to be largely identified with the brain (*vide infra*), but the personality seemed rather to be a matter of the entire organism. Every one thought, as the average man thinks now, of his hand as of a piece with himself. To move his finger voluntarily was an act of mind in itself, not an event caused by a previous act of mind. To separate the movement in time from the event of will that caused it was in a sense to separate the body from the mind, and almost from the personality or self. At any rate, Helmholtz's discovery was a step in the analysis of bodily motion that changed it from an instantaneous occurrence to a temporal series of events, and it thus contributed to the materialistic view of the psychophysical organism that was the essence of nineteenth century science. Johannes Müller's doctrine of the specific energy of nerves had served the purpose of a similar analysis on the sensory side. In Helmholtz's experiment lay the preparation for all the later work of experimental psychology on the chronometry of mental acts and processes. Certainly the point was not clear at the time, but certainly also it existed half-consciously in the thought of scientists or there would not have been such incredulity expressed about Helmholtz's measurement, nor would Johannes Müller have accepted so readily—while still exhibiting considerable empirical caution—the virtual instantaneousness of nervous transmission.

Notes

Bell and Magendie

Sir Charles Bell's famous statement of his law of the spinal nerve roots is entitled *Idea of a New Anatomy of the Brain: Submitted for the Observations of His Friends.* It was a privately printed monograph of only 100 copies, but known to have been printed in 1811. It is no wonder that Magendie did not know of this brochure. It has been reprinted first in *J. Anat. and Physiol.*, 3, 1869, 153-166, together with relevant letters and notes, 147-182; and again, with a German translation, as *Idee einer neuen Hirnanatomie*, 1911.

There was also a third contender for priority in the matter of the discovery of this law—Alexander Walker, who published in 1809. This statement seems, however, rather a faulty plagiarism of Bell's lectures, and in it the functions of the anterior and posterior roots are, by some strange mistake, reversed.

On this controversy, on Bell's contributions to physiological psychology, on the life of Bell, and for references on all these subjects, see the excellent account by L. Carmichael, Sir Charles Bell: a contribution to the history of physiological psychology, *Psychol. Rev.*, 33, 1926, 188-217. See also C. Eckhard, Geschichte der Leitungsverhältnisse in den Wurzeln der Rückenmarksnerven, *Beiträge zur Anat. u. Physiol. von C. Eckhard*, 10, 1883, 135-169.

Bell published a systematic text on dissection in 1798. In 1804 he contributed the sections on the nervous system to an *Anatomy of the Human Body* under the authorship of himself and his brother. His most important books from our point of view are the *Anatomy of Expression*, 1806, and *The Nervous System of the Human Body*, 1830, which sums up his researches from 1807 to 1829.

F. Magendie's first publication on the functions of spinal nerve roots is to be found in *J. de physiol. expér. et pathol.*, 2, 1822, 276-279, 366-371. See pp. 369 ff. for his comment on Bell's priority and his own independence. His most important book from our point of view is *Leçons sur les fonctions et les maladies du système nerveux*, 1839.

Müller and Reflex Action

For the life and letters of Johannes Müller, see W. Haberling, *Johannes Müller: Das Leben des rheinischen Naturforschers*, 1924. For an appreciation of his influence in physiology, see T. L. W. Bischoff, *Ueber Johannes Müller und sein Verhältnis zum jetzigen Standpunkt der Physiologie*, 1858; R. Virchow, *Johannes Müller: Eine Gedächtnisrede*, 1858; E. Du Bois-Reymond, *Abh. der berl. Akad. der Wiss.*, 1859, 25-191.

The *Handbuch der Physiologie des Menschen* appeared from 1833 to 1840. Müller worked continuously on it during his last years at Bonn and his first years at Berlin. The first *Abtheilung* of the first volume appeared in 1833, the second *Abtheilung* in 1834. The first edition of the first volume is rare, and the author does not know the date of the two parts bound together. The first *Abtheilung* of this volume had, however, reached a third revised edition in 1837, and the second in 1838, when the two were issued together with a single title bearing both dates. Meanwhile, the second volume was beginning to appear in three *Abtheilungen:* the first in 1837, the second in 1838, and the third, and thus also the entire second volume, in 1840. The immediate revisions, with the addition of much citation of new work, indicate how active research in experimental physiology had become. The need for such a book, and doubtless Müller's

prestige, led to its immediate translation into English by W. Baly, the first volume in 1838 and the second in 1842. There was a second edition of the translation of the first volume in 1840, and all editions contain notes, added by the translator, on very recent discoveries.

On the history of reflex action, see C. Eckhard, Geschichte der Entwicklung der Lehre von den Reflexerscheinungen, *Beiträge zur Anat. u. Physiol. von C. Eckhard*, 9, 1881, 29-192; G. S. Hall, A sketch of the history of reflex action, *Amer. J. Psychol.*, 3, 1890, 71-86; C. F. Hodge, same title, *ibid.*, 149-167, 343-363.

Whytt's paper can be found in the posthumous publication, *The Works of Robert Whytt*, Edinburgh, 1768. Marshall Hall reported his observations to the Committee on Science of the Zoölogical Society in 1832, and read his paper before the Royal Society, of which he was a Fellow, in 1833; it was published in *Philos. Trans.*, 1833, 635-665.

The English translation of Müller's *Handbuch* (1838-1842) uses the terms *reflex motion* and *reflected motion* interchangeably. The English noun *reflex*, in the sense of a reflection from a mirror, was used in the eighteenth century and occurs poetically in Tennyson in 1830, the time of Marshall Hall's writing. As an adjective applied to light, its usage is both earlier and later. *Reflected* and *reflection* have been more common. The same two nouns, *Reflex* and *Reflexion*, occur in German in the same sense, but *Reflexion* is more usual, and is the word that Müller used. It is perhaps fortunate that the more unusual term has been retained in both languages, since the original notion of reflection of the animal spirits by the spinal columns after the manner of light proved to be wrong, but the historical justification for the word is nevertheless thus lost.

For Müller's discussion of reflex motion, see bk. iii, sect. iii, chap. 3 (1834); bk. iv, sect. ii, chap. 1 (1837). The author has not found the word *Reflexion* in the portion of the first volume published in 1833, but Müller asserts that he presented the essential notion there in his discussion of respiration; see foot-note at the beginning of the first reference, where he also yields priority to Hall.

The quotation apropos of Astruc is from Hodge, *op. cit.*, 157. The other quotation is directly from Marshall Hall.

Claude Bernard

Claude Bernard wrote *Leçons sur la physiologie et la pathologie du système nerveux* in 1858. The quotation is from the *Leçons sur les phénomènes de la vie*, 1878, I, 45. An excellent account of his life and work is that by J. C. Bay, *Bull. Soc. Med. History of Chicago*, 2, 1919, 119-130. See also H. C. Green's English trans. of Bernard's *Introduction to the Study of Experimental Medicine*, 1927, with an introduction by L. J. Henderson, and a short life by his pupil, P. Bert.

Rate of the Nervous Impulse

Helmholtz sent his first note on the rate of transmission of the nervous impulse to Du Bois-Reymond to read before the Physikalische Gesellschaft in Berlin in order to establish priority for his discovery. See H. L. F. von Helmholtz, *Ber. d. könig. preuss. Akad. d. Wiss. zu Berlin*, 1850, 14 f. (cited erroneously by both König and Koenigsberger as *Berliner Monatsberichte;* the *Berichte* were published 1836-1855, and the *Monatsberichte* succeeded them in 1856). Du Bois-Reymond immediately asked Humboldt to give the note publicity in Paris; see *Comptes rendus*, 30, 1850, 204-206; 33, 1851, 262-265. Helmholtz published at length in the summer: [*Müller's*] *Arch. für Anat. und Physiol.*, 1850, 276-364 (esp. 328-363); and again two years later, *ibid.*, 1852, 199-216.

Haller had chanced upon an ap-

proximately correct value in 1762 by considering the rate of the movement of the tongue in pronouncing the letter *R;* A. v. Haller, *Elementa physiologiæ corporis humani,* 1762, IV, 373. His 9,000 *pedes in minuto* is equivalent to 45.4 meters per second, which is just about Helmholtz's higher value for the frog.

Joh. Müller's discussion of this matter occurs in the first volume of the *Handbuch,* bk. iii, sect. iii, introd., and, since a last edition of this volume is dated 1844, it has often been remarked that Müller's dictum that the rate was unmeasurable antedated its actual measurement by only six years.

The velocity of light is about 297,500,000 meters per second; the velocity of sound is about 330 meters per second. A recent determination gives the velocity of the nervous impulse in man as about 123 meters per second. Thus the speed of light is about 2,500,000 times the speed of the nervous impulse, and the speed of sound about two and two-thirds times the speed of the nervous impulse. Helmholtz's values for the nervous impulse were still less.

Helmholtz's determinations for the frog's nerve were 0.0014 second for 60 mm. of nerve, and 0.0020 second for 50 mm. These figures give respectively 42.9 and 25.0 meters per second. They were determined in laboratory temperatures of 11-15° C. We know now that the rate increases geometrically at a ratio of about 1.06 for every degree centigrade, and Helmholtz noted the importance of temperature in 1850. If we apply the modern coefficient to the average of Helmholtz's first values in 1850, we get for the temperature of the human body 145 meters per second, as against the modern observed 123 meters per second.

Du Bois-Reymond, who was in intimate appreciative correspondence with Helmholtz, received the first two-page note. Müller, to whom Du Bois-Reymond tried to explain it, insisted on rejecting the conclusion, arguing that Helmholtz had not eliminated the time for the contraction of the muscle. Humboldt, Du Bois wrote Helmholtz, "war ganz depaysiert," and at first refused to send the paper to Paris for publication there. Du Bois-Reymond had first to edit it and then Humboldt, won over, had it published in the *Comptes rendus,* adding a further explanatory foot-note of his own. By summer Müller had also been won, and then Helmholtz published his longer paper, in which he included a measurement of the time of the muscular contraction and additional determinations of the rate of nerve transmission.

The attitude of the times, which made the acceptance of this discovery difficult, is well set forth in a letter to Helmholtz from his father, a teacher of classics and philosophy in a *Gymnasium.* He wrote, in reply to Helmholtz's brief and enthusiastic account of his discovery: "As regards your work, the results at first appeared to me surprising, since I regard the idea and its bodily expression, not as successive, but as simultaneous, a single living act, that only becomes bodily and mental on reflection: and I should as little reconcile myself to your view, as I would admit a star that had disappeared in Abraham's time should still be visible."

On Helmholtz's life and work, see L. Koenigsberger, *Hermann von Helmholtz,* 3 vols., 1902-1903 (Eng. trans., 1906); J. G. M'Kendrick, *Hermann Ludwig Ferdinand von Helmholtz,* 1899. On the measurement of the rate of nerve transmission in particular, see Koenigsberger, I, 116 ff. (trans., 62 ff.); M'Kendrick, chap. 6. In general, on Helmholtz, see chap. 14 of this book.

For very brief statements about the biological significance of the work of the men mentioned in this chapter, see E. Nordenskiöld, *History of Biology,* 1928, 374-388, on Bell, Magendie, Bernard and Johannes Müller; and 408-413 on Helmholtz and Du Bois-Reymond.

Chapter 3

PHRENOLOGY AND THE MIND-BODY PROBLEM

IF in 1850 there could be resistance to the acceptance of Helmholtz's demonstration that nervous transmission is not practically instantaneous on the ground that the idea and its bodily expression are simultaneous, 'a single living act,' then it is clear that modern common sense, which identifies the mind with the brain, had not yet become the common view, and that even the localization of the mind at or within the brain was a matter of some doubt.

In phrenology, however, we find a movement, almost exactly contemporaneous with the developments which have been considered in the preceding section, which sought to establish the brain as the 'organ of mind,' and even particular parts of the brain as particular organs of separate mental faculties.

The more general of these two ideas was not entirely new. It is true that the idea was not favored by Aristotle, who referred the seat of life to the heart. The Egyptians had localized thought in the heart, but judgment in the head or kidneys. Pythagoras, however, considered the brain as the seat of the mind and the intellect, and Plato held a similar view. It was the Pythagorean doctrine that prevailed. The Alexandrian anatomists held this belief, and they suggested an even more specific localization. Erasistratus referred sensation to the membranes of the brain, and movement to the brain substance itself. Herophilus regarded the brain ventricles as reservoirs of the vital forces, and Galen established this view by teaching that the animal spirits flow from the brain ventricles to the heart, and are thence distributed to the body by the arteries.

Even the belief in particular localization is quite old. Albertus Magnus (1193-1280) referred feeling to the anterior ventricle of the brain and, at times, memory to the posterior ventricle. Imagination was variously localized. Many views of like nature were expressed during the succeeding centuries. Willis, an anatomist

(1621-1675) just before Newton's day, placed memory and will in the convolutions of the brain, imagination in the corpus callosum, sense-perception in the corpus striatum, and certain emotions in the base of the cerebrum.

At the same time that the anatomist-physiologists were puzzling over the organ of mind, the philosopher-psychologists were concerned with establishing the place of the seat of the soul. It is a familiar fact that Descartes (1596-1650) localized the soul in a sense in the entire body, but most specifically in the pineal gland in the brain, where, he supposed, its interaction with the body took place. He did not, however, by any means identify the brain with the mind. He held to a complete dualism between the two, and thought of the pineal gland merely as the point at which the mind affected the flow of animal spirits, changing their course. The soul, being immaterial, did not occupy space, but it needed a definite point of contact with the spatial brain. A similar view without such specific localization is presented by Lotze in his medical psychology of 1852.

All these guesses were, however, more matters of philosophical view than of empirical proof. The effective impetus to the consideration of the brain as the organ of mind did not come until the nineteenth century. Late in the eighteenth century there was, nevertheless, some specific preparation for it. It was then that Benjamin Rush (1745-1813) in America, William Tuke (1732-1822) in England, and Philippe Pinel (1745-1826) in France independently began their agitations for reform in the treatment of the insane. Up to this time the insane had been outcasts, supposedly possessed of demons, and had been scourged or cast into chains in dungeons. These reformers advocated the iew of demoniacal possession as a disease, and accomplished much n the way of liberation and sympathetic treatment of those so fflicted. The work of change was slow and had not even been xtended from Paris to the French provinces at the time of Pinel's death in 1826. Nevertheless, it was an important movement of great influence that tended to fix the conception of mental disease as against the notion of demoniacal possession for which the afflicted person, at least in popular belief, shared the responsibility. To recognize the mind as subject to disease is a step in the direction of realizing its dependence upon the body, the usual seat of disease. It is, however, the prominence that the

movement gained, rather than the specificity of its bearing upon the problem of mind and body, that makes mention of it important here.

Phrenology

It was out of this milieu that phrenology emerged at the hand of Franz Joseph Gall (1758-1828). Gall was an anatomist who concerned himself primarily with the head and brain. As a schoolboy he believed he had observed a relationship between some of the mental characteristics of his schoolmates and the shapes of their heads, especially that those with prominent eyes had good memories. This idea he carried with him into adulthood, and subjected it to investigation. His first observations were made upon the lower classes of society which he found in jails and lunatic asylums, where the mental characteristics can be regarded as established because they have led their possessors to their present situations. For instance, the 'bump' on the head which phrenology takes as a sign of the faculty of acquisitiveness is the place which Gall thought was especially prominent in pickpockets. Later Gall extended his studies to his friends and to casts of the heads of persons whose mental traits were well known. A German by birth, he began lecturing on his new doctrine of physiognomy in Vienna, where in 1800 he was joined by Spurzheim as a pupil. The doctrine attracted a great deal of popular attention, which was not diminished by the fact that Gall in 1802 was ordered by the government, at the instance of the church, to discontinue his lectures. Spurzheim was now associated with Gall as a collaborator, and, after a lecture tour in Germany, they settled in Paris in 1807. They began to publish jointly, but in 1813 they agreed to separate; Gall remained in Paris to continue his lectures, writing, and research, while Spurzheim fared forth to preach the new gospel in France and England, and finally in America. The great first treatise on phrenology appeared under Gall's authorship, with Spurzheim's collaboration in the first two volumes, in 1810-1819. It is entitled: *Anatomie et physiologie du système nerveux en général, et du cerveau en particulier, avec observations sur la possibilité de reconnaître plusieurs dispositions intellectuelles et morales de l'homme et des animaux par la configuration de leurs têtes.* In 1822-1825 Gall published a new and much altered edition of this work under the title *Sur les fonctions du cerveau.*

The *Anatomie et physiologie du système nerveux* is a much more conservative and scientific work than many who now sniff at phrenology might suppose. The first volume, published in 1810 with an atlas, deals with the *système nerveux en général* and is a very careful consideration of the more important nerves, the spinal cord, the cerebellum, and the five senses. The last three volumes deal with the *physiologie du cerveau en particulier* and enter more properly into phrenology, although that term was not employed by Gall. Even some of Gall's physiological critics give him credit for his contributions to the anatomy of the nervous system. It is said that, when Gall and Spurzheim were proposed for election to the Institut de France in 1808, Cuvier, then permanent secretary for physical science, was favorable to their election until he was dissuaded by Napoleon, who had been irritated by the recent election of an Englishman (Sir Humphry Davy) and opposed the further election of foreigners.

The modern character of phrenology was given it by Spurzheim (1776-1832), who adopted the term *phrenology*. Gall's doctrine had been popularly referred to as his *Hirn- und Schädellehre,* and Gall spoke of physiognomy and craniology. Spurzheim was more of a propagandist than a scientist. He was influential, while with Gall, in dignifying phrenology by bringing it more into relation with the respectable traits of mankind than with those to be found in the inmates of jails and asylums. It was presumably he who first saw the more important relations to society, although Gall learned from him the importance of this extension. After his separation from Gall, Spurzheim worked out many details of the system, established a new and more complete topography of the skull together with a revised terminology for the faculties, wrote a series of books presenting his views and controverting the numerous and caustic derogators of phrenology, and finally died in Boston while advocating the doctrine in America.

It is plain that there are three fundamental propositions inherent in the phrenology of Gall and Spurzheim. In the first place, it is necessary for the doctrine to show that the conformation of the exterior of the skull corresponds to the conformation of its interior and of the brain. Gall held that it did, that the form of the brain was determined, at least, early in life, and that the skull was formed to conform to it. His view seems not to

be correct as against small differences, for the thickness of the skull varies greatly and apparently adventitiously. In the second place, if the doctrine is to be allowed, it is necessary to believe that the mind can meaningfully and satisfactorily be analyzed into a number of faculties or functions, and to perform this analysis. Modern psychology has largely failed of any such scientifically significant analysis, but Gall assumed that analysis into the mental traits admitted by common sense was adequate to his purpose. Finally, there is in the doctrine the central proposition that the faculties will be found to correlate with the conformation of the skull; a protrusion of the skull must mean an excess of the faculty corresponding to that spot, and a recession a lack of the corresponding faculty.

The corollary to these propositions, which Gall sought to establish by independent argument, is that the brain must be the organ of the mind, and that, furthermore, particular regions of the brain must be the particular organs of particular units of the mind.

The work of the phrenologists has centered upon the central problem of correlation, and it must be admitted that, had the correlation been satisfactorily made out, we should have been in possession of a very important fact. If it were shown later that the outside of the skull did not conform to the brain, then we should have had to explain in some other way the relationship between the form of the skull and the nature of the mind. The established correlation itself would have been sufficient test of the adequacy of the mental analysis.

In the proof of this central theme, the modern anthropometrist is struck with the inadequacy of the method. Nowadays we should seek to avoid the danger of selection of cases to fit the theory by choosing in advance an 'unselected' group of persons, measuring all their 'bumps' accurately, estimating the degrees of all of their faculties in the accepted list without knowledge of their head measurements, and then determining the correlation between the two sets of data. Gall could have done this even without the modern mathematics of correlation, but physiology a century and more ago was still in the stage where personal observation and checks and safeguards depended more upon the rigor of the investigator than upon the recognized sanctions of

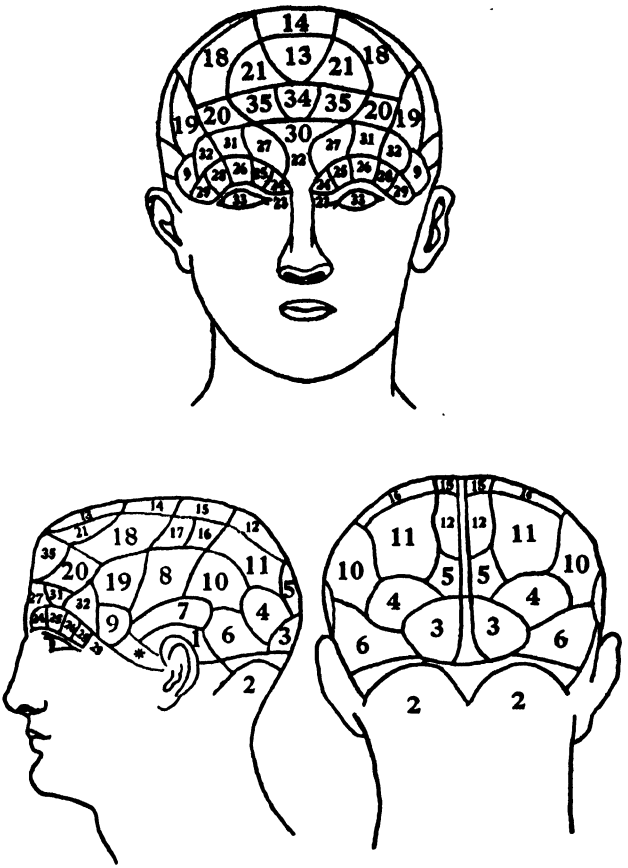

FIG. 3. THE "POWERS AND ORGANS OF THE MIND," ACCORDING TO SPURZHEIM, *Phrenology, or the Doctrine of Mental Phenomena*, 1834.

AFFECTIVE FACULTIES		INTELLECTUAL FACULTIES	
PROPENSITIES	SENTIMENTS	PERCEPTIVE	REFLECTIVE
? Desire to live	10 Cautiousness	22 Individuality	34 Comparison
* Alimentiveness	11 Approbativeness	23 Configuration	35 Causality
1 Destructiveness	12 Self-Esteem	24 Size	
2 Amativeness	13 Benevolence	25 Weight and Resistance	
3 Philoprogenitiveness	14 Reverence	26 Coloring	
4 Adhesiveness	15 Firmness	27 Locality	
5 Inhabitiveness	16 Conscientiousness	28 Order	
6 Combativeness	17 Hope	29 Calculation	
7 Secretiveness	18 Marvelousness	30 Eventuality	
8 Acquisitiveness	19 Ideality	31 Time	
9 Constructiveness	20 Mirthfulness	32 Tune	
	21 Imitation	33 Language	

science. (Psychology is perhaps only now, a century later, emerging from this stage.)

Gall's correlation as extended and modified by Spurzheim rec-

ognized thirty-seven "powers" of the mind which corresponded to an equal number of "organs" of the mind, the development of which might lead to enlargements of the skull. The skull was therefore divided into thirty-seven contiguous patches, some large and some small, and the table of the corresponding faculties was drawn up. The mental analysis began with a dichotomy into affective powers and intellectual powers, and for each of these there was a two-fold subdivision. First there were the "propensities," affective powers, like "destructiveness," "amativeness," and "philoprogenitiveness." All these were grouped together at the lower part of the back of the head and at the sides above the ears. The other affective powers were the "sentiments," "cautiousness," "benevolence," "hope," which lay in a single region above the propensities on the back, sides, and top of the head. The intellectual powers were all related to the forehead, and consisted mostly of "perceptive" faculties like the perception of "size," "weight," "coloring," "time," and "tune." There were two "reflective" intellectual powers, "comparison" and "causality," in the very center of the forehead.

It is impossible to evaluate the evidence for these relationships. The region for "adhesiveness" (a propensity), for example, was originally designated thus because it was prominent in a lady who had been introduced to Gall as a model of friendship and because it was said to be the region of contact when persons closely attached to each other put their heads together. (The region is just to the side of the middle of the back of the head!) However, the phrenologists claimed that these dubious initial findings were accepted because they were verified in all persons without exception. Thomas Brown, the philosopher, remarked that the theory would never gain acceptance because any one could test it simply by looking at heads; and Spurzheim rejoined that this was the very reason that it had gained acceptance. Perhaps the seriousness of the phrenologists' belief, as well as the dangers inherent in their interpretative method, can best be illustrated by a later phrenologist's phrenological biography of Gall. This writer closed his biography by a detailed examination of Gall's personality and cranium in respect of two dozen faculties. He began: "The organs of Amativeness, Philoprogenitiveness, Adhesiveness, Combativeness, and Destructiveness were all very well developed in Gall. His Secretiveness was also rather large,

but he never made bad use of it. He was too conscious of his intellectual powers to obtain his ends by cunning or fraud." Our superficial acquaintance with Gall lets us see how the 'bump' of combativeness could be justified. Destructiveness also seems intelligible until we note that it was discovered because prominent "in the head of a student so fond of torturing animals that he became a surgeon," and "in the head of an apothecary who became an executioner." But the author of the biography himself found a need for explaining away secretiveness. Herein seems to lie the fundamental fallacy of this interpretative method: if a particular correlation obviously failed, it could be accounted for by other more dominant faculties that might be supposed to suppress it or to alter its direction.

Phrenology had a tremendous popular appeal. The most important and greatest puzzle which every man faces is himself, and, secondarily, other persons. Here seemed to be a key to the mystery, a key fashioned in the scientific laboratory and easy to use. It actually constituted a new intellectual gospel. Moreover, it was supported by other men of prominence and intelligence than Gall and Spurzheim. Perhaps the most important of these men was George Combe (1788-1858), a Scotchman, who, after deriding phrenology, was converted by Spurzheim and took up the cause ardently from about 1817 until his death in 1858. He wrote and lectured much on phrenology, like Spurzheim visiting America to spread the doctrine. He was a strong candidate for the chair of logic at the University of Edinburgh, although he was rejected in favor of Sir William Hamilton. In America, as well as in England, the new 'science' spread. In America the Fowler brothers were chiefly instrumental in furthering the cause, and the Institute of Phrenology still existed in New York at a recent date. At one time there were twenty-nine phrenological societies in Great Britain and several journals. The *Journal of Phrenology,* although passing through a series of changes of name and amalgamations, was born in Edinburgh in 1823 and died in Philadelphia only in 1911. Phrenology had flourished for a century!

It was never, however, generally accepted in science. In the days of Gall, when it was still scientifically possible if not plausible, it was opposed by Sir Charles Bell, Sir William Hamilton, Thomas Brown, and other men of equal prominence. It was also

derided and ridiculed. Later, when knowledge of the physiology of the brain rendered it impossible, it still held its popular appeal, with scorn only from the scientifically informed. Quite early it came to occupy the position of psychic research to-day, looked at askance by most men of science because unproven, using unscientific methods, and indulging in propaganda, and yet still not absolutely disproven.

The importance of phrenology for us lies in its effect upon the scientific thought of this period. While many expressed skepticism about the actuality of the correlations between faculties and prominences of the skull, there were also separate attacks upon the two other fundamental principles. The physiologists disbelieved the relationship between the exterior of the skull and the brain, but the philosophers objected to the analysis of mind into faculties with spatially distinct organs. Such a conception seemed to violate the principle of the unity of mind. Descartes assigned the pineal gland for the point of action of the mind because every other organ in the brain was in duplicate, one on each side. What would Descartes have said to thirty-seven organs, every one of them in duplicate? In the nineteenth century, the notion of the unity of mind was still too strongly entrenched to abide the phrenological analysis. We have already seen that it was responsible for the resistance that Helmholtz encountered when he measured the rate of nervous transmission.

The theory of Gall and Spurzheim is, however, an instance of a theory which is essentially wrong, but just enough right to further scientific thought. It was right in the first place in establishing the brain as the 'organ of mind,' a phrase which Gall used—or, if it was not right, at least by establishing that belief, it left science free for all the progress that resulted in physiological psychology. As long as the seat of the soul remained a matter for metaphysical speculation, to be affirmed or denied on other than empirical grounds, there was no scientific approach available for the study of the mind. But as soon as this metaphysical objection was overcome, not so much factually as in the acceptance of modes of thought, the physiology of the brain and the psychophysics of sensation were ready to be undertaken. For this reason it is almost correct to say that scientific psychology was born of phrenology, out of wedlock with science.

Another great impetus that phrenology gave to science was

its suggestion of a localization of function in the brain. If the phrenological correlations were not correct, still it was reasonable to suppose that different parts of the brain would have different physiological, perhaps even psychophysiological, functions. It was an important thing to have this belief established, but the account of the first attack upon the problem must be left for the next chapter.

Notes

René Descartes's discussion of the seat of the soul in the pineal gland occurs in his *Les passions de l'âme*, 1650, arts. xx-l. Most of these sections are translated in B. Rand, *Classical Psychologists*, 1912, 173-183. It is important to note that the soul, being unextended and therefore not occupying space, was not thought of as confined within the pineal gland. It was associated with, but was not within, the body, and the pineal gland was the organ through which it affected the body, but not its container. Descartes's mechanics of the body was such as should have left him no difficulty in accepting Helmholtz's demonstration that the conduction of the nervous impulse takes an appreciable time; nevertheless, Descartes preserved the unity of the soul, which was really the underlying difficulty in the acceptance of those results. For R. H. Lotze's view, see his *Medicinische Psychologie*, 1852, 115-122.

Pinel is known mostly for his work in the liberation of the insane, which was carried on after him by his pupil Esquirol. The English reform was an important, but not the principal, event in the life of Tuke. In America, a new country, tradition was less well established and the reform came much more easily than in France and England. Rush is primarily famous as a friend of Franklin, a signer of the Declaration of Independence, and a surgeon concerned with many humanitarian reforms. Both Tuke and Rush were Quakers. Rush wrote a book called *Diseases of the Mind* in 1812.

On the psychological antecedents of phrenology, see M. Bentley's article with that title, *Psychol. Monog.*, 21, 1916, no. 92, 102-115. He traces the primary thread of development from Locke through the French sensationists, and expresses a very proper appreciation of Gall's work.

Gall and Spurzheim

F. J. Gall began his lectures in Vienna in 1796. His doctrine came to be well known before his printed publication, largely by way of his, and later Spurzheim's, lectures, and also because some of his students published notes on his lectures.

The first publication of importance is the *mémoire* presented by Gall and G. Spurzheim as candidates for election to the Institut de France: *Recherches sur le système nerveux en général, et sur celui du cerveau en particulier*, 1809. The *mémoire* was presented on March 14, 1808, and a committee of five, including Pinel, with Cuvier as chairman, was appointed to examine the researches and the doctrine. Whether Napoleon actually interfered is not certain, although it is plain that he was opposed to the doctrine. Cuvier's long negative report of fifty-one pages is cautious and conservative, and shows that his committee realized that it was dealing with a difficult controversial matter. It concludes: "It is necessary again to repeat, if only for the instruction of the public, that the anatomical questions with which we have been occupied in

this report do not have an immediate and necessary relation to the physiological doctrine put forth by M. Gall on the functions and the influence of the relative volume of the different parts of the brain, and that all that we have brought out concerning the structure of the encephalon would be equally true or false without there being the least thing to conclude from it for or against the doctrine which can only be judged by totally different means." In other words, Cuvier and his committee withdrew from a difficult position by judging that the essential thesis of Gall and Spurzheim was irrelevant to the field of the mathematical and physical section of the Institut. For Cuvier's report, see *Mémoires de la classe des sciences mathématiques et physiques de l'Institut de France,* 1808, 109-160. On Napoleon and Gall, see Capen (cited below), I, 22-26.

(The *mémoire* was addressed to what we should now call the Académie des Sciences. The old Académie des Sciences had been suppressed by the Revolution in 1793, and the Institut established by Napoleon in 1796. It was not until 1816 that the mathematical and physical division of the Institut came again to be known as the Académie des Sciences. Had the Académie des Sciences Morales et Politiques, suppressed by Napoleon in 1803 and reëstablished in 1833, been in existence in 1808, it could not very well have made Cuvier's report of no jurisdiction.)

We have seen that the four volumes of the *Anatomie et physiologie* appeared in 1810-1819, and that Spurzheim collaborated on the first two. In 1825, Gall had completed the six volumes of *Sur les fonctions du cerveau et sur celles de chacune de ses parties.* This work reprints much of the *Anatomie et physiologie,* but omits the descriptive anatomy and includes a great deal of new material more directly relevant to 'physiognomical' doctrine. It was translated into English by W. Lewis in 1835. The editor, N. Capen, added a biography to the translation, I, 1-52. See also F. J. Möbius, *Franz Joseph Gall,* vol. VII of Möbius's *Ausgewählte Werke,* 1905; here there is also a biography, pp. 3-17.

Spurzheim, after he separated from Gall in 1813, published many books in English. They include *The Physiognomical System of Gall and Spurzheim,* 1815 (it was only in 1815 that Firster suggested the term *phrenology*); *Phrenology or the Doctrine of the Human Mind,* 1825; *The Anatomy of the Human Brain,* 1826; and *Outlines of Phrenology,* 1832. The introduction to the second work mentioned includes an historical account of phrenology and of Spurzheim's relation to Gall (3d Amer. ed., 1834, I, 9-12). The figure and its legend in the text are taken from this book.

Later Phrenology

Combe was even more prolific than Spurzheim. His first work is *Essays on Phrenology,* 1819, and other books on phrenology followed in 1824, 1825, 1827, 1839, and 1847. See also C. Gibbon, *Life of George Combe,* 1878.

O. S. Fowler (1809-1887) wrote very many books on phrenology and his brother, L. N. Fowler (1811-1896), a few. Together they founded the *American Phrenological Journal* in 1838, and they did more than any others to establish phrenology in America. The British *Phrenological Journal* was published from 1823 to 1847. The American journal later took this name and combined with the British *Phrenological Magazine* in 1880. It ceased publication with the 124th volume in 1911.

Chapter 4

PHYSIOLOGY OF THE BRAIN: 1800-1870

IN emphasizing the importance of Gall's work in localizing the mind within the brain, we must not be misled into believing that physiologists at the end of the eighteenth century were in doubt about the existence of an intimate connection between the mind and the brain. Bichat (1771-1802), for instance, held that the brain was the center for intelligence, memory, perception, imagination, and judgment, but that the emotions have their seat in the internal organs. Many other French physiologists of this period held similar views, and because of their tendency to localize the emotions in the viscera they are the logical and perhaps actual progenitors of the James-Lange theory of emotion. All in all, the point of view about the body and the mind was not so very different from that of Descartes, whose influence is shown in this manner. Thus the relation of the mind is to the whole body, not merely to the brain; nevertheless, there are special points of contact which can be regarded as the seat of the mind or the seats of some of its functions, and for these the brain is more likely to serve than are other organs. The necessity for the relationship is obvious. The human mind expresses itself in action; action depends upon the nerves; the nerves arise in the spinal cord and the brain. Were emotions known best by the actions to which they lead, rather than by the obvious disturbance of the viscera, Bichat and the others would doubtless have looked to the brain for *le siège des passions* also. As to whether such a gross anatomical analysis accounted for all of the mind, that was plainly not a problem for a physiologist then, any more than the physiologist to-day seeks to find an account of all the accepted facts of psychology.

Gall's extremely specific psychophysiology accomplished two things. In the first place, it forced the problem of the correlation of mind and brain to the fore because of the tremendous vogue of phrenology and because Gall was an anatomist of such

scientific excellence that his movement could not be ignored by the scientists. In the second place, by going to extremes, Gall made a radical but less extreme view actually seem conservative. Without Gall, Flourens might never have conceived the problem of finding different functions for the cerebrum, the cerebellum, the medulla, and the cord; and Flourens's position was much strengthened because he could appear in this position as a conservative correcting the pseudo-science of Gall and Spurzheim. It is the familiar case where the truth is more nearly approximated because a traditional belief that deviates from the truth in one direction is offset by the dramatic and vigorous promulgation of a view that deviates in the other direction. While no scientist consciously cultivates error, it is nevertheless true that error may sometimes be more effective than truth in yielding, at any given time, what is then regarded as true.

Pierre Flourens

Flourens (*ca.* 1822) is the most important figure in the advance of the physiology of the brain away from both the too vague tradition and the too specific doctrine of the phrenologists. First, however, we must mention Rolando, the anatomist after whom the cerebral fissure is named.

Luigi Rolando (1770-1831) was interested in the anatomy and pathology of the brain, but he also indulged in some physiological experimentation and speculation, which, published in 1809, led him to claim priority over Flourens in determining correctly the functions of the different parts of the brain—one says "correctly" because he, like most of the scientists, rejected Gall's localization as incorrect. In Rolando's view, the cerebral hemispheres are "the principal seat of the immediate cause of sleep, of dementia, of apoplexy, of melancholia and of mania." Such a statement is certainly equivalent to a localization of the higher mental functions in the cerebrum, although Flourens pointed out that it is not a statement that assigns perception and intelligence exclusively to the cerebral lobes. Rolando drew upon pathological observations and post-mortem examinations. He held further that activity of the cerebrum is due to movement of the fibers, thus erroneously imputing movement to the fibers and making the white, rather than the gray, matter fundamental to the psycho-

physiological correlation. Sensations, however, he localized in the medulla oblongata and not in the cerebrum. There was already anatomical evidence for this opinion, for all the sensory nerves except the olfactory and the visual were thought to run to the medulla. (The sensory function of the trigeminal nerve, which is also motor, was not recognized; otherwise the belief was correct.) The medulla was already known to be essential to life, to contain the 'vital knot'; and it is to this vital knot that Rolando assigned also the principal center of sensibility. His chief argument was, however, directed to the cerebellum, which was, he thought, the organ for the preparation and secretion of nervous force. This conclusion came from his own experiments. Volta (also an Italian) had devised the Voltaic pile in 1800, thus rendering electric current available. Rolando used such a pile to stimulate the brain, and found that violent muscular contractions became more violent the nearer the electrodes approached the cerebellum through the brain substance. The experiments were crude and one does not know exactly where his stimulus was effective, but the gross fact that movement was more vigorous the nearer he came to the cerebellum was obvious enough, and led him to characterize the cerebellum as the battery from which the nervous energy is derived.

If Rolando was unconvincing in his experiments and vague and essentially incorrect in his theories, Pierre Flourens (1794-1867) was precise in his technique, clear and trenchant in his writing, and essentially right in his conclusions—at least he marks a definite step in both method and fact toward what is accepted as truth to-day. He early became a protégé of Cuvier's in Paris, where he lectured on the physiology of sensation and attracted much attention. His important researches on the brain were presented by Cuvier to the Académie des Sciences in 1822 and 1823, and, collected and printed together with an explanatory preface, they form his first important book in 1824. A few later papers were printed in another small book in 1825. In 1828 he was elected to the Académie on the occurrence of a vacancy, and in 1833 at Cuvier's dying request he was made *secrétaire perpétuel* in Cuvier's stead. He then held a professorship of comparative anatomy especially created for him at the museum of the Jardin du Roi. In 1840 he was elected to the Académie de France over Victor Hugo. In the interval, he published and also engaged to

a small extent in politics. In 1842 he put out a revised edition of the papers of 1824-1825, and also his *Examen de la phrénologie,* in which he brought forward Descartes to confound the doctrine of Gall and to establish the scientific physiology of the brain. In 1855 he was made professor of natural history in the Collège de France: and he died twelve years later.

In all things his touch was sure. As a writer he is clear and forcible. In the preface to the *Examen* he says: "J'ai voulu être court. Il y a un grand secret pour être court: c'est d'être clair." And he is both brief and clear. He had the gift of annihilating an opponent, Rolando or Gall, in fair play and without bitterness.

His experiments were equally precise and simple. He operated cleanly and precisely without mutilating the tissues, and in the light of a few carefully formulated principles which rendered the results of his operations crucial. His problem was the determination of the functions of the different parts of the brain. He had two main principles. His first principle was that the experiments should bear "directly" upon the conclusion; that is to say, he would substitute the immediate observation of a correlation between a part of the brain and its function for the vague and indirect inferences that had previously been formed on the basis of pathological cases or by a more exclusively rationalistic process. His method was the method of extirpation of parts, and he is thus father of this extremely important method in the physiology of the brain. His emphasis upon "direct" observation amounts to an insistence upon carefully planned experiments as preferable to 'nature's experiments' that occur in accidental lesions and disease. It is the argument of the laboratory against the clinic. Flourens's second principle requires the isolation of the part whose function is to be determined. The satisfaction of this principle requires a clear notion in advance of the functional relationships that one seeks to study, and, on anatomical grounds, Flourens accepted as separate units for investigation the cerebral hemispheres, the cerebellum, the corpora quadrigemina, the medulla oblongata, the spinal cord, and the nerves, six units in all. Given this precise setting for the experiment, the method becomes one of operative technique. The given part must be removed cleanly, not merely mutilated, and without mutilation of the other parts. Flourens possessed this requisite skill; at any rate Cuvier and his contemporaries believed that he did.

We can best see Flourens's results and their great importance by a summary which notes the relation of his conclusions to the state of the problem in the twentieth century.

"The function of the cerebral lobes is willing, judging, remembering, seeing, hearing, in a word perceiving." The clean removal of the lobes at once abolishes voluntary action. The animal undisturbed may remain still until it dies of starvation; a bird will not fly unless thrown into the air. It also abolishes perception. With the lobes removed, the animal is blind and deaf; it does not respond to ordinary visual or auditory stimuli. Nevertheless it is sensitive to light, for the pupil still contracts to strong light. The modern view would be that the perception is abolished and that the sensory reflex remains. Presumably the same distinction must be made for the other senses, although it is not so easy to make the distinction in the tactual sphere. Perception occupies the superordinate relation to sensation or mere sensitivity that volition occupies to the immediate cause of movement. Thus the cerebrum is the seat of perception, intelligence, and the will.

"All the perceptions, all the volitions, occupy concurrently the same seat in these organs; the faculty . . . of perceiving, or willing constitutes thus only one faculty essentially a unit." The physiology of the late nineteenth century challenged this statement in its determination of various cerebral centers; but the tendency of research in the twentieth century is again in the direction of Flourens's statement; the belief in fixed centers is now yielding ground.

"The function of the cerebellum is the coördination of the movements of locomotion." With the cerebellum removed, an animal may attempt to walk, but falls. It is sensitive and moves; it can perceive and will; but it cannot accomplish the complex coördinated movements of walking, flying, or maintaining position. This conclusion is still good doctrine.

The medulla oblongata is the organ of conservation. As such, it is the 'vital knot' and is essential to the life of the organism, including the nervous system itself, for if the nervous system be divided caudad to the medulla, the distal region dies and the proximal lives, and if it be divided cephalad to the medulla, still the distal region dies and the proximal lives. The medulla is thus the vital center of the nervous system. It orders the sensations

before they are perceived; it brings together the volitions before they are executed in movement.

The corpora quadrigemina function for seeing; without them the animal is blind, though the cerebrum be intact. The function of the spinal cord is that of conduction; of the nerves, that of excitation.

"In the last analysis . . . all these essentially diverse parts of the nervous system have all specific properties, proper functions, distinct effects; and, in spite of this marvellous diversity of properties, functions, and effects, they nevertheless constitute of it a unique system." Thus the nervous system has a unity which comes about because in addition to the *action propre* of each part there is also an *action commune,* for the removal of any part reduces the energy of every other. "One point excited in the nervous system excites all the others; one point enervated enervates them all; there is a community of reaction, of alteration, of energy. Unity is the great principle that reigns; it is everywhere, it dominates everything. The nervous system thus forms but a unitary system." Such a statement anticipates by a century the view of the psychophysiological mechanism taken by the *Gestalt* psychologists of the present day.

Flourens's analysis of the brain into its essential unitary parts, although made on anatomical grounds, was justified by the results that the parts were actually differentiated in function. He felt that the analysis was still further substantiated by his discovery that a dose of opium produced the effect of the removal of the cerebral hemispheres and caused observable changes in the appearance of the hemispheres; that belladonna had the same relation to the corpora quadrigemina, and alcohol to the cerebellum.

In his notion of the unity of each part of the nervous system, that each represented essentially only a single function, Flourens was reinforced by his discovery that the parts "can lose a portion of their substance without losing the exercise of their functions," and that "they can reacquire it after having totally lost it." These facts of the recovery of function after its abolition by the extirpation of a part constitute the outstanding problem of the physiology of the nervous system to-day and are the primary reason for its gravitation back toward the position of Flou-

rens from the "new phrenology" of centers of the late nineteenth century.

It is plain that Flourens's formulation of his findings is couched against the functional atomism of phrenology. It is, however, primarily a justification of the experimental method, and as such it tells against Gall for his lack of experimental control, against Rolando for ill-defined isolation of factors, especially in clinical material, and against the philosophers who reasoned about the nature and seat of the soul without arranging a crucial empirical test. He found both unity and diversification of function. The *action propre* of each of the six principal divisions of the nervous system is an analysis that looks in the direction of Gall; the *action commune* of all of these parts is the assertion of a unity for which the philosophers had contended. The position is intermediate between the two opposing fields, and it is intermediate because it was thither that the experiments led.

It was in this way that the phrenologists and physiologists fixed in the first half of the nineteenth century the notion that the brain is the seat of the mind, and that the mental functions of the different structures of the brain form a fundamental physiological problem in the scientific study of this portion of the nervous system. Magendie, for example, although interested primarily in the nerves, and believing that the problems of the mental functions belong to ideology and the problems of intelligence to metaphysics, nevertheless represented this general view. He argued that the seat of the sensations is neither in the cerebrum nor in the medulla, but in the spinal cord, a most natural argument for the joint discoverer of the sensory function of the posterior nerve roots of the cord, and in a sense a correct argument. He appealed for proof to the phenomena that we now call reflexes, phenomena that occur after the removal of the brain and cerebellum. This view, however, is really also Flourens's in so far as it was perception rather than sensibility that Flourens ascribed to the cerebrum, and Magendie also admitted that the cerebrum perceives the sensations of the cord. He noted, moreover, that the cerebrum can reproduce these sensations and is thus the seat of memory, and he suggested further that there are different kinds of memory, memory for proper names, for substantives, for numbers, and so forth. Whether there are different organs for these different memories in the cerebrum he did not pretend to guess; he was

an opponent of Gall and waited upon the ideologists for a better formulation of the problem. Magendie also suggested, because of the differences in the brains of animals at different levels in the scale of animal development, that the number of convolutions of the brain might be correlated with the degree of "perfection or imperfection of the intellectual faculties." The principle that the brain is the organ of the mind was still further indicated to the scientists (for the public that accepted phrenology needed no further demonstration) by Desmoulins, a pupil of Magendie's, who in 1825 published a book which included his discovery that the brains of old people are lighter than the average for adults. He thus was able to attribute senility to atrophy of the brain. He failed, however, to gain acceptance for this view even among scientists, for his report on this matter had been indignantly rejected by the Académie des Sciences.

Histology of the Nervous System

After the period of which we have been speaking, a new interest, coupled, as is nearly always the case, with a new method, developed in the physiological study of the brain. Indirectly, this interest came also to bear on the problem of localization of function, but it was not this problem that determined it. It was rather the improvement of the microscope about 1830, which, as we have already seen, led to a decade of illuminating histological research. It was Rolando who, in 1824, had first thought of cutting thin sections of the tissue of the brain, chemically hardened, for microscopical examination. A little later, Johannes Müller discovered that potassium bichromate formed an excellent material for preserving and hardening tissue. A section, of course, does not give the solid structure that is under consideration, and it was not until 1842 that Stilling perfected a method of cutting a continuous series of sections so that structures, like nerve fibers, could be traced beyond the plane of the section. Meanwhile in 1833, shortly after the Lister who improved the microscope had described cells, Remak discovered that the gray matter of the brain is cellular, and in the same year Ehrenberg described the fibers of the white matter. It was not until 1858 that Gerlach discovered that staining with carmine brings out the details of a microscopical preparation, thus again, by the discovery of a

method, stimulating a profound interest in the minute structure of the tissues. Only now was it that the nerve-cells sprang, as it were, into sudden view.

In this same period, there was still another method of research that came into use. Nasse, in 1839, had found that a severed nerve trunk degenerates in its peripheral portion only. In 1852, Waller came to the conclusion that every nerve fiber is connected to a nerve cell, and that this 'secondary degeneration,' as it is called, occurs in the part of the nerve fiber distal to its cell. This fact led Waller to the formulation of a method for the tracing of nerve tracts: if a tract be severed, its course away from the center where its cells lie can be traced by following the degeneration, whatever the course of the fibers through the complicated structure of the brain and cord.

It is our purpose here simply to inquire into the state of the physiology of the nervous system at the beginning of the second half of the nineteenth century, the time at which 'physiological psychology' branched off from physiology on the one hand and philosophy on the other as an independent discipline. We should not, then, go further in this interesting development except to note that the much superior method of staining nervous tissue with nitrate of silver was not discovered by Golgi until 1873, nor was it until later that Golgi developed his theory of the nervous system as a network formed by the axon fibers and their collaterals. (To the dendrites he assigned only a trophic function.) The nature of the synapse, which showed the true function of the dendrites and the fact that each nerve cell and its fibers form an independent unit waited upon the research of Cajal; this discovery belongs to the year 1889. Cajal is thus the father of the neurone theory, so named by Waldeyer in 1891. At the period in which our present interest centers, it was supposed that the fibers merely formed a complicated network, anastomosing and dividing, and that the physiological account of mind was somehow to be gained from a further knowledge of this network.

At first glance this histological work seems to have but little bearing upon psychology; nevertheless, there is a very definite connection. Flourens left the brain a fairly simple organ. It consisted for him of a few gross parts, principally the cerebrum, the cerebellum, and the medulla; and each of these parts had its own peculiar functions. Within each part, however, Flourens

thought that no differentiation of function was to be found. Perceiving, willing, and judging, which had their common seat in the cerebrum, were after all simply different names for the single mental function of the cerebrum. To this extent the philosophers were right in their insistence on the unity of mind. So it was that, with the cerebrum, for example, serving only a single function and any part of it being able to perform that function (as recovery from operative lesion indicated), there was no demand for a study of differentiation within the cerebrum or any other of the principal parts of the brain. The histological work changed all this. The brain was now seen to be composed of an almost infinite number of separate cells, each bearing several processes, and some of these processes giving rise to long fibers, which pass in definite tracts through the brain and presumably connect the whole mass into a single complicated network. The brain seemed to be a tremendously tangled skein of fibers bearing in its structure a very large number of cells like beads on threads, with the whole mass arranged in a definite way, the full usefulness of which was not yet understood.

We shall see later that the psychology of associationism was within philosophy the dominant psychology of this period, and the scheme of mind for which the associationists stood is a mental arrangement that much resembles this physical arrangement of the brain. For the associationists, mind is composed of an infinitude of separate ideas, just as the brain is constituted of an infinitude of cells. But these ideas are bound together into more complex ideas or into higher mental processes by a huge number of associations, just as the nerve cells appear to be connected by fibers. There are laws of association and laws of nervous connection, though neither was yet sufficiently well established to raise explicitly the question of the explanation of the one in terms of the other. The important point is that the new picture of the brain, arrived at unpsychologically by discoveries in histological technique, nevertheless bore a close resemblance to the new picture of the mind that associationism yielded. It was not that men were thinking explicitly of a cell for every idea, although the apparent infinitude of both made the correlation reasonable as to numbers, but rather that the new knowledge of the division of the brain in its finer structure into many tiny units implied

that somewhere further separation of localized mental functions, like the ideas, was to be sought.

The Speech Center

The next definite step in the knowledge of the physiology of the brain was again immediately concerned with the localization of function. In 1861, the year after Fechner published the *Elemente der Psychophysik* and thus inaugurated an experimental method that is the exclusive property of a scientific psychology, Paul Broca (1824-1880) announced the localization of a center for speech at the base of the third frontal convolution of the left cerebral hemisphere. This date is, therefore, usually taken as marking the first scientific discovery of the localization of a mental function within a circumscribed region within one of the major divisions of the brain. Flourens's doctrine of the unity of the cerebrum had not before been successfully challenged.

As is usual in such cases, it appears that Broca did not possess absolute priority in his discovery. In 1825, J. B. Bouillaud had advanced the view on clinical evidence that the center for articulate speech lies in the anterior portion of the cerebral lobes. Bouillaud was a physician and an admirer of Gall, although not what one might call a phrenologist. He opposed Flourens's belief in the unity of the cerebrum, and suggested the existence of separate motor, perceptual, and intellectual organs in the brain. He also presented some evidence to the Académie des Sciences in 1827 for an experimental distinction between the anterior and posterior portions of the cerebrum, stating that the removal of the posterior portion does not abolish sensation. (This is not necessarily an incorrect statement; its correctness depends upon what is regarded as the posterior portion of the cerebrum, and whether sensory reflexes are to be taken as indices of sensation.) Bouillaud, however, did not prevail, nor did Dax, who presented a similar view in 1836. Bouillaud continued to hold his views, while making valuable contributions to a knowledge of the treatment of diseases of the heart, and in 1865, four years after Broca's discovery and forty years after his own, presented an elaborate paper to the Académie de Médecine extolling Gall, equating phrenology to scientific psychology, and reaffirming his own priority. It seems to be a case where Bouillaud chanced to be approximately right,

but Broca's conclusion was more precise and more carefully established. It is also not improbable that Bouillaud's championship of phrenology led the scientists to regard him less seriously than they otherwise would have done.

Broca's famous observation was in itself very simple. There had in 1831 been admitted at the Bicêtre, an insane hospital near Paris, a man whose sole defect seemed to be that he could not talk. He communicated intelligently by signs and was otherwise mentally normal. He remained at the Bicêtre for thirty years with this defect and on April 12, 1861, was put under the care of Broca, the surgeon, because of a gangrenous infection. Broca for five days subjected him to a careful examination, in which he satisfied himself that the musculature of the larynx and articulatory organs was not hindered in normal movements, that there was no other paralysis that could interfere with speech, and that the man was intelligent enough to speak. On April 17 the patient—fortunately, it would seem, for science—died; and within a day Broca had performed an autopsy, discovered a lesion in the third frontal convolution of the left cerebral hemisphere, and had presented the brain in alcohol to the Société d'Anthropologie.

There was nothing new about this method. For many years French surgeons, especially those connected with the École de la Salpêtrière (the Salpêtrière is a large government home and hospital for women that includes among its inmates many insane), had been doubting Flourens's doctrine of the unity of the nervous system and believing that they must find more specific localization of function within it. The mere fact that in mental disease disturbances of motor, of sensory, and of intellectual functions are not necessarily associated, kept them looking for a constant difference of brain lesion in the different cases. Some localizations had been seriously proposed, even though Bouillaud had not met with approval. Broca's merit lay first in his careful examination of a clean-cut case which chance threw into his hands, and secondly in the immediacy with which he seized upon the broader implications.

Citing now other cases in support of the crucial one, and showing for it that the defect did not lie in muscular movement, he concluded that he had to do with the loss of the memory for words, and that the left third frontal convolution contains the center for language. (Here Broca differs from Bouillaud, who was

inclined to emphasize the articulatory aspect of his aphasic case.) The fact that monkeys have this region and yet lack language is not, Broca thought, determinative, for they lack the intelligence to acquire language. Thus it appears that the center does not act in isolation, but in relation to intellectual development. Another fact of importance was Broca's conclusion that the convolutions of the brain furnish adequate topographical marks to use in connection with the problem of localization. The differences between the brains of animals, even between different mammals, had left the physiologists uncertain how to identify accurately a given point in the brain. Now it suddenly appeared that the convolutions may be significant as fixing the place of certain organs or centers, and it is even possible that differences between animal brains may mean some difference in mental functions.

More important, however, than either of these conclusions was Broca's enunciation of what he regarded as the necessary consequence of his discovery, the general principle of localization of function. "Il y a, dans le cerveau, de grandes régions distinctes correspondantes aux grandes régions de l'esprit." Here is a most instructive situation for the student of science. Thirty years before, both more and less, Gall and Spurzheim had argued vehemently to a receptive public for localization of mental functions in the brain, and the scientific world had refused to believe, at first on general considerations and later on the specific experimental evidence of Flourens. Now we find the scientific world accepting localization as a great discovery and listening willingly to Broca's demolition of Flourens: "Du moment qu'il sera démontré sans réplique qu'une faculté intellectuelle réside dans un point déterminé des hémisphères, la doctrine de l'unité du centre nerveux intellectuel sera renversée, et il sera hautement probable, sinon tout à fait certain, que chaque circonvolution est affectée par des fonctions particulières." Is science fickle? No, there was a difference, a difference of method. Flourens and Broca, although they appear to be upon opposite sides of a great controversy, both belong in the straight course of scientific progress because they held to the experimental method, which Gall did not do, and did not seek to transcend their observations, as did Gall. (Broca in generalizing added the saving "sinon tout à fait certain.") For the same reason, Broca's discovery was a great discovery even though the modern tendency is toward a return to Flourens's

position. The shift of view is not merely a swinging of the pendulum; it is rather the sinusoidal course that an advancing pendulum traces. We know more and more, even though we take back something that we once knew. And never are we at the end. "La science n'est pas; elle devient."

Electrical Localization of Function

Broca's contention, based on his clinical evidence, was not long in receiving support of a different kind from experimental physiology. In 1870, Fritsch and Hitzig announced the experimental discovery of the localization of motor functions in the cerebral cortex. This result reveals a very interesting situation with respect to scientific opinion. For half a century preceding, practically all physiologists had accepted the dogma of the 'inexcitability of the cerebral cortex.' Operations upon the cerebra of animals do not produce movements, and it was known that operations upon the brains of clearly conscious persons were accompanied by no sensory or other conscious phenomena. Mechanical and chemical stimulation of various sorts had been tried without results. Apparently the brain was both inexcitable and insensitive to any direct stimulation then known to physiologists. Magendie, Flourens, and many other more recent physiologists of note concurred in this view. It was the accepted dogma. It had not, however, been a universal finding. Haller had reported convulsive movements on forcing an instrument into the substance of the cerebral hemispheres. There had been other scattered reports of the direct stimulation of the brain. We have seen ourselves that Rolando stimulated electrically what he took to be the cerebellum. Fritsch and Hitzig offered an explanation of the growth of this dogma on two possible grounds. They found motor centers in only a limited region of the brain, and since, except in the conscious human subject, sensation could only be judged by movement, it seemed probable that the negative results meant merely that the chances were against the stimulation of the right region when there was not a systematic exploration of the entire surface of the cerebrum. They also discovered that hemorrhage reduced or even abolished excitability of the cortex, and that death also immediately abolished excitability. It is possible that some negative results were to be accounted for in this manner. That precautions

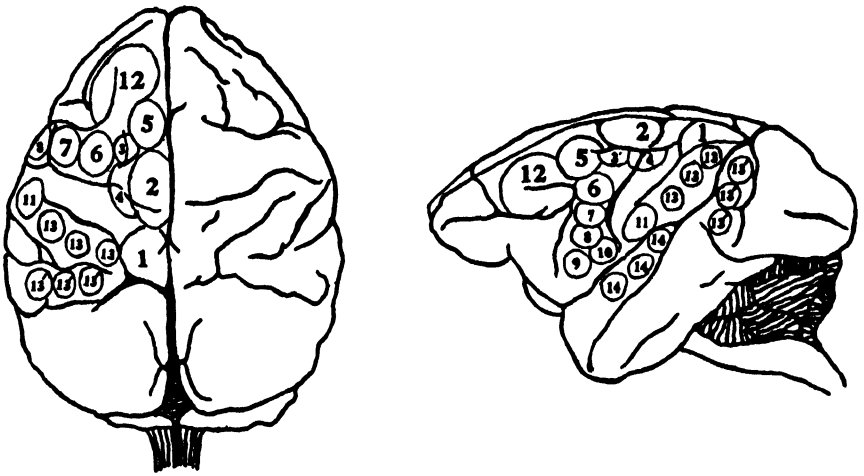

FIG. 4. LOCALIZATION OF MOTOR FUNCTION IN THE BRAIN OF THE MONKEY. After D. Ferrier, *Functions of the Brain*, 1876, 142.

1. Advance of opposite leg.
2. Movements of thigh, leg, and foot.
3. Movements of tail.
4. Retraction of opposite arm.
5. Apprehensile movements of opposite arm, hand, and fingers.
6. Flexion of forearm.
7. Retraction and elevation of angle of mouth.
8. Elevation of nose and upper lip.
9. Opening of mouth and protrusion of tongue.
10. Opening of mouth and retraction of tongue.
11. Retraction of opposite angle of mouth.
12. Opening of eyes, dilation of pupil, turning of head and eyes to opposite side.
13. Turning of eyes upward and to opposite side.
13'. Turning of eyes downward and to opposite side.
14. Pricking of opposite ear, turning of head and eyes to opposite side, dilation of pupil.
15. Torsion of lip and nostril on same side.

A comparison of this figure with Fig. 3 shows not only the difference between the old and the new phrenology in the kind and range of faculties localized, but also how much less clear-cut a localization the experimental method yielded as against the uncontrolled empiricism of Gall and Spurzheim.

against hemorrhage and death should not have been taken is very reasonable, when it is remembered that neither of these factors interferes with the excitability of the motor nerves. At any rate, they referred the discrepancy between dogma and experiment to technique; "the method," they remarked, "creates the results."

This famous joint experiment originated in Hitzig's observation that the electrical stimulation of the cortex of a man led to movement of the eyes. Hitzig verified this observation on a rabbit, and then, with the assistance of Fritsch, undertook a systematic study of electrical stimulation of the cerebral cortex of the dog. In a certain region of the anterior portion of the cerebral cortex they found that they could get movement uniformly. If the current was strong, the movements were convulsive and general. With weak current, however, they succeeded in finding different 'centers' for different groups of muscles—five in all in the first experiment: one for the neck, one for extension of the foreleg, one for flexion of the foreleg, one for the hind leg, and one for the face.

It was thus that the new and scientific 'phrenology' came into being. A tremendous amount of experimentation according to the new method was immediately begun. The findings of Fritsch and Hitzig were verified, and in a few years a very much more detailed map of the motor centers had been made out. We cannot trace the details of this development here, because our present purpose is simply to show how experimental physiology prepared the way for an independent experimental psychology, and we have already reached the '70's, when the separation of the two had been, perhaps, as completely established as it ever was to be. In this decade there were many men who added to our knowledge of the localization of the motor centers: Ferrier in England, Nothnagel in Germany, and Carville and Duret in France. It is also important to note that the clinical evidence of Hughlings-Jackson and of Brown-Séquard, as well as the work with the method of extirpation by Vulpian and Goltz in the preceding decade, all played into the hands of the men who were seeking a full account of the functions of the various parts of the brain. Ferrier's book on *The Functions of the Brain,* published in 1876, shows how rapidly experimentation had advanced and isolated observations had been assimilated to the central theory. The independent localization of a sensory center for tactual sensation, posterior to the motor region and separate from it, was not, however, accepted

as proved until about 1900, although the recognition of the visual center in the occipital lobes dates from Munk in 1881.

Notes

The history of the physiology of the brain, with especial reference to localization of function, from Aristotle to Fritsch and Hitzig (1870) is given most excellently and completely by J. Soury, "Cerveau," in C. Richet's *Dictionnaire de physiologie,* II, 1897, 547-670. Soury has expanded this discussion in his two-volume work, *Système nerveux central,* 1899, 1863 pp. The reader who consults either of these sources will realize how very scanty indeed is the discussion of the text.

For Bichat, see X. Bichat, *Recherches physiologiques sur la vie et la mort,* 1799-1800 (An VIII), Eng. trans., 1827. For the French physiologists on the visceral seat of the passions as anticipating James and Lange, see E. B. Titchener, *Amer. J. Psychol.,* 25, 1914, 427-447.

For Rolando's experiments and theory, see L. Rolando, *Saggio sopra la vera struttura del cervello e sopra le funzioni del sistema nervoso,* 1809. The work is summarized in French by Coster, Expériences sur le système nerveux de l'homme et des animaux; publiées en Italie en 1809, et répétées en France en 1822, *Arch. gén. de méd.,* 1, 1823, 359-418. If Flourens, whose memoirs were first reported in 1822, overlooked the Italian book, he at least made ample amends by reprinting Rolando's experiments in his own book, *q.v.*; P. Flourens, *Recherches expérimentales,* etc. (*vide infra*), 1824, 273-302.

Flourens

For a brief account of Flourens's work, see Soury, *op. cit.,* 1897, 616-619, or 1899, I, 518-522. Soury fully appreciates the lucidity of Flourens's mind. He remarks that, although the structure and the functions of the central nervous system are things infinitely complex and obscure, Flourens succeeded in realizing the complexity but not the obscurity. Flourens is himself so clear and concise that the reader need not seek to learn about him at second hand.

The fundamental reference is M. J. P. Flourens, *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés,* 1824, 331 pp. The additional memoirs were published as *Expériences sur le système nerveux,* 1825, 53 pp. The second edition of these two works bears the title of the first, and is dated 1842. Flourens observed in the preface that revision has been necessary, but the reader will find the essentials of the discussion unchanged. The work provides its own summaries; notably in the preface (1824, i-xxvi), at the close of the first three memoirs (1824, 121 f.), and in the chapter called "De l'unité du système nerveux" (1824, 236-241). Most of the quotations of the text are from these portions.

In an attempt to follow Flourens's rule for securing clarity by brevity, the text translates *sensation* and *sentir* as meaning "perception" when reference is to the cerebral hemispheres. The ambiguity of the French *sensation* is well known, and Flourens uses it in two senses, although his meaning is always clear in the larger context. The sensation that belongs to the medulla or the corpora quadrigemina is bare sensitivity as evidenced in a reflex, like the pupillary reflex. In one place he calls it *sentiment* for the medulla. The sensation of the cerebrum is the sensory experience that belongs to perception, contributes to judgment, and is bound up with the will. It is true that Flourens says that "les lobes cérébraux sont le siège exclusif des sensations, des perceptions et des volitions"; but then he

also says "de vouloir, de juger, de se souvenir, de voir, d'entendre, en un mot de sentir"; and we must remember that all these are not separate but "une faculté essentiellement une." Cerebral sensation is plainly perception in the modern sense, and thus Joh. Müller, in summarizing Flourens, notes that in the cerebrum "die Empfindungen nicht bloss bewusst werden, sondern zu Anschauungen, Vorstellungen umgeshaffen." On the ambiguity of *sensation* and *sentiment* as applied to feeling, see E. B. Titchener, A note on *sensation* and *sentiment*, *Amer. J. Psychol.*, 25, 1914, 301-307.

It is quite clear that Flourens has given a positive meaning to the problem of the 'seat' of the faculties, a meaning that ought to have rendered obsolete such contentions as Bichat's, only a quarter of a century earlier, that the seat of the passions is in the viscera. On this point in Bichat, see Flourens, *De la vie et de l'intelligence*, 2d ed., 1858, 142-160, 251-261.

The key to Flourens and the doctrine of Gall is Flourens's *Examen de la phrénologie*, 1842. It puts the case of science against phrenology, and it also shows how seriously the phrenological vogue had to be taken by scientists. In the preface Flourens wrote: "The 17th century enthroned the philosophy of Descartes; the 18th that of Locke and Condillac; should the 19th enthrone that of Gall? . . . I cite Descartes often; I do more, I dedicate my book to him. I write against a bad philosophy, and I call back the good."

The cycle of fashions in theories of localization in the brain is interesting. First there was little thought of localization of any exact kind. Then came phrenology, with a most minute and specific system of localizations. Then Flourens ushered in the new view with a compromise: localization in the gross, but not in the fine. With Fritsch and Hitzig in 1870 and the subsequent physiology, the pendulum swung back toward Gall: a center for everything, if a center could but be found. Now it is going the other way back toward Flourens, and the physiology of brain centers has been called the "new phrenology," as an opprobrious epithet. See S. I. Franz, New phrenology, *Science*, N. S. 35, 1912, 321-328; K. S. Lashley, Studies of cerebral function in learning, *Psychobiol.*, 2, 1920, 55-128; *J. Comp. Psychol.*, 1, 1921, 453-468; *Amer. J. Physiol.*, 59, 1922, 44-67; *Brain*, 44, 1921, 255-285; *Arch. Neurol. and Psychiat.*, 12, 1924, 249-276; etc.

For the similarity of Flourens's view on the unity of the nervous system with the view of some modern *Gestalt* psychologists, see W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, esp. *Abs.* ii and iii.

The text omits reference to much important work of Flourens that does not belong in the present context. The work of 1824, first reported in 1822, describes experiments on the nerves which show that the primary division among nerves is between the sensory and motor functions. Thus Flourens verified the distinction made by Bell's law in the very year that Magendie was reporting this same law as an independent discovery. Flouren's experiments on the semicircular canals (1824, 1830) are, with Purkinje's work (1820, 1827), the pioneer studies on 'vestibular equilibration.' And Flourens was also interested in anthropology.

Brain Physiology

The reader who wishes a picture of the physiology of the brain as it was known in the second quarter of the nineteenth century will do well to read Joh. Müller's account of it in *Handbuch der Physiologie des Menschen*, I, bk. iii, sect. v, chap. 3, or the English translation. He will note that Müller relied principally upon Flourens and the knowledge gained from pathology. For Magendie, see his *Précis élémentaire de physiologie*, 1816-1817, but esp. 2d ed., 1825, Eng. trans., 1826; and also his *Anatomie comparative du*

cerveau, 1826. For Antoine Desmoulins (1796-1828), see his *Anatomie des systèmes nerveux des animaux vertébrés*, 1825, esp. II, 595-637. Magendie was Desmoulins's collaborator in the physiological portions of this book, although he is referred to in the third person.

While the discovery of secondary degeneration is due to Nasse, [*Müller's*] *Arch. für Anat. und Physiol.*, 1839, 405-419, it is more often associated with Waller and sometimes called Wallerian degeneration, because he employed it as a method of tracing nerve tracts. See A. Waller, *Philos. Trans.*, 1850, 423-429. Golgi's original paper of 1873 was in Italian, but his later book was translated into German: C. Golgi, *Untersuchungen über den feineren Bau des centralen und peripherischen Nervensystems*, [1885], German trans., 1894. S. Ramon y Cajal wrote in Spanish and is available largely in secondary sources. The reference to the original description of the synapse is said to be *Riv. trimestr. micrograph.*, 1, 1889, 2 ff. The neurone theory was formulated by W. Waldeyer, *Ueber einige neuere Forschungen im Gebiete der Anatomie des Centralnervensystems*, 1891. For an excellent historical summary of the work of Golgi and Cajal, see J. Soury, Histoire des doctrines contemporaines de l'histologie du système nerveux central; théorie des neurones, *Arch. de neurol.*, 2me sér., 3, 1897; Golgi, 95-118; Cajal and the theory of neurones, 281-312.

On the general development of microscopy and cytology in this period, and the effect of the improvement of the microscope upon research, see E. Nordenskiöld, *History of Biology*, 1928, 389-405.

Speech Center

J. B. Bouillaud's original paper on the speech center is Recherches clinique à démontrer que la perte de la parole corréspond à la lésion des lobules antérieurs du cerveau, et à confirmer l'opinion de M. Gall sur le siège de l'organe du langage articulé, *Arch. gén. de méd.*, 8, 1825, 25-45. His later defense of localization was read before the Académie de Médecine in 1865. The other claimant for priority in this matter is M. Dax, who read a paper to the Congrès méridional at Montpellier in 1836, which was not published until 1865; *Gazette hebdomadaire de méd. et de chir.*, 2me sér., 2, 1865, 259-262. For Broca's original paper, see C. Broca, *Bull. de la Soc. anat.*, 2me sér., 6, 1861, 330-357.

Localization

On localization in the '70's, see G. Fritsch and E. Hitzig, Ueber die elektrische Erregbarkeit des Grosshirns, [*Reichert und Du Bois-Reymond's*] *Arch. f. Anat. und Physiol.*, 1870, 300-332; D. Ferrier, *The Functions of the Brain*, 1876, 2nd ed., 1886, describing researches which began in 1873; H. Nothnagel, [*Virchow's*] *Arch. f. pathol. Anat. und Physiol.*, 57, 1873, 184-214; 58, 1873, 420-436; 60, 1874, 128-149; 62, 1875, 201-214; C. Carville and H. Duret, *Arch. de physiol.*, 2me sér., 2, 1875, 352-491; and many other writers of this and the subsequent period.

Chapter 5

SPECIFIC ENERGIES OF NERVES

WE have seen that the establishment of Bell's law of the sensory and motor functions of the spinal nerve roots served once for all to separate these two functions within physiology, creating a primary dichotomy within the nervous system. So far, we have sought to ignore the sensory problems in order to concentrate upon other topics, but it is plain that 'the unity of the nervous system' is such that we have not been completely successful. Bell's law was as much a law of sensation as of movement. Reflex action is movement where the sensory impulse is 'reflected' into motor channels. The faculties of phrenology are as much intellectual as conative, and at least seven, like coloring and tune, are purely perceptual if not sensory. Gross localization of function in the brain assigned a seat to sensation, in the medulla or in the cerebrum, according to what was meant by sensation. The new phrenology of cerebral centers (1870) dealt first with motor functions because movement could be easily observed, and, at this degree of refinement, introspection rather than inference from behavior was necessary for the establishment of sensory centers; nevertheless, the finding of motor centers, together with large regions of the cortex for which excitation gave rise to no movement, immediately implied that somewhere in the seemingly inexcitable regions sensory centers might lie. If there had been no other development in the physiology of the nervous system, we should, nevertheless, have been tracing a certain development in the physiological theory of sensation.

Bell and Müller on Specific Energies

The most general and probably, therefore, the most important event in sensory physiology in the first half of the century is the formulation of the theory of the specific energies of nerves. This doctrine traditionally bears the name of Johannes Müller, and,

when it is to be distinguished from its extension by Helmholtz, it is called the Müllerian theory. It is interesting to discover that there is in this doctrine not one single principle that is new with Müller; everything of importance had already been stated by Sir Charles Bell, and there is no doubt that Bell himself thought as clearly in these matters as did Müller. For this reason, some persons have argued that Bell's name rather than Müller's should be attached to the doctrine. It seems probable, however, that, were this to be done, the critics would have little difficulty in showing that the essential facts were all known before Bell also; that, of the two most important principles, Aristotle implied one and the other has been explicitly a familiar doctrine in philosophy, at least since Descartes and Locke. In other words, we are dealing here simply with the continuity of thought that so frequently makes it impossible to date a discovery or to assign a theory to its originator. Certainly enough was known and already established in doctrine for the theory to have been formulated before the nineteenth century. Certainly Bell deserves credit for his insight in bringing various observations together and showing their meaning clearly. What Bell did not do was to organize this view as a distinct theory or to adopt a terminology that would serve to give it a name. Had he been less modest about his own achievement and in the degree of printed publicity which he gave it, we might have come to regard this theory as one of Bell's laws. Müller, because he comes later, deserves less credit for his insight; if this view was after all more or less the obvious fact to a thoughtful person in 1811 when Bell wrote of it, it must have been more nearly obvious in 1826 when Müller first wrote, and in 1838 when he gave it systematic position. One champion of Bell has even pointed out that Müller was familiar with Bell's early writing and may have been influenced by him, but the solution of this question bears not at all upon the importance of the theory in experimental physiology and psychology.

It seems quite clear, that, had it not been for Müller, many things that did happen would not have happened. Müller gave the theory explicit and precise formulation. It occupies almost 2 per cent of the entire volume of his *Handbuch.* He gave it, practically, a name. By including it in the *Handbuch,* he gave it also the weight of his own great personal authority and of the publicity of that important compendium. In short, though he may

have originated nothing in it, he placed the seal of orthodoxy upon it. Had this not occurred, we might never have had Helmholtz's theory of hearing, now classical in itself. Conceivably we might not have had Helmholtz's or Hering's theories of vision. We should not have had the discovery of the sensory spots in the skin in the manner of its occurrence, for these researches were suggested explicitly by the theory. In time the doctrine became almost a dogma, and the fact that it is coming to be doubted nowadays should not detract from its importance in stimulating discovery. Whether the doctrine should bear Müller's name is a matter to be decided, not by the historian, but by those who are more concerned with ethics than facts.

Johannes Müller formulated the doctrine of the specific energy of nerves under ten laws. To the modern reader who has not to combat the same current beliefs as Müller, these laws seem somewhat repetitious. We shall, at any rate, not quote them as they were given, for they can readily be found thus in other sources; rather we shall attempt to examine the principles contained in them, referring to the laws by number (I-X), and noting the empirical and historical background of each principle.

1. The central and fundamental principle of the doctrine is that we are directly aware, not of objects, but of our nerves themselves; that is to say, the nerves are intermediates between perceived objects and the mind and thus impose their own characteristics upon the mind.

Müller's dictum was: "Sensation consists in the sensorium receiving through the medium of the nerves, and as the result of the action of an external cause, a knowledge of certain qualities or conditions, not of external bodies, but of the nerves of sense themselves" (V). "The immediate objects of the perception of our senses are merely particular states induced in the nerves, and felt as sensations either by the nerves themselves or by the sensorium" (VIII).

Bell's position was similar: "It is admitted that neither bodies nor the images of bodies enter the brain. It is indeed impossible to believe that color can be conveyed along a nerve; or the vibration in which we suppose sound to consist can be retained in the brain: but we can conceive, and have reason to believe, that an impression is made upon the organs of the outward senses when we see, hear or taste." "The idea in the mind is the result of an

action excited in the eye or brain, not of anything received, though caused by an impression from without. The operations of the mind are confined not by the limited nature of things created but by the limited number of our organs of sense."

Now there is nothing new in the idea that the nerves are intermediaries between the external world and the brain; that had been said by Herophilus and Erasistratus (*ca.* 250 B.C), and had been common doctrine since Galen (*ca.* 200 A.D.). That the nerves would thus impose their own nature upon the mind is a necessary consequence of the materialistic view of immediate causes, and of the brain as the organ of mind. Epistemologically, this principle in Müller has been characterized as "a fruit of the anthropocentric standpoint, as the newer philosophy from Descartes down to Kant and Fichte has developed it," and also as the mere "physiological counterpart of a Kantian category." It is more likely that Bell felt unconsciously the tradition of British empiricism.

It was essentially on this matter that Hartley wrote in his *Observations on Man* in 1749: "The white medullary Substance of the Brain is also the immediate Instrument, by which Ideas are presented to the Mind: or, in other words, whatever Changes are made in this Substance, corresponding Changes are made in our Ideas." "External Objects impressed upon the Senses occasion, first in the Nerves on which they are impressed, and then in the Brain, Vibrations of the small, and as one may say, infinitesimal, medullary Particles."

That the idea in the mind is the result "not of anything received" (Bell's phrase) from the external object by way of the nerves, that "the nerves of the senses are not mere conductors of the properties of bodies to our sensorium" (Müller's phrase), was apparent in Locke's doctrine of secondary qualities in 1690. Locke wrote: "To discover the nature of our *ideas* the better, and to discourse of them intelligibly, it will be convenient to distinguish them, as they are ideas or perceptions in our minds: and as they are modifications of matter in the bodies that cause such perceptions in us; that we may not think (as perhaps usually is done) that they are exactly the images and resemblances of something inherent in the subject; most of those in sensation being in mind no more the likeness of something existing without us than the names that stand for them are the likeness of our ideas, which yet upon hearing they are apt to excite in us." While the correspon-

dence between the idea and the perceived body is close in the case of the primary qualities, it is not in the case of secondary qualities: "such qualities, which in truth are nothing in the objects themselves, but powers to produce various sensations in us by their primary qualities, *i.e.*, by the bulk, figure, texture, and motion of their insensible parts, as colours, sounds, tastes, &c., these I call *secondary* qualities." "If, then, external objects be not united to our minds when they produce ideas therein; and yet we perceive these *original* qualities in such of them as fall singly under our senses, it is evident that some motion must then be continued by our nerves, or animal spirits, by some parts of our bodies, to the brains or seat of sensation, there to produce in our minds the particular ideas we have of them."

2. No less important in the doctrine than this conception of the relation of the nerves to the mind is the principle of specificity. There are five kinds of nerves, and each imposes its specific quality upon the mind.

Müller's statement is: "Sensation consists in the sensorium receiving . . . a knowledge of certain qualities . . . of the nerves of sense themselves; and these qualities of the nerves of sense are in all different, the nerve of each having its own peculiar quality or energy" (V). "The nerve of each sense seems to be capable of one determinate kind of sensation only, and not of those proper to the other organs of sense; hence one nerve of sense cannot take the place and perform the function of the nerve of another sense" (VI).

Similarly Bell: "The operations of the mind are confined . . . by the limited number of our organs of sense." "If the retina were sensible to the matter of light only from possessing a finer sensibility than the nerve of touch, it would be a source of torment; whereas it is most beneficially provided that it shall not be sensible to pain, nor be capable of conveying any impression to the mind but those which operate according to its proper function producing light and colour." "The nerve of vision is as insensible to touch as the nerve of touch is to light."

Now this principle adds to the first only the notion that there are a few specific qualities or energies, which are immutable functions of the separate senses. There is little here that is not contained in Aristotle's original doctrine of the five senses. We may cite Aristotle directly: "In discussing any form of sense-perception

we must begin with the sensible object . . . By the 'peculiar object of sense' I mean a sense-quality which cannot be apprehended by a sense different from that to which it belongs, and concerning which that sense cannot be deceived, e.g. color is the peculiar object of vision, sound of hearing, flavor of taste. Touch, however, discriminates several sense-qualities. The other particular senses, on the contrary, distinguish only their peculiar objects, and the senses are not deceived in the fact that a quality is color or sound . . . To the objects of sense, strictly regarded, belong such properties as are peculiarly and properly sense-qualities, and it is with these that the essential nature of each sense is naturally concerned." "It is necessary that if any sensation is lacking, some organ must also be lacking in us." This is the doctrine which had long been the common belief about senses. It is to be noted that it even asserts for touch a multiplicity of sense-qualities, an assertion that anticipated the nineteenth century discovery of several 'specific energies' within this sense-mode.

3. The third principle of the doctrine of specific energies deals with the nature of the empirical evidence for the first two principles. It asserts that the same stimulus affecting different nerves gives rise to the different qualities appropriate to the particular nerves, and, conversely, that different stimuli affecting the same nerve always give rise to the peculiar quality for that nerve.

Müller devoted three laws to this matter. "The same internal cause excites in the different senses different sensations;—in each sense the sensations peculiar to it" (II). "The same external cause also gives rise to different sensations in each sense, according to the special endowments of its nerve" (III). "The peculiar sensations of each nerve of sense can be excited by several distinct causes internal and external" (IV). In support of these laws, Müller brought a very great deal of simple empirical evidence to bear. A blow on the head may be enough 'to give a person what will make his ears ring,' or 'what will make his eyes flash fire,' or 'what will make him feel.' "Pressure on the eye-ball gives rise to colors." An electrical stimulus can, according to the evidence which Müller accepted, become the cause of any one of the five sensations as it affects one nerve or another. Müller is especially complete in enumerating instances of this kind. Plainly then, among a multiplicity of causes the quality of sensation depends,

not upon the nature of the cause, but upon the nature of the nerve which the cause affects.

If Bell was less replete with instances than Müller, still he was not less sure. "It is also very remarkable that an impression made on two different nerves of sense, though with the same instrument, will produce two distinct sensations; and the ideas resulting will only have relation to the organ affected." "In the operation of couching the cataract . . . the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision, . . . but, . . . when the needle pierces the eye, the patient has the sensation of a spark of fire before the eye." "When the eye-ball is pressed on the side, we perceive various coloured light. Indeed the mere effect of a blow on the head might inform us that sensation depends on the exercise of the organ affected, not on the impression conveyed to the external organ; for by the vibration caused by the blow, the ears ring, and the eye flashes light, while there is neither light nor sound present." Even touch and taste, Bell thought, can be discriminated in mechanical stimulation of the tongue.

Now some of these illustrations come from what was then modern technique and some from common sense. To the former group belong Bell's instance of the operation upon the eye, all the experiments on stimulating the senses electrically (Volta, the inventor in 1800 of the Voltaic pile, described some of these), Magendie's observations that the retina, the optic nerve, and the olfactory nerve give no signs of pain on being pricked, and Tourtual's observation that, in extirpating the human eye, the section of the optic nerve gives rise to "the perception of a great light." In the antiquity of the notion, however, Müller himself believed. He suggested that even Plato possessed an imperfect idea that internal causes other than light could give rise to the sensations of light and color. He cited Aristotle on dreams to the same end, and noted that Spinoza observed that the colors seen after gazing at the sun occur in the absence of light. Another historian goes further and cites Aristotle as knowing that light sensations may follow upon mechanical stimulation.

The contribution of Bell and Müller to theory really lies in this point. There was evidence long before them of the independent variability of stimulus and sense-quality, but it was scattered, meager, and casual. They brought the new evidence together; they

found more of it; and some of it partook of the nature of experimentation.

4. When we take up the doctrine in this order, the fourth point seems trivial. It consists merely in Müller's emphasis upon the equivalence of internal and external stimuli. It seemed important to Müller because the localization of the mind in the brain had only recently become unquestionable doctrine, and, unless the mind were limited to some restricted portion of the body, any internal condition could be thought of as acting directly upon the mind and not by way of the nerves.

Müller thus devoted his first law to this point. "External agencies can give rise to no kind of sensation which can not also be produced by internal causes, exciting changes in the condition of our nerves." For this reason he separated the second and third laws, quoted above, so that he might deal separately with the internal and external causes. Bell did not raise this point because he was taking it for granted that only the brain is the organ of the mind: "all ideas originate in the brain: the operation of producing them is the remote effect of an agitation or impression on the extremities of the nerves of sense."

The entire history of the notion of the brain as the seat of the mind is the necessary preparation for this view. When this matter is settled, the point becomes redundant, although Müller avoided an appearance of redundancy by dealing with the matter first.

5. To say that the mind is directly aware only of the state of the nerves is to raise at once the problem of how it becomes aware of external objects, the fundamental problem of knowledge. Müller's answer to this question lay first in the relation of the nerves to external objects. The nerves, like all other objects, have definite relations to external objects. Unless an external agent possess certain properties, it affects a particular nerve not at all or only exceptionally. Plainly, the eye ordinarily perceives light, but not pressure. Exceptionally it may perceive pressure, but then it perceives it as color. And so with the other senses.

Müller's manner of expressing this point was as follows. "Inasmuch as the nerves of the senses are material bodies, and therefore participate in the properties of matter generally occupying space, being susceptible of vibratory motion, and capable of being changed chemically as well as by the action of heat and electricity, they make known to the sensorium, by virtue of the changes thus

produced in them by external causes, not merely their own condition, but also the properties and changes of condition of external bodies. The information thus obtained by the senses concerning external nature, varies in each sense, having a relation to the qualities or energies of the nerve" (VIII).

Bell met the problem almost as explicitly. Of the ideas, which "originate in the brain," he said: "Directly they are consequences of a change or operation in the proper organ of the sense which constitutes a part of the brain . . . It is provided, that the extremities of the nerves of the senses shall be susceptible each of certain qualities in matter; and betwixt the impression of the outward sense, as it may be called, and the exercise of the internal organ, there is established a connection by which the ideas excited have a permanent correspondence with the qualities of bodies which surround us."

This view really takes an implicit account of what has later come to be regarded as a distinction between the adequate and, as it is called, the 'inadequate' stimulus; and although such terminology is recent, the recognition of the essential fact is as old as Aristotle. It is plain that the eye most readily and naturally perceives light; the ear, sound; the skin, pressures; and so on. A pressure is not actually an 'inadequate,' but a less adequate, stimulus to vision. Sounds may be felt, but less readily than pressures. In other words, because there is a predominant and more adequate relation of the stimulus to the nerve, we come predominantly to perceive objects truly by way of the nerves. When an inadequate stimulus is effective, illusion results.

Müller further explained the fact that we believe that we perceive objects rather than the states of our nerves by pointing out that a sensation may not be localized in its actual seat. "That sensations are referred from their proper seat towards the exterior, is owing, not to anything in the nature of the nerves themselves, but to the accompanying idea derived from experience" (IX). Bell dealt with the object in a similar manner: "By induction we know that things exist which are not yet brought under the operation of the senses." All this is little more than the old problem of the object in idealistic philosophy; and Müller's discussion dealt further with the empirical genesis of perception through experience.

6. There is still the question of the locus of the specific prin-

ciple, whether it lies in the nerve or at one or the other termination. Müller had no certain answer to this question. "It is not known," he said, "whether the essential cause of the peculiar 'energy' of each nerve of sense is seated in the nerve itself, or in the parts of the brain or spinal cord with which it is connected; but it is certain that the central portions of the nerves included in the encephalon are susceptible of their peculiar sensations, independently of the more peripheral portion of the nervous cords which form the means of communication with the external organs of sense" (VII). Bell noted of the general theory that "there is here no proof of the sensation being in the brain more than in the external organ of sense. But when the nerve stump is touched the pain is as if in the amputated extremity." We have already quoted Bell's assertion that "all ideas originate in the brain"; that "directly they are consequences of a change or operate in the proper organ of the sense which constitutes a part of the brain."

This addendum to the doctrine is important, for it tended to localize the specificity in the brain. The stimulation of the proximal ends of severed nerves shows that the specificity is not in the sense-organ or in the peripheral portion of the nerve. If it is not in the peripheral portion, there is a certain presumption against its being in the central portion, and thus by a process of elimination it would have to be in the central termination. Such a view plays at once into the hands of those who would find localization of function in the brain. A belief in sensory centers for the five senses is only a step further. Such a belief was not for the moment a popular view, not because it was new, but because it was too familiar in phrenology. With the 'new phrenology' that followed Fritsch and Hitzig (1870), science was ready to accept a belief in such centers and to find evidence, some of it rigorously experimental, for localizing them. Müller's seventh law, in a way, then, foreshadowed the accepted gospel of the late nineteenth and early twentieth centuries.

7. We need finally but to mention briefly that Müller in his last law discussed the selective power of the mind over against the specific energies. This point is an entirely gratuitous addition, which goes to show the degree of completeness which Müller sought in his compendium. The mind "has a direct influence" upon sensations, "imparting to them intensity"; that is to say, we can attend to parts of the visual field to the exclusion of others, or to

parts of the tactual field, or we can make the same discrimination with respect to time in the case of hearing. Moreover, the mind "also has the power of giving to one sense a predominant activity." Bell barely touched on the point: "over these organs [of sense in the brain], once brought into action by external impulse, the mind has an influence." Of course, selection, attention, or determination (and there are many other terms for the same thing) has remained a persistent problem in psychology. It is interesting to see that Müller, a physiologist, was unable to avoid the problem; but then, Müller perhaps ought also to be considered as an early nineteenth century experimental psychologist, for one of the eight major sections of the *Handbuch* is entitled "Of the Mind," and deals, not with sensation and movement, which are treated elsewhere, but with what have since been called the 'higher mental processes.' The matter, of course, was not new with Müller; the problem of attention is very old, but had remained with the philosophers and not with the scientists.

As is always the case with a broad doctrine presented in elaborate detail, Müller's theory led to criticism, which came in this instance from Lotze, E. H. Weber, and others. It was possible, in particular, to criticize the factual evidence marshaled in support of the doctrine. Some facts were not easily verifiable, and remained doubtful as to their exact nature. Especially is it not always clear that the effect of the stimulus is as simple as the name of the stimulus implies. For instance, Weber wondered whether the electrical excitation of sound might not be due to the electric stimulation of the muscles of the middle ear, which might thus act upon the drum-skin and produce at least the mechanical equivalent of sound at that point. In spite of the argument from the effectiveness in certain cases of inappropriate stimuli, it became obvious that most inappropriate stimuli are completely inadequate. It is plain that light is never the stimulus to hearing, taste, or smell (though it may be to warmth); that heat or cold never gives rise to sight, sound, or odor; that the stimuli for taste and smell are quite ineffective in producing sights or sounds. If they were, illusion might be so common that the perception of reality would be seriously hampered. Such criticism does not alter the central doctrine which rather depends upon these facts. It did serve, however, to show the naïveté of the distinction between appropriate and inappropriate stimuli. If a metallic rod gives rise to pressure on the

nerve of touch and to taste on the nerve of taste, still there is no reason to believe that this distinction occurs for any other reason than that the rod is sapid as well as heavy; no one is surprised because a lump of sugar on the skin arouses pressure. If sounds are sometimes felt, it is simply because they consist of vibrations, and as such are mechanical as well as acoustic stimuli. In other words, the entire argument from 'inadequate' stimuli simply reflects the then popular conception that somehow or other the nerves conducted to the brain, not exactly properties of the object, but rather incorporeal copies of the object. In spite of the philosophical sophistication of the times, this was the view that both Bell and Müller explicitly opposed, and as against that point of view, it is important to see that very different objects produce the same effect by the same nerve, and the same object very different effects by different nerves, provided the nerves are affected at all. Certainly the criticism cleared the air of a misleading epistemology, although it left the theory unscathed. It became plain that the doctrine was essentially nothing more than the fact that a given nerve, however affected, if affected at all, gives rise to a sense-quality that depends only on the specific character of the nerve. The matter of adequate stimuli thus became another problem, and here it was clear that the relationships were to be understood by reference not to the common-sense classes of objects, but rather to the physical nature of the stimuli and their effects. The physiologist was brought back to Locke's doctrine of secondary qualities, dependent upon the "powers" of the object, "which in truth are nothing in the objects themselves."

Extension of the Doctrine of Specific Energies

While this criticism was going on, the so-called extension of the Müllerian theory had already begun. The extension consists merely in applying the principle of the dependence of qualitative difference upon specific nerves to differences of quality within a single modality of sense. This development is usually attributed to Helmholtz, who made such effective use of it in his theory of hearing; but once again we find that the man who gave his name to the theory was not the very first to propose it. Priority seems to belong to Natanson, who made this suggestion in 1844, only six years after Müller had formally promulgated the complete,

systematized doctrine in his *Handbuch.* Volkmann followed him immediately with a similar view.

Natanson laid down the fundamental principle that every organ of the nervous system has but a single function. It follows that there must be as many organs as functions—that is to say, as sensory qualities. This proposition led Natanson to posit separate nerves for temperature, for touch, and for the perception of resistance; for sweet, sour, and bitter; for the simple smells (which he could not of course at that time name); and for the fundamental colors, red, yellow, and blue. In other words, he divided up four of the five senses into what he regarded as simple sensations. No such analysis was available for the tones, and Helmholtz's boldness in supposing several thousand specific auditory energies was too great a complication for him to dare.

There was, however, nothing so bold in Helmholtz's theory of vision. This part of the *Handbuch der physiologischen Optik* Helmholtz published in 1860 and it follows the lines of an earlier paper of 1852. He stated the theory concisely, attributing it to Thomas Young. "The eye is provided with three distinct sets of nervous fibers. Stimulation of the first excites the sensation of red, stimulation of the second the sensation of green, and stimulation of the third the sensation of violet." It is true that Helmholtz did not at this point mention Johannes Müller or the doctrine of the specific energies of nerves (although he added such a paragraph in the second edition, completed in 1896); but this omission must have been because of its obviousness. Helmholtz dealt with the Müllerian doctrine explicitly and at length earlier in the same section of the *Handbuch,* developing it for various kinds of stimulation of the eye. He is known to have regarded the theory as one of the great fundamental principles in physiology, comparable to Newton's theory of gravitation in physics. It is not clear, however, just how conscious Helmholtz was that he was extending Müller's dogma. In one place in the *Handbuch* he presented the original theory of specificity for the five senses, and then three sections later wrote the sentence quoted above, as if Natanson's fundamental principle were to be taken as a matter of course.

That Helmholtz attributes this theory to Young, who wrote in 1807, makes one wonder whether Young anticipated all the others in both the old and the new theory of specific energies. Such, however, is not the case. Young's theory, where it was directly pre-

sented, was stated very concisely without mention of the nerves. In another remote passage he devoted a few lines to the physiology of the theory. "Each sensitive point of the retina," he observed, must contain "a limited number of particles" that vibrate in resonance with the frequencies of "the three principal colours, red, yellow, and blue." These particles excite the nerve, and it is possible that "each sensitive filament of the nerve may consist of three portions, one for each principal colour." This was the doctrine of the specific energy of nerves only in so far as it was the reasonable way of thinking about a particular problem even before Charles Bell.

Helmholtz's resonance theory of hearing first appeared in 1863. In it he made explicit and conscious use of Müller's doctrine. Continuous homogeneous sounds can be analyzed into harmonic components. Resonators make such an analysis, and we also seem to be able to make it introspectively. The thing to look for in the ear is, therefore, a set of resonators, each of which will give rise separately to a tonal sensation. Helmholtz looked and found (for the first edition of *Die Lehre von den Tonempfindungen*) the arches of Corti. Later he thought that transverse fibers of the basilar membrane were better, but the argument remained the same. There had been estimated to be about 4,500 outer arch fibers. If every one of these arches resonates best to a different frequency, is tonal hearing explained? Yes, if we but take "a step similar to that taken in a wider field by Johannes Müller," for each arch fiber excites a different nerve fiber, and the separate tonal sensations as well as the harmonic analysis of complex clangs by the ear are accounted for. Of course, this step means 4,500 specific auditory energies (and later research doubles or trebles this number), but it was the logical, almost irresistible step to take, and Helmholtz did not hesitate to take it.

We may merely indicate here the later developments of the theory. For a considerable period it was the only theory available for the physiological explanation of qualitative difference. Blix and Goldscheider, independent discoverers in 1883-84 of the separate sensory 'spots' in the skin for warmth, cold and pressure, were consciously seeking separate endings because they seemed to be required by this theory. The analysis of taste into sweets, sours, salts, and bitters came to be taken as indicating four specific gustatory energies. Hering's theory of vision assumed six visual ener-

gies—a further modification, since a pair of nerve fibers would have to be excited by the two antagonistic processes of a single visual substance: assimilation of one substance would give rise to red by exciting the appropriate fibers; dissimilation of the same substance would give green by exciting the other fiber; and so for the other two pairs. In the present century, there has been some tendency to examine frequency of excitation as a possible correlate of quality within the sense-mode, but in the nineteenth century, the doctrine of Müller and Helmholtz, in spite of persistent criticism, reigned almost supreme.

One important effect of this acceptance of the specific energies was the support that it ultimately gave to the theory of cerebral localization. We have seen that Müller was led to conclude that the seat of the specific differences must lie in the brain or the central portion of the nerves. Helmholtz, who in 1863 created the analogy of the nervous system to a telegraph system, argued that the nerves were all alike and indifferent conductors of excitation, and that the specificity must therefore lie in the brain. Du Bois-Reymond, whose work on the electrical nature of nervous conduction gave him a right to speak, held also to this view, and even went so far as to say that, were it possible to cross-connect the auditory and optic nerves, we ought to see sounds with our ears and hear light with our eyes.

There was opposition, of course. Lotze had argued for a locus of specificity in the peripheral sense-organs, and Hering held a similar view. Munk is said to have been the first to point out the relation of the doctrine of Müller and Helmholtz to the 'new phrenology' of cerebral localization. The Müllerian doctrine meant that five separate sensory centers ought to be found in the brain, and these, with more or less uncertainty, were later indicated. The Helmholtzian view fitted in with the network theory of Golgi: within each sensory center, there may be a cell or group of cells for every sensory quality. While this extreme view has not been openly championed, it is logically the ultimate consequence of Müller's doctrine, and has therefore come rather subtly to be taken for granted in many discussions which turn on the physiology of qualitative difference.

We should note in passing that this outcome of the evolution of the doctrine of specific energies has hardly made for clarity of thinking. In vision and touch, at least, there has developed a con-

fusion between the perception of spatial differences and the perception of qualitative differences. In the middle of the nineteenth century, Weber was contending for an *Ortsinn*, and local signs were thought by some to be consciously qualitative. Nowadays, however, with the projection theory of Cajal before us, we may assume one day that adjacent points on the retina or skin correspond to adjacent points in the cerebral cortex, and then the next day we may find ourselves thinking that different points in the cortex correspond to different qualities. The inconsistency presumably does not often become apparent, because the one view is usual in explaining visual and tactual space and the other in accounting for auditory quality, and this difference of sense-mode keeps the two conceptions separated in thought.

Notes

An excellent history and examination of the theory of specific energies of nerves is R. Weinmann, *Die Lehre von den Sinnesenergien*, 1895. Other sources are A. Goldscheider, *Die Lehre von den specifischen Energien der Sinnesorgane*, 1881; M. Dessoir, *Arch. f. Anat. und Physiol.*, 1892, 196-232; W. Nagel, *Handbuch der Physiologie des Menschen*, 1905, III, 1-15. The only complete reference in English is not very satisfactory: A. J. McKeag, *The Sensation of Pain and the Theory of Specific Sense Energies*, 1902; but there are scattered accounts in textbooks, and E. B. Holt's partially philosophical discussion in *The New Realism*, 1912, 314-330, is especially enlightening.

Bell and Müller

For Johannes Müller's doctrine, see his *Handbuch der Physiologie*, II, bk. v, the introductory section, in any edition or the English translation. This portion of the *Handbuch* first appeared in 1838. The first eight of the ten laws and most of the discussion of them are reprinted in English in B. Rand, *Classical Psychologists*, 1912, 530-544. Müller originally advanced the theory in *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, 44-55. See also his *Ueber die phantastischen Gesichtserscheinungen*, 1826, 6-9.

Charles Bell's similar discussion, which forms the basis for the claim of priority for him, is the privately printed monograph of 100 copies distributed personally to his friends, the monograph which we have already discussed in connection with the law of the spinal nerve roots: *Idea of a New Anatomy of the Brain*, 1811. The reprint, *J. Anat. and Physiol.*, 3, 1869, 154-157, gives his view. It has also been reprinted in the original English, with a German translation, by E. Ebstein, *Charles Bell: Idee einer neuen Hirnanatomie*, 1911; and Carmichael (cited below) again reprints nearly all the significant passages.

L. Carmichael, *Psychol. Rev.*, 33, 1926, 188-217, esp. 198-203, is Bell's champion for his priority over Müller. Dessoir, *op. cit.*, 202, gives Bell credit; Weinmann, *op. cit.*, 20, does not. An historian must necessarily speak of "the Müllerian doctrine" because, in spite of Bell's priority, it was Müller's formulation of the theory that was known and that exerted its great influence. Bell's views and discoveries,

unformulated in a 'doctrine' and incompletely explicated, would not have had the same influence upon the history of psychology. It was Müller's theory, even though he got it from Bell, that was so effective.

The quotations from the philosophers are from Aristotle's *Treatise on the Principle of Life* (Hammond's trans., 1902), bk. ii, chap. 6; bk. iii, chap. 1; John Locke's *Essay Concerning Human Understanding*, 1690, bk. ii, chaps. 8, 9; and David Hartley's *Observations on Man*, 1749, pt. i, chap. 1, sect. i. They will all be found in context in Rand, *op. cit.*, 59-62, 242-249, 315-320. In this connection see also Weinmann, *op. cit.*, 11-21, 76-94; and, especially for the relation to Locke's secondary qualities, Holt, *op. cit.*, 313 ff.

For R. H. Lotze's criticism, see his *Medicinische Psychologie*, 1852, 182-197, and other references cited by Weinmann, 39-42, *q.v.* For E. H. Weber on the same matter, see his *Der Tastsinn und das Gemeingefühl*, in R. Wagner's *Handwörterbuch der Physiologie*, 1846, III, ii, 505-511 (pp. 37-46 of the 1905 separate reprint).

Extension of the Doctrine

The extension of the theory dates from Natanson, *Arch. f. physiol. Heilkunde*, 3, 1844, 515-535; and A. W. Volkmann, Wagner's *Handwörterbuch* (*op. cit.*), 1844, II, 521-526.

H. L. F. von Helmholtz's discussion of specific energies is in his *Handbuch der physiologischen Optik*, 1867 (but this section was first published in 1860), 192-208, 839 f., and his implicit extension of the theory in the interests of a theory of vision is also here, 290-293. The account is similar to an early paper (1852), where it seems as if Helmholtz took Young's theory as this "extension"; see Helmholtz's *Wissenschaftliche Abhandlungen*, II, 1883, 3-23, esp. 6 f. The first edition of the *Handbuch* is available as reprinted in the third; see *ibid.*, II, 1911, 3-20, 119-122; or the English trans., II, 1924, 1-21, 142-146. In the second edition, Helmholtz noted that the use of Müller's doctrine for the theory of vision is a special case; *ibid.*, 1896, 349; see also Rand, *op. cit.*, 573-581. Helmholtz had, however, already recognized the fact that he was extending the orthodox view in formulating his theory of hearing. For it see Helmholtz, *Die Lehre von den Tonempfindungen*, 1863, Abth. i, the end of Abs. vi (pp. 219-223 in the 1865 *Ausgabe*, but slightly revised); the text is but little amplified in the English translation by Ellis, pt. i, chap. 6. This point is discussed again in chap. 14.

Thomas Young's theory of color vision is to be found in his *Course of Lectures on Natural Philosophy and the Mechanical Arts*, 1807, I, 440, a single paragraph which is quoted entire by Rand, *op. cit.*, 573. The mention of the possibility of three portions of the nerve fiber functioning for the three principal colors is in a remote paragraph, *ibid.*, II, 617. Certainly a sentence of this kind cannot be taken as the setting-forth of a theory of specific nerve differences.

On the last three paragraphs of the chapter, see Weinmann, *op. cit.*, 31-35, 63-68. Holt, *op. cit.*, 321-330, gives the argument for the dependence of qualitative difference upon frequency of excitation rather than difference of fiber.

Specificity and Localization

It is in the *Tonempfindungen, loc. cit.*, that Helmholtz gives the 'telegraph theory' of the nervous system, as popular later as it is now misleading. Here he argues that there are no specific differences between nerves or the natures of their conductions, that the nerves are like telegraph wires which passively conduct electricity. The specificity must therefore lie in the terminations, just as is the case with the various instruments that can be attached to the end of an electric wire. It is interesting to note that

Helmholtz is here arguing for a view of the nerves as passive conductors in the interests of a theory of the specificity of nerve terminations. Müller argued against a belief in the nerves as passive conductors in practically the same interests, but in order to combat an older theory that had been safely disposed of later.

The reader must not assume from the text that this idea of the specificity lying in the brain was new with Helmholtz. We have seen that Müller definitely left the issue open, although some writers have supposed that his constant use of the term *nerves* was meant to exclude their terminations in the brain. The title of Weber's discussion, cited above, is *Endigung der Sinnesnerven in besonderen Organen des Gehirns*. Even Bell, who later derided the phrenology of Gall and Spurzheim, had this view in 1811. He wrote then: "The idea or perception is according to the part of the brain to which the nerve is attached" (*op. cit.*, 154). "The operations of the mind are seated in the great mass of the cerebrum, while the parts of the brain to which the nerves of sense tend, strictly form the seat of sensation, being the internal organs of sense" (*op. cit.*, 157).

Chapter 6

PHYSIOLOGICAL PSYCHOLOGY OF SENSATION: 1800-1850

WHILE the doctrine of the specific energies of nerves was destined to play a dominant rôle in the formulation of the psychophysiological problems of sensation, it was rather the result of a general scientific interest in sensation than the cause of this interest. The first half of the nineteenth century saw great progress in the knowledge of the laws of sensation, a progress that was the natural result of growing interest in the physiology of the nervous system and, in particular, of the division of the peripheral nervous system, by the formulation of Bell's law, into motor and sensory nerves. Much of this research into sensation was concerned with the physics of the sense-organs. Nearly all of it lies in a continuous line of development with the modern experimental psychology of sensation, and for this reason its careful examination cannot be undertaken here, where we are seeking merely to give the broad picture of the development of experimental psychological physiology up to the middle of the last century. The details of the separate discoveries and their organization by the German systematists belong in the particular histories of the five senses.

It is obvious, after the event, that the more psychological part of this work made use of an informal method of introspection; that is to say, it depended upon the sensory experiences of human beings, usually upon the experience of the experimenter himself. As long as such an uncritical method of introspection was able to produce results that were readily verifiable by any other scientist, there was no need to refine the method or even to give it a name, nor was there any need to bring up the solipsistic issue raised by modern behaviorism and to say that the data were not the observer's experiences, but only his reports of them. The modern scientist has habitually avoided consideration of such epistemological issues. Newton could find that blue and yellow

light when mixed look white; Tartini could observe that two simultaneous tones of different pitch may seem to be accompanied by a third tone of lower pitch; Weber could note that two points on the skin, when close enough together, feel like one; all these observations could be made without critical discussion of the nature of the experience that is a factor in them.

At first this interest in the dependence of experience upon its physiological conditions was but incidental; the physiological attack upon the problem of sensation consisted in bringing together anatomical and physical knowledge, for both physics and anatomy were well advanced with respect to light and the eye, and to sound and the ear. Thus it was in vision and hearing that sensory physiology was first extended. It would seem as if a similar appeal might have been made to mechanics in extending the knowledge of the sense of touch, for mechanics was as well advanced as were optics and acoustics; but here there was no detailed anatomy into which physical knowledge could fit. Anatomy had shown the eye to be an optical instrument and the ear an acoustic machine, but of the skin it had no detailed structural picture to present. The other two senses, smell and taste, remained even more obscure, and most of what little we know of them even now was not forthcoming until the end of the century.

The development of sensory physiology was also aided by the increasing necessity for the publication of scientific handbooks. Scientific information was increasing so rapidly that it was becoming more and more important to bring all the material available at a given date together in compendia. It has often been said that science progresses unevenly, that the frontier of knowledge is ragged, that unknown territory always lies between the outposts, and that the conditions for advance are more or less fortuitous. While such a statement is correct, it is also true that the organization of systematic compendia exhibits baldly the existing lacunæ and directs attention upon them. It may require a stroke of genius, or at least a happy thought, before the gap can be filled in; nevertheless we should not minimize the importance of systematization in bringing scattered facts together, and exhibiting both the unexplored regions and the known territory that impinges upon them.

Of systematic physiological texts, which, being systematic, had to treat of sensation, there were many. In 1803 Charles Bell pub-

lished the third volume of *The Anatomy of the Human Body* (a joint work with his brother). This volume is devoted to the nervous system and the organs of sense and is an excellent compendium of the knowledge of sensation at that date. Here Bell found ten times as much to say about sight and hearing as about the other three senses. In general, however, it is the Germans who have excelled in the organization of compendia. Beside works dealing with a single sense, there were several early in the century devoted to sensation in general: Steinbuch in 1811, Purkinje in 1823-1825, and Tourtual in 1827. We have already seen the tremendous importance that is to be attached to the publication of Johannes Müller's *Handbuch der Physiologie des Menschen* (1833-1840). More than 15 per cent of these volumes is devoted to sensation, and a third as much to "the mind." The *Handbuch* treats most fully of vision and hearing; there was still but little said of the other three senses. It was followed, however, almost immediately by Wagner's *Handwörterbuch der Physiologie* (1842-1853), which contains E. H. Weber's famous section on *Der Tastsinn und das Gemeingefühl,* thus largely eliminating one deficiency. This practice has continued almost up to the present. Hermann's *Handbuch der Physiologie* (1879-1880), devotes an entire volume of more than a thousand pages to sensation; its sections are written by Hering, Hensen, Vintschgau, and other well-known physiologists of that period. Schäfer's *Text-Book of Physiology* (1900) is a still more recent example of similar character, but the coming of age of experimental psychology and its especial activity in the field of sensation has tended rather to remove the treatment of these problems from the physiological texts.

Vision

Vision was the best known of the five senses. For this the publication of Newton's *Optiks* (1704) a century earlier is doubtless responsible. Not only did this book and the subsequent work of the physicists render a fairly complete knowledge of the laws of refraction and of optical instruments available for application to the problem of the eye, but it is also true that the *Optiks,* especially in respect of color, contributed some incidental psychological information. At the beginning of the nineteenth century, the anatomy of the eye was well understood, and its study as an

optical instrument was the natural course for physiologists first to take. Beside the texts already mentioned, there were several dealing solely with vision. Johannes Müller published in 1826 *Zur vergleichende Physiologie des Gesichtssinnes* and *Über die phantastischen Gesichtserscheinungen.* These books, as we have seen, contained in them the beginning of his doctrine of the specific energies of nerves. The first is much broader than a mere comparative physiology, for it deals also with the problems of external reference, the unity of the two fields in binocular vision, convergence and accommodation, and even Goethe's color theory. The second, a much smaller book, is concerned with more purely psychological problems. Treviranus's work on the visual organ, published two years later, gave very complete tables of the dimensions and other optical constants of the eyes of a great variety of animals, and treated mathematically the optical system that is the eye. We saw that even Flourens (1824) in his gross anatomy of the brain had to distinguish between what we may call sensation and perception. This distinction was elaborated by Heermann for vision in 1835 in his *Die Bildung der Gesichtsvorstellungen aus Gesichtsempfindungen.* In 1836, A. W. Volkmann wrote *Neue Beiträge zur Physiologie des Gesichtssinnes,* a book that Müller frequently referred to in his section on vision in the *Handbuch* two years later. This Volkmann (who is not the Volkmann who wrote the *Lehrbuch der Psychologie* in 1856) contributed the section on vision in Wagner's *Handwörterbuch der Physiologie* in 1846. Another important work by Burow on the physiology and physics of the human eye appeared in 1841. It dealt with eye-movement and the problems of convergence and accommodation. Most of the experimental observations appeared, of course, in separate monographs. These books are the milestones that mark the progress of the physiology of sight.

In this period, almost every account of vision concerns itself primarily with the physics of the stimulus, the anatomy of the eye, and consequently the relation between these two subjects—that is to say, with the eye as an optical instrument. Bell's account is typical, but the emphasis had shifted only a little thirty-five years later with Johannes Müller. Of the stimulus, beside the general fact that it is normally light, we are told of colors produced by refraction, by reflection, by transmission, and by interference. All the essential structures of the human eye were known

except the variability of the curvature of the lens. The comparative anatomy of the eye was studied for its own sake, but also because it seemed as if a knowledge of simple vision in subvertebrate forms would lead to a recognition of the more important elements in vertebrate vision.

Starting with physics and the stimulus, the progress of knowledge was from without in. Thus a knowledge of the eye as an optical system, which results in the formation of an image on the retina, was the all-important problem. Bell (1803) treated it in detail. Treviranus (1828) was primarily concerned with it. Müller (1838) made it the primary problem, dealing with refraction by the lens and other media, the achromatic character of the system, and the defects of the system in myopia and presbyopia and their correction by lenses. In Wagner's *Handwörterbuch,* beside Volkmann's section on sight (1846), there is a section by Listing on dioptics (1853), which contains the principle of the reduced eye and Listing's law of the relation of the image to the curvature of the retina and to torsion of eyes in movement. The problem by this time had been extended to binocular vision and eye-movement. Such was the setting upon which Helmholtz's classical analysis of physiological optics (1866) supervened. Wundt (1862) and Hering (1868) dealt with the same problem. It was at first the fundamental problem of vision.

The facts were fundamental, partly because the doctrine of the specific energies of nerves raised the question of the mechanism of perception. If we put the matter at the philosophically unsophisticated level of Johannes Müller, we shall only be presenting the problem as the physiologists of his day saw it. The common view was that perception consisted in the transmission, in some way or other, by the nerves to the brain of properties emanating from perceived objects. Müller argued that we perceive directly not the properties of objects, but the properties of the nerves themselves. How, then, did we come to know about objects correctly? Because the state of the nerves corresponds to the state of objects in ways that can be formulated under certain definite laws. Now this means that we perceive by sight, not an object nor even the light from it, but the state of the optic nerve and of the retina which is but the extension of the optic nerve. Aside from color, the most obvious thing about visual perception is that it yields correct information about space, size, shape, and position. This

fact comes about because the eye as an optical instrument projects an image of the perceived object upon the retina, an image that is as correct a copy of the object as a flat picture could be. It seemed to Müller and other physiologists, therefore, that, by showing how the image on the retina resembled the object, one had come near to explaining perception. If the excitation on the nerve is a pattern, and the sensorium perceives directly the state of the optic nerve, no wonder then that it perceives a pattern; and if this pattern is the optical image of an object, no wonder that it perceives it correctly.

Müller was also, of course, quite clear that the retina would thus at times misrepresent external space. All the arguments for specific energies that are dependent on 'inadequate' stimulation of the retina show that illusion is possible, that the sensorium is not always correctly informed. The size of the visual field is simply the size of the retina, for it is the retina that the sensorium perceives directly. Absolute size is thus dependent upon the size of the retinal image—that is to say, upon the visual angle and not upon the size of the object. The perception of direction depends upon the point of the retina stimulated, both relatively and absolutely. The image on the retina is inverted, but Müller saw no problem as to why we do not see things upside down. It was plain to him that there is no meaning to 'up' except the sensation that results from the excitation of the bottom of the retina, and that only a man with a knowledge of physiological optics would ever, in directly perceiving his retina, know that this sensation actually came from the bottom of the retina. There is even here in Müller the hint that one learns 'up' from 'down' by experience. In general, Müller remains good doctrine to-day, although we know that perceived size is neither entirely relative nor entirely proportional to visual angle.

It is plain that, for Müller, the theory of vision is merely the theory of the excitation of the retina by the optical image. In these terms he compared vision in mammals with vision in lower forms, and deduced the functions of the lens and iris, citing Magendie's experiment in which images projected by an actual eye were seen on an artificial screen. He fixed upon the retinal cone as the essential organ, and noted that acuity must depend on the density of the cones, being less in the periphery than in the center of the retina. He even raised the problem of the spatial limen. Two cones in the

fovea subtend an angle of about 40″ of arc and Müller cited Volkmann and Weber as agreeing that two points separated by this angle are but just discriminable. Treviranus's threshold of acuity was too fine for the size of the cones.

The retina further imposes its nature on external reality by being blind at the point of entrance of the optic nerve. A knowledge of the blind spot existed, however, more than a century before the time under discussion. Mariotte discovered it and described it in 1669 and in 1682-1683. It must have been fairly common knowledge, for Charles II (died 1685) is said to have used it in jest with his courtiers to show them how they would look when their heads were off.

There was also in this half-century some knowledge of the laws of color. Both Bell and Müller cite Herschel's measurement of the spectrum (1800), and Bell at least pointed out that the colors are not equally bright, and that maximal brightness in the spectrum occurs in the yellow-greens. The change of color values in twilight was observed by Purkinje in 1825. The first two laws of color mixture had been laid down by Newton (1704), and involve, of course, a knowledge of complementaries. The physiologists knew these facts, and the third law was formulated by Grassmann in 1853. Color mixing by means of rotating disks was first accomplished by van Musschenbroek in 1820; the law of such mixture was worked out by Plateau in 1853, and the method was perfected by Maxwell in 1857. The colored disks are often called Maxwell's disks.

The persistence of sensation after the cessation of the stimulus was known to Newton, who described the circle of light seen on whirling a luminous body. Müller cited Plateau as finding the time to be about a third of a second. Bell made use of a projected after-image in an experiment upon nystagmus, and Müller noted especially that after-images move with the eyes. Müller was also vaguely aware of the significance of the facts of light and dark adaptation and the consequent sensitization of the eye to the opposite illumination. His account of both positive and negative after-images, which he called "spectra," is full. He knew that the former are best produced with closed eyes and the latter with open eyes. His explanation of the complementary color of the negative images is almost exactly that of the Young-Helmholtz theory, though he did not here cite Young.

Beside this anticipation of Helmholtz, Müller seems also to have anticipated Hering in his explanation of contrast. Not only did he give the essential facts of color contrast, noting especially the facts of colored shadows described by Goethe, Tourtual, and several others, but he discussed the phenomena under a general section on "the reciprocal action of parts of the retina." Here one finds the facts of color contrast, light contrast, irradiation, the filling-in of the blind spot, and complete adaptation.

The knowledge of the blind spot stimulated interest in peripheral vision. Thomas Young (1800) described the decrease in acuity from the center to the edge of the retina, and Purkinje (1823) verified earlier experiments (1804) which showed that colors fade out rapidly at the periphery. Szokalsky (1839-1840) and Hueck (1840) noted that the retina is less sensitive to color in its peripheral portions, but the mapping of color zones did not occur until Aubert's work (1857). The era of measurements and of disputes as to the meaning of the measurements was still to come.

Color-blindness had been known ever since the famous case of Dalton, the Quaker, was described in 1798, giving rise to the term *Daltonism*. Thomas Young discussed the case in 1807, and Goethe referred to these abnormalities (1810). In 1837 Seebeck gave an account of an investigation of color-blindness, and, from lists of color confusions, concluded that there are two types of the defect.

Of color theories there were Thomas Young's (1807) and Goethe's (1810). The German writers of handbooks mentioned Goethe's most frequently, of course; Müller, however, criticized it. It remained for Helmholtz to give Young's theory its full importance in Germany.

Binocular vision furnished one of the most obvious conundrums in this field. Touch something with both hands and there seem to be two touches; but look at something with both eyes and there is only one 'look.' Gall had sensed the problem and had concluded that one eye always predominates and that we never see with both at once. Bell came nearer the truth in suggesting corresponding points on the two retinas which function together, since in movement the eyes remain parallel. The geometrical optics of the eye did much to further this view, and it was soon understood that much of what is seen is double, and that only a limited part

of the binocular field, the horopter, is single. Müller discussed this matter in 1826 and corrected his view of the horopter in 1838. It had been shown by Gehler (1828) that the horopter must be a circle. The development of the theory of the horopter, however, depended upon the notion of corresponding points, and the conundrum was not solved. If the mind perceives directly the retina, why does it not then perceive two retinas, and why does it sometimes seem to perceive two and sometimes one? Müller was in a fair way to answer what might have been his own questions. He observed that one could not tell in double vision which image belonged to which eye; we are not conscious of the retinas separately as such. He noted also the experiments of Heermann and Volkmann, which showed that when one color is presented to one eye and another color to the corresponding part of the other eye, there may be a blending of the two colors, although other experiments showed that under other conditions there may be rivalry. The latter case would have supported Gall's theory, but the former was consistent with single vision in the horopter. Now the relation of the optic chiasma to this problem had already been brought to attention, even by Newton. Müller discussed various theories and concluded that, since the two nerves are the same size before and after the chiasma, it is probable that the fibers do not divide, but that half the fibers cross over and that half do not. This conclusion led him to the view that obtains to-day, that each half of the brain functions for half of the unitary binocular field of vision, and that presumably fibers from corresponding points on the retinas go to the same point in the brain. Apparently, then, the sensorium does not directly perceive the retina after all, but the central terminations of the nerves where corresponding points become one. (Such might have been a further argument for the locus of specificity in the brain and not in the nerves; but this comment is the author's and not Müller's.)

Müller just missed in the *Handbuch* Wheatstone's invention of the stereoscope and his experiments with that instrument, which were also published in 1838. It is perhaps fortunate, for the dependence of the perception of solidity upon retinal disparity of small degree would have presented then, as it does now, a seemingly insoluble problem. Brewster's stereoscope followed Wheatstone's shortly. He described the principle in 1843-1844, and the

instrument fully in 1858. Here, then, were another method and fact ready for the new experimental psychology.

In this period the mechanisms of convergence and accommodation were well recognized. All the work on binocular vision had to take convergence into account; for instance, the size of the circular horopter obviously varies with the degree of convergence. The means of eye-movement were clear; the muscles that move the eyes were well described by anatomists. Accommodation was, however, puzzling. It is plain that we can adjust with only one eye to distinctness of vision at different distances, and that with change of distance adjustment is required. Bell noted the fact, but missed a physiological explanation by appealing to attention as the adjusting factor. When Müller wrote the *Handbuch,* a great deal of information on "adaptation," as he then called it, had become available. The near point of fixation had been worked out by several investigators; Scheiner's experiment, which shows that the eye changes its optical focal length, was history (1619); and some theories were in the field. Magendie had said that the eye was so constructed that it could not change. Mile had tried to explain the matter by reference to the changing iris. Young thought the lens contracted of itself. Some thought that the convexity of the cornea was changed; others, that the lens moved back and forth. Müller was inclined to believe that the eyeball was elongated by its muscles, but left the matter open as to whether it might not be the curvature of the lens that changed. The term *accommodation* for this function seems to have come into use with Burow (1841), whose book, dedicated to Müller, deals mostly with accommodation and convergence. It was left for Helmholtz (1866) to give us the modern theory of the mechanism of accommodation, and for Wundt (1862) to perform the first experiments on the relation of these two mechanisms to the perception of distance.

In all this discussion, the physiologists touched upon psychological matters more or less according to their temperaments. Even Bell, who consciously sought to avoid vague speculation, was occasionally driven back upon 'attention' for an explanation. Müller, of course, was as near to being an experimental psychologist as any one could be in this half of the nineteenth century, and he frequently sought explanations in terms of the mind. He discussed attention to visual phenomena as against those of the other senses, and the attentive selection of one object within the visual field.

The spatial reference of objects to the external world was for him, as we have seen, a matter of "judgment," not at all a matter of vision itself. So, he tells us, perceived form depends not only on sensation, but also on association; and the perception of distance, not on sensation, but on reasoning. In retinal rivalry, attention favors the persistence of one image over the other. While such statements seem vague and therefore useless, we must remember that each one is in a way the formulation of a psychological problem. Later we shall understand better how physiological psychology sprang into a separate existence, if we see first how physiology was unable to avoid raising its problems for it.

Hearing

After sight, the sense-physiologists were most interested in hearing. The physics of acoustics was well advanced, and the anatomy of the external and middle ears, as well as the grosser structure of the inner ear, was fairly well understood. Nevertheless, on the whole, less was known of hearing than of vision, and thus less was written. Bell found about half as much to say about hearing as about sight; Steinbuch about a quarter as much; Müller about three quarters as much. Harless wrote the section on hearing in Wagner's *Handwörterbuch* in 1853. Most of the observations upon which these books drew occurred in monographs and articles, and not in separate books. We shall get an excellent picture of the period if we follow, in the main, Johannes Müller.

The texts all emphasized physical acoustics: the conduction of sound in solids, liquids, and gases, the physics of sonorous bodies like strings and columns of air, reflection and interference of waves, resonance, and the rate of propagation. Müller performed numerous experiments on the conduction of sound from one medium to another, in order to analyze the acoustic properties of the ear. The anatomy of the ear was brought into relation with acoustics. Both Bell and Müller discussed at length the comparative anatomy of the ear and the nature of hearing in different animal forms. The outer ear was obviously for the collection and concentration of the sound against the tympanic membrane, which would thus vibrate with the sound. Both Bell and Müller showed that the chain of small bones in the middle ear would conduct the sound to the oval window. Müller supposed that this conduction

occurred in the way that sound vibrations are transmitted through any solid, and not by the movement of the bones as a system of levers. The stapes could not move, he thought, because it presses against the incompressible liquid of the inner ear. He did not know that the structure of the inner ear is such as to allow the round window to relieve pressure exerted at the oval window, so that mass movement can occur. To the tensor tympani he assigned what is probably its correct function: the maintenance and adjustment of tension on the tympanic membrane. He showed by experiments that low and high tones require different tensions for the best transmission through a membrane, and he concluded that the tensor tympani acts reflexly in such adjustments. He believed that he could contract this muscle in himself voluntarily, and mentioned ways in which he thus substantiated his conclusion. He confessed complete ignorance of the function of the other muscle of the middle ear, the stapedius. The function of the Eustachian tube puzzled him especially. He outlined nine theories of its use, and favored the theory that the tube is designed to equalize air pressure between the atmosphere and the middle ear. Hearing, he showed by experiment, is diminished when the pressure is unequal.

In his theory of the inner ear Müller swung wide of the truth. The eighth nerve, which is called the auditory nerve because it leads to the ear, is distributed to both the cochlea and the semicircular canals, and Müller naturally believed that both the cochlea and the canals were organs of hearing. He noted that neither (presumably because they are closed tubes filled with liquid) had very good acoustic properties. He assumed that sound has two routes through the middle ear; that it passes from the tympanic membrane by way of the bones to the oval window and by way of the air to the round window. Presumably the former route, he thought, is more effective upon the canals, and the latter upon the cochlea. Indeed, it seemed that the former route must be more effective, for Müller showed by experiments that sound is transmitted from solids (the bones) to liquids more readily than from gases (the air) to liquids. The round window is probably, so he believed, merely a less effective auxiliary, and is not essential to hearing, for frogs do not have it. Still it must operate in man, for a person can hear, through one or both windows, when the bones of the middle ear are lost. If this mechanism is poor acoustically,

the defect is partially compensated by the disposition of the auditory nerve, which is spread out upon the spiral lamina so as to gain extended contact with the acoustic undulations. In this way Müller got the sound to the auditory nerve, just as in dealing with vision his primary object was to get the light by way of an optical system to the optic nerve; and, as we have seen, this is really the main problem of the author of the theory of the specific energies of nerves. Applied to hearing, its solution seems more naïve, because Müller knew less of the nature of the auditory endings than of the structure of the retina.

Of the nature of the auditory stimulus there was little for Müller to say except what is implied in the physics of sound. He noted in addition that sounds may be heard by conduction through the bones of the skull, but he was not sure whether these vibrations acted directly or by affecting the tympanic membrane. He cited, of course, the usual instances of subjective sounds that were taken to support the theory of specific energies.

On the problem of tone and successive noise, Müller referred to Savart's experiments. In these experiments a note was produced by a card in contact with the teeth of a revolving wheel. One tooth gives a single sound that is not a note. Several may give a rattle. At the correct rate, however, several give a note, and a note may be got from only two teeth.

Müller also cited Savart on the limits of hearing. The upper limit he took as 24,000 d.v. or more; the lower as 16 d.v. and less. The differential limen was not a problem in 1838; later Weber (1846) set it as 1/160 in terms of musical interval.

The question of analysis of compound wave-forms, the question that largely determined Helmholtz's theory of hearing, was not to be satisfactorily dealt with by Müller. He noted that wave-length must determine pitch, that amplitude must determine intensity, and that two tones of the same wave-length would reinforce each other and give a more intense sound. That we hear both tones when they are of different wave-lengths must be due, he thought, to the fact that the ear appreciates the maxima of each and thus hears both. In this discussion he represented the frequencies by series of dots rather than wave-forms, and he remarked that the perception of one tone in the presence of another is a more difficult perception than that of one tone alone. Bell, although extremely vague, is thought to have come nearer the truth, as Helmholtz

saw it, by guessing that the ear must be something like a musical instrument with strings of different lengths.

Beats offered no problem to Müller. They seem to be explained physically by interference. In this discussion, however, he drew the compound wave-forms, instead of representing maxima by dots. The facts of difference tones, then known as the sounds of Tartini, he presented without explanation. Plainly, a theory was less possible then than now.

The perception of the direction of sound Müller explained as dependent on the difference of intensity of the sound at the two ears—a modern theory. The perception of distance he supposed to be a matter of absolute intensity. Neither, he noted, is very accurate in itself, but auditory localization is usually aided by vision, as the facts of ventriloquism show.

Fusion is a modern term, but Müller discussed harmony, giving the well-known frequency ratios for the natural scale.

As with sight, so with hearing Müller raised some purely psychological problems. Attention may select among sounds, he observed, and sounds not attended to may not be heard at all. All sounds last a little longer than the stimulus, but long-continued sounds may last for many hours afterward: this statement of Müller's seems to refer to what has since been called the 'recurrent image.' Finally, we may note that hearing is said by Müller to yield many reflex movements, of which the bodily start at a loud sound is perhaps the most familiar.

Such, then, was the picture of the physiology of hearing in the first half of the last century. Again it was Helmholtz (1863) who was to extend and revise it.

Touch

While the psychophysiology of sight and hearing was developing, there seemed to be very little to do or to say about touch. Bell and Müller dismissed it with but a few pages. It was sometimes classed with taste and smell as a 'simple sense.' It is indeed simple in that the physical stimulus would seem to act directly upon the nerve endings in the skin and other tissues. The theories of sight and hearing had been concerned mostly with getting stimulation to the nerve—that is to say, with the projection of the optical image on the retina and the conduction of sound to the

auditory endings in the inner ear. The tactual stimuli—pressures, temperatures, movements, and the rest—act immediately on the nerves in the skin without any transmitting mechanism.

It remained for Ernst Heinrich Weber (1795-1878), professor of anatomy and later of physiology at Leipzig (1818 *et seq.*), to bring the facts and problems of touch into due prominence. Most of these facts were of Weber's own experimental finding. In 1834 he published some experiments on touch in a Latin monograph: *De tactu: annotationes anatomicæ et physiologicæ.* The important work, however, that is now regarded as one of the classics in the psychology of sensation, was his *Der Tastsinn und das Gemeingefühl* (1846), which constituted the section on touch in Wagner's *Handwörterbuch.* Let us see what Weber knew about touch.

In the first place, it was necessary for him to limit his problems. *Touch* was a vague term. Seemingly all of the skin and much of the insides of the body were supplied with sensory fibers leading to the dorsal roots of the spinal cord. Sensitivity was quite general, and *Gemeingefühl* was a term practically synonymous with *touch.* Weber, however, distinguished clearly between touch and the *Gemeingefühl* (common sensibility). Touch belongs to the skin. Common sensibility is possessed by the skin and internal organs in common; and it includes all pain.

The sense of touch (*Tastsinn*) provides us with three classes of sensation: pressure sensations (*Druckempfindungen*), temperature sensations (*Temperaturempfindungen*), and sensations of locality (*Ortempfindungen*). Although Weber often spoke of a pressure 'sense,' a temperature 'sense,' or a local 'sense,' it is quite clear that he was using the term loosely, and that these 'senses' were for him all parts of the sense of touch. Pressure and temperature he thought of as the two kinds of touch sensation; locality, the *Ortsinn,* appeared to be secondary and to depend upon the other sensations for its arousal, although separate from them. Warmth and cold Weber regarded as positive and negative sensations of temperature, like light and dark in vision. In support of this last relationship he mentioned a pathological case where warmth and cold could not be distinguished. The relation of temperature to pressure within the *Tastsinn* is shown, he argued, by the fact that cold bodies appear heavier than warm bodies of equal weight. Weber's experiment with the *Thaler* is fairly well known. He found that one of these coins, when removed from

cold water to the forehead of a subject, felt heavier than two of the same coins superposed and warmed in warm water before being put on the forehead. He noted also the relation of cold and warmth in the facts of successive contrast (facts that were described by John Locke).

In this connection it is interesting to note that Bell, while not intimately concerned with this problem, was clearer than Weber with respect to the relation of the qualities of touch. Pressure, heat, and cold seemed to him to be distinct sensations when considered "in relation to the body," although otherwise cold is obviously but "the privation of the quality of heat."

To understand Weber's view of the *Ortsinn,* we must realize that the sense of locality is not an essential part of other sensations. Ordinarily it seems as if locality and pressure, for example, were inseparably connected, as, when one finger is touched and then another, we think we have two different sensations. We shall know better, Weber observed, if we touch one finger with another: then both fingers are stimulated equally, each by the other, and we feel only one sensation. Sensation itself varies, Weber held, only in quality and degree; its spatial characteristics depend on the activity of the mind and the relationship of sensations.

Nevertheless, to assign the *Ortsinn* in part to the 'mind' did not mean to Weber that it could not be investigated. It is against this problem that Weber brought his famous 'compass test' to bear, the experiment that has since become the measurement of the limen of dual impression. Weber found that the threshold for the perception of two points varied greatly in different parts of the body; for example, the upper arm gave a threshold thirty times as great as the volar surface of the little finger. These differences led him to conclude that they must depend on the distribution of the nerve fibers, and that spatial discrimination is most accurate where the fibers are most dense. If one regards the cutaneous zone in which any given nerve fiber would be affected as a circle, then it is clear that the skin can be divided up into a very large number of sensory circles. In the compass experiment, two points applied within the same sensory circle would give rise to the stimulation of but one fiber, and thus to the perception of a single point; two points on adjacent circles, however, would stimulate adjacent fibers and give the perception of a line; and the perception of two separated points would occur only when the

sensory circles stimulated were separated from each other. Such was Weber's view of the *Ortsinn.* The rôle of the mind in all this is to interpret spatially the pattern of excitation, for the sensations themselves are possessed of no spatial characteristic.

Weber was not able to deal so specifically with the *Gemeingefühl* as with touch. He noted that common sensibility is distributed all over the body; that it supplements touch in the skin; and that the other organs of sense—the eyes, the ears, the nose, and the tongue—also possess it. It is finest in the skin and the muscles. Pain is its most marked characteristic, and is elicited by the stimuli appropriate to touch. Both pressure and pull upon the skin may give rise to pain as well as pressure. Similarly, both heat and cold may arouse pain. Weber worked out the thermal thresholds for pain very carefully. Moreover, there are many other sensory items that belong in the *Gemeingefühl,* like shudder and tickle, but there is little to be said of them.

Weber recognized the sensitivity of muscles and placed their sensations in the *Gemeingefühl,* because they are internal, because fatigue seems qualitatively akin to pain, and because some muscular contractions (e.g., that of the uterus) are very painful. It would seem that Weber did not know about Charles Bell's discussion of the "muscle sense," or the sixth sense, as it came later to be called. The sensitivity of muscles had long been known—since Aristotle, it is said. Bell simply developed the functions of the sense and named it. His important paper was published in 1826, although it is said that the germ of the distinction is to be found in the paper of 1811 which announces the law of the spinal roots. Neither Müller nor Weber mentioned Bell in his discussion of muscular sensibility.

Weber's knowledge of the sensitivity of the skin is the result of a great many experiments which he performed himself and reports in *Der Tastsinn und das Gemeingefühl.* From his own determinations he knew that the degree of perceived weight varies for the same weight with different parts of the skin; that the capacity for discrimination between weights and between temperatures varies with the region of the skin, much as does the *Ortsinn;* that a difference of about half a degree centigrade could readily be perceived, and that discrimination was very much better when the stimulus was large. He also was ready to assert that the intensity of a thermal sensation is greater, for the same temperature,

if the stimulus is larger, and he concluded that there must consequently be summation at the brain, which is greatest in the case of adjacent fibers.

The most notable of these experiments on the measurement of sensitivity were, however, those that form the basis of Weber's law, as Fechner afterwards named it. Weber had reported these results in *De tactu* in 1834; now he enlarged upon them a little. However, he formulated no specific law. He simply made it clear that the smallest perceptible difference between two weights can be stated as a ratio between the weights, a ratio that is independent of the magnitudes of the weights. He extended his original experiments on weights to the visual discrimination of the lengths of lines and the auditory discrimination of the pitches of tones. His findings for the ratios that represent the least perceptible differences were: weights, 1/40; lines, 1/50 or even 1/100; tones, about half of a musical comma, or 1/160. Weber believed that he had here established an important general principle, but he could not have realized that these simple experiments would lead eventually to the whole structure of psychophysics.

Of the actual physiology of touch Weber knew little. He thought that the papillæ of the skin and the hair-bulbs must be the organs. He believed that touch is confined to the skin, for he tried in vain to elicit pressure, warmth, and cold from the internal organs and from the deep tissues exposed in wounds. The *Gemeingefühl* he described as pretty generally distributed over the body, although the tendons, cartilage, and bones are insensitive. The nerves of touch and of common sensibility run to the cord (Bell's law); do they then pass to the brain? Weber brought out a great deal of evidence to show that they must terminate in the brain.

Weber was less inclined than Müller to run into psychological vagueness; nevertheless, having got the tactual excitations to the brain, he could not but note that they would then come under the influence of the mind. One function of the mind is of course perception (*Vorstellung*); Weber described perception as consisting in bringing sensations under the categories of space, time, and number. In general, however, he tended to avoid metaphysical problems. In demonstrating the influence of the mind upon sensation, he preferred to cite the experiments of the astronomer Bessel on the personal equation, experiments to which we shall recur later.

Taste and Smell

At the beginning of the nineteenth century, as little was known about taste and smell as about touch, and exact knowledge of these senses had changed very little by the middle of the century. Such progress as was to be made was reserved for the very end, the 1880's and 1890's. There was, to be sure, one very thorough treatise on smell written early in the century: it is a book of over 700 pages by Cloquet entitled *Osphrésiologie,* published about 1821. Half of it, after the French fashion, was devoted to the pathology of olfaction. All later works used it as a foundation. There was no similar work on taste.

Bell (1803) knew that the papillæ of the tongue are the organs of taste, and that where there are no papillæ, there is no taste. Horn (1825) showed that different papillæ are differently sensitive. Here was a hint of difference of function, but as yet there was no adequate classification of taste. Nausea was thought of as a taste quality.

In smell there was Linnæus's classification of odors into seven groups, as well as Haller's three-fold distinction between sweet, foul, and indifferent odors; but there was no fine structure that could be correlated with the classes. The olfactory organ showed no mechanism that could give a clue.

The normal stimulus to taste was seen to be a substance in solution. Müller held that gases could also be tasted, and that mechanical stimulation might elicit tastes. For smell, the normal stimulus seemed to be either a gas or a substance finely divided in the air. Müller believed that the mucous membrane must be moist for stimulation to occur.

Notes

The reader must bear in mind that this section gives the picture of the physiology of sensation only in its broad outlines. It is incomplete even as against the works cited. A more detailed picture of the same sort can be had by reading successively the sections on sensation in Bell (1803), Müller's *Handbuch,* and Wagner's *Handwörterbuch.*

Plainly such a sketch as this cannot be fully documented with references. We may, however, recapitulate here the more important works already cited:

Vision

I. Newton, *Optiks,* 1704. C. Bell, *Anatomy of the Human Body,* III, 1803, 224-372; the entire work is in

four volumes by both John and Charles Bell. J. G. Steinbuch, *Beytrag zur Physiologie der Sinne*, 1811, 140-270. J. E. Purkinje, *Beobachtungen und Versuche zur Physiologie der Sinne*, 2 vols., 1823-1825; the subtitles are *Beiträge*, and *Neue Beiträge, zur Kenntnis des Sehens in subjectiver Hinsicht*. Joh. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Tiere nebst einem Versuch über die Bewegungen der Augen und über den menschlichen Blick*, 1826, 462 pp.; *Ueber die phantastischen Gesichtserscheinungen*, 1826, 177 pp. C. T. Tourtual, *Die Sinne des Menschen*, 1827. G. R. Treviranus, *Beiträge zur Anatomie und Physiologie der Sinneswerkzeuge des Menschen und der Thiere*, 1828; the first *Heft* deals solely with the eye, and apparently no other *Hefte* were published. Heermann, *Die Bildung der Gesichtsvorstellungen aus Gesichtsempfindungen*, 1835. A. W. Volkmann, *Neue Beiträge zur Physiologie des Gesichtssinnes*, 1836. Müller, *Handbuch der Physiologie des Menschen*, II, 1838, bk. v, sect. i (pp. 1088-1214 in the Eng. trans.). C. A. Burow, *Beiträge zur Physiologie und Physik des menschlichen Auges*, 1841. V. Szokalsky, *Ueber die Empfindungen der Farben in physiologischer und pathologischer Hinsicht*, 1842. A. W. Volkmann, R. Wagner's *Handwörterbuch der Physiologie*, III, i, 1846, 264-351. J. B. Listing, *ibid.*, IV, 1853, 451-504. The author has not seen the works by Purkinje, Tourtual, and Heermann, and cites them at second hand.

Hearing

Bell, *op. cit.*, 373-453. Steinbuch, *op. cit.*, 270-300. Müller, *Handbuch, op. cit.*, bk. v, sect. ii (pp. 1215-1311 in the trans.). C. Harless, Wagner's *Handwörterbuch, op. cit.*, IV, 1853, 311-450. E. H. Weber was interested in hearing when he was first at Leipzig. He wrote a comparative anatomy of the ear, *De aure et auditu hominis et animalum*, 1820; and a physics of sound in 1825.

Touch

In the same works: Bell, 472-480; Steinbuch, 53-140; Müller, bk. v, sect. v (1324-1332 in the trans.); and E. H. Weber, *De tactu: annotationes anatomicæ et physiologicæ*, 1834; *Der Tastsinn und das Gemeingefühl*, Wagner's *Handwörterbuch*, III, ii, 1846, 481-588. This last article has been reprinted separately in 1851 and later.

Smell

Bell, 454-457. Steinbuch, 303-307. H. Cloquet, *Osphrésiologie, ou traité des odeurs, du sens et des organes de l'olfaction;* the second revised and enlarged edition is dated 1821, and there is a German translation in 1824. Müller, bk. v, sect. iii (1312-1318 in the trans.). F. Bidder, Wagner's *Handwörterbuch*, II, 1844, 916-926.

Taste

Bell, 458-471. Steinbuch, 300-303. Müller, bk. v, sect. iv (trans., 1318-1323). Bidder, Wagner's *Handwörterbuch*, III, i, 1846, 1-11.

Chapter 7

HYPNOTISM

WHILE all the main threads of experimental psychology lie wholly within physiology during the first half of the nineteenth century, there were during this period two discoveries important for psychology, that came from other fields. One, which we shall consider later, is the discovery of the personal equation by the astronomers. The other is hypnotism.

The state of hypnosis is undoubtedly as old in the history of man as other mental phenomena. Similar states have long been recognized in sleep-walking and religious ecstasy, and seem to have been induced intentionally in some savage ceremonials. It was not, however, such phenomena that first led to the scientific consideration of hypnosis.

Magnetism had long been regarded as a mysterious natural force, and Paracelsus (1493-1541), the physician and mystic, had designated magnets as bodies which, like the stars, especially influenced the human body by means of a subtle emanation that pervades space. Van Helmont (1577-1683) inaugurated the doctrine of animal magnetism by teaching that a magnetic fluid radiates from all men and may be guided by their wills to influence the minds and bodies of others. In the century and a half following van Helmont, there arose numerous men in Europe who appeared to effect mysterious cures of the sick by the laying-on of hands or even without contact. Greatrakes (1629-1683), an Irishman, is one of the best known, and his remarkable cures in England of many in the crowds that flocked to him attracted both public and scientific attention. Still, aside from the theory of animal magnetism, which was certainly not an illuminating theory, these phenomena remained inexplicable and lay outside of science.

Mesmerism

The matter came to a head with Friedrich Anton Mesmer (1734-1815), who discovered how to produce these phenomena

and lent them his name, for the process of inducing them came to be called 'mesmerism.' Mesmer was a physician in Vienna who at first held a view similar to van Helmont's. His early views grew out of an attempt to explain the supposed effect of the stars upon human beings, the fundamental principle of astrology. There must be, he thought, some effective principle that permeates the universe, a principle that is probably to be identified with electricity or magnetism. This belief led him to experiment with the effect of magnets upon persons, and, in stroking their bodies with magnets or in making passes with magnets over their bodies, he discovered that he could induce what we now call hypnosis. These facts and this view he presented in a work published in 1766. As a physician, he used the magnetic method for healing certain diseases. Ten years later he met a Swiss priest, Gassner, who effected cures by a similar method, but without the use of magnets. In this way Mesmer came to realize that the magnets were irrelevant to his method. He abandoned them, and of necessity altered his theory. It now seemed to him that there must be some occult force capable of influencing others and residing in his own person. He called this force animal magnetism, in contradistinction to physical magnetism, because that was a term and conception already established by van Helmont and others. Actually, the only thing common to both physical and animal magnetism was the fact that they were both mysterious influences acting at a distance without a visible intermediate agent.

In 1775 Mesmer sent a letter on the subject of animal magnetism to the various scientific academies. The letter was ignored by all but one, which replied unfavorably. In Vienna his doctrine aroused enemies, and principally on this account Mesmer removed to Paris in 1778.

In Paris he constructed his famous *baquet*. It seems to have been an oak chest, containing chemicals and fitted with many appendages of iron. It was supposed to have been magnetized by Mesmer and to be capable of transmitting the magnetism to the subjects who sat in a circle about it, with their hands joined or connected by cords. (This circle appears to be the ancestor of the circle of spiritualistic sitters of the present day.) The room which contained the *baquet* was dimly lighted and hung with mirrors; strains of soft music sounded through it at intervals; Mesmer appeared, sometimes in magician's dress, and passed about the circle

of sitters, touching one, making passes over another, and fixing a third with a glance. The effects were emotional and various; many cures occurred; and hypnosis undoubtedly played an important rôle in the results. In Paris, public interest in the affair was very great. Scientific commissions were appointed to examine the phenomena, and reported negatively (1784); that is to say, while not denying the effects produced, they found in 'animal magnetism' no similarity to magnetism as it was known in minerals. After all, such a report was not so very different from Mesmer's own view.

What happened next is a comment on the power of words in human affairs. Mesmer had discarded magnets as irrelevant to the phenomena, but he kept the phrase *animal magnetism*, thinking that there must be some similarity between the new power and magnetism. The investigators found no such relationship, and therefore declared against 'animal magnetism' in the sense that it was not magnetism. Since the phenomena could hardly be produced by collusion of the subjects with Mesmer, it seemed plain that there must be some force which, if it were not magnetism, must be something else. This 'something else' came to be supposed to be a secret of Mesmer's, for how could he produce these effects without knowing how he did it? The French government offered him 20,000 francs to disclose the secret, but Mesmer refused. Actually he had no secret to disclose: he knew that he obtained certain results in a certain way, and he believed that the results were due to a magnetic power within himself, but there was nothing to say except what every investigator of his methods knew. He was, however, from the first opposed by the medical profession and the scientists, and when he "refused" to reveal his "secret," he gradually fell into disrepute, and finally, denounced as an impostor, withdrew from Paris to Switzerland, where he died in 1815.

It is interesting to see just what factors were at work in this rapid rise and decline of mesmerism, especially because the little drama was reënacted more than once afterward. The affair was a conflict between radicalism and conservatism in science and the medical art. Mesmer was seeking something new, just as his scientific contemporaries were engaged in discovery; but the new thing that he found was very new in that it appeared, as Mesmer incorrectly formulated the principle, to break with accepted scien-

tific tradition and medical practice. Had he been content to work quietly on the nature of animal magnetism, there would have been no conflict. The discovery, however, seemed to have in it immediate practical possibilities, and Mesmer's temperament led him to utilize these powers to the full without further effort to understand their nature. This course led to publicity and thus to increased opportunity for therapeutic aid. Few men can resist the pressure of popular acclaim, and it may be that Mesmer yielded to the public's love of mystery in much of the hocus-pocus that surrounded the *baquet*. On the other hand, he may have discovered that it was an important condition in inducing the 'magnetic' effects. Now science felt that it had disposed of magic long before, and the similarity of Mesmer's methods to the procedure of the magician and charlatan provoked its opposition. Had Mesmer known the secret of his own success, he might in part have met the opposition, but that secret presupposes a state of psychological knowledge that did not then exist, and is only imperfectly revealed to us almost a century and a half later. In any case, however, he would have had to contend with scientific conservatism. New discoveries make headway in science but slowly, even though discovery is the business of science. This is, so it seems to the author, essentially as it should be. Scientific progress is most sure when it is the resultant of a force operating for change and the opposing inertia of criticism. Too often the scientists are blamed when wrong and applauded when right, although a satisfactory judgment of right and wrong may not have been possible until long after the event in question. The views of both Mesmer and Gall were too broadly drawn to command acceptance, and both men were read out of the body scientific; yet neither man was wholly wrong. We reject to-day mesmerism and phrenology, but we accept hypnosis and, with certain limitations, localization of cerebral function. The historical perspective shows, however, that the conservative scientists were simply fulfilling their critical function in 1784 and 1808. Neither party to either of these controversies can be blamed. Nor can we pass judgment on the form of the controversy. Both sides, when the cleavage is wide, are bitter and personal; presumably it is only through such emotional conflict that important steps in progress occur. It is by some such mechanism that the personal bias of the human mind ultimately corrects itself.

Elliotson and Esdaile

There were many individuals in the first third of the nineteenth century who practised mesmerism, as the new power was now called, but there was no such general interest as had been excited by Mesmer, nor did the practice then come again to an issue with the scientists. A little later, however, on a less public scale, the drama of Mesmer was reënacted in England. The occasion for controversy was John Elliotson (1791-1868).

In 1837, Elliotson was professor of the practice of medicine at University College, London, and senior physician at the University College Hospital. For twenty years Elliotson had been a pioneer in medical science, distrustful of the old, but believing profoundly in the possibilities of the new. First as physician at St. Thomas's Hospital he gained consent to give clinical lectures. Later he was appointed to University College and was largely instrumental in the establishment of the hospital in connection with the college. He was a man of great imagination, fertile in new ideas. In the traditions of the past he saw mainly a force that prevents progress. But he was not a visionary, for many of his ideas were sound. The establishment of University College Hospital was one of these ideas. He saw what is accepted as obvious to-day, the fact that a medical school needs a hospital attached to it for demonstration and research. He was vigorously opposed in this venture by his conservative colleagues, but he won his fight. He was the first man in England to adopt the use of the stethoscope, which had been invented on the Continent. This practice his colleagues condemned, ridiculed, or ignored. They spoke about his "hocus-pocus," and one is said to have remarked of the stethoscope, "Oh, it's just the thing for Elliotson to rave about!" Elliotson also introduced several novel practices in the use of drugs, practices which after the first resistance of the medical profession have become established. Altogether it is plain that he was a radical in temperament, although an effective radical from the point of view of medical progress. It is not surprising that, being met with contempt and ridicule, he should have replied in kind. He did not hesitate to point out some of the absurdities of conventional medical practice of his day, and he thus hindered the progress that he sought to promote by obscuring it behind the barrier of personal prejudice which controversy and recrimination

erected. In such a case it is hard to say whether the more blame attaches to inflexible conservatism or to tactless radicalism.

It was in 1837 that Elliotson's imagination was aroused by a demonstration of mesmerism by Dupotet. He had seen a demonstration in 1829, but the possibilities of the phenomenon seem not to have occurred to him before. He immediately began to mesmerize patients in the hospital, with important results in the treatment of certain nervous cases. Mesmerism was, however, then in the depths of scientific disrepute. His colleagues refused to witness his demonstrations, and annoyed him in many petty ways. The dean urged him to desist on the ground that the reputation of the medical school was of more importance than scientific research and medical progress, and Elliotson indignantly refused. In 1838 the council of University College passed a resolution forbidding "the practice of mesmerism or animal magnetism within the Hospital." Elliotson immediately resigned from both the college and the hospital, which he had been instrumental in founding, and never entered either institution again. He complained that the council had acted without discussion of the matter with him and without witnessing any of his mesmeric demonstrations.

As so often happens, this crisis fixed Elliotson's determination to study and to utilize mesmerism still further, and thus also fixed the opposition of the medical profession toward him. Moreover, he was not to be left alone. Braid's interest in mesmerism dates from 1841, and Esdaile's, in India, from 1842. In England in 1842, Ward amputated a thigh with the patient under mesmeric trance and reported the case to the Royal Medical and Chirurgical Society. The evidence indicated that the patient had felt no pain during the operation. The society, however, refused to believe. Marshall Hall, the pioneer in the study of reflex action, urged that the patient must have been an impostor, and the note of the paper's having been read was stricken from the minutes of the Society. It was further urged that the method, if correct, was immoral, since pain is "a wise provision of nature, and patients ought to suffer pain while their surgeons are operating." Eight years later, Marshall Hall informed the society that the patient had confessed to an imposition, but that the source of his information was indirect and confidential. The patient, however, then signed a declaration that the operation had been painless.

In 1843 Elliotson began, under his own editorship, the *Zoist*,

which described itself as "a journal of cerebral physiology and mesmerism, and their applications to human welfare," and which served as a place where new biological and even sociological ideas could be reported and discussed free from the inhibitions of conservative tradition. There are several instances about this time of the refusal of scientific journals or proceedings to publish accounts of mesmeric phenomena. The *Zoist* continued until 1856, when its mission was considered to have been accomplished.

In 1846 it became Elliotson's turn to deliver the Harveian Oration. He accepted in spite of bitter opposition, and reviewed the history of scientific opposition to great medical discoveries, not omitting Harvey's discovery of the circulation of the blood. In founding the oration, Harvey had specified that the orator should present his own work "with an exhortation to others to imitate, and an exhortation to study and search out the secrets of nature by way of experiment." Elliotson did not fail at the close of the oration to exhort his audience to the experimental study of mesmerism.

In 1849 a mesmeric infirmary was opened in London, and soon similar clinics were established in other cities of Great Britain. At Exeter, the surgeon claimed at one time that he had mesmerized 1,200 patients and performed 200 painless operations. All this activity the medical journals ignored, but the *Zoist* served as a medium for those who were interested in the work.

Elliotson, like Mesmer, had originally been interested primarily in the therapeutic value of mesmerism, but interest had meanwhile been shifting toward its possibilities as an anesthetic agent. As a matter of fact, with so little understood of hysterical diseases, one might expect that the anesthetic use of mesmerism, more than the therapeutic use, would be the one to lead the medical world to accept mesmerism. The medical profession was earnestly desirous of alleviating the pain of surgical operation, in spite of the remark about Ward's case which we noted above. What happened was that a more reliable, comprehensible, and 'respectable' anesthetic came to hand at exactly the same time. While the anesthetic effect of certain drugs seems always to have been known (e.g., Homer mentions the effects of nepenthe), the regular surgical usage of modern times dates only from 1846. In 1844 an American dentist, Wells, had one of his own teeth extracted painlessly while he was under the influence of nitrous oxide. The fail-

ure of a public demonstration in Boston prevented him from continuing his plan for painless extraction, but another American dentist, Morton, using ether vapor, put the practice into effect in Boston in 1846. News of these results reached England a few months later, ether anesthesia was tested at once, and it rapidly became the general practice in surgical operations. The use of chloroform began in 1847. The dramatic appearance of ether, chloroform, and nitrous oxide on the medical stage in the middle '40's may have forestalled the acceptance of mesmerism as an anesthetic agent.

Another historical accident of these times, that affected the status of mesmerism, was the sudden rise of spiritualism. Clairvoyance had been regarded as a possibility, although looked at askance by scientists, for some time. Elliotson, however, had sought to connect clairvoyance with mesmerism, a natural mistake for his radical temperament. In 1848 spiritualism was born in the mysterious rappings that began in the home of the Fox sisters in Hydesville, New York. These phenomena developed sensationally in Rochester, where the Fox sisters went to live, and the hypothesis that they represented communication from the world of the dead transmitted through the "medium" of certain persons was formed. The word *medium* was attached to the person possessing this power, and sittings for the purpose of eliciting these phenomena were found often to produce new mediums. The development was rapid. By 1852 the interest spread to England, and in 1853, in the form of a craze for table-tipping, all over the Continent. Elliotson himself rejected spiritualism, while believing, although less surely, in clairvoyance. Nevertheless there was a striking similarity between some of the phenomena of spiritualism and those of clairvoyance, and especially between the nature of the spiritualistic sitting and the séances of Mesmer about the *baquet*. Because of this similarity, the revived mesmerism suffered still more in repute.

While all this was going on in England, James Esdaile (1808-1859) was advocating and practising mesmerism in India. Here the British government proved more open-minded than the medical profession. Esdaile had read of Elliotson's work, and in 1845 it occurred to him to try mesmerism upon a patient who was suffering great pain. To his surprise he was successful in inducing the mesmeric state and in rendering his patient entirely without

pain. He began, then, to practise mesmerism for anesthetic purposes. A letter to the Medical Board describing his work remained unanswered, but a later report to the government, after he had more than 100 cases to describe, led to the appointment of a committee of investigation. The report of the committee was cautious, but favorable to further research, and the government accordingly in 1846 established a small mesmeric hospital in Calcutta where Esdaile might continue his work. After almost a year, the official visitors of the hospital were convinced of the effectiveness of mesmerism as an anesthetic and of its partial effectiveness in reducing operative shock. Three hundred native citizens of Calcutta signed a petition for its continuance. Nevertheless, the government closed the hospital. In 1848, a new mesmeric hospital, dependent upon private resources, was opened, and Esdaile was placed in charge. In six months, however, the government transferred Esdaile to another hospital for the express purpose of effecting a combination of mesmerism with the common practice of medicine. Here he remained at work until he left India on account of the climate in 1851. He settled in Scotland and continued his active interest in mesmerism, corresponding with Elliotson, until his death in 1859. Elliotson, seventeen years his senior, lived until 1868.

Had not the Indian government supported Esdaile's work, he would have had as difficult a time as Elliotson. The Indian medical journals attacked him and suggested that his success was due to the fact that the natives liked to be operated upon and were trying to please Esdaile. Esdaile, before he left India, had performed about 300 major operations and innumerable minor ones, all of them in mesmeric trance and seemingly painless. Natives came to him when they avoided operation by others. Esdaile could attempt certain operations that other surgeons feared to undertake. It seems that Esdaile reduced mortality in the operation for scrotal tumors from about 50 per cent to about 5 per cent. The mesmerized patients lay relaxed and quiet during operation. Detailed descriptions and statistics were available. It was quite unreasonable to suppose that these natives were merely indulging a fancy for pleasing Esdaile.

Neither in India nor in Great Britain could Esdaile get the medical journals to print accounts of his work. For a time, the only available descriptions lay buried in reports of the Indian

government. In 1846 and again in 1852, however, he published a separate book, describing his work in India. Sir James Simpson, the first to discover and apply the anesthetic properties of chloroform, had urged Esdaile before the later report to publish, but Esdaile could not get his paper accepted.

Esdaile's predominant interest in the anesthetic aspect of mesmerism led him to prefer mesmerism to the new-found agents, ether and chloroform. In this matter he may have been prejudiced by his own past interests, but it was also apparent that ether and chloroform left deleterious after-effects and, if not expertly administered, might be fatal. From both these evils mesmerism seemed free. When the Congress of the United States in 1853 sought to bestow an award of ten thousand dollars on the discoverer of the anesthetic powers of ether, and described ether as the first anesthetic, Esdaile, without claiming the award, sent a letter of protest on the ground that mesmerism was prior. As a matter of fact, the priority was not very great. The important events were these. Ward in England amputated a leg under mesmeric trance in 1842. Wells in America had a tooth painlessly extracted under nitrous oxide in 1844. Esdaile in India began and accomplished many operations with mesmeric analgesia in 1845. Morton in America extracted teeth under ether anesthesia in 1846, and the practice spread to England in the same year. Simpson introduced chloroform as an anesthetic in 1847.

James Braid

We must now turn our attention to James Braid (*ca.* 1795-1860), famous as "the discoverer of hypnotism," and much better known than Elliotson or Esdaile. Unlike them, Braid never broke with the medical profession, and he thus provides us with an opportunity for gaining insight into the causes of conservative opposition to mesmerism.

In the first place, we must note that Braid was never regarded as a mesmerist either by himself or by the mesmerists. He described the mesmeric trance as a "nervous sleep" and invented for the underlying doctrine the term *neurypnology* (a contraction of *neuro-hypnology*). In general, the prefix of the term came to be dropped, a change which led to the words *hypnotic*, *hypnotize*, and *hypnotism*. Braid was a hypnotist; Elliotson was a mesmerist.

The importance of words in the discussion and comprehension of men is very great indeed. We have just seen that Mesmer's failure to drop the word *magnetism* when he found that magnets had nothing to do with his phenomena obscured the real importance of his discovery, probably in his own mind as well as in the minds of others. Conversely, the introduction of the new word *hypnotism* tended to set off Braid's theory from every other that went by the name of 'mesmerism.'

There is, however, something more than words to be considered. The very fact that Braid chose a new word means something about him. Let us compare him with Elliotson. Elliotson was undoubtedly the more brilliant of the two; he was a discoverer of several important medicinal facts and a leader among his colleagues (when he could persuade his colleagues to be led), but also an extremist in his enthusiasms. Braid was a sound medical practitioner of Manchester; he was unusually, but not remarkably, skilful as a surgeon; he is known only for his work in hypnotism; and he was not an extremist. The most impulsive and dramatic act of his life, so far as our knowledge of him goes, was when, only five weeks after a public mesmeric demonstration by Lafontaine on the stage at Manchester, he himself sought the stage to refute Lafontaine and to present his own theory and demonstrations that had been elaborated in that brief interval. In the issue thus raised, both circumstances and his temperament made him a champion of the middle ground. On the one side, the medical profession looked askance; on the other, the mesmerists disowned him. Nevertheless, he kept calmly on, experimenting and writing, seeking always to give scientific meaning to his findings, and thus narrowing the gulf between himself and medicine. As the personal separation between Braid and medical men became less, the breach with the mesmerists became greater. Elliotson in general ignored him, but on two occasions the *Zoist* made its contempt for Braid quite clear. On the other hand, in phenomena and even in theory it is plain that Braid belonged in interests with the mesmerists. It would appear, then, that the breach between medicine and mesmerism was after all more a matter of personalities than of science. If Elliotson, when unjustly rebuffed, could have adjusted himself to the situation, changed his obnoxious terminology, and gone on quietly with research while cultivating the good-will of his colleagues, the story of mesmerism might

have been different. It is hard in a controversy of recrimination to say which side is at fault. It was fortunate for science that Braid's first impulse in connection with mesmerism was his desire to refute Lafontaine, for thus the breach with the mesmerists started at the very beginning, and this breach helped to preserve his scientific reputation. We can trace some of these motives as we tell the story of Braid.

In 1841, Lafontaine gave a series of public exhibitions of mesmerism in Manchester. This was three years after Elliotson's resignation from University College, two years before the *Zoist* began publication, and four years before Esdaile undertook to induce mesmeric trance in India. There was much public interest in these exhibitions; the subject has always created interest, and in this case the *Manchester Guardian* had printed very unfavorable comments in advance. The first meeting was small, but the audience rapidly grew to huge proportions. Braid was present at the second meeting and is said to have been "loud in his denunciation of the whole affair." Other medical men joined with him; but the sympathies of the audience were with Lafontaine. It was easy to charge fraud, because the mesmerist operated principally upon two subjects whom he brought with him. At the next exhibition, however, Braid became suddenly convinced that there was something more than collusion beneath the phenomena. According to his own statement, he came to this conclusion because he observed the inability of one of the subjects to open his eyes. He is said by another physician present on the stage to have tested the subject by forcing a pin under one of her nails without eliciting any evidence of pain. The "leading ophthalmist" of Manchester, who was also on the stage, likewise changed his opinion when he noted, on forcing the subject's eyes open, that the pupils were contracted to two small points. Plainly, Braid was in a dilemma. He had publicly committed himself against Lafontaine and mesmerism, and now he had evidence which he would have accepted in the clinic as showing that the phenomena were 'real.'

It is here that he showed the scientific balance of mind that ultimately led to the discovery of the essential truth hidden in mesmerism. He might have withdrawn, reiterating his denunciation, and then have kept his counsel, but he was too open-minded. He might have become a complete convert to mesmerism as Combe had to phrenology under similar circumstances, but he

was too conservative. What he did do was to observe the next exhibition carefully, and then go home to plan experiments and to elaborate a theory that would bring these facts into relation with scientific physiology.

The mesmerists' theory, so far as it can be called a theory, was that the cause of the phenomena resided in the person of the mesmerist; it was animal magnetism. This view was too vague and mysterious to satisfy Braid; he was sure that there must be some more immediate physiological cause residing in the person of the subject. To his surprise, he found that he could induce an artificial sleep in the members of his family and in his friends by having them stare fixedly at some bright object above the line of vision. He thus came to the conclusion that the mesmeric phenomena were merely the evidences of a sleep "caused by paralyzing the levator muscles of the eyelids through their continued action during the protracted fixed stare." In his enthusiasm for this discovery, he sought the stage only a few weeks after Lafontaine had left it, mesmerized many persons before large and intensely interested audiences, and expounded his physiological theory. He presented phenomena similar to Lafontaine's, but he was not branded as a mesmerist because his theory fitted in with conservative physiological belief, and because he presented it as an attack upon mesmerism. It is also probable that the new theory could be more readily tolerated since it did not magnify the theorist as a man possessed of peculiar power over his fellowman. Modesty begets sympathy, and an egotistical theory, even if right, would be bound to meet with resistance. It was for this reason that Elliotson failed.

Braid and his biographers have regarded this formulation of a theory as the "discovery" of hypnotism. Braid at least showed all the enthusiasm of a discoverer, developing his theory by experiment and writing. He immediately met with opposition from the medical men, from the mesmerists, and even from laymen. His first book is a brochure entitled: "Satanic Agency and Mesmerism Reviewed, in a Letter to the Rev. H. McNeile, A.M., of Liverpool, in Reply to a Sermon Preached by Him at St. Jude's Church, Liverpool, on Sunday, April 10th, 1842." In 1843 he published his fundamental work, *Neurypnology, or, the Rationale of Nervous Sleep; Considered in Relation with Animal Magnetism,* and the term *hypnotism* and its derivatives date from this

year. Half a dozen other books and many articles in journals followed in the next decade. Later, as opposition waned, his publication also decreased; and he died suddenly in 1860. Just before his death he had the satisfaction of having his work taken up by Azam in France, and of learning that Broca had presented a paper on the subject to the Académie des Sciences, a paper which seems to have met with a favorable reception and which resulted in the appointment of a committee of investigation.

Braid's earlier views of hypnotism emphasized the importance of sensory fixation. His method of inducing hypnosis by having the subject stare fixedly at some object naturally stressed this notion. It is, however, but a step in thought from visual fixation to the fixation of attention, and from the very beginning, when he used the word *monoideism* to describe the hypnotic state, Braid had this broader and more psychological view in mind. Later he came to recognize more clearly the importance of the factor of suggestion in inducing the phenomena, and his emphasis shifted even more from the physiological to the psychological aspect of the state. He was also, it is interesting to note, quite clear about the division of consciousness in so far as he found that memories would persist from one hypnotic state to another, although unavailable to the subject when awake. Though, as the champion of the middle ground, he had to defend himself on both sides, he was nevertheless not a propagandist, but a man primarily interested in the nature of the phenomena; and it is thus that we get with Braid a great step forward from vague theory and personal controversy to an analytical description of empirical fact. The scientific knowledge of hypnosis begins with Braid, and it cannot be said that the fundamental nature of the hypnotic state is so very much better understood to-day.

Viewed in its entirety, the history of hypnosis consists of three short periods of intense interest, separated by long periods in which there was little general interest, but in which the thread was maintained continuously by a certain amount of practice along the lines of the immediately preceding development. In the decade of 1780-1790, Mesmer flourished in Paris; but he was soon discredited, and for a half-century mesmerism became the property of a few honest disciples and many charlatans. In 1840-1850 came the revival of mesmerism within medicine and the new theory of hypnotism. In the '50's there was a decline in interest,

for the therapeutic value of hypnotism was not clearly established, as an anesthetic it was of less general application than ether or chloroform, and as a controversial subject it was dead as soon as the nature of the phenomena had been indicated by Braid. This time the period of quiescence was but little more than two decades, for the next revival belongs to the '80's.

It is the period of Braid that is of particular importance to us here. In it we see how physiological thought of the middle nineteenth century took up with the problem of mesmerism, a problem essentially psychological in so far as it was not merely occult. It cannot be argued that the development of hypnosis contributed immediately and directly to the beginning of physiological psychology; rather is this development a symptom of the thought of the times out of which physiological psychology emerged. Later, it is true, the method and facts of hypnosis were ready for assimilation by psychology, especially in France, and for further elaboration within psychology.

Later Hypnotism

With the later period we are not primarily concerned at present, but we may nevertheless profit by tracing its bare outlines. After the Académie des Sciences, under the stimulation of Azam and Broca, had considered hypnotism in 1860, and Braid had died in the same year, little of importance occurred until 1878. 'Durand de Gros' had in 1860 coined the word *Braidism* as a substitute for *hypnotism,* because the emphasis in describing the state had shifted from sleep to suggestion. Liébeault, who began the study of hypnotism in 1860, settled in 1864 at Nancy, where for twenty years he practised hypnotism as a therapeutic agent. Braid's work meanwhile sank into obscurity in England, whereas it had never become well known on the Continent. In 1875, Richet in France called attention to the phenomena of hypnosis and attested to their genuineness. In 1878, Charcot began his demonstrations of hypnosis, and his particular view of hypnosis came to be an important doctrine of the school of the Salpêtrière, of which he was the leader. Almost simultaneously, interest in hypnosis sprang up in Germany. Heidenhain's work belongs to these years, and in 1881-1882 Preyer translated Braid's works into German. It was in 1882 that Liébeault converted Bernheim to the new art.

Liébeault had cured a case of sciatica that had resisted Bernheim's treatment. Bernheim's subsequent work created the Nancy school of hypnosis. Thus it came about that Braid's work, after being lost sight of, was again revived. Interest was again quickened by controversy, but this time the controversy did not turn upon the 'genuineness' of hypnosis. Hypnosis was accepted as a fact, and the dispute was concerned with its nature. The theory of the Nancy school, which differed but little from Braid's later views, held that the phenomena were to be understood in terms of suggestion, and were thus entirely normal phenomena. The school of the Salpêtrière believed that the phenomena were essentially hysterical in nature and thus symptoms of abnormality. The verdict of time has favored the Nancy school, but the controversy was fortunate, for it dismissed forever the suspicion that hypnosis was not a proper subject for scientific inquiry.

Notes

There are several excellent accounts of the facts of hypnosis: A. Binet and C. Féré, *Le magnétisme animal*, 1887, Eng. trans., 1888; A. Moll, *Der Hypnotismus*, 1889 and four subsequent editions with Eng. trans. of all; J. M. Bramwell, *Hypnotism, Its History, Practice, and Theory*, 1903; and W. McDougall, article on *Hypnotism* in the *Encyclopædia Britannica*. All of these accounts treat of the history of hypnosis. Binet and Féré, chaps. 1-3, are most complete on its origins; Moll, chap. 1, is extremely detailed and lacks on this account perspective; Bramwell, pp. 3-39, deals mostly with Elliotson, Esdaile, and Braid. On Braid, see below.

The account in the text sacrifices detail to perspective, but the reader should realize that the total literature on hypnosis is very extensive. Dessoir's bibliography in 1888-1890 noted 1,182 titles by 774 authors: M. Dessoir, *Bibliographie der modernen Hypnotismus*, 1888, with a *Nachtrag* in 1890. B. Rand, in J. M. Baldwin's *Dictionary of Philosophy and Psychology*, III, ii, 1905, 1059-1067, gives a bibliography of 411 titles. See also the bibliography in Bramwell, 440-464.

Mesmer

F. A. Mesmer's early work, before his discovery of the irrelevancy of magnets, is *De planetarum influxu*, 1766, His own account of the beginnings of that doctrine of animal magnetism that came to be called mesmerism is his *Mémoire sur la découverte du magnétisme animal*, 1781, Germ. trans., 1781. There are also several later books by Mesmer; the last is a general account, *Mesmerismus*, 1814, published long after he had been driven from Paris and but a year before his death.

Elliotson

On John Elliotson, see Bramwell, *op. cit.*, 4-14. Practically all of his writings on mesmerism were perforce in the *Zoist*, since the medical journals refused them. There was published in London and Philadelphia a pamphlet by him, describing among other things Ward's amputation under mesmeric

anesthesia: *Numerous Cases of Surgical Operations without Pain in the Mesmeric State; with remarks upon the opposition of many members of the Royal Medical and Chirurgical Society and others to the reception of the inestimable blessings of mesmerism*, 1843. His *Harveian Oration* was also published in London in 1846 with an English translation of the Latin.

Esdaile

On James Esdaile, see Bramwell, *op. cit.*, 14-21. Some of his publications are now rare; see the list in Bramwell, 456. His *Mesmerism in India, and Its Practical Application in Surgery and Medicine*, 1846 (second printing, 1847), is not rare and gives a good account of his work. The separate publication of 1852, which was originally solicited by Simpson for a journal and then rejected on the advice of his editorial colleagues, is *The Introduction of Mesmerism, as an Anæsthetic and Curative Agent, into the Hospitals of India.*

Anesthesia

On the discovery of anesthesia in America, see C. A. H. S[illegible]h, *Scientific Monthly*, 24, 1927, 64-[illegible] Wells and Morton had been associated in early dental practice, although not directly in this discovery. Wells's public failure to demonstrate nitrous oxide as an anesthetic in Boston in 1845, owing to his lack of skill in administering the gas, led to his being ridiculed, to his retirement from practice, and thus indirectly to his suicide. Morton, though a dentist, administered ether for a surgical operation at the Massachusetts General Hospital in Boston in 1846. He was successful, but strange to say he met with violent opposition from the newspapers and the public. One of the clergy argued that he was interfering with the Divine will in abolishing pain (the same argument that had been raised against Ward's amputation of a leg under hypnosis), and others, including many of the medical profession, heaped abuse upon him. The case is very similar to Elliotson's, except that the more open-minded surgeons immediately adopted the new method. In a few years, the greatness of the discovery was so well recognized that a dispute arose between Morton and Jackson, his one-time patron and adviser, as to priority, a dispute that prevented the United States Congress from making the award that Esdaile claimed belonged to hypnosis.

Braid

On James Braid, see the reprint of his *Neurypnology* (1843) in 1899, with editorial additions by A. E. Waite. This book contains a biographical account of Braid by Waite, pp. 1-66. Braid tells the story of the inception of the idea of hypnosis, in connection with Lafontaine's exhibitions, in chap. 1. (See also another account of Lafontaine's Manchester meeting in Bramwell, 465-467.) Waite adds a bibliography of thirty-four titles for Braid, pp. 364-375. Bramwell, 460-464, gives forty-nine titles of books and articles by Braid and twenty-seven contemporaneous titles about him. The translation of Braid into German by W. Preyer is *Der Hypnotismus; ausgewählte Schriften*, 1882. The French translation was by J. Simon, *Neurypnologie*, 1883. When the historians state that Braid discovered hypnotism in 1843 and that the discovery was lost sight of for forty years, they have in mind this revival about 1880, and that the translations of his work into German and French occurred forty and forty-one years, respectively, after its publication.

Braid, as we have seen, coined the word *hypnotism* and all its common variants. He did not use the word *hypnosis*, which appeared later in the '80's. Cf. A. Lehmann's lectures of 1889, *Die Hypnose*, 1890. The word was used medically of narcotic sle in the '70's.

The term *Braidism*, analogous to *mesmerism*, was invented by J. P. Philips, writing under the *nom de plume* 'Durand de Gros': *Cours théoretique et practique de Braidisme, ou hypnotisme nerveux considéré dans ses rapports avec la psychologie, la physiologie, et la pathologie*, 1860.

Later Hypnotism

A. A. Liébeault's first work was *Du sommeil et des états analogues, considérés surtout au point de vue de l'action de la morale sur le physique*, 1866. He published other books on hypnosis in 1883 and later, but he was in general more of a practitioner than writer. He had, nevertheless, the scientist's enthusiasm for the new method, for in Nancy he would give hypnotic treatment to the poor gratis, though he charged for medical treatment.

The revival of hypnosis begins with Charles Richet's favorable pronouncement in his article, Du somnambulisme provoqué, *J. de l'anat. et de la physiol.*, 11, 1875, 348-378. J. M. Charcot's first accounts of his demonstrations are in the *Gazette des hôpitaux civils et militaires*, 51, 1878, 1074 f., 1097, 1121; and *Compt. rend. de la soc. de biol.*, sér. 6, 5, 1878, 119, 230, but the School of the Salpêtrière is to be known through his later writing; see his *Œuvres complètes*, IX, 1890, 213-480, where twenty-seven papers on hypnotism and metallotherapy have been reprinted. Hippolyte Bernheim's first book after his conversion by Liébeault is *De la suggestion dans l'état hypnotique et dans l'état de veille*, 1884; and there are numerous later books. For further comment on psychopathology in France, see the appendix of this book.

The revival in Germany seems to have begun with R. Heidenhain's *Der sogenannte thierische Magnetismus*, 1880, which appears to have run through four editions in the same year. W. Preyer published *Die Katapledie und der thierische Hypnotismus* in 1878, and *Die Entdeckung der Hypnotismus* in 1881. He was the German exponent of Braid.

The result of all this new interest in hypnosis is shown by the establishment of the *Revue de l'hypnotisme* in France in 1887 and of the *Zeitschrift für Hypnotismus* in Germany in 1892. In England a great deal about hypnosis appeared in the *Proceedings* of the Society for Psychical Research, which was founded in 1892.

Chapter 8

THE PERSONAL EQUATION

WHILE the physiologists were concerning themselves with problems of nerve conduction, localization of function in the brain, and sensation, and the medical men were combating hypnosis as an anesthetic and curative agent, the astronomers were taking serious account of a physiological or psychological source of error in their observations, a personal difference between individual astronomers in their observation of the times of stellar events. Although Maskelyne, the astronomer royal at the Greenwich Observatory, observed and recorded such difference in 1795, the real discoverer of the personal difference was Bessel, the astronomer at Königsberg, who saw the significance of the event at Greenwich and investigated the matter during the '20's of the last century. His positive results led some astronomers in the next decade to measure the personal equation, as it came to be called, and to correct for it. In the '40's, this practice became frequent and the astronomers sought for ways to eliminate the error. The perfection of the chronograph in the '50's, and other methods of 'doing away with the observer' were developed. While these methods succeeded, it is nevertheless true that there was more about the personal equation written in astronomical journals in the '60's than in any other decade, and interest was maintained through the '70's and '80's.

The astronomers thought of the error as "physiological," but it is obviously the sort of physiological problem that, along with sensation, was destined to become the property of the new physiological psychology.

Maskelyne and Bessel

At Greenwich in 1796 Maskelyne, as every psychologist knows, dismissed Kinnebrook, his assistant, because Kinnebrook observed the times of stellar transits almost a second later than he did. Maskelyne was convinced that all through 1794 there had

been no discrepancy between the two of them. Then in August, 1795, Kinnebrook was found to be recording times about a half-second later than Maskelyne. His attention was called to the "error," and it would seem that he must have striven to correct it. Nevertheless, it increased during the succeeding months until, in January, 1796, it had become about eight tenths of a second. Then Maskelyne dismissed him. The error was serious, for upon such observations depended the calibration of the clock, and upon the clock depended all other observations of place and time.

The accepted manner of observing stellar transits at that time was the "eye and ear" method of Bradley. The field of the telescope was divided by parallel cross-wires in the reticle. The observational problem consisted in noting, to one-tenth of a second, the time at which a given star crossed a given wire. The observer looked at the clock, noted the time to a second, began counting seconds with the heard beats of the clock, watched the star cross the field of the telescope, noted and "fixed in mind" its position at the beat of the clock just before it came to the critical wire, noted its position at the next beat after [illegible] the wire, estimated the place of the wire between [illegible] in tenths of the total distance between the po[illegible] added these tenths of a second to the time in seconds [illegible] counted for the beat before the wire was reached. [illegible] complex judgment. Not only does it involve [illegible] en the eye and the ear, but it requires a [illegible] dent upon a fixed position (the wire), an actual but [illegible] neous position of a moving object, and a remembered [illegible] no longer actual. Nevertheless, "the excellent method of Bradley" was accepted and regarded as accurate to one or at least two tenths of a second. In the face of this belief, Kinnebrook's error of eight tenths of a second was a gross error and justified Maskelyne's conclusion that he had fallen "into some irregular and confused method of his own," and his consequent dismissal.

Had it not been for Bessel (1784-1846) this event, recorded in the pages of *Astronomical Observations at Greenwich,* might have passed into oblivion. In 1816 von Lindenau mentioned the incident in a history of Greenwich Observatory which he published in the *Zeitschrift für Astronomie,* and Bessel noticed it. Bessel was the astronomer at Königsberg, where in 1813 a new observatory had been erected under his supervision. He was a

man of unusual intellect, a pioneer in the more exact measurements of modern astronomy, and an investigator with a special interest in instrumental errors in measurement. A great mind is least bound by tradition, and it was natural, in view of his special interest, that the incident of Maskelyne and Kinnebrook should suggest to him the possibility of a personal error of observation which the accepted method of Bradley did not guard against. It struck him that Kinnebrook, when informed of his "error," must have tried to correct it, and that his failure to succeed might mean that the error was involuntary. Moreover, it is probable that Bessel had been thinking about errors of observation, for Gauss, at the Göttingen observatory, had discussed the theory of them in 1809. At any rate, Bessel sent to England for a copy of Maskelyne's complete observations, and, after studying them, determined to see whether this personal difference, which seemed incredibly large in view of the supposed accuracy of the method, could be found amongst observers more experienced than Kinnebrook.

His first attempt in 1816, while he was visiting Encke and von Lindenau, was [illegible] cloudy weather, but a year later he found an opportunity to prepare himself with Walbeck at Königsberg. They [illegible] stars; each observed the transit of five one night and [illegible] of the other five the next night, and so on, for five [illegible] Bessel was found always to observe earlier than Walbeck. The average difference was 1.041 secs., with but little variability from the average. If Kinnebrook's error of 0.8 sec. was 'in [illegible] this difference was even more so, though Bessel recorded: "We ended the observations with the conviction that it would be impossible for either to observe differently, even by only a single tenth of a second."

It was fortunate that the difference was so large, for it stimulated Bessel to further work, and, when published in 1822, it attracted immediate attention. As a matter of fact, this difference is so large that it has been questioned. How was it possible for there to be a deviation of more than a second in estimating fractional intervals between clock-beats a second apart? Some have suggested that Bessel and Walbeck counted their beats in different ways. The clock beats as its hand moves from one second to the next, and one person might assign the beat to the second from which the hand had moved and another to the second toward

which the hand was moving. Others have pointed out that, although this difference is almost the largest on record, it is simply the limiting case of personal differences that vary from zero up to a second. This latter view is supported by the belief that so keen an observer as Bessel would hardly have missed so gross an artifact as a difference in methods of counting. Whichever way it was, an artifact or a true difference, it was fortunate, for it led Bessel to continue the investigation.

In 1823 Bessel had an opportunity to observe with Argelander. This time Bessel had Argelander observe seven stars while he himself determined by observation the clock-corrections. From these data the right ascensions of the stars were computed and compared with similar observations and computations for the same stars in 1821, when Bessel had both observed the stars and made the clock-corrections. The personal difference between the two astronomers is represented by the equation B — A = — 1.223 secs. From the first, Bessel presented the differences in this manner, with the result that a difference between two observers came to be referred to as a "personal equation."

Bessel next conceived the notion of determining the personal equation indirectly by means of a third observer. He was especially anxious to compare himself with Struve of Dorpat who, like himself, was more practised in the observation of transits than were Walbeck and Argelander. No opportunity for a direct comparison with Struve immediately presented itself, but Walbeck compared himself with Struve on passing through Dorpat in 1821, and Argelander compared himself with Struve on a similar visit in 1823. The first four equations given below were therefore known, and it became possible to eliminate both Walbeck and Argelander algebraically and to find without direct observation the relation of Bessel to Struve; thus:

By direct comparison:	B–W	= –1.041 secs. (1820)	B–A	= –1.223 secs. (1823)
By direct comparison:	S–W	= –0.242 sec. (1821)	S–A	= –0.202 sec. (1823)
Hence, indirectly:	B–S	= –0.799 sec.	B–S	= –1.201 secs.

The difference between the two values of B–S suggests that there is variability in the personal equation. Ultimately, Bessel established the fact of variability. In 1825, a visit of Knorre to Dorpat and then to Königsberg gave another indirect value for B–S. Only in 1834 did Bessel and Struve find an opportunity

to compare themselves directly, but certain joint observations made in 1814, before Bessel had ever heard of Kinnebrook's dismissal, furnished data for another correct comparison. In all, then, there were finally available five determinations of B–S, three indirect and two direct, ranging from 1814 to 1834. The values in seconds are:

Direct	*Indirect*	*Indirect*	*Indirect*	*Direct*
(1814)	(1821)	(1823)	(1825)	(1834)
−0.044 sec.	−0.799 sec.	−1.021 secs.	−0.891 sec.	−0.770 sec.

Thus Bessel may be said to have discovered not only the personal equation, but also its variability. He found a gross error, and he showed that one could not reliably 'calibrate the observer' in order to correct for it.

Bessel's interest did not stop here. He found for himself that his error was less with a clock beating half-seconds, and again for himself that the rate of motion of a star did not affect the error. The latter point was not, however, verified later. He was also concerned to discuss the nature of observation by the 'eye and ear' method, but to this point we shall return later.

Obviously the first result of Bessel's discovery was to lead the astronomers to determine personal equations and to correct for them. There was no reason to believe that the variability of the personal equation was so great as to render correction entirely useless. In the four values for B–S determined after the discovery of this individual difference (1821-1834), all show that Bessel always observed in advance of Struve by an amount varying between 0.770 and 1.021 secs. No correction can apparently then be accurate to one tenth of a second, but presumably correction in this case would reduce error from about a second to about one fourth of a second.

Astronomical Use of the Personal Equation

About 1830, Robinson, the director of the Armagh Observatory in Ireland, determined the probable errors of observations of the sun's limbs, and, publishing his report much later, described these values as personal equations. In 1833, two astronomers, Wolfers and Nehus, at the Göttingen observatory in Germany, where Gauss was director, determined their relative personal equation and corrected their observations for it. In 1837, Gerling, the

director of the observatory at Marburg, planned to investigate, with the coöperation of Gauss at Göttingen and Nicolai at Mannheim, the differences in longitude of the three observatories, which form a triangle with mountains near one visible at the other two. The method involved the observation of heliograph signals flashed in daytime and flashes of gunpowder at night. Before computing the results, Gerling visited the two other observatories and determined his personal equations with the other observers for flashes of light and for stellar transits. Beside the three principal astronomers, Gauss's assistant and Gerling's assistant also observed; thus there were five observers in all, and the equations between Gerling and the other four made it possible to reduce all the results to Gerling's times. In 1838, Airy, the astronomer royal at Greenwich, began the practice of recording personal differences in the observation of transits, computed as Maskelyne had done from the calibration of the clock. This practice was continued until 1853, and the annual variations in the differences for Main-Rogerson and for Main-Henry became available for fourteen and thirteen successive years respectively. There were at least three other determinations of personal equations for the purpose of correction in the early '40's: that by Quetelet, in comparing the longitudes of Brussels and Greenwich; that by Struve, in comparing the longitudes of Pulkova and Altona; and that by Goujon, in determining the diameter of the sun. Had the personal equation been as little variable as other errors of observation, undoubtedly the practice of determining it and correcting for it would thus have grown up. Its repeated determination, however, served principally to exhibit its variability, and astronomers turned their attention to its control and elimination.

Various alternative methods of dealing with the personal equation or determining it were suggested quite early. Gerling, in the work just mentioned, had sought to determine it by observations of the transits of a spring pendulum, instead of a star. This method had in it the possibility of determining the true time as well as the observed time, but this problem was not faced until the chronograph was perfected later. In 1843 Arago sought to avoid the difficulty which seemed to be caused by the divided attention required in the 'eye and ear' method. He had one observer at the telescope make a sharp stroke when the star appeared to cross the wire, and another observer estimate the tem-

poral position of the sound of this stroke between the two beats of the clock. The first observer had thus to attend only to a visual stimulus, the second only to an auditory one. Under these conditions, the personal difference practically disappeared; nevertheless, the method did not come into use. Arago even went further by eliminating the second observer. He constructed a device in which the observer at the telescope could pull a trigger at the instant of transit and thus cause the pointer of the chronometer to make a mark upon the dial so that the fraction of a second could be read off later. In 1849 Faye described a photographic method, especially applicable to the sun (where there is abundance of light): instantaneous photographs are made of the field, including the cross-wires, at times determined electrically, thus 'excluding the observer' entirely. In 1852 a binocular eyepiece was developed at Greenwich such that two observers could observe the transit of the same star simultaneously. This method ruled out the calibration of the clock as a factor, because, even if the clock had been wrong, a personal difference in the estimation of tenths of a second when two observers were listening to the same beats would be immediately obvious. Just at this time, however, a satisfactory chronograph became available and distracted attention from these new methods.

Repsold had constructed a chronograph at the Hamburg observatory in 1828, but the speed of movement was not constant, and Repsold died two years later, before the instrument was perfected. There was, therefore, no satisfactory device of this sort until one was developed by workers in the United States Coast Survey about 1850. The superintendent of the Survey, Bache, was the leading spirit in this work, but the credit is due jointly to six men who worked in coöperation.

The chronographic method was adopted at Greenwich in 1854. The chronograph was simply the forerunner of the modern kymograph, a drum on which a pointer traces a long spiral line. An electromagnet, connected with a seconds-clock, causes the pointer to draw a jag in the line every second. At the instant of transit the observer taps a key, which causes another pointer, tracing a line on the drum parallel to the first line, to make a jag. By comparison of the two lines the astronomer can note the time of transit and measure it to a fractional part of a second. The observer's task is as simple as in Arago's device where he pulled

a trigger to make the hand of the chronometer mark on the dial between the divisions for the seconds. In the first two years of the use of the chronograph, the Greenwich observatory discovered that the instrument reduced the personal equation to less than one tenth of a second, the goal of precision that the astronomers had originally been seeking.

The chronograph, however, made easy the measurement of the absolute personal equation. Heretofore, astronomers had had to be content with the measurement of relative personal differences for pairs of observers, but it was not possible for them to say how much any one of them deviated from the truth. With the development of electrical devices that were, as compared with the times under consideration, practically instantaneous in operation, it became possible to arrange an artificial star or point of light that would move across the telescopic field and would record automatically on the chronograph the instant when it was bisected by the crucial cross-wire. The astronomer might observe the transit of this artificial star by any method he wished. He could use the 'eye and ear' method, or he could tap a key at the instant of transit in order to make a mark on the chronograph. At last it was possible to write a personal equation for one astronomer without regard to any other astronomer, and to attempt to reduce all observations to their 'true' values instead of to a system of times based upon one man.

This new method seems first to have been suggested by Prazmowski in 1854, although not actually used by him. In 1858 Mitchel reported similar experiments (begun in 1856) which showed the amount of the absolute personal equation as varying between one tenth and two tenths of a second. Beside determining the "absolute personality of the eye," as he called it, he also investigated the absolute "personality of the ear" and the absolute "personality of touch" by using auditory and tactual instead of visual stimuli. In the same year (1858), Hartmann published results obtained by a similar method, results which are notable in that they indicate a partial reduction of the variability, that still persisted, to but one of its psychological conditions.

We must also note that, even before the chronograph had been generally adopted as an astronomical instrument, F. Kaiser of Leyden had begun to measure the absolute personal equation by means of another device. He had the usual standard clock

and in addition another clock which beat at a slightly different rate from the standard clock. The artificial star, moved by clockwork, caused the auxiliary pendulum to be released automatically at the instant of transit, and an assistant then counted the beats of one pendulum until the beats of the two pendulums coincided. Since it is known how much the faster pendulum overtakes the slower in a single swing, it is easy to compute, when the number of swings required for the two to reach coincidence is known, at just what fraction of a second the one pendulum was released after the other had beat. In Kaiser's experiment, this computation gave the true time of transit, which he compared with the time observed by the 'eye and ear' method. The experiments were made from 1851 to 1859 and published four years later.

During the '60's, the decade when work on the personal equation reached its culmination, there were many measurements of the absolute equation by means of artificial transits and the chronograph or the chronoscope. The Hipp chronoscope, an instrument which measures time-intervals in thousandths of a second, and which is familiar to all psychologists, was used in 1862 by Hirsch (assisted by Plantamour) for this purpose. Interest had now, however, definitely centered on the variability of the personal equation. It is true that the new methods had reduced the indeterminate variability to an amount which, had the methods been in use, would have caused neither Kinnebrook's dismissal from the Greenwich observatory nor Bessel's astonishment at the inaccuracy of observation; nevertheless, refinement is never at an end in scientific measurement, and the astronomers still wanted to discover the causes of the variability and thus either to eliminate it or to take account of it.

A great deal of the investigation resulting from this interest in both the '60's and the '70's took the form of measuring the dependence of the size of the personal equation upon various astronomical conditions. It was found that the personal equation varies from the sun or moon to the stars, from the first limb of the sun or moon to the second, with the magnitude of a star, with the direction of motion of a star, with the rate of a star, and with still other less fundamental changes.

The discovery of so many astronomical conditions of variation suggested that there might be even more, and rendered hopeless the attempt to take them all into account by allowing for them.

It was becoming quite plain that what was needed was not an astronomical, but a psychological, analysis of the variants. If the personal equation is different for the sun and the stars and even for different magnitudes of stars, presumably it depends upon the brightness of the visual sensation. If it varies for different rates of motion of the star, one wants to know how the time-element enters into the observation itself. Bessel, we have seen, recognized how importantly personal variability entered into these differences, and thus really set the problem as a psychological problem. Forty years later, just as experimental psychology was coming into independent existence, it was becoming plain that the problem could not be solved without a psychological analysis of the kinds of observation involved. The beginnings of this analysis were made by the astronomers themselves, but the continuation of the problem was taken over by the physiological psychologists. It is for this reason that experimental psychology can be said to have grown in part out of astronomy: astronomy furnished it with a problem, some facts, a method, and some apparatus.

Psychophysiology of the Personal Equation

There was from the very first a certain amount of interest in the psychophysiological explanation of the personal difference. Bessel himself discussed the problem in 1822: "If it is assumed that impressions on the eye and the ear can not be compared with each other in an instant, and that two observers use different times for carrying over the one impression upon the other, a difference originates; and there is a still greater difference if one goes over from seeing to hearing, and the other from hearing to seeing. That different kinds of observation are able to alter this difference need not seem strange, if one assumes as probable that an impression on one of two senses alone will be perceived either quite or nearly in the same instant that it happens, and that only the entrance of a second impression produces a disturbance which varies according to the differing nature of the latter."

Here lies the germ of the theory of prior entry, which came later to be good psychological doctrine. If a coincidence of two events is observed later by some persons than by others, there is a loss of time in the later observations to be accounted for; but since the different observations must be fundamentally similar

in their mechanisms, it thus seems probable that there is actually a delay in both cases, and that the delay is simply greater in the one than in the other. The problem is one of the cause and locus of the delay. Most men who thought at all about this problem in 1822 believed that the transmission of the nervous impulse was practically instantaneous, but the most casual introspection shows that the processes of the mind consume time. Hence Bessel placed the locus of time in the mind; it occurred, he thought, in "the carrying over of one impression upon the other."

The astronomer Nicolai, however, in 1830 suggested that the delay might occur in the nerves, or at least in the reflex times for the eye and the ear. He wrote of the personal difference: "In my opinion it can scarcely have its basis in any other causes than that a difference exists in different individuals for mental reflexes from external impressions of the eye and of the ear. Thus one comes to believe that in one individual the mental reflex from the eye, for example, occurs earlier than the mental reflex from the ear, or, otherwise expressed, that in a united activity directed upon the same object, the object of both senses is in such an individual seen earlier than it is heard; and that in another individual both reflexes either differ in lesser degree or occur at the same time or even in the opposite order (*i.e.*, the reflex from the eye is later than the reflex from the ear). In this manner the reported phenomenon is explained quite completely and univocally. Thence there would follow the important result already discussed that the opposing interaction between two organs of consciousness is not quite instantaneous."

Nicolai may not have meant that the delay was in the nerve, since he spoke of "mental reflexes," but Johannes Müller certainly so understood him. Müller, as we have noted elsewhere, believed that the conduction of nervous action is practically instantaneous. He, therefore, after quoting Nicolai in 1834, pointed out that the delay might be occasioned by the time required for the sensorium to take cognizance of impressions. He wrote: "It is well known that the sensorium does not readily perceive with equal distinctness two different impressions; and that, when several impressions are made on the nerves at the same time, the sensorium takes cognizance of but one only, or perceives them in succession. When, therefore, both hearing and sight are directed

simultaneously to one object, this will necessarily be first heard and then seen. The interval of time, however, between the two perceptions by the sensorium may be greater in some individuals than in others: some persons may receive and be conscious of many impressions at the same moment, for which others require a considerable interval." Thus Müller very nearly stated the principle of prior entry as a condition of attention, for, although he did not mention attention, he appealed actually to the limited range of attention in his explanation.

In 1850, Müller's belief in the instantaneousness of nerve action was overthrown by Helmholtz's measurement of the velocity of the nervous impulse, but his view nevertheless persisted as the basis of Wundt's psychology of the 'complication.' Helmholtz's discovery, coming as it did in the very year of the perfection of the chronograph, paved the way for a simple explanation of the absolute personal equation, which depends upon nothing more than reaction time. If nervous conduction is slower even than sound, then reaction times have a simple physiological meaning, and no vague process of the mind needs to be invoked.

The subsequent investigations of the astronomers brought out many new facts concerning the personal equation and suggested three different types of explanation.

In the first place, there was the peripheral explanation in terms of the retina. C. Wolf, who published in 1864 an elaborate study of absolute personal equations with artificial transits, sought experimentally to demonstrate that an important factor in the personal equation may be the persistence of the visual image upon the retina. His view never gained acceptance, and it is too involved to be described here. Newcomb (1867) and Gill (1878) accepted the fact that the personal equation varies with the magnitude of the star, and, although their views differ in detail, were in accord in believing that a star of large magnitude, approaching a cross-line, would appear to coincide with the line earlier, since the periphery of its image would approach the line sooner than the periphery of a star of lesser magnitude.

Then there was the afferent explanation, which took account of possible differences in times of conduction of the auditory and visual impression. However, Nicolai's theory of individual differences for the times of the two senses never gained ground, in spite of Helmholtz's demonstration that the rate of nervous action

is measurable and relatively slow. Investigation of such a view should have been favored by the invention of the chronograph, the chronoscope, and the method of measuring reaction times, but the issue was confused by the fact that the astronomers with the new method still remained interested in personal equations more than in reaction times. Wolf, for instance, used the 'eye and ear' method for observing artificial transits, where the true time was known, in preference to the reaction method.

Indirectly, however, Wolf contributed to the problem of explanation by showing that the personal equation depends upon the rate of movement of the star, thus contradicting Bessel's negative conclusion with respect to rate. This finding tells against Nicolai. If the error of observation is greater when the star is moving rapidly, then it is plain that the essential cause of the delay must be sought in the retina, as Wolf believed, or in the brain. The error would seem to be due to some constant observational lag which allows for a greater displacement of the more rapidly moving star. If it were simply a matter of different rates of nervous conduction for sight and hearing, then the rate of the star ought to make no difference. The theory based on rate of conduction presupposes that the observational stimulus is an instant taken out of a continuous series and thus independent of change within the series.

Finally, there was the central mode of explanation. It was favored by the inadequacy of the other two modes, and also by Hartmann's experiments in 1858 which showed 'expectation' to be a very important determinant of the personal equation. Bessel had discovered that the equation was greater for sudden phenomena, like occultations and emergences, than for definitely anticipated events like transits. In the observation of a transit by the 'eye and ear' method, there are two rhythmical series, the successive beats of the clock and the successive coincidences of the star with the cross-wires of the reticle. Hartmann, using artificial transits of a point of light shining through a rotating disk, allowed the disk to continue in rotation so that successive transits occurred at equal intervals. He used the 'eye and ear' method, and he discovered that the absolute personal equation became, on the average, very small indeed, and that the single times of observation were sometimes positive and sometimes negative, i.e., the observer seemed sometimes to note the coincidence before it

occurred. The inference is that in these experiments the observer was responding to his expectation of the event as established by the preceding rhythmical series, and might have made the observation even if the event had somehow been forestalled immediately before the expected moment of its occurrence. Later, the psychologists discovered this same fact for simple reaction times: with a warning signal at a fixed short period before the stimulus, some reaction times are negative and the average is very small. The reaction is to the estimated interval and not to the stimulus.

The importance of all this astronomical work for experimental psychology becomes evident when it is recalled that the most active period of experimentation upon the personal equation belongs to the '60's and '70's, the period of the birth of physiological psychology. It was already plain that at bottom the problem is psychological, that expectation, preparation, and attention are factors in the explanation. The explanation also raises questions of the time-relation of ideas and impressions, and it was this problem that first interested Wundt. In 1861, he constructed a simple pendulum that swung across a scale and caused a spring to give a click at a given point in its excursion. He described these experiments in 1863 in his lectures on the human and animal mind. In the first edition of the famous *Physiologische Psychologie* (1874), he devoted an entire chapter, entitled "Course and Association of Ideas," to these matters, and described a new pendulum which is still known in many laboratories as Wundt's complication clock. It was even later, however, that the complication experiment and the reaction experiment came to be clearly differentiated, though the results of each were referable to attentional predisposition. In Wundt's new psychological laboratory at Leipzig, Lange showed in 1888 that reaction time varies with the attentional predisposition of the observer, and von Tschisch found in 1885 that in the visual-auditory complication (as Wundt named mental integrations of impressions from different sense-departments) the point of coincidence depends also upon the attentional predisposition. It was these two experiments that astronomy furnished psychology.

The reaction experiment developed first in the '80's and '90's into a complex chronometry of mental acts (compound reactions) which proved to be less scientifically productive than had been

expected. Nevertheless, it remains a fundamental psychological method for the study of action and for the determination of the times of psychological phenomena where time is a significant variable (e.g., in diagnostic association reactions).

The complication experiment, while still regarded as a classical experiment, has proved less useful. The meaning of the experiment has been somewhat obscured by the fact that the original astronomical situation has been kept: a continuously moving visual impression is complicated by the addition of a discrete sound. This form of presentation is known to introduce eye-movements which unnecessarily complicate the result. Only very recently has the experiment been tried of comparing a discrete sound and a discrete touch, with the apparent result that the impression for which the observer is predisposed (sound or touch) is sensed earlier than the synchronous impression (touch or sound) for which the observer is indisposed.

Notes

There are at least three good accounts of the history of the personal equation, all excellent as of their respective dates. They are: C. A. F. Peters, *Astronomische Nachrichten*, 49, 1859, 2-30, esp. 16-24; or, with slight modifications, his *Ueber die Bestimmung des Langenunterschiedes zwischen Altona und Schwerin*, 1861; R. Radau, [*Carl's*] *Repertorium für physikalische Technik*, 1, 1866, 202-218, 306-321, and, less importantly, 2, 1867, 1-9; 4, 1868, 147-156; or the corresponding French account, *Le moniteur scientifique du Dr. Quesneville*, 7, 1865, 977-985, 1025-1032; 8, 1866, 97-102, 155-161, 207-217; 9, 1867, 416-420; and finally E. C. Sanford, *Amer. J. Psychol.*, 2, 1888-89, 3-38, 271-298, 403-430. Sanford gives a bibliography of 108 titles. An account of the physiology and psychology of the personal equation, with less emphasis upon its history, is S. Exner, *Arch. f. d. ges. Physiol.*, 7, 1873, 601-660; 11, 1875, 403-432; or, in condensed form, Hermann's *Handbuch der Physiologie*, 1879, II, ii, 255-277.

Maskelyne and Bessel

N. Maskelyne's original account of Kinnebrook's persistent "error" and his subsequent dismissal is found in *Astronomical Observations at Greenwich*, 3, 1799 (the portion for 1795), 319, and esp. 339 f.

For Bessel's discovery, see F. W. Bessel, *Astronomische Beobachtungen in Königsberg*, 8, 1823 (for the year 1822), iii-viii; 11, 1826 (1825), iv; 18, 1836 (1832), iii; *Abhandlungen*, 1876, III, 300-304. That Bessel should see the significance of Maskelyne's note about Kinnebrook appears less surprising when we recall that this was a period of great interest in observational and instrumental errors and in the mathematical theory of errors. Laplace was the pioneer in the theory of errors, but the contributions of Gauss were so striking that the normal law of error is sometimes nowadays (incorrectly, it would seem, because of both de Moivre's and Laplace's priority) called the 'Gaussian law.' Gauss was director at Göttingen, and we have seen in the

text the intellectual rapport that existed between the different German directors. C. F. Gauss's mathematical theory of errors was put forth in his *Theoria motus corporum cœlestium*, 1809. For his application of the fundamental principles to astronomical and geodetic observation, see his *Abhandlungen zur Methode der kleinsten Quadrate*, 1887, 54-91 (1826), 92-117 (1809), 129-138 (1816), 139-144 (1822). In other words, the determination and control of errors of observation was a much discussed matter. We even have the personal equation for Gauss and Gerling.

Astronomical Use

For T. R. Robinson's early determinations of personal equations with the sun's limbs, see his report almost thirty years later, *Places of 5,345 Stars Observed at the Armagh Observatory (First Armagh Catalogue of Stars)*, 1859, x f. For the equation of Wolfers and Nehus in 1833 at Altona, see Peters, later the director at Altona, *op. cit.*, 1859, 18. For C. L. Gerling's experiment, see *Astron. Nachrichten*, 15, 1838, 250-278, esp. 259 f. Airy had the transit observations recorded at Greenwich by observers from 1838 on, and computed by the method of least squares the average personal equation between Main and Rogerson and between Main and Henry for the years 1846-1853. Peters computed them for 1841-1845. See Peters, *op. cit.*, 19 f. For the other instances of the use of the personal equation in the early '40's, see R. Sheepshanks and A. Quetelet, *Sur la différence des longitudes des observatoires de Greenwich et de Bruxelles*, esp. 4-13, in *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-lettres de Bruxelles*, 16, 1843, no. 1; O. W. Struve, *Determination de la longitude entre Pulkova et Altona*, 1843; J. J. E. Goujon, *Comptes rendus*, 28, 1849 (observations, 1835-1848), 220-223.

The astronomers wished to 'exclude' the ever-variable observer from the observations. Of course, such a desire is a paradox; there can be no observation without an observer. Most of the plans, however, meant the reduction of the observation to visual space with observation made at leisure: this is what the physical scientist always attempts—to translate the observation automatically into the visual reading of a scale. Such readings are the most acute and the least variable of all sensory discriminations. Arago's method is to be found in *Comptes rendus*, 36, 1853, 276-284. Faye's photographic methods are in *Comptes rendus*, 28, 1849, 241-244; 46, 1858, 705-710; 50, 1860, 965-967. There were also other methods not mentioned in the text. One was to illumine the cross-wires rhythmically and to adjust the time and rate of the illuminations so that the star crossed the successive wires at successive flashes. The time of transit could be computed from the adjustment of the flashes. Another method was to move the telescope by clockwork with the star and at the same rate, and to adjust the setting of the telescope so that the star remained bisected by the critical cross-wire. The time of transit was then computed by observing the adjusted setting of the telescope. These methods do not actually eliminate the observer, but allow him to make a spatial reading visually at leisure.

Chronograph and Chronoscope

For a general description of the chronograph and the chronoscope and their use in the reaction experiment, see W. Wundt, *Grundzüge der physiologischen Psychologie*, III, 1911, 359-388; E. B. Titchener, *Experimental Psychology*, II, 1905, i, 142-167; ii, 326-356. The development of a satisfactory chronograph by Bache and his colleagues in the United States Coast Survey is described by B. Peirce, *Proc. Amer. Acad. Arts and Sci.*, 4, 1859, 197-199.

As a practical instrument, the chronoscope antedated the chronograph. Wheatstone constructed a chronoscope in 1840 and used it for measuring the velocity of cannon-balls. Mathias Hipp, a watchmaker and mechanic, conceived the idea of a chronoscope in 1842, and, after having seen Wheatstone's model, constructed one in the following year. Improvements in both Wheatstone's and Hipp's chronoscopes were made during the decade, and in 1849 Oelschläger described experiments upon the times of falling bodies measured with a new form of the Hipp chronoscope. All this happened before the perfection of the chronograph, but the chronoscope does not seem to have been taken up by astronomers until Hirsch's experiment, in 1862. On the early history of the chronoscope, see B. Edgell and W. L. Symes, *Brit. J. Psychol.*, 2, 1906, 58-62, 86-88, and references there given.

Later Development

The first suggestion for the determination of absolute personal equations is said to have been made by Prazmowski in *Cosmos*, 4, 1854, 545 (cf. the relevant paragraph by Le Verrier in *Comptes rendus*, 38, 1854, 748 f.). O. M. Mitchel's experiment is reported briefly in *Roy. Astron. Soc., Monthly Notices*, 18, 1858, 261-264, and *J. Franklin Inst.*, 66, 1858, 349-352. J. Hartmann's study is in [*Grunnert's*] *Arch. der Math. und Physik*, 31, 1858, 1-26, and (slightly abbreviated) *Astron. Nachrichten*, 65, 1865, 129-144.

For F. Kaiser's method of the double vernier pendulum, see *Verslagen en mededeelingen der koninklyke Akademie van wetenschappen, Amsterdam, afdeeling natuurkunde*, 15, 1863, 173-220; 2de Reeks, Deel 2, 1868, 216-236. The first article is in Dutch, but the second is in German. Kaiser notes that he is utilizing the principle of the nonius or vernier, and Sanford's vernier chronoscope, employing the observation of visual instead of auditory coincidence of the two pendulums, was patterned after it; cf. E. C. Sanford, *Amer. J. Psychol.*, 3, 1890, 174-181; 9, 1898, 191-197.

The reference to A. Hirsch's first use of the Hipp chronoscope for the observation of artificial transits is *Bull. de la Soc. de Sci. nat. de Neuchâtel*, 6, 1863, 365-372.

For a complete discussion of the dependence of the personal equation upon numerous astronomical variables, see Sanford, *Amer. J. Psychol.*, 2, 1889, 271-298.

Psychophysiological Theories

Bessel's discussion of the carrying-over of one impression upon the other occurs in his first publication on the personal equation, *op. cit.*, 1822, p. vii. Nicolai's theory and account of the personal equation is reported by Treviranus in *Isis von Oken*, 23, 1830, 678-682. Joh. Müller's discussion of the problem occurs in his *Handbuch der Physiologie des Menschen*, bk. iii, sect. iii, introd. (any edition or the Eng. trans.).

The later experiments mentioned are: J. Hartmann, *op. cit.* and C. Wolf, *Annales de l'Observatoire Impérial de Paris, Mémoires*, 8, 1866, 153-208. For the later discussion of stellar magnitude and the personal equation, see S. Newcomb, *Astron. and Meteorol. Observations, U. S. Naval Observatory*, 1867, append. 3, 27, and D. Gill, *Roy. Astron. Soc., Monthly Notices*, 39, 1878, 98.

W. Wundt's early discussion of the matter and his mention of his complication pendulum of 1861 is to be found in his *Vorlesungen über die Menschen- und Thierseele*, 1863, I, lect. 23, which is amplified in the second edition, 1892, lect. 28 (also Eng. trans.). In this connection it is interesting to note that Wundt in 1861 made an address at Speyer in which he argued for individual differences in the order in which sight and hear-

ing enter into these observations: cf. E. B. Titchener, *Amer. J. Psychol.*, 34, 1923, 311. For the more formal incorporation of these data into systematic physiological psychology, see Wundt, *Grundzüge der physiologischen Psychologie*, 1874, chap. 19, esp. pp. 727-780; cf. the corresponding section in later editions, e.g., 1911, III, 44-79, 357-451. W. James has an excellent and clear summary and criticism of the Wundtian view: *Principles of Psychology*, 1890, I, 409-416, 427-432. The classical experiments from Wundt's Leipzig laboratory are Lange on reaction time and "attention," and von Tschisch on prior entry and "attention": L. Lange, *Philos. Stud.*, 4, 1888, 479-510; W. von Tschisch, *ibid.*, 2, 1885, 603-34.

On compound reactions, the subtractive procedure, and mental chronometry, see J. Jastrow, *Time-Relations of Mental Phenomena*, 1890; Wundt, *op. cit.*, III, 1911, 424-451.

K. Dunlap has shown the dependence of the complication experiment upon eye-movements, *Psychol. Rev.*, 17, 1910, 157-191, and S. Stone has recently found a time-difference of the order of 0.05 sec. with eye-movement eliminated when attention is directed differentially upon sound and touch, *Amer. J. Psychol.*, 37, 1926, 284-287.

THE PREPARATION FOR EXPERIMENTAL PSYCHOLOGY WITHIN PHILOSOPHICAL PSYCHOLOGY

Chapter 9

BEGINNINGS OF MODERN PSYCHOLOGY: DESCARTES, LEIBNITZ, AND LOCKE

BY this time it is clear to the reader that problems of scientific psychology grew up within science and forced themselves in the natural course of development upon scientific attention. Still *psychology*, as a subject-matter with that name, before the middle of the nineteenth century was a formal division not of science, but of philosophy. In the very beginning, with the Greeks, there was, of course, no such clear distinction. Aristotle, for example, did not need to distinguish between the rationalistic and empirical methods. It was later, as we have seen, that the two diverged, and still later (*ca.* Locke, 1690) that philosophy became predominantly psychological, thus making psychology philosophical and not scientific. All these differences, however, are ultimately artificial and appear only at the veneered surface of knowledge. They are essentially matters of human convenience in the formalization of knowledge at a particular time. Fundamentally knowledge is one, and it may be that philosophy and science are again approaching each other.

That such an essential unity may force a formal synthesis is exactly what the history of psychology in the nineteenth century demonstrates. Here was empirical science, on the one hand, rapidly developing, proving very fertile in its research, and therefore perpetually facing new and difficult problems. The advance of the physiology of the nervous system in particular persistently raised, and sometimes solved, psychological problems: problems of sensation and the sensory nerves and organs, of the brain and its functions, of the 'mental organs.' Other formal divisions of science contributed to psychology incidentally: physics, the perceptual laws of color and of sound; medicine, the phenomena of hypnosis; astronomy, the facts and partial explanation of the 'personal equation.' None of these advances took place because some psychologist said: "Here is a definition and method of psychology which will yield results. Let us go ahead!" The men who

made these discoveries did not call themselves psychologists. Nevertheless, here also at this time there was a psychology, an essentially rationalistic psychology, which was the property of the philosophers. Because of the continuity of the name, it seems to have been older than the 'mental knowledge' of the scientists. It—'psychology'—had been dignified by becoming focal in the philosophical method, but it gained, rather than lost identity, nevertheless: at the beginning of the nineteenth century, there was *a* psychology. What more natural than that these two 'psychologies' should fuse and become one?

If we place the beginning of 'scientific psychology' at 1860, we are merely choosing a convenient year, the date of Fechner's *Elemente der Psychophysik*. There are no such abrupt occurrences in the history of scientific thought. Lotze tried to write a 'medical psychology' in 1852, but nevertheless remained largely the metaphysician throughout. Johannes Müller had essayed a chapter "On the Mind" in 1840, yet scarcely excelled the layman-philosopher in result. Nevertheless, the fusion of the two psychologies had been impending ever since Hartley (1749), or perhaps Descartes (1650). For all this, however, in the middle of the nineteenth century with Fechner and especially with Wundt there was a formal, conscious recognition of the essential identity of the two psychologies, and since the philosophic member had been called "psychology" and the scientific member "physiology," the birth of "physiological psychology" was a natural consequence and of the utmost importance in the history of psychology.

In this history of experimental psychology, we must, therefore, go back into philosophical psychology in order to see what it was that, married to physiology, gave birth to physiological, experimental psychology. Yet we need not stress this pre-scientific psychology so much as we have stressed the 'pre-psychological science,' partly because the early philosophical psychology is already better known, and partly because our dominant interest in experimental psychology throws the greater weight upon the scientific antecedents. We scarcely need to go back of Descartes.

Aristotle

Nevertheless, Descartes is simply a convenient beginning. Many ideas that are still with us as constant tools of thought became

fixed with him. The history of thought is, however, continuous and no history of any kind of psychology can quite avoid the mention of Aristotle's name.

Aristotle (384-322 B.C.) has been acclaimed as the greatest mind in the history of thought. Be that as it may, the work and thought attributed to him (for some of the writings raise questions of authenticity) have been more influential than is the case with any other man. Only in part is his influence to be ascribed to his valid insight, for his errors have held back the progress of truth even as his correct dicta have advanced it. He was, however, an encyclopedic genius, and his researches in geometry, astronomy, mechanics, botany, zoölogy, anatomy, logic, and philosophy, as well as his philosophical presentation of all of these subject-matters, were epochal. No wonder, then, that traces of his psychology can be found in all modern psychology, or that there are still modern Aristotelian psychologies (e.g., Brentano, 1874).

If we glance briefly at the frame of Aristotle's psychological system and at just a few of the details, we shall be able to see in how many ways this great man anticipated modern psychology. Unfortunately, the bare frame lacks much of its intended meaning without the rest of the structure and without the earlier views of mind which Aristotle was seeking to correct.

Of the ten kinds of entities (we follow Aristotle's argument) one is *substance*. Substance may be living or not living, and all living substance possesses a soul. (Thus a modern author can say that the definition of psychology as "the positive science of the behavior of living things" is a return to Aristotle.) All substance is analyzable into *form* and *matter*, which are two logical aspects of substance, different because independently variable, but inseparable from each other. (The argument resembles the modern one for attributes of sensation.) There cannot be formless matter or immaterial form. Form and matter are thus interdependent, but form must be thought of as *superior* and matter as *inferior*. (Here we get the first inkling of degrees of dignity which we have to-day in such phrases as "the higher mental processes"; but the question of degree is older than Aristotle; cf. Plato's notion of the rational soul elevated in the head and the inferior souls below in the body.) The notion of degree becomes clearer when we find that form is *actual* and matter *potential*. Matter is

the possibility of form, but form is the realization of matter. This statement gives some meaning to the notion of superiority. (Cf. Meinong's *inferiora,* and *superius,* which is a *Gestaltqualität,* for a recent similar notion of superiority.) In living substance, the body is matter and the soul is form. Thus the soul is superior to the body and is the actualization of its potentialities. This actuality Aristotle calls *entelechy.* The soul is the entelechy ('end,' 'realization') of the body. (Thus Driesch to-day reverts to the word *entelechy* for a similar purpose.)

All living substance has *soul,* but there is a hierarchy of forms of the soul. At the bottom there is the nutritive or vegetable soul, which plants possess as well as animals. It is the only form of soul that all living substance, and therefore all soul, has. Next there is the *sentient soul,* which all animals have and plants lack. Beyond this the hierarchy continues pyramidally; the higher forms contain the lower, but the lower may not contain the higher. There are in order the kinetic, the appetitive, the phantastic, and finally at the apex the noetic, souls. The *noetic soul* or *nous* or intelligence is the most exalted of all, occurs only in man, and is godlike. It is free and, as nous, is immortal, though apart from the body it is no longer individualized; the individual is therefore not immortal.

This is the scheme in barest outline. We may now turn to some details that still appear in modern psychology.

The soul is unitary; it is one and indivisible; though it has several distinct faculties, it has no parts. Aristotle was combating Plato's plurality of souls. The obvious persistent fact is that the mind appears as a single totality without parts, and thus the unity of the soul has been an echo from Aristotle to Descartes, from Descartes to William James, and is to-day the central dogma of *Gestalt* psychology.

Nous is *superior* to the other souls, e.g., the sentient. Thus to-day we talk about the 'higher mental processes,' thinking, if not saying, that sensations are 'lower mental processes.' The modern hierarchy is plainly from sensation up to intelligence. Experimental psychology has been most successful with these 'lower' processes, and much of the complaint of academic philosophers about modern psychology has been that it does not deal adequately with the higher mental phenomenon, nous.

The soul is *free* and *immortal.* The problem of freedom of the

will is still with us, and the deterministic psychologist who denies it may be said to shut his eyes to an obvious fact of mind. On the other hand, it is not clear that the voluntarist is always unmotivated by an interest in immortality.

In the noetic soul we must recognize an *active* intellect and a *receptive* intellect. Here we have the distinction between will and feeling or between will and intellect, which still persists, reinforced in some quarters by the distinction between motor and sensory.

Substance is unitary, but nevertheless it has form and matter, which in living substance becomes *soul* and *body*. This is not the mind-body dichotomy of Descartes that furnishes the ground of so much psychological thinking to-day, but it is a recognition of the necessity for some such distinction.

The *seat of the soul,* in so far as this phrase can be used at all for Aristotle, is the *heart*. The heart is the center of animal heat and life and thus of the sentient soul, although the noetic soul is not dependent upon any bodily organ. This dogma is one of Aristotle's 'errors,' and it is probably remotely responsible for the confused state of thought as to the 'organ of mind' in the beginning of the nineteenth century.

Aristotle's discussion of *sensation* is full and complete and shows perhaps better than any other subject how empirical observation was constantly interwoven by him with rationalistic dogma. We find here the doctrine of the *five senses,* which is still good doctrine, for even Aristotle noted the relative complexity of touch. When we have more recently found sixth and seventh senses, it has always been by analysis of common sensibility or 'somesthesia,' as it has recently been called.

Another point that Aristotle had to explain was how the telesthetic senses can perceive their objects. He assumed a diaphanous and a trans-olfacient *medium* (and perhaps a trans-sonant medium, though he may have meant only the air) as interposed between the organ and the object. We have already seen that it became necessary much later in the argument for the doctrine of the specific energies of nerves to emphasize the importance of the media that are interposed between the brain and its perceptual object.

Nowadays we call this knitting-up of psychology with physics *objectivism,* but we must remember that there was no distinction between 'psychology' and 'physics' for Aristotle, living so long

before the Cartesian dualism had become impressed upon thought. Nevertheless, the modern psychologist will see that Aristotle's 'monism' has much in common with modern behaviorism.

Aristotle's distinction between direct *sensing* and *judgment* anticipates the later problem of the difference between sensation and perception. We may perceive a white object directly as white by vision; to perceive it as the son of Diares, however, requires a judgment. Thus perception, as distinct from sensation, has been in a sense the central problem of systematic psychology from Locke to the most recent *Gestalt* psychology.

In this connection we note further that cogitation proceeds out of sense and experience and that prior to these, mind is but a *tabula rasa,* a notion which is the starting point of Locke's psychology. Thus we get even a hint of empiricism in Aristotle.

Another anticipation of the modern psychology of perception lies in Aristotle's discussion as to whether we can perceive two distinct sensations at once. He thought that we cannot perceive them as distinct, but that two sensations may *combine into one;* we can perceive a sweet white object. Here is a foretaste of association, and of its Wundtian descendants, fusion and complication.

Finally there is the matter of *association* itself. This is a formal concept foreign to Aristotle, and yet every one knows that the traditional laws of associational psychology—similarity, contrast, and contiguity—and the essentials of the doctrine are based directly upon Aristotle's discussion of memory.

It is plain that very many modern fundamental concepts of psychology can be found in Aristotle, and the historian can show for some of them a continuous descent. For this history, however, the reader must go to other sources. It is enough, as we begin with Descartes, for us to realize that he is by no means the first systematic psychologist.

René Descartes

René Descartes (1596-1650) marks the actual beginning of modern psychology when the historical divisions are drawn broadly as ancient, medieval, and modern. The psychology of two of the three centuries since Descartes we have called 'prescientific,' but even were we to limit modern psychology to the seventy years since Fechner, we should nevertheless be forced

to note that Descartes was in a sense a physiological psychologist.

Descartes must have been born to lead the intellectual life. In spite of certain years as a soldier (and they must have been uncomfortable years!) he sought intellectual seclusion even as a youth. Once he left Paris in disgust because his friends insisted upon disturbing him in his quarters. In 1629 he was advised to devote himself to philosophy, and he then settled in Holland to avoid acquaintances and to live with philosophy. During the very productive period of the next twenty years before his death he is said to have lived in thirteen different places and twenty-four different houses, usually with his whereabouts unknown except to a few intimates who respected his seclusion and forwarded communications to him. He never married. In 1649 he felt obliged to yield to the request of the Queen of Sweden to instruct her in philosophy, but the change resulted disastrously. He died of pneumonia in 1650.

Descartes was not only in the social sense an individualist. He was impatient of interference by the great men of the past. In short, he was an intellectual 'radical.' He had little respect for tradition or the classics. Students of the history of science will realize the danger of such an attitude, but the proof of the pudding is in the eating. Descartes's independence had the effect of freeing him from the limitations of tradition and thus yielded more original work than might otherwise have been possible.

Primarily Descartes was a philosopher. In his early years he was also a mathematician. His secondary interest in adult years was science. He wrote a physics containing his theory of vortices, and an optics. He both wrote and experimented in physiology, and his knowledge of physiology was extensive for the times. His belief that animals were without souls and therefore automata led him into vivisection, with its attendant advantages for physiological discovery. He contributed to the theory of sensation and the theory of visual space-perception.

A dominant motive with him was the application of physics to physiology. The human body he regarded as a machine, and physics gave the natural approach to its understanding. This machine was, of course, no less a machine because it was controlled by the soul and in turn affected the soul. His physiology was thus thoroughly mechanistic and anticipated the mechanistic

physiology of to-day, when all but a few 'vitalists' look to physics and chemistry for the ultimate account of living processes. Thus, too, he set the stage for mechanistic physiological psychology, although we must remember that modern 'objectivists' are following only half of Descartes's psychology: they accept the body and reject the soul.

At the bottom of Descartes's psychology lies his *dualism* of soul (mind) and body. It is a clean-cut dualism, not a mere logical erection of antithetically related aspects, like Aristotle's form and matter. The definitions of each member of the dichotomy are related to the belief that animals are automata. Body is all that pertains to the inanimate and, in the way of further explanation, extended substance. Soul is all that cannot pertain to the inanimate; it is non-extended substance and hence conscious.

This doctrine has been exceedingly influential. To-day it is still basal to the view of mind possessed by the man in the street, the view that mind is some thing in the head that does not take up space. It creates the problem that all theories of perception, including incidentally the doctrine of the specific energies of nerves, sought to solve: that is naïvely to say, how can the object get into the mind when the two are in separate worlds? Even Descartes, however, did not always succeed in following out strictly all the implications of his view. He sought, for example, to explain the fact that we see things right side up in spite of the inversion of the retinal image by supposing that the nerve fibers are reversed spatially in their terminations in the brain. There should be no such problem for a thoroughgoing dualism.

It should also be noted that this dualism is fundamental to most technical psychological thinking since Descartes. All theories of mind and body imply it in some form, for there is no need for a theory of the relationship of mind to body until the two have been separated. Those who try to write about a mind without a body do not succeed, and often reassert the dualism by their very care to leave the body out. On the other side, those who try to work with a psychological body that has no consciousness may fare well enough as physiologists, but as psychologists persistently find themselves influenced, confused, and often controlled by this Cartesian dogma. Historically it has been a very important principle indeed.

Descartes solved the problem of mind and body by establishing

what we now call the theory of *interaction*. Mind affects body and body affects mind, but this statement does not mean that the soul is a link in a causal chain. The body, a perfect machine, is a closed system, except that the soul is a directing agent that determines which causes shall operate. Such a relation of the soul to the causal chain resembles somewhat the modern notion of a 'release mechanism.'

The body is a *machine*. The human body considered without the soul is only a machine. Animals lacking thought are automata. This mechanical view is not open to argument, for it follows necessarily from the definition of body as all that pertains to the inanimate, and it frees Descartes to proceed with his physics of physiology. It is not similarly necessary that animals should be automata, but theology required that they be without souls. It was thus that theology gave the sanction for vivisection!

Of the body, that extended substance to which the laws of the inanimate are applicable, Descartes had considerable knowledge. Harvey had just discovered the circulation of the blood in 1628. Descartes knew the gross facts of circulation and of digestion correctly. He knew that muscles operate in opposing pairs. He knew that the nerves are necessary for sensation and for movement, but he thought of the nerves, according to the belief of the times, as hollow tubes conducting the animal spirits indifferently in either direction. Thus he was led to a 'pathway' theory of the peripheral nervous system that is not unlike Helmholtz's telegraph theory with the brain as a switchboard, or the modern theory of the reflex arc. It is thus also an anticipation of the projection theory. Descartes could write:

"It is to be observed that the machine of our bodies is so constructed that all the changes which occur in the motion of the spirits may cause them to open certain pores of the brain rather than others, and reciprocally, that when any one of these pores is opened in the least degree more or less than is usual by the action of the nerves which serve the senses, this changes somewhat the motion of the spirits, and causes them to be conducted into the muscles which serve to move the body in the way in which it is commonly moved on occasion of such action; so that all the movements which we make without our will contributing thereto . . . depend only on the conformation of our limbs and the course which the spirits, excited by the heat of the heart, nat-

urally follow in the brain, in the nerves, and in the muscles, in the same way that the movement of a watch is produced by the force solely of its mainspring and the form of its wheels."

The reader will note here an anticipation of the projection theory of the brain, for the important thing in connection with the 'switching' of the animal spirits about in the brain is the location of the pores of entrance and exit, that is to say, the central nerve terminations. He will also note the recognition of involuntary action in man and of the facilitation of action in habit.

However, Descartes was two centuries in advance of a knowledge of the brain as a network of fibers. He thought, nevertheless, of the localization of ideas within the brain; in fact, it was easier to think of the brain as a storehouse when it was not known to be a mass of pathways. Thus, when the mind wishes to recall something, the animal spirits—so Descartes thought—are impelled "toward different parts of the brain until they come upon that where the traces are left of the thing it wills to remember," where the pores are which were used when the thing was perceived. When one imagines something, the spirits, according to Descartes, have opened pores in the proper part of the brain. When one keeps the attention fixed, the direction of the spirits within the brain is preserved constant. All this theory takes on meaning only in connection with the history of the localization of function in the brain, a history with which we are already familiar.

This, then, is the body. What is the nature of the soul?

The unextended soul, which is "all that is in us and which we can not conceive in any manner possible to pertain to a body," perceives and wills. It thus interacts with the body. Perceptions and passions are primarily dependent upon the body, but the soul knows them. Most actions originate with the will, but not all, for there are some that the will cannot bring about directly. Descartes mentions the pupillary reflex and speech. In the latter, the will acts indirectly, for we can will to say the words, and thus the muscles move correctly; but we cannot directly will to move these muscles thus and thus so that the words are formed. In general, there is less to say about the soul than about the body. In so far as the soul is free, there is nothing to say, for freedom is without laws and cannot be generalized under them. The important thing then is to inquire the manner in which the soul interacts

with the body, but first we must note Descartes's view of introspection.

The persistent problem of introspection is the question as to whether in a perception, for example, the perception itself is also a knowledge of itself, or whether we have also to 'perceive' the perception to know that we have it, thus presumably establishing an indefinite regress. Such a regress has been objected to on several grounds, and yet the opposite view, viz., that to perceive is to know that one perceives, raises the question as to why a true knowledge of one's own mind is difficult to come at. If the mind is there, one ought also to have a true and irrefutable knowledge of it. Descartes discusses this matter in connection with emotion. He takes the position that the knowledge of mind is immediate, but that we have nevertheless to learn about it because the ancients, for whom the reader will recall Descartes had little patience, have misled us.

"Everyone, feeling the passions in himself, stands in no need whatever of borrowing any observation elsewhere to discover their nature, nevertheless, what the ancients have taught on this subject is of such slight intent, and for the most part so untrustworthy, that I can not have any hope of reaching the truth, except by abandoning the paths which they have followed."

Others, we shall see (cf. Mach), have made the same decision, though they have explained the dilemma differently.

The interaction between the soul and the body occurs at the pineal gland, the "conarium," which is the only part of the brain which is single; that is to say, the only part that is not duplicated in the two halves. Descartes was convinced that the soul, being unitary (cf. Aristotle), could not affect the body at two separate points, and, since the brain seems to be the organ to which sensation proceeds and where motion originates, he selected the only unduplicated part of the brain.

Descartes's mechanistic bias is evident in his whole discussion of the manner in which the mutual action between soul and body takes place. His thought is entirely spatial and has to do with the direction and loci of the animal spirits. The gland, by inclining to this side or that, directs the spirits here or there in this or that particular recollection, or imagination, or movement. Similarly in perception, the spirits come from different directions: we have

already seen how Descartes felt it necessary for the visual nerves to be redistributed in order to compensate for the inverted retinal image. The top of the world, it would seem, is the top of the brain for the soul that can see it only through the pineal gland!

The reader must not, however, get the impression that Descartes thought of the soul as shut up within the pineal gland. This belief is a very common misapprehension of Descartes. The gland is simply the point of interaction, and not the seat of the soul in any more complete sense. The body is extended; the soul is not extended; but the extended, when acted upon by the non-extended, requires some definite point of action, which is the pineal gland. However, "the soul is united to all parts of the body conjointly." The entire body is its seat, so long as the body remains intact. It is true that when a member is cut off, the soul is not divided, because, since it is unitary, only as much of the body as remains a unit is its seat. When the unity of a body is lost in death, the body is no longer a seat of the soul. One might think, of course, that the soul, being united to the entire body, might act directly on any part or be acted upon by any part. Such a view, however, is untenable for Descartes, because the body would become an unaccountable mechanism instead of a perfect machine. Moreover, we should find that, while animal physiology is capable of being stated under exact physical laws, human physiology would differ in being unpredictable, for the soul might interfere at any point.

With this general summary we must leave Descartes. He had a great deal to say about psychology. In particular, he contributed to the psychology of the emotions, and the present account of his general point of view is drawn from his *Passions de l'âme* (1650). He made some original contributions to the psychology of visual space-perception (the nativity of areal space; convergence and the third dimension; secondary criteria of depth) which are important because they are correct and thus anticipate modern fact. We cannot, however, go into the details of the various chapters of experimental psychology. It is enough if the reader realizes Descartes's more fundamental systematic conceptions which still influence or even dominate psychology: the mechanistic approach, the dualism of mind and body, their interaction, the brain as the important locus for the mind, the localization of the mind nevertheless in the entire body, and yet the specific localizations within the brain.

Gottfried Wilhelm Leibnitz

German psychology is sometimes said to have begun with Gottfried Wilhelm Leibnitz (1646-1716), and indeed, if we are to speak of national tendencies in psychology at all, it could hardly have begun sooner, for the period of Leibnitz's life is the period of the emergence of German culture. For this reason, the principal contemporary intellectual life lay west of the Rhine: Malebranche and the Cartesian tradition in Paris, Spinoza (d. 1677) in Holland, Locke and Newton in England. Leibnitz was one of the great mathematicians of his day, although he was surpassed by Newton. Both he and Newton independently discovered the calculus as a general method, invented a system of notation for it, and used it in the solution of problems. (It is customary now to give Newton the credit for the method and Leibnitz for the system of notation, and Isaac Barrow anticipated them both.) The publication of Locke's *Essay* in 1690 stimulated Leibnitz to a reply, the *Nouveaux essais,* which he never printed because Locke's death occurred at the time of its intended publication (1704). The *Essais* were first published half a century after Leibnitz's death. Throughout his life Leibnitz was engaged in political writing. He traveled on the Continent, sometimes for political purposes, and exerted a great intellectual influence, although the final recognition of his greatness was posthumous. The growth of his philosophy formed a persistent background to his other numerous activities, and it is with a part of his philosophy that we are here concerned.

Leibnitz is less important for experimental psychology than Descartes or Locke; than Descartes, because Descartes really stands at the beginning of modern psychology and many of its concepts, and even anticipates vaguely physiological psychology; than Locke, because Locke initiated English empiricism and thus associationism, from which, on the philosophical side, physiological psychology directly sprang. Nevertheless, Leibnitz had a psychological view of the world and thus heads a tradition of activity-psychology, which has persisted mostly in Germany and Austria, but also in England, until the present. This act school of psychology, which can also count Aristotle among its direct ancestors, borrowed less from physiology and has become less involved in experiment than has the other half of modern psychology. It therefore interests us less than does the tradition which

Wundt represents; nevertheless, the whole psychological family is so intimately connected that it is impossible to ignore one branch and yet understand the other in anything like its entirety.

Let us see if we can in a few words comprehend Leibnitz's view of nature, his monadology, and its relation to modern psychology. *Activity* lies at the very base of the system.

"Substance is being, is capable of action. It is simple or compound. Simple substance is that which has no parts. Compound substance is a collection of simple substances or monads. *Monas* is a Greek word which signifies unity, or that which is one."

The *monad* is the element of all being and partakes of its nature, and being is activity. If we ask further concerning the nature of the monad or activity, we can only be told that it is most like *perception. Activity* and *consciousness* are thus two words for the same thing and lie at the bottom of nature.

The monad is *indestructible, uncreatable,* and *immutable,* but it is not static. It undergoes a continuous process of *development* in accordance with its own laws, but loses in developmental change neither its identity nor its unity. We see in the calculus a similar situation. A function is a unity with its own internal laws that characterize it. To understand it we may break it up into atomic differentials, but this is an artificial analysis. In reality there are no differentials, and the true function is revealed only as the infinitesimal differentials reach their limit, zero, and disappear.

Immutable, uncreatable, indestructible monads can have no effect upon each other, for what could they do but change, create, or destroy each other? Thus it appears that the world is an infinite *pluralism* of independent monads. Thus, too, there are no causes. A cause would either imply mutual effects between monads (and there are none) or else an analysis of the monad so that its internal development is a causal chain (but the monad is unitary and has no parts). Cause as anything more than coincidence is sheer illusion. A monad is like a watch, perfectly constructed, wound up, and set going forever. It will continue without an external agent according to the laws of its own nature. Two such watches will be found always to agree, and yet neither is the cause of the other. Thus harmony in nature comes about without effective causation because harmony preëxists in the laws of the monads.

It is the same way with *compound substance.* In compounding, there is no creative synthesis, nor any synthesis at all. The apparent synthesis is simply the synchronous arrival of many monads at a given point in their development.

This development is in a way a process of *clarification.* If the essential essence of being is something like perception, its development would naturally be a clarification. Substance thus shows *degrees of consciousness.* The supposedly unconscious is really only relatively unconscious and has the possibility of becoming conscious (cf. the potentiality of matter and the actuality of mind in Aristotle, whence comes this doctrine). The lower degrees are for Leibnitz *petites perceptions;* the conscious actualization of these is *apperception.* The sound of the breakers on the beach is apperception; it is compounded, however, of the *petites perceptions* of all the falling drops of water, no one of which is conscious alone.

In this miniature sketch of Leibnitz's monadology we see the beginnings of many big things.

In the first place, there is the *psychological view* of the universe. It is not idealism, for consciousness does not explain or create matter. Consciousness is matter, and matter consciousness.

In the second place, there is the insistence on *activity* as essential to substance. This is the view of all modern act psychologies: Brentano's, James's, Stumpf's, Külpe's (in the later days), or McDougall's. It is argued that the most obvious thing about mind is its activity, that mental activity is so immediately patent to all psychological observation that not only can it not be denied, but it must also necessarily be the starting point for all psychologizing. Leibnitz, moreover, leaves the scientific psychologist not even the appeal to a static physics; all substance for him is active.

Hardly separate from the notion of activity is the principle of *unity.* A persistently, actively evolving mind is continuous and therefore unitary. Unity as a mental attribute we have met in Descartes. It also, like activity, has seemed obvious as applying to the mind, and the appeal to the unity of mind has persisted. The most recent form it has taken has been in *Gestalt* psychology, where analysis is decried and unitariness of mind repeatedly exhibited.

Leibnitz also gives us a doctrine of degrees of consciousness and thus also of the *unconscious.* The *petite perception* of the monad is unconscious. The sound of the single falling drop of

water may be an unconscious perception. The continuum passes through perception to apperception. There are many echoes of this view. The negative sensations of Fechner were *petites perceptions*. There is the apperception of Herbart and Wundt. There is the whole doctrine of the unconscious, recently become so important in psychopathology, but sometimes related to apperception and the degrees of consciousness.

Finally it must be observed that Leibnitz gave us *psychophysical parallelism,* the theory that in the matter of mind and body has been the usual alternative to the 'interactionism' of Descartes. The relation between the monads is one of parallelism; the two self-contained perpetual watches agree, not because of any causal interrelation, but because their laws are parallel. The case of the soul and the body is simply a special case. The two are not causally related; they follow parallel courses, and the resultant correlations appear like causes.

In all this we are to remember that Leibnitz's philosophy was a great and influential philosophy. It is not enough to note mere anticipations of later thought. We have here the actual beginnings of later theories in the sense that the development of thought was continuous and the lines of influence are more or less clear.

John Locke

We must now turn our attention from the Continent to England, where empiricism and associationism were beginning. It is this tradition more than any other that has influenced modern psychology. It has had a great effect upon modern act psychology, upon modern British psychology, and upon James in America, but it is most peculiarly the philosophical parent of experimental psychology. It is hard to see how physiology alone could have given rise to anything more than a sense-physiology or at best a psychology of sensation. The English tradition was the necessary complement for experimental psychology. By furnishing at first the problems of psychology, it defined psychology as something broader than that which the physiological methods were ready to attack. At times it has held back experimentation by providing a speculative content for the chapters of psychology that could not yet be founded upon experimental data, but in the same way it has stimulated effort to extend laboratory technique to the 'higher

mental processes.' It is the English tradition that made perception the primary problem in psychology and that indicated the fundamental line of attack. Thus Wundt (1862) began his psychology with an experimental study of perception, and Helmholtz (1866) founded his psychology of perception upon empiricism. It was the English tradition that made association the key to the 'higher' processes. Thus Ebbinghaus (1885) could see how to extend the scope of experimental psychology to association and memory. It was the English tradition, now embodied in physiological psychology, that insisted upon a psychological problem of the 'still higher' mental processes. Thus Külpe became dissatisfied with the 'sensationism' of Wundt and developed the systematic experimental introspection of the Würzburg school (*ca.* 1901-1909). Very important for psychology in general and for experimental psychology in particular is this English tradition.

Chronologically Thomas Hobbes (1588-1679), a contemporary of Descartes, a political philosopher, and the author of *Leviathan,* begins the English school. Hobbes sought, like Locke after him, to refer the content of the mind to sense-experience and thus to do away with the innate ideas, which Descartes espoused. Hobbes also outlined the doctrine of association as dependent upon the "coherence" of past ideas. This theory, however, was vague and incomplete in Hobbes. His primary importance is for political philosophy, and it is only because of his priority that it is necessary to mention him here. Chronologically he begins the new school, but Locke heads it spiritually. Locke did not, it seems, obtain his inspiration from Hobbes. We may rest content, then, with the mere mention of Hobbes, and pass on to Locke.

On the surface, the life of John Locke (1632-1704) not unlike the life of Leibnitz, seems to be a political life, for politics in those days offered a practical medium for philosophical thought. Locke, however, did not attain fame as a philosopher until the publication of his *Essay* in 1690, when he was fifty-seven years old. Only in the fourteen years remaining to him did he live the life of a great philosopher, but it is not accurate to say that he matured late. His fundamental beliefs in liberty and tolerance, which guided his thought until his death, were formed early, as were also his habits of philosophical and scientific thought and discussion. The *Essay* was begun in 1671. After experimenting with life as a tutor at Oxford and as a physician, he had in 1666 formed a friendship

with the great man who was shortly to become the Earl of Shaftesbury. They were drawn together by common ideas upon the matter of political freedom and tolerance, and Shaftesbury took Locke as his private secretary. Locke lived with Shaftesbury until his political eclipse in 1675. It was during this period that he was accustomed to have meetings with his intimates in which questions of science and theology were debated. On one such occasion, the group "found itself quickly at a stand by the difficulties that arose" in their discussion of morality and religion, and it occurred to Locke that what was needed first was a criticism of human understanding itself. He thought then that he could write the criticism itself on one sheet of paper, but the undertaking grew in the intervals that Locke returned to it until it resulted twenty years later in the famous *Essay*.

This sort of work had to be sporadic. With Shaftesbury's fall in 1675, Locke went for a few years to France, where he was able to engage in philosophy and to meet many of the thinkers of the day. He returned to London in 1679 upon Shaftesbury's restoration, but three years later his patron was arrested, tried, and acquitted, and went to Holland, where he died within a few months. Locke, under suspicion, followed him to Holland for another period of thought and writing. Here the *Essay* was completed. Locke returned to England with William and Mary in 1690, and the *Essay Concerning Human Understanding* was published a few months later.

It had been a turbulent setting for the contemplative life, but now he became rapidly famous as a philosopher, refused important political commissions, and for the most part lived quietly, in ill health, with friends near London until his death in 1704. By 1700 the *Essay* had gone into its fourth edition, and to this edition Locke added a chapter on the "Association of Ideas." French and Latin translations followed shortly. English empiricism was fairly begun.

For Locke, ideas are the terms of mind. An idea is "the object of thinking." Ideas are such things as are "expressed by the words, whiteness, hardness, sweetness, thinking, motion, man, elephant, army, drunkenness, and others." In other words, they are logical concepts; some modern psychologists would call them 'meanings'; they are, as it were, items of knowledge. If we can divide what we know consciously at any given time into components, we have

ideas. It is this meaning of *idea* that we have in the phrase *association of ideas*. The Lockian conception approximates present-day common sense: the man of the street believes that his 'head is full of ideas,' and that these ideas are 'what one thinks about,' e.g., whiteness or elephants.

This view, we are about to see, persisted through the English school, at least as far as James Mill (1829), and its correctness is a matter of controversy even to-day. Introspective psychology (cf. Titchener) fails to find such ideas immediately obvious in consciousness, and seeks to explain the common belief of men that they have just such ideas by calling them 'meanings,' which do not exist in the mind, but are immediately implied by mind. Nevertheless, the Lockian notion has persisted. The strict introspectionists have been accused of blindness to all conscious contents but sensations and images. Ward recently began his psychology by defining the presentation as the equivalent of Locke's idea. The Würzburg school admitted such ideas as "imageless thoughts." Some modern act psychologists seem to have an equivalent in their 'acts' or 'functions.' The experimental phenomenologists of the modern *Gestalt* school deal with them as phenomena. Even Titchener would seem to think that the psychologist can, if he wishes, do something with 'meanings.' It is plain, then, that the Lockian idea is not yet dead, although in arriving at maturity it has continuously changed its appearance.

It is also important to note that Locke's idea is an element. The mind is capable of *analysis* into ideas. The full significance of this fact will become more apparent as we deal with the compounding and association of ideas. We may note, however, that the elementary status of the idea has led to an entirely different controversy from the one mentioned in the preceding paragraph. Wertheimer to-day can object to analysis and elements, while welcoming the Lockian material into consciousness; Titchener could cultivate analysis into elements while rejecting the Lockian stuff.

Locke's interest in philosophy had come about by his reading of Descartes while he was a student at Oxford. He was destined, however, to become an opponent of Cartesian psychology. It was Descartes's doctrine of innate ideas to which Locke objected and to which his empiricism was opposed. Ideas, Locke thought, are not inborn; they come from *experience*.

"Let us suppose the mind to be, as we say, white paper, void of all characters, without any ideas; How comes it to be furnished? Whence comes it by that vast store, which the busy and boundless fancy of man has painted on it with an almost endless variety? Whence has it all the materials of reason and knowledge? To this I answer, in one word, From *experience*. In that all our knowledge is founded, and from that it ultimately derives itself."

Neither the conception nor the figure is new. Aristotle, as we have seen, had the notion of the mind as a *tabula rasa*, but it was only incidental to him. Locke makes the principle the central dominating point of his whole psychology. English *empiricism* is the result.

It was out of this view that the dilemma of idealism and realism arose. Locke himself wrote:

"Since the mind, in all its thoughts and reasonings, has no other immediate object but its own ideas, which it alone does or can contemplate, it is evident that our knowledge is only conversant about them."

He illustrated the problem with his famous experiment of the three basins of water: one hand is placed in cold water, the other in warm; then both are placed together in water of a neutral temperature and the one feels it as warm and the other as cold. Error and illusion always furnish the crucial situations for the discussion of this problem. But Locke was not an idealist. He believed in the reality of the neutral water, even though it was known in that instance only falsely as warm and cold. He distinguished between adequate and inadequate, between true and false ideas. It is possible to transcend illusion by reflection.

We come thus to the doctrine of ideas. There are two sources of ideas: *sensation* and *reflection*. Sensation is the obvious source: by the senses, sensible qualities are conveyed into the mind from external bodies and there produce perceptions. There remains, however, the question as to how the mind obtains knowledge of its own operations. The answer lies in the existence of reflection, which "might properly enough be called internal sense," and which is thus a second source of ideas—of ideas about ideas and the manner of their occurrence. This is the doctrine of the *inner sense*, subsequently important in act psychology. It is not that Locke believed that being aware of an idea is different from the mere

having of an idea; in this point he held with Descartes. But Locke added the act, the 'operations of the mind,' as a second object of immediate knowledge. The dichotomy resembles closely some modern dichotomies (Witasek, Messer, Külpe), where act or function, on the one hand, and content, on the other, are the two materials of which mind is constituted.

Ideas may be simple or complex, and either kind may be ideas of sensation or of reflection. The simple ideas are unanalyzable, but the complex ideas may be resolved into simple ideas. The compounding of complex ideas out of simple ideas is one of the operations of the mind that reflection reveals. This notion of mental combination and analysis is very important, for it is the beginning of the 'mental chemistry' which characterizes associationism, which (in the guise of *Vorstellungen* as *Verbindungen*) is the core of the Wundtian tradition, and which is at the bottom of all the controversy about analysis and elements to-day.

Locke was not very clear about the nature of these *compounds*. He thought there were three kinds: modes, like "triangle," "gratitude," "murder"; substances, like "a sheep," or collectively "sheep"; and relations, which arise from comparing one simple idea with another. We shall see later how this principle was developed almost to an absurdity in the duplex and complex ideas of James Mill, who held that the idea of a house is a compound of all the ideas of every item that enters into the construction of a house, and who asked then how complex the idea "called Everything" must be. Locke similarly mentions "the universe" as an example of a complex idea.

The chapter "Of the Association of Ideas" was added by Locke, as we have noted, to the fourth edition of the *Essay*. It is supposed first to have been written for the Latin translation: "De idearum consociatione." It adds little to the doctrine except to give prominence in the English title to the word *association*, by which the theory was later to be known. In the text of this chapter, as elsewhere, Locke speaks mostly of connections or combinations of ideas; he uses the word *association* and the word *associate* only once each.

Locke's doctrine of *association* is, then, his doctrine of the combination of ideas. It is plain that he is thinking of both simultaneous and successive association. Simultaneous association, however, was for him simply the complex idea, very much as it was

for Wundt. The important thing thus is to see that he thought of successive combinations as essentially the same sort of thing. In the chapter on association he stressed the importance of custom as establishing these connections, and thus anticipates the law of repetition which emerged very slowly within associationism.

For psychology, perhaps one of the most important of Locke's special theories is his doctrine of primary and secondary qualities, as they apply to simple ideas of sense. There are in the doctrine three kinds of qualities or powers.

1. The *primary qualities* are those that inhere in bodies and are singly perceived by the senses. They form the main avenue of contact between the mind and the external world. These qualities are

"such as are utterly inseparable from the body, in what estate soever it be; and such as, in all the alterations and changes it suffers, all the force can be used upon it, it constantly keeps; and such as sense constantly finds in every particle of matter, though less than to make itself singly perceived by our senses; *e.g.*, take a grain of wheat, divide it into two parts, each part has still solidity, extension, figure, mobility; divide it again, and it retains still the same qualities: and so divide it on till the parts become insensible, they must retain still each of them those qualities. . . . These I call *original* or *primary* qualities of body, which I think we may observe to produce simple ideas in us, viz., solidity, figure, motion or rest, and number."

2. The *secondary qualities* of an object are powers that the object possesses for producing ideas which do not exist within the objects in the form in which they are perceived. They are

"such qualities, which in truth are nothing in the objects themselves, but powers to produce various sensations in us by their primary qualities, *i.e.*, by the bulk, figure, texture, and motion of their insensible parts, as colours, sounds, tastes, *etc.*"

3. For the sake of completeness, Locke adds a third category: *powers*. Objects have powers to affect other objects beside the organs of sense, which are also objects. "The sun has a power to make wax white, and fire, to make lead fluid." These powers, however, have by definition nothing to do with the production of ideas.

Actually all of the qualities are powers of objects to affect the nerves (about which Locke has very little to say) and thus to produce ideas. The essential difference between the primary and

the secondary qualities is that, in the first case, the ideas are like the properties of the objects that produce them, and thus the properties are perceived directly as such; whereas in the second case, the ideas do not resemble the properties of the object at all, but are produced indirectly by the action of the primary qualities (properties). There is a certain confusion of thought because the qualities, when primary at least, reside in the object; they are not subjective; it is ideas that are the subjective data produced by objective qualities. However, the secondary qualities, although they are not ideas but only produce ideas as do the primary qualities, are "in truth nothing in the objects themselves but powers" of the primary qualities.

The doctrine can be stated more clearly if we partly surrender the Lockian terms. All objects, let us say, have properties which have power to affect other objects. When they do not arouse ideas of sense, they are called by Locke simply "powers." When they affect the nerves of sense and thus arouse simple ideas of sense, they are called "qualities." When these resultant ideas are like the original properties of the object, so that the properties of the object are directly perceived in them, the qualities are "primary." When, however, the resultant ideas do not directly represent properties of the object nor resemble them, but are aroused indirectly, then the qualities are "secondary." Thus extent is a primary quality, because it can give rise directly to the idea of visual extension, which is like the extension of the stimulus; but vibration-frequency is a secondary quality when it arouses a tone, because it does not resemble the tone and is in itself not a primary aspect of the stimulus.

While Locke's use of the term *quality* is in one way the exact opposite of the modern usage (for nowadays quality is an attribute of the subjective sensation and not of the stimulus-object), nevertheless it foreshadows the modern use. Color, sound, smell, and taste are qualities for us and were secondary qualities for Locke. So, too, our other attributes of sensation were primary qualities for Locke.

A more important thing about this doctrine is that it raises implicitly the question of the whole mechanism of relation between the stimulus and the resultant sensation (or idea). The secondary qualities have to be introduced because there is not exact correspondence between the two. The mind does not mirror the external

world; it knows about it for the most part indirectly. It was this problem of the difference between the properties of objects and the characteristics of sensations that the doctrine of the specific energies of nerves sought in part to meet, and we have seen in discussing that doctrine how Locke can be said to have anticipated one of its principles.

We have finished our brief survey of Locke's psychology. We have not been complete. There are many other interesting items, like Locke's recognition of the range of consciousness or attention, when he shows that we cannot distinguish the difference between a 1,000-sided figure and a 999-sided one, though we can distinguish such a difference when the numbers are small. We have, however, said enough for our purposes in pointing out in Locke the nature of the idea, the empirical principle of knowledge, the rôle of reflection, the compounding of complex ideas and their analysis into simple ones, the origin of the phrase *association of ideas,* and the theory of primary and secondary qualities. In the next chapter we shall see how English empiricism and associationism in the eighteenth century developed from this beginning.

Notes

In connection with this chapter, the reader may wish again to refer to chap. 1 on the rise of science and its emergence from philosophy. Descartes and Hobbes were contemporaries of Francis Bacon; Leibnitz and Locke, of Newton.

For the history of systematic psychology prior to Descartes, a history with which this book does not deal, see especially G. S. Brett, *History of Psychology,* I, 1912, II, 1921; also M. Dessoir, *Outlines of the History of Psychology,* 1912 (Eng. trans. from German), 1-88; portions of O. Klemm, *History of Psychology,* 1914 (Eng. trans. from German); and W. B. Pillsbury, *History of Psychology,* 1929. Brett is the most complete. These four books (including vol. III of Brett) are the available general histories of psychology from ancient times to the present, and the reader may supplement the present text by references to them. They all, however, present psychology more as an outgrowth and branch of philosophy than as an experimental science, and it is for this reason that the present work was undertaken. B. Rand, *Classical Psychologists,* 1912, gives in English psychologically important excerpts from representative great psychologists from the Greeks to the present day. The student who is unfamiliar with the original sources will find this book extremely useful in connection with the present chapter and the immediately succeeding chapters. There is also G. Villa, *Contemporary Psychology,* 1903 (Eng. trans. from Italian), which begins with Descartes but is less satisfactory than the other texts mentioned. H. C. Warren, *History of the Association Psychology,* 1921, is an excellent text for English empiricism and associationism, and is thus generally relevant to this chapter and the text.

In these chapters, the author is deal-

ing with philosopher-psychologists only as they furnish the background for experimental psychology or incidentally enter into it. He makes, therefore, no attempt to treat the great names completely in the text or in these bibliographical notes. For complete accounts, the reader must consult the histories of philosophy and other special texts. In general, B. Rand's bibliography in J. M. Baldwin's *Dictionary of Philosophy and Psychology*, III, 1905, may be relied upon for a list of the complete works of a given author and the secondary sources, biographies, commentaries, and criticism, up to the date of its publication.

Aristotle

Aristotle's most important psychological work is *De Anima;* next in importance are *De Sensu et Sensili*, which contains much of the doctrine of sensation, and *De Memoria et Reminiscentia*, where occur the laws of memory and 'association.' The two latter are parts of the *Parva naturalia*, and both it and the *De Anima* have been translated into English by W. A. Hammond, *Aristotle's Psychology*, 1902. Another excellent English account of Aristotle's psychology is given by Grote in the appendix to A. Bain, *Senses and Intellect*, 3d ed., 1872, 611-667. Aristotle's writings were very numerous, and the literature upon them is very great indeed. See, e.g., Rand in Baldwin's *Dictionary*, III, 75-99; G. Sarton, *Introduction to the History of Science*, I, 1927, 127-136.

Descartes

The important psychological work of René Descartes is *Les passions de l'âme*, published in 1650, the year of Descartes's death. There is an Eng. trans. by H. A. P. Torrey (1892); cf. also the excerpts in Rand, *Classical Psychologists*, 168-190. For discussion of his psychology, see Brett, *op. cit.*, II, 196-217; Dessoir, *op. cit.*, 89-96; and scattered references in Klemm, *op. cit.* (see index).

Leibnitz

G. W. Leibnitz's philosophy is mostly in scattered writings and in letters. It is best to consult compilations of his works. His *Œuvres philosophiques* (containing his French writings and translations of the Latin), edited by R. E. Raspe, 1765, contain the first printing of the *Nouveaux essais sur l'entendement humain*, which Leibnitz never published because Locke, to whose *Essay* they were a reply, died (1704) at the time they were completed. These *Essais* are the chief psychological work of Leibnitz. They are given in Eng. trans. by A. G. Langley, as *New Essays Concerning Human Understanding*, 1896.

Our text, however, has concerned itself with the more fundamental and therefore more influential portion of the philosophy. For the explication of it, the reader should consult the *Système nouveau de la nature et de la communication des substances*, 1795-1796; *La monadologie*, 1714; *Principes de la nature et de la grâce*, 1714. All of these, as well as some extracts from the *Essais*, are given in Eng. trans. with notes by G. M. Duncan, *Philosophical Works of Leibnitz*, 1890 (2d ed., rearranged and somewhat revised, 1908). Rand, *Classical Psychologists*, 208-228, reprints some of Duncan's translations.

For a general bibliography of Leibnitz, see Rand, Baldwin's *Dictionary*, III, 330-338. For secondary discussion of Leibnitz in the history of psychology, see Brett, *op. cit.*, II, 301-308; Dessoir, *op. cit.*, 126-132; Klemm, *op. cit.*, scattered references (see index) and on the unconscious, 172-177; L. Binswanger, *Einführung in die Probleme der allgemeine Psychologie*, 1922, esp. 187-193 (but see index).

It is not strictly true to say that psychophysical parallelism was first held by Leibnitz; Spinoza held a paral-

kelistic view before him. Cf. J. M. Baldwin, *History of Psychology*, 1913, I, 131-156, esp. 142-146.

Hobbes

Thomas Hobbes's important psychological works are *Humaine Nature: or the Fundamental Elements of Policie*, 1650, and *Leviathan, or the Matter, Form and Power of a Commonwealth, Ecclesiastical and Civil*, 1651. See Rand, *Classical Psychologists*, 147-167, for excerpts from the first. See also Brett, *op. cit.*, II, 219-222; Warren, *op. cit.*, 33-36.

Locke

John Locke's *Essay Concerning Human Understanding* (1690; 4th ed., 1700) has been reprinted in many editions, with and without notes, separately and in his *Works*. The edition by A. C. Fraser (1894), with many notes and biographical and critical introductions, is very useful. The literature is large; see Rand, Baldwin's *Dictionary*, III, 341-347. The reader who is following the present text with the histories of psychology should see Rand, *Classical Psychologists*, 232-255 (excerpts from the *Essay*); Brett, *op. cit.*, II, 257-264; Klemm, *op. cit.* (see index); Warren, *op. cit.*, 36-40.

It is interesting to note that in general we are dealing with men of superior intelligence. Some indication of this fact is given by Cox's estimates of their 'intelligence quotients' (cf. notes to chap. 1). Her estimates indicate that in intelligence, as measured by modern tests, Hobbes, Shaftesbury, and Locke would have been at least very superior; that Descartes was certainly brilliant (IQ = 150 at least); and that Leibnitz was one of the three most intelligent men in her study of 300 geniuses (IQ = 185). See C. M. Cox, *Early Mental Traits of Three Hundred Geniuses*, 1926.

For biographical references, see Baldwin's *Dictionary*, *op. cit.*; also Cox, *op. cit.*, 578 (Descartes), 705 (Leibnitz), 472 (Hobbes), 329 (Locke).

Chapter 10

ENGLISH PSYCHOLOGY IN THE EIGHTEENTH CENTURY: BERKELEY, HUME, AND HARTLEY

FROM Locke, English psychology passed to Berkeley, Hume, and Hartley, and then, in the nineteenth century and after an ineffective interval when the Scottish school was predominant in Great Britain, to the Mills and Bain.

George Berkeley

Locke's immediate successor in British philosophy was George Berkeley (1685-1753), later the Bishop of Cloyne. In one important respect the intellectual biographies of the two men are in striking contrast. Locke published his important contribution which marks him as a great philosopher when he was fifty-seven, after a varied life of political and intellectual activity. Berkeley published his two important contributions in successive years when he was about twenty-five. At this time, he had never been out of Ireland to associate with thinkers on the Continent or even in England; his intellectual background was that of a student and junior fellow at Trinity College, Dublin.

Berkeley published the *New Theory of Vision* in 1709 and the *Principles of Human Knowledge* in 1710. Actually, we know very little of the intellectual history that led up to these two remarkable books. Berkeley was born of an English family in Ireland. He was a precocious youth. He was matriculated at Trinity College, Dublin, less than two weeks after his fifteenth birthday (1700). During the next decade he was to become a philosopher of the first order. He received his bachelor's degree in 1704, the year of Locke's death, and his master's degree in 1707, when he was almost immediately made a junior fellow. In 1705 he joined enthusiastically with college friends in the formation of a philosophical society for the discussion of "the new philosophy of Boyle, Newton, and Locke." He kept in these days a *Common-Place Book*, a

diary of philosophical queries, memoranda, and propositions, in which the trend of his thought is evident. Before he was twenty years old, he was referring in this book to a "new Principle," which he seemed already to believe would be the key to unlock the mystery of nature. As the principle develops, we presently discover it to be the beginning of the very doctrine for which Berkeley is now famous, 'subjective idealism.' The *New Theory of Vision* was founded upon the new principle, but vision was an inadequate medium for the convincing presentation of a principle so fundamental, for it was possible to argue that what was true of one sense might not be true of another. Perhaps Berkeley feared to give so radical a view its full importance all at once. Be this as it may, he took the bull by the horns in the following year, and the *Principles of Human Knowledge* presents his whole philosophy.

The remainder of Berkeley's life is much less interesting to us because it comes after this formative period and the publication of these books. From 1713 to 1728 he spent much time in England as well as Ireland, and visited France and Italy. This period is marked by Berkeley's prosecution of his great project for founding a university in Bermuda for the Indians and the colonists. In this matter he was motivated by his educational ideas, by a conviction that the future of civilization lay in the West, and perhaps also by his personal desire for the academic-philosophic life, a desire that may have been temporarily enhanced by the numerous unexpected delays that he experienced in obtaining a charter and grant for the university while at the court of George I. The charter was given and the grant finally approved by George II; Berkeley, newly married, sailed, not to Bermuda but to Newport, Rhode Island, where he remained for three years hoping that his plans would be furthered; but the grant was never paid. He then returned to London for a few years, and in 1734 was made Bishop of Cloyne in County Cork in Ireland. Here he remained for eighteen years, again actively engaging in philosophical speculation and writing, at a time when Hume and Hartley were writing and publishing their important works. The retired bishopric of Cloyne was not, however, the academic-philosophic seat for which Berkeley longed. In 1752 he moved to Oxford and took up private residence close to New College, but in the following year he died.

We shall now turn our attention to three important and inter-

related contributions of Berkeley to psychology: his "new principle," his theory of visual space-perception, and what we may call for want of a better name his theory of 'meaning.' That these three contributions are not separable one from another will shortly be obvious.

1. Berkeley in his philosophy was historically naïve. He knew Descartes and Locke thoroughly and the *Common-Place Book* of his college days abounds in explicit references to Locke's *Essay*. He was probably not influenced by Malebranche, although certain similarities between the philosophies of the two men have led some critics to infer a direct relationship. Of Leibnitz and Spinoza, and of the "ancients," as Descartes called them, he knew little. His contemporary interest deviated in the direction of the science of Newton and Boyle, but, unlike Descartes and Leibnitz, he entirely lacked the scientific temperament. However, he shared with Descartes, who emphatically rejected the older philosophy, a freedom from the constraints of tradition. Actually, what he set out to do was to improve upon Locke. If Locke's greatness had been temporary, Berkeley's might have been too; but as it is, his "new principle" has the distinction of being toward the universe an extreme point of view that is the most obvious and immediate consequence of Locke's position: it is the left wing of empiricism.

This principle consisted essentially in the denial of matter as such and in the *affirmation of mind as the ultimate reality*. Locke had denied the innate ideas of Descartes, but had not transcended the dualism. There were still two worlds, the one knowing about the other through experience. Berkeley simply cut the knot, as a young man in his early twenties without the force of accepted tradition upon him can so often do. The ideas themselves are the one thing of which we are sure. *Esse* is *percipi*. Perception is the reality (as indeed Leibnitz had said). The problem is not as to how the mind is related to matter (Descartes) nor as to how matter generates mind (Locke), but rather as to how mind generates matter. It was a bold, clean stroke, and the logical next step after Locke. It failed of becoming accepted truth largely because it was a suicidal step for philosophy to take, for it leads to solipsism, the belief that there is only one mind, in which other minds exist as ideas, and thus abolishes the social nature of science and philosophy as collective thought. The position is not capable of disproof;

it is merely a *reductio ad absurdum,* which must be rejected along with other things that seem absurd, where reasoning is law.

The solution of many problems of *visual perception,* Berkeley found, is aided by this view. Take, for instance, the question of the size of the moon and its distance from the earth. It is said to be just so big and such a distance from the earth, but it is plain that these measures cannot apply to the visible moon, "which is only a round luminous plane, of about thirty visible points in diameter." This description would not apply if the observer could be moved from the earth to a point near the moon, but the simple fact here is that the moon has changed, if indeed one still calls the changed object the moon. In the same way we could remove the problem of the familiar illusion of the size of the moon and in fact all illusions, for perception is not illusion when *esse* is *percipi;* it is the constancy of objects that is the illusion and that requires explanation.

Thus we can pass at once to the problems of visual perception where Berkeley anticipated modern fact, but in passing we must note that the "new principle," because of its generality, went a long way toward settling a psychological approach upon philosophy. The relation of *esse* to *percipi* is still with us, a fact which explains in both historical and rational terms many of the formal connections between psychology and philosophy to-day, that persist in spite of psychology's assertion of its independent status as a science.

It is also desirable at this point to note that Aristotle, Locke, and Berkeley tended to fix upon psychology one of its primary principles of classification. Aristotle established the primary division of the senses into five. Locke emphasized the sensory nature of ideas. Berkeley, insisting upon the primacy of ideas, was obliged in the first place to separate ideas by sense-departments. Thus vision and touch, as systematic classes, are prior, for example, to form. There are no abstract forms. "The extensions, figures, and motions perceived by sight are specifically distinct from the ideas of touch, called by the same names; nor is there any such thing as one idea, or kind of idea, common to both senses." In Locke's famous hypothetical case of the man born blind and suddenly receiving his sight, the seen sphere would not look round because it was already known by touch to be round. In some such way, 'quality,' which marks off the senses from one another, has come

quite generally to be considered a primary sensory attribute and classificatory principle, even though it has never been the only principle, and though space quite early demanded independent consideration (cf. Wundt).

2. In the *New Theory of Vision* Berkeley began by separating distance from areal space and dealing with *distance*. "Distance of itself, and immediately, can not be seen. For distance being a line directed endwise to the eye, it projects only one point in the fund of the eye—which point remains invariably the same, whether the distance be longer or shorter." Thus most persons, he thought, agree that the perception of distance "is rather an act of judgment grounded on experience." Here he mentioned the equivalents of many of the 'secondary criteria' listed to-day: interposition, areal perspective, relative size. He knew about light and shade from Locke's discussion of the sphere which is perceived as a sphere and not as a disk, though he did not mention it here. Linear perspective as a criterion is only with difficulty separated from interposition and relative size. Relative movement is the only remaining criterion of those usually given to-day. We see, then, in 1709 the third visual dimension separated from areal dimensions of the retina and made secondary, as a psychological problem, to them, and we find listed most of the secondary criteria of distance.

Berkeley also was able to indicate the nature of the primary criteria of distance. He listed three. First, there is the distance between the pupils, which is altered by turning the eyes when an object approaches or recedes—what is now called 'convergence.' Then there is the blurring that occurs when the object is too close to the eye. This criterion, in spite of controversy about it nearly two centuries later, is probably not valid, for objects blur beyond, as well as within, the focus of the eye. Finally Berkeley noted the "straining of the eye," by which, when objects are brought too near, "we may nevertheless prevent, at least for some time, the appearance's growing more confused"—accommodation, in other words. We must not deceive ourselves about the extent of Berkeley's knowledge. He understood vaguely the mechanism of the perception of distance. He was essentially correct in two of his three primary criteria, but he was a long way off from a knowledge of the physiology of convergence or the theory of the horop-

ter and corresponding points, and he knew nothing of Helmholtz's theory of the physiology of accommodation.

Perhaps more important than the recognition of the mechanism of perception was the idealistic (introspective) slant that Berkeley gave it. Descartes had recognized convergence: the eyes, he noted, feel out distances as if the lines of vision were two staffs attached to them. Such a view, however, leads to a geometry of binocular vision; the perception of distance becomes the perception of angles. Berkeley avoided even mentioning angles, except to expostulate against their introduction into the discussion. He talked about distance between the pupils and thus of the positions of the eyes on turning. What he meant is that distance, in this instance, is an awareness of the positions of the eyes and is—as every one knows at once from experience—not an awareness of angles. So, too, the straining of the eyes when an object approaches is sensory. Blurring, if we consider it at all, is also sensory. What he did, then, was to make the perception of distance, even though it is mediate, a matter of sensation or idea. This is essentially the introspectionist's context theory of the visual perception of distance. Presently we shall see how this context theory was in general anticipated by Berkeley.

Having disposed of distance, Berkeley turned to *magnitude.* One might suppose that, with a true image upon the retina, magnitude would be directly perceived. Extension for Locke was a primary quality. Berkeley's position, however, admitted of no such belief as Locke's, which was both the common belief of his time, and that of a century later before the law of the specific energies of nerves. Magnitude, by which Berkeley meant objective size, is no more directly perceived than distance. In the first place, magnitude depends upon distance: far objects are small and near objects large. If we perceive magnitude at all, it is by taking distance into account, and the perception of distance itself is a secondary matter of judgment. Secondly, perceived magnitude does not accord with the geometry of space: there is a *minimum visibile* and a *minimum tangibile,* both of which are finite quantities and not the infinitesimal points which are the minima of geometry. This argument, of course, shows the psychological principle of the limen entering in to distinguish mind from matter. Thus Berkeley rescued even the idea of size from an objective world.

3. We have been speaking entirely of perception. The reader

must not suppose that Berkeley's denial of the primacy of matter obliterated the problem of perception, for it merely inverted it. We have to ask, not how the mind apprehends matter, but rather how it dispenses it. In the empiricism of Locke, matter generates mind. In the empiricism of Berkeley, mind generates matter. We must substitute for a theory of knowledge about objects a psychological description of objects; and it is plain that these objective ideas are formed through experience, and that Berkeley was thus no less an empiricist than Locke.

To the author it seems that Berkeley's *theory of objects* is a direct anticipation of Titchener's context theory of meaning, and that both theories imply association without mentioning it—Berkeley because he wrote too long before the theory, Titchener because he wrote so long after it. Be this as it may, it is plain that Berkeley undertook the solution of the problem of meaning, and that he solved it in terms of the relation or connection between ideas. We can do no better than to quote the text of the *New Theory of Vision.*

"It is evident that, when the mind perceives any idea, not immediately and of itself, it must be by means of some other idea. Thus, for instance, the passions which are in the mind of another are of themselves to me invisible. I may nevertheless perceive them by sight, though not immediately, yet by means of the colours they produce in the countenance. We often see shame or fear in the looks of a man, by perceiving the changes of his countenance to red or pale."

"Moreover it is evident that no idea which is not itself perceived can be to me the means of perceiving any other idea. If I do not perceive the redness or paleness of a man's face themselves, it is impossible I should perceive them by the passions which are in his mind."

Elsewhere he wrote:

"Sitting in my study I hear a coach drive along the street; I look through the casement and see it; I walk out and enter it. Thus, common speech would incline one to think I heard, saw, and touched the same thing, to wit, the coach. It is nevertheless certain the ideas intromitted by each sense are widely different and distinct from each other; but, having been observed constantly to go together, they are spoken of as one and the same thing."

The last sentence contains the theory of association in principle, if not explicitly in words. So also, as we have already implied,

Berkeley appealed to "an habitual or customary connexion" between ideas in his explanation of the perception of distance.

"Not that there is any natural or necessary connexion between the sensation we perceive by a turn of the eyes and the greater or lesser distance. But—because the mind has, by constant experience, found the different sensations corresponding to the different dispositions of the eyes to be attended each with a different degree of distance in the object—there has grown an habitual or customary connexion between those two sorts of ideas; so that the mind no sooner perceives the sensation arising from the different turn it gives the eyes, in order to bring the pupils nearer or farther asunder, but it withal perceives the different idea of distance which was wont to be connected with that sensation. Just as, upon hearing a certain sound, the idea is immediately suggested to the understanding which custom has united with it."

Similarly Berkeley discussed the meanings that come to be attached to words in the process of their becoming language, and how in the perception of objects the secondary (associated) idea is often taken note of to the exclusion of the primary idea which gives rise to it. Is this view so different from the modern introspectionist's theory of a conscious context yielding meaning by accruing to a conscious core? Perhaps the terms of the modern theory are more sensory and less like Locke's ideas than were Berkeley's, but then Berkeley too was trying to depict ideas as much like sensations as was possible in 1709.

David Hume

David Hume (1711-1776) was Berkeley's philosophical successor. Biographically there is a superficial similarity between the two men, for both were somewhat precocious, both were given to mature philosophical thought and writings in youth, both developed their philosophies in relative isolation without the social stimulus of intimacy with other great men, and both published their most important books while still in their twenties—Berkeley at twenty-five, Hume at twenty-eight. However, except for this precocious seriousness, there is little psychological similarity between the two. Hume was a young man much concerned with his own personality, extremely ambitious, but at the same time a perfectionist, dissatisfied with poor work in himself. A restless, nervous personality he was, subject, it would seem, to an internal

conflict, for he was persistently driven on by an urge for production and greatness, and constantly held back by his own standards of accomplishment. He completed "a college education in Scotland," at about fifteen (the usual age for its completion, he himself said), and presumably went to the university at Edinburgh, although he never graduated. He tried to study law, but he could not shift his interest from philosophical to legal matters. He tried business, but it was less "suitable" than the law, and the venture lasted only a few months. He was the second son in the Hume family at Ninewells, not far from Edinburgh, a secondary estate in an old family. He had thus a meager competence, and when he was twenty-three, after the failure of the law and of business to provide a satisfactory means of support, he undertook to make his slender income support him in seclusion in France, while he continued with the studies that had long been his compelling interest. It is impossible to say how far his philosophy had developed at this time, but at any rate it took shape rapidly during three years in France. He returned home when he was twenty-six with a manuscript almost completed, and two years later, in 1739, the first two volumes of what he called *A Treatise on Human Nature* were published. The third volume appeared the following year.

The *Treatise,* while lacking a finished style or even a clearly defined subject-matter, is generally regarded as showing the vigor of a young man and thus as Hume's most important work. It sold well, yet not so well but that its ambitious author was greatly disappointed in its reception. He began almost immediately the writing of *Philosophical Essays* which should make his points more succinctly and clearly, and these resulted in the publication of the smaller *Enquiry Concerning the Human Understanding* in 1748. Hume advised his public to read the latter work as shorter and simpler, and thus "really much more complete"; but he seems to have been influenced in this judgment by his disappointment that the *Treatise* had not brought him greater fame.

The last thirty years of Hume's life concern us less than the brief period that led up to the *Treatise.* During them, fame and, in its wake, wealth came to him. He lived as a writer, although he engaged in numerous other occupations incidentally. He was companion to an insane but wealthy marquis under very trying circumstances; he was judge-advocate in a minor military expedition against the coast of France; he was secretary to a general

on a Continental diplomatic mission; he was a librarian; he entered actively into politics. All these activities he undertook between 1745 and 1763. Twice he sought a university chair of moral philosophy (or "pneumatic philosophy," the equivalent of psychology), but failed, for his orthodoxy was insufficient for such a post. Later his writing turned toward politics and his fame consequently increased. In 1753, he began his *History of England*, which was completed in its entirety in 1761, and which was an unusual financial success, in part perhaps because he allowed his account to reflect a Tory bias. In 1763, he went to Paris with a secretarial connection in the British embassy, and was there received with great acclaim at the court and among the European scholars who gathered in Paris. No man could ask for greater contemporaneous recognition, and Hume was happy in Paris, although he always underrated his success. After three years, he returned home, became an under-secretary in London for two years, and finally in 1769 settled in Edinburgh for the last seven years of his life. His greatest claim to immortality, however, lies in the work of his secluded youth. His keen ambition, ingrafted upon a sensitive, self-depreciatory personality, led him always to impose high standards of excellence upon himself, but it led him also to court fame, society and its approval, and it is probable that these latter ends distracted him from greater philosophical accomplishment.

Nevertheless, he was a great philosopher—the last great British philosopher, in the opinion of those who think that Kant was his intellectual successor. Our interest, however, centers not in his philosophy, but in certain of his contributions to psychology. In general, it is to be noted, he preserved the tradition that philosophy is at bottom psychological; he reëmphasized Locke's notion of the compounding of simple ideas into complex; and he developed and made more explicit the notion of association. His most important direct contribution to modern psychology is, however, his clear distinction between impressions and ideas. When psychology was to seek much later for systematic classifications through which to view its complex material, it was to find fewer distinctions than it required for adequacy, and nothing, except the Aristotelian division of the five senses, proved so useful as this distinction between sensation and perception (impression), on the one hand, and image and idea, on the other. We shall also have

to consider Hume's doctrine of causation, not because that problem is necessarily psychological, but partly because Hume made it psychological, and partly because his solution throws light on the perplexing problem of psychological causation as many psychologists deal with it to-day.

Hume made the distinction between *impressions* and *ideas* fundamental. He sought to "restore the word, idea, to its original sense, from which Mr. Locke had perverted it," when he used it to include sensation. An idea is obviously the experience we have in the absence of its object; it was used by Hume in the sense of the words *idea* and *image* to-day. Over against it may be set *impression,* a word that carries the modern meaning of *sensation* and *perception.* Hume himself did not like the word, since it seemed to indicate the manner of the production of perceptions in the soul, but he knew of no other suitable name "either in English or any other language." Both impressions and ideas are then simply the respective experiences themselves; they are not defined physiologically, or by reference to external object present or absent; they are recognizably distinct kinds of experience both of which Locke included under the term *idea.*

What is, then, the difference between them? The important difference seems to lie in their *vivacity.* The impressions are more vigorous, lively, violent than the ideas; the ideas are relatively weak and faint.

"Those perceptions, which enter with most force and violence, we may name impressions; and under this name I comprehend all our sensations, passions and emotions, as they make their first appearance in the soul. By ideas I mean the faint images of these in thinking and reasoning; such as, for instance, are all the perceptions excited by the present discourse, excepting only those which arise from the sight and touch, and excepting the immediate pleasure or uneasiness it may occasion. Everyone of himself will readily perceive the difference betwixt feeling and thinking."

The systematic usefulness of this distinction has tended to escape criticism because of the readiness with which every one perceives "the difference betwixt feeling and thinking"; nevertheless, there is a difficulty. Perhaps ideas are generally fainter than impressions; but are they always so? May not the faintest impression be weaker than the strongest idea? Is the idea of thunder never more vigorous than the impression of the barely perceptible tick

of a watch? Some psychologists have had to adopt Hume's criterion in addition to Külpe's valid physiological distinction of peripheral and central excitation; but the criterion was later dismissed experimentally by showing that it was easy to compare images and sensations in intensity, and that images could be more intense than sensations, although perhaps not as intense as the strongest sensations.

We are not to suppose, however, that this difficulty escaped Hume's keen mind. He noted that ideas in dreams, in madness, in violent emotions may approach impressions in degree; that impressions are sometimes "so faint and low, that we cannot distinguish them from our ideas." Nevertheless "in their common degree" they are easily distinguished. Yet Hume failed us in our desire for a universal definition. He never told why the faintest impression is still an impression and not an idea, nor why the strongest idea, in a dream for example, is not an impression. Had he been physiologically minded, like Descartes or Hartley, he might have solved the problem as Külpe did; but he was not. One feels that he was sure of the distinction in immediate experience, that vivacity is the usual conscious difference, but that his assurance held him to the dichotomy even when the criterion of vividness failed. After all, most psychologists who have faced this problem have followed him. Memory and imagination are obviously such different experiences from sense-perception, and so rarely confused with it, that we think there must be some primary difference, if not in vivacity merely, then in qualitative texture, like the difference between a sight and a sound.

The fact that Hume must have believed in a difference in 'quality' between impressions and ideas is favored by his view that ideas are *faint copies* of impressions. Both ideas and impressions, he thought, may be *simple* or *complex*. A simple idea always resembles some simple impression, although a complex idea, since it may be constituted of simple ideas in some novel manner, need not resemble any impression. Thus it falls out that there is a one-to-one correspondence between simple ideas and simple impressions, in exactly the same manner as we picture the relationship nowadays between images and sensations. But how can an idea resemble a sensation and yet not be identical with it? It would resemble it without identity if it were of the same quality and different intensity, but intensity, we have seen, is not always an

adequate criterion. By a "faint copy" Hume must have meant that there was some difference other than faintness, the difference that always exists without respect to vivacity between a copy and the original.

Hume regarded the impressions as *causing* their corresponding ideas. We shall understand this matter better when we have seen presently what Hume meant by a cause. Here we may record that he noted four facts as establishing the causal relation: (1) simple ideas resemble their simple impressions; (2) the two concur (are correlated in time); (3) the impression is always prior to its idea; and (4) ideas never occur when their corresponding impressions cannot occur (e.g., visual ideas in the congenitally blind). The last three facts satisfy Hume's conditions for establishing causation; the first seems to be unnecessary. It is possible that this relationship is all that Hume meant by calling the idea a "copy"; a "copy" is to its original as an effect is to a cause. In any case, we must not miss the historical importance of the enunciation of this relationship. The dependence of images upon prior sensations is taken so much for granted to-day, that we are likely to forget that the conception needed clearly to be introduced into empiricism and that, in the face of the Cartesian innate ideas, it was not obvious.

Here we may pause briefly to note some incidental systematic matters in Hume's psychology. He used the word *perception* loosely to include both impressions and ideas. Impressions may be either *sensations* or *reflections*. A reflection is an impression that is aroused by an idea. Thus a sensation of pain may be copied by the mind as an idea of pain which, recurring, may arouse a reflection of aversion, which in turn may be copied as an idea of aversion, and so on. It is plain that the reflection must play a rôle in the passions. It was three years earlier (1736) that Anstruc (it is said) first applied the word *reflexion* to what we now call reflex movement or action.

Ideas Hume divided, as ideas of *memory* and ideas of *imagination*. Ideas of memory he held were more like impressions, because more vivid and because they were limited by the form of the original impression. Ideas of imagination were fainter and free. The distinction in freedom is almost nothing other than what is meant by the difference between memory and imagination. The difference in vivacity is, however, the reverse of a recent experi-

mental finding, but this experiment has led to so much controversy that the results have never been generally accepted.

We have already seen that Hume espoused Locke's notion of *complex ideas,* thus tending to establish some kind of 'mental chemistry' as the psychological method. These complex ideas may be of *relations,* of *modes,* or of *substances.* Hume named seven ideas of relation, but it seems rather that among them resemblance, quantity, quality, and perhaps contrariety are primary, and that identity, space and time, and cause and effect, although listed with the others, really reduce to them. Hume's modes are our present-day modalities: colors, sounds, tastes, and so on. Substances or objects are ideas that have names and are referred to as things, but are really explained associatively as complexes. Here Hume actually followed Berkeley in an associative theory of meaning.

Association with Hume came more nearly into its own as the fundamental law of connection among ideas, although Hume, like Locke, did not emphasize the word. It is also true that association now took on something of the character of an act of associating, and was not merely a description of the constitution of a complex.

"As all simple ideas may be separated by the imagination, and may be united again in what form it pleases, nothing would be more unaccountable than the operations of that faculty, were it not guided by some universal principles, which render it, in some measure, uniform with itself in all times and places. Were ideas entirely loose and unconnected, chance alone would join them; and 'tis impossible that the same simple ideas should fall regularly into complex ones (as they commonly do) without some bond of union among them, some associating quality, by which one idea naturally introduces another."

Hume thought of association as an *attraction* or force among ideas whereby they unite or cohere. He was thus anticipating, not so much a 'mental chemistry,' as a mental mechanics. He noted that the connections are not necessary nor inseparable, but merely such as commonly prevail; thus he spoke of the attraction as a "gentle force." In this thought he anticipated the modern notion that associations are *tendencies* in that they do not always occur and must therefore be dealt with statistically. He laid down three *laws* of association: resemblance, contiguity in time or place, and

cause and effect. Later, however, he virtually reduced cause and effect to contiguity, so perhaps there are left but two.

Hume seems also to have believed in mediate association, an addition to the theory that adds much more power to the associative principle. Ideas that are but "cousins in the fourth degree" may, he said, be connected by association.

"We must consider, that two objects are connected together in the imagination, not only when one is immediately resembling, contiguous to, or the cause of the other, but also when there is interposed betwixt them a third object, which bears to both of them any of these relations. This may be carried on to great length; tho' at the same time we may observe, that each remove considerably weakens the relation. Cousins in the fourth degree are connected by causation, if I may be allowed to use that term; but not so closely as brothers, much less as child and parent."

There remains now for our consideration only the famous doctrine of *cause and effect*. The relationship is puzzling because causes seem to *act* to produce their effects, and the action is ordinarily entirely unobservable. Hume's analysis of the relationship led him to define it by reference to three conditions.

1. In the first place, a cause and its effect are always *contiguous* in both space and time: there cannot be causal action at a distance, nor after the lapse of an interval of time. This notion of the spatial and temporal immediacy of cause and effect explains in a large measure the popular belief that a cause somehow or other 'does something to' its effect. Nevertheless something more is needed, as we shall see.

2. In the second place, a cause is always *prior* to its effect. This is an obvious simple principle which serves to differentiate the two. It is perhaps a little more obvious than it has any logical right to be, for common sense anthropomorphizes the situation and thinks of an irreversible action which can proceed only forward in time, whereas neither term is logically prior to the other and the relation is symmetrical. Nevertheless, there must be some way of naming the two terms, even though the definition makes a 'final cause' a verbal paradox.

3. Finally, there must be between a cause and its effect a *necessary connection*. Hume's giving to this condition a status correlative to the other two is a tribute to the fact that contiguity alone does not seem to yield sufficient immediacy or intimacy.

However, Hume, after insisting upon it, proceeded to annihilate it. He asked how we can observe necessity, and replied that necessity is an illusion that exists in the mind and not in objects. We have, then, rather to ask how the idea of necessity arises, and he found that necessity is an impression of reflection, which is "that propensity, which custom produces to pass from an object to the idea of its usual attendant." "Necessity is nothing but that determination of thought to pass from causes to effects and from effects to causes, according to their experienc'd union." In other words, it is contiguity after all; nevertheless, it is a little bit more. Contiguity is a 'gentle force'; necessity is stronger. Contiguity becomes necessity when there is a "constant conjunction of two objects." Things may be associated by contiguity when they occur together. They are associated by cause and effect when they always occur together. Having been indissoluble in experience, they are conceived by the mind as necessarily connected.

This view of cause and effect as correlation is extremely important for psychologists. Many physical laws are causal, and psychology, struggling consciously to be a science and taking physics as the model, has sought laws of mental causation. Physics, however, had become quantitative before psychology had asserted its scientific independence of philosophy, and there came to be added to Hume's notion the additional condition that a cause and its effect must be quantitatively equivalent. This was a condition that psychology could not satisfy: there was no common quantitative mental measure, like energy, to which all psychological phenomena could be reduced. Those who believed that cause and effect ought to imply quantitative equivalence said that psychology could not be causal, but others reverted to an approximation of Hume's view. Ernst Mach and Karl Pearson, for instance, have looked upon cause in the entire psychophysical domain as a correlation of events in time. If their view differs from Hume at all, it is mostly in the fact that they cannot ask for contiguity in space as well as time, since mental events are not localized. There is even nowadays some tendency to get away from time also and to express the contiguity with reference to some other medium. The method of statistical correlation has grown up, and the question is sometimes asked whether the constant concurrence of two characters in the same individual does not mean a causal relation between them. Plainly, we are not ready to say that every such

correlation is causal; the occurrence of one term must certainly be prior to the occurrence of the other, and even then we may doubt. The supposed causal relation of sun-spots to terrestrial weather would certainly avoid contiguity in both space and time, but would insist upon the priority of the sun-spots. There are cases of equilibria, in both physics and economics, where any point on one function is referable to some previous point on the other. In such cases, however, the fact is best pictured as a total complex process in time. Hume's view would therefore seem to be only a method for dealing with nature and applicable where scientific analysis already has separated events in time. It is because so many psychological data are found by such an analysis that Hume's doctrine is so important to psychology.

David Hartley

David Hartley (1705-1757) is an important figure, but not, like Hume, a great man. He is important because he was the founder of associationism. He was not the originator; that was Aristotle or Hobbes or Locke, as one pleases. The principle had been used effectively and greatly developed by Berkeley and Hume. Hartley merely established it as a doctrine. He took Locke's little-used title for a chapter, "the association of ideas," made it the name of a fundamental law, reiterated it, wrote a psychology around it, and thus created a formal doctrine with a definite name, so that a school could repeat the phrase after him for a century and thus implicitly constitute him its founder. It is apt to be thus with 'founding.' When the central ideas are all born, some promoter takes them in hand, organizes them, adding whatever else seems to him essential, publishes and advertises them, insists upon them, and in short 'founds' a school. Hartley was less of a 'promoter' than some present-day founders of psychological schools or other men of his own times, for he was not temperamentally a propagandist. Nevertheless, as we pass from Hume to him, it is important to see that origination and founding may be very different matters. Whoever discovered 'association,' there is not the least doubt that Hartley prepared it for its *ism*.

There could scarcely be a greater contrast of personality than that between Hartley and Hume. From the restless, versatile, ambitious, energetic Hume, always dissatisfied, generous but also

a little quarrelsome, we turn to a calm, persistent, self-complacent man, always assured, benevolent and tolerant. Hume's life could hardly be reduced to a paragraph; Hartley's could. We can do no better than quote a part of the description of Hartley by his son, making allowances for the fact that it is a son who writes, but realizing that the account at least fits the superficial facts.

"The philosophical character of Dr. Hartley is delineated in his works. The features of his private and personal character were of the same complexion. It may with peculiar propriety be said of him, that the mind was the man. His thoughts were not immersed in worldly pursuits or contentions, and therefore his life was not eventful or turbulent, but placid and undisturbed by passion or violent ambition. . . . His imagination was fertile and correct, his language and expression fluent and forcible. His natural temper was gay, cheerful, and sociable. He was addicted to no vice in any part of his life, neither to pride, nor to sensuality, nor intemperance, nor ostentation, nor envy, nor to any sordid self-interest: but his heart was replete with every contrary virtue. . . . His countenance was open, ingenuous and animated. He was peculiarly neat in his person and attire. He was an early riser and punctual in the employments of the day; methodical in the order and disposition of his library, papers and writings, as the companions of his thoughts, but without any pendantry, either in these habits, or in any other part of his character. . . . He never conversed with a fellow creature without feeling a wish to do him good . . ."

This is human perfection, and yet perfection seems to be but *l'homme moyen* in greatness. It is not merely that the ordinary man reacts against assured, tolerant, benevolent poise in self-defense (for one could still love Hume after two centuries, but scarcely Hartley). It is more that the driving force that leads to greatness runs to extremes of good and bad, or of truth and error. Some error is the price of much truth, some smallness of much greatness.

Hartley was the son of a minister and was prepared for the church, but he was prevented from taking orders because of conscientious scruples against signing the required Thirty-nine Articles, in particular the one dealing with eternal punishment. This heterodoxy is the most dramatic thing in Hartley's life, but the reader will note that an insurmountable objection to the doctrine of eternal punishment is but the price of the tolerance and benevolence of the man. Unable to enter the church, Hartley

studied for the medical profession and continued to practise throughout his life, a kindly physician of scholarly habits, who dispensed his philosophy for mental ills along with his other drugs. He knew his Latin, and wrote in Latin on the association of ideas in 1746. He knew science in general, as one still could in those days. He extended his admiration especially to Locke and to Newton, and his psychology was a result of a fusion of Locke's theory of the association of ideas and Newton's theory of vibrations. His friends for the most part were within the clergy. His one great book is the *Observations on Man,* published in 1749. But a little more than six years later he died of 'the stone,' after taking great quantities of a medicine which he thought he had shown would cure it; however, since this book was eighteen years in preparation and since Hartley was not of a polemical nature, it is doubtful if a longer life would have been productive of much more psychology.

Hartley first got the notion of his vibrational, associational psychology from reading Newton and Locke. As a physician, it was natural that he should apply Newton's conceptions to the nervous system. He was thus in a sense a physiological psychologist, the first important one in England. The general application of the theory of association was suggested to him by a publication of the Reverend John Gay (*ca.* 1731), but some idea of the final result is said to have been in Hartley's mind even earlier—that is to say, in his early twenties, perhaps while he was still at Cambridge. At any rate, his book was definitely begun about 1731 and completed about 1747, although it was not published until 1749. Thus it is plain that Hartley wrote relatively independently of Hume. Hume's *Treatise,* not widely read at first, was published in 1739-1740, and his *Essays,* brought together as the *Inquiry,* a year after the completion of Hartley's *Observations* (though a year before its publication). Hartley's Latin essay appeared in 1746. It is plain that the *Observations* represent a philosophical avocation, secondary to Hartley's busy life as a physician, and completed as leisure permitted during a long period of about eighteen years.

Hartley was a *dualist* in his psychology. He was anxious to make it clear that he did not hold to a materialistic conception of the free and immortal soul. Although his laws of ideas in the mind and of vibrations in the body are very like, he insisted that

they are parallel and not the same. Thus Hartley is for all practical purposes a psychophysical parallelist, although he does not represent the direct tradition of Leibnitz and Bain in this view. Still, Hartley's view is a parallelistic dualism, and not a dualism of interaction like Descartes's. Superficially, his systematic view resembles Descartes's. Hartley is the first clear dualist of mind and matter after Descartes, and like Descartes he had particularly before him the problem of mind and body. Both men were brought to this problem by an interest in physiology, and thus they were both primitive physiological psychologists. Hartley's was a physician's and Descartes's a scientist's physiology, and the analogy cannot be pressed; nevertheless there is an analogy.

On the physiological side, Hartley had first to indicate the parts of the body that have to do with mind, that are 'the seat of' the mind. He was quite explicit: the brain, the spinal cord, and the nerves have to do with sensation and motion; ideas, however, depend upon the brain alone. Within the nervous system he embraced Newton's notion of *vibratory action,* instead of the currently accepted theory of the flow of animal spirits in tubes. There occur (he thought) in nervous substance "vibrations of the small, and as one may say, infinitesimal, medullary particles." These are exceedingly small, longitudinal vibrations of the particles; "that the nerves themselves should vibrate like musical strings, is highly absurd." Within the nerves these small vibrations give rise to sensation or motion; but within the brain there may be even smaller vibrations, "diminutive vibrations, which may also be called Vibratiuncles, and Miniatures, and which are the physiological counterpart of ideas." The small, but larger, vibrations in the nerves arouse these miniatures in the brain, and the vibratiuncles thus correspond directly to the vibrations. Hartley's thought resembles Hume's transferred to the physiological sphere. Hume said that ideas were faint copies of impressions. Hartley said that the vibrations for ideas are miniatures of the vibrations for sensations. Hume said that the ideas resembled the impressions in a one-to-one correspondence. Hartley said the vibratiuncles resemble their respective vibrations in "kind, place, and line of direction" (and he seems also to have meant *rate*), differing only "in being more feeble." Thus Hartley really strengthened Hume's fundamental view.

Hartley's argument for the dependence of sensations upon vi-

brations in the nerves was drawn primarily from the facts of the *persistence of sensation* after the removal of stimulus, facts already advanced by Newton. Hartley mentioned the persistence of vision in whirling a burning coal and in the mixture of colors on their quick succession (both illustrations of Newton's), the positive after-image, the persistence of colors when the eyeball is pressed, the persistence of the experience of heat after the removal of the hot object, and the fact that tones, although consisting of separate pulses, are nevertheless heard as smooth. What sort of a mechanism would make it the general rule that sensation would not disappear instantaneously upon the removal of its object? Vibrations, Hartley urged; vibrations, though they are effects from a cause, also involve some self-perpetuation, for it takes a little while for them to die out.

When we have finished on this simple level with the body, there is little left to say about the mind, for the parallelism is so close that the one implies the other.

"Sensations, by being often repeated, leave certain Vestiges, Types, or Images, of themselves, which may be called, Simple Ideas of Sensations."

This statement is against Hume's doctrine. In it the word *image* appears, but it is not stressed in the text.

After the object of sensation has been removed, the sensation and its vibrations persist briefly, but become fainter the while. Thus a sensation may be as weak as an idea. Moreover, by repetition, an idea and its vibratiuncles may become strong, as strong as a sensation (e.g., in dreams). Thus we see that for Hartley, as for Hume, the separation of ideas and sensations with regard to their relative intensities is only a matter of common occurrence and admits of exceptions. The fundamental distinction for Hartley is not that the vibrations for ideas are always smaller than those for sensations, but that they lie always in the brain and are aroused on their first occurrence by vibrations from the nerves.

We are now ready to consider the laws of *association*. There must really be two laws, one for the mind and one for the body; and Hartley gave both in different places. Here again we must note, however, that the parallelism is so close that the one seems only a repetition of the other with a change of phrase.

"Any sensations A, B, C, *etc.*, by being associated with one another a sufficient Number of Times, get such a Power over corresponding Ideas, a, b, c, *etc.*, that any one of the Sensations A, when impressed alone, shall be able to excite in the Mind, b, c, *etc.*, the Ideas of the rest."

The parallel law is identical in wording, except that the word *Vibration* is substituted for *Sensation*, and *Miniature Vibration* for *Idea*.

It is apparent, first, that Hartley in his laws of association appealed fundamentally to contiguity as a principle, although he did not name it thus. There was no law of resemblance (cf. Hume) to interfere with the simplicity. Hartley was thus directly in accord with the modern view of association.

Association, Hartley thought, may occur among sensations, ideas, or motions, or among all of them. The addition of 'motion' to the list is the logical outcome of his physiology, but the point gains interest for us because of the modern behavioral conception of association as conditioned reflex.

Beside mere contiguity, a further condition of association for Hartley is *repetition*. The sensations, ideas, motions, vibrations, or vibratiuncles must be associated with one another "a sufficient number of times." With Ebbinghaus's invention of experimental methods for measuring memory (1885), repetition was seen to be the all-important condition. The infinite variety of chance contiguities in the psychological life render a single concurrence scientifically negligible. Necessarily, then, the thought of repetition as an important condition has always been present in the discussion of association, but it had never been given its proper emphasis. Even in Hartley the notion was only just emerging.

There are many other ways in which Hartley anticipated modern doctrine. He noted the validity of both *simultaneous* ("synchronous") and *successive* association, thus making association adequate for the explanation both of trains of thought and of the fusion of simple ideas within a complex—Wundt's view, approximately. In successive association, he argued that associations might occur between *remote* members, and argued further that, the more remote the members, the weaker must be the associations, a conclusion reached experimentally by Ebbinghaus. He thought that associations in successive trains must always be *forward* and never backward. This is perhaps not the modern

view, but the real facts are obscured by the complexity of the processes that enter into observation.

On the physiological side, it is interesting to see that Hartley, even more than Descartes, thought naturally of some sort of *brain localization*. He spoke of "the primary seat" of a given vibration in the medullary substance, and he drew several conclusions from the manner in which separate vibrations, under certain conditions, must be spatially related in the brain. We have seen elsewhere how the question of brain localization was vehemently argued back and forth by the physiologists of the nineteenth century. When one reads the arguments against phrenology and its localizations in the early part of that century, one is apt to conclude that brain localization had never had a respectable position as a tenet of intellectual men. It is then that one may do well to go back fifty years to Hartley—or 150, to Descartes.

We have to note finally that association for Hartley, because it can be simultaneous as well as successive, becomes clearly the basis of mental *compounding*, and that we are thus carried one step further toward the 'mental chemistry' of the Wundtian school.

"Upon the whole, it may appear to the reader, that simple ideas of sensation must run into clusters and combinations, by associations; and that each of these will, at last, coalesce into one complex idea, by the approach and commixture of the several compounding parts.

"It appears also from observation, that many intellectual ideas, such as those that belong to the heads of beauty, honour, moral qualities, *etc.*, are, in fact, thus composed of parts, which, by degrees, coalesce into one complex idea."

These quotations contain the key for associationism as a systematic principle. They also reveal Hartley's total program.

In the continuation of this program he first applied association to a variety of psychological matters much simpler than morality. He pointed out that the relation of perceived visual distance to visual size, which Berkeley discussed, is a matter of association. Pleasure and pain, he observed further, are habitually related to sensations by association, and thus emotions are nothing more than aggregates of sensations, pleasures and pains, or their ideas. Words—and here Hartley is again building on Berkeley in the matter of meanings which we have already examined—acquire

their meanings by way of their associated ideas. Recollection is a use of association; memory is accurate association; even imagination proceeds by association, as in dreams, though inaccurately and not in the original form. There can be no doubt that Hartley is right in assuming that Locke's principle of association is capable of being made into the fundamental psychological law, provided only that we raise no objection to his method.

Hartley's further extension of the universal principle to the problems of "the moral qualities" need not concern us. It was these matters presumably that interested Hartley most, but, as often happens, the important contributions to history of this kindly, scholarly, assured physician were not entirely what he valued most himself.

Psychology from Hartley to James Mill

To pass directly from Hartley (1749) to James Mill (1829) is to omit a period of seventy years, and yet, in a history of experimental psychology, the immediate transition is justified. There is really nothing of great importance in this period that bears directly upon experimental psychology. There is the Scottish psychology that leads up to Thomas Brown, who influenced James Mill. There is French associationism, tracing one line of descent from Dugald Stewart of the Scottish school, and accounting for the psychology of recent French psychologists like Ribot. The Scottish school, however, did not influence experimental psychology directly, and France, which has led in abnormal psychology, has never, in spite of Binet and like-minded men, played an important rôle in what we call experimental psychology. There was also in England some psychology; great developments in thought do not entirely die and then reëmerge all within seventy years. There were, however, in England no truly great psychological books or men, as there had been in the preceding sixty years from Locke to Hartley. We shall pass, then, in the next chapter to James Mill, because he continues the direct line from Locke, the line which more than any other philosophical psychology predetermined the eventual nature of experimental psychology.

Here we must content ourselves with the bare mention of some of the names that pertain to this period.

In England associationism was settling in. Less important men,

like Tucker (1768), Priestly (1775), Alison (1790), and Erasmus Darwin (1794), were writing and thinking associational psychology, mending it here and ornamenting it there, bringing it, with combing and brushing, into such form that it might appear in society. It is because of this period and the way in which philosophical thought, given time, gradually permeates common sense, that it is impossible to say, for example, that Charles Bell, the physiologist, was entirely uninfluenced in 1811 by English philosophy, even though he may never have read Locke or Hartley.

In Scotland there was Thomas Reid (1764); the more influential Dugald Stewart (1792 *et seq.*); Thomas Brown (1820), who influenced James Mill; and finally, after the revival of English associationism, the great Sir William Hamilton (1830 *et seq.*). As philosophies go, this Scottish school is closely related to the English school, and the sharp division is arbitrary. Brown's discussion of perception, for instance, is even more modern than any we have mentioned yet; but at best the school is a collateral branch, and can be neglected when we are picturing the main trend that led up to the modern era.

The independent French tradition goes back to Descartes. There was La Chambre (1664), a contemporary of Descartes, and Malebranche (1674), a contemporary of Locke. At the very middle of the eighteenth century there were Hartley's contemporaries: Condillac (1754), La Mettrie (1748), Bonnet (1760), and Helvétius (1758). Then at the beginning of the next century there were de Tracy (1801 *et seq.*), Cabanis (1802), and Maine de Biran (1812). These writers gave the historical background for French psychology. Within them also can be found the roots of some modern psychological thought which is not French, like the James-Lange theory of emotion. In general, however, as history has taken its capricious way, they have not been as important for psychology as other men like James Mill, to whom we shall next turn.

Notes

Like the last chapter, the present one deals with philosopher-psychologists—not completely, but only as they form the pertinent background of modern psychology in general and experimental psychology in particular. The reader cannot be urged too strongly, if he wishes a thorough knowledge of these men, to go to their texts and to the numerous commentaries and criticisms that are in existence. There are many methods of approach. We may content ourselves with again mentioning B. Rand's bibliography in J. M.

Baldwin's *Dictionary of Philosophy and Psychology*, III, 1905.

He who reads merely for psychology can follow the text with the histories of psychology: G. S. Brett, *History of Psychology*, II, 1921; O. Klemm, *History of Psychology*, Eng. trans., 1914; M. Dessoir, *Outlines of the History of Psychology*, Eng. trans., 1912; W. B. Pillsbury, *History of Psychology*, 1929; H. C. Warren, *History of the Association Psychology*, 1920. The last is the most useful for the present chapter.

A compromise is to be had in the reprinted psychological excerpts in B. Rand, *Classical Psychologists*, 1912. After reading the twenty-odd pages of excerpts in this book for each author, the reader should find that the present text takes on new significance.

Berkeley

The two important works of George Berkeley are *An Essay towards a New Theory of Vision*, 1709, and *A Treatise Concerning the Principles of Human Knowledge*, 1710. There are various edited reprints. *The Works of George Berkeley*, edited by A. C. Fraser, may be especially mentioned; also C. P. Krauth's edition of the *Principles*. See Rand, Baldwin's *Dictionary*, III, 120-122; and for excerpts from the *New Theory of Vision*, see Rand, *Classical Psychologists*, 256-278.

For Berkeley, in the history of psychology, see Warren, *op. cit.*, 40-42; Brett, *op. cit.*, II, 264-270; Klemm, *op. cit.* (see index).

For Berkeley's biography and an account of his philosophy, see A. C. Fraser, *Life and Letters of George Berkeley*, 1871. See also J. S. Mill, *Three Essays on Religion*, 1874, 261-302. It is possible that the sharp contrast between Berkeley's "academico-philosophic" life (Swift's phrase) at Dublin and the busy political worldly-meticulous life of the project for an American university is to be partly accounted for by Berkeley's unexpectedly coming into a small fortune in 1723. When Esther Vanhomrigh, immortalized as "Vanessa" by Swift, discovered that her passion for Swift was insufficiently requited, she changed her will, making Berkeley and another her beneficiaries instead of Swift, and presently died, as the biographers put it, of a broken heart. Berkeley, it seems, had met her only once at a dinner in London with Swift; but Berkeley was in Dublin as Dean of Dromore, Swift was in Dublin as a voluntary political exile, and "Vanessa" was living near Dublin to be near Swift. This completely unexpected acquisition of wealth may have changed Berkeley from the unpopular, eccentric student-philosopher of Trinity College days to a promoter; certainly as a joint executor of "Vanessa's" will he had no end of other business detail to attend to at this time.

It was Berkeley who wrote: "Westward the course of empire takes its way"; and his belief in the decay of European civilization may have been enhanced by his experiences with the politics of the courts of Anne and the first two Georges. It is doubtful whether he had any very practicable ideas for the Bermudian university, and the congregation at Newport, Rhode Island, which received him unannounced (the pastor stopped his sermon to go to the wharf), can hardly have been more surprised by Berkeley than was Berkeley ultimately by his three years' visit to America. Berkeley, California, was named for him and has since become the seat of a great university. Berkeley, the city, in 1860 occupied a position in the westward course of empire similar to that of Bermuda in 1728.

The passages quoted in the text in connection with Berkeley's (context) theory of perception are from the *New Theory of Vision*, pars. 9, 10, 16-18, 45-47. The interested reader will do well to read these sections and the related text, and also pars. 50 and 51, which sum up the matter excellently. Cf. here E. B. Titchener, *Text-Book of*

Psychology, 1910, 367-371; *Beginner's Psychology*, 1915, 26-30. Then compare again the latter with Berkeley, par. 46.

Hume

The full title of David Hume's chief work is *A Treatise on Human Nature being an Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects* (1739-1740), but it was not an early experimental psychology. There are numerous editions. One with notes and a long expository introduction is edited by T. H. Green and T. H. Grose, 1878. For excerpts, see Rand, *Classical Psychologists*, 279-312. There are also several editions of *An Enquiry Concerning the Human Understanding*, 1748, which came out of the *Philosophical Essays*, published in the '40's. In general, see Rand in Baldwin's *Dictionary*, III, 271-277. See also Brett, *op. cit.*, II, 270-278; Warren, *op. cit.*, 43-47.

There are several biographical accounts, including his own *Life*. A full account is J. H. Burton, *Life and Correspondence of David Hume*, 1846, 2 vols., upon which the text is based. See also T. H. Huxley, *Hume*, 1879; James Orr, *David Hume*, 1903, esp. 14-84.

Hume's philosophy is skeptical, and he also criticized the church. For both reasons, for they are only remotely the same, he was not a strong candidate for a chair of moral philosophy.

Nowadays, because of his personal sensitiveness and self-depreciation, we should call him an introvert, but we see in him a case where these factors led to success and not to futility. His quarrel with Rousseau, then almost insane, for whom he found a refuge in England, and who then turned upon him with ill-grounded suspicions, illustrates both his generous nature and his emotional reaction to an affront.

When Hume is said to have attained wealth, the statement means that in his later years the income from his writings was about £1,000 per annum, a sum which meant much more then than now.

T. H. Green (*op. cit.*, 3) notes that the proper succession is from Hume (and Leibnitz) to Kant. It was Hume's *Treatise* that woke Kant from his "dogmatic slumber." In psychology, however, Hume's more immediate relationship seems to be to English empiricism and associationism; but we must not forget that he was a much greater man than these contributions alone imply.

The experiment that established the intensity of images as a fact and as comparable to the intensity of sensations, with the possibility that a given image may be more intense than a given sensation, is one by A. deV. Schaub, *Amer. J. Psychol.*, 22, 1911, 346-368. See 347-349 for a brief resumé of Hume and modern psychologists on this point. The experiment might seem unnecessary were it not for the fact that competent psychologists held the other view!

The question of a textural or qualitative difference between images and sensations is difficult. E. B. Titchener (*Text-Book of Psychology*, 1910, 198 f.) was not able, at the time he was writing, to conclude whether there is a textural difference or not. The difficulty is, of course, that we never experience merely a simple impression or a simple idea (in Hume's terms); all experience is always a matter of complex impressions and ideas, and even Hume never held that a complex idea resembles a complex impression or is a 'faint copy' of it. The difference is obvious, but it is obvious for complexes, and one never knows about the elements.

The author does not know the exact meaning of the word *reflexion* for Hume, whether as in thought or as of light. In the former sense, as if it were an activity of the mind, Hume used it in the discussion of cause and effect; but the definition which contrasts it with *sensation* is susceptible of the latter meaning. In this latter sense, also,

the same word had already been used as applied to reflected (reflex) movement (Astruc, 1736); but it is not likely that the youthful Hume, writing the *Treatise* in seclusion in France, ever heard of a physiological use of the word. The converse relationship, that the *Treatise* affected physiology, is possible but improbable.

The (disputed) experimental basis for the assertion that ideas of imagination are more vivid than ideas of memory is reported by C. W. Perky, *Amer. J. Psychol.*, 21, 1910, 422-452.

On Humian causality in modern science, see K. Pearson, *Grammar of Science*, 1892, chap. 4 on "Cause and Effect," and also in the later editions (1900, 1911); E. Mach, *Principien der Wärmlehre*, 1896, 430-437, the chapter on "Causalität und Erklärung," and also in the later editions (1900, 1919); *Analyse der Empfindungen*, 2d ed., 1900, and later eds., chap. 5, on causality and teleology; also Eng. trans. of the 2d ed. or later. In general, Pearson (1892) borrowed from Mach (*Analyse*, 1st ed., 1885), but in this discussion Pearson seems to have the priority. At any rate, it all goes back to Hume.

Hartley

David Hartley's book is *Observations on Man, His Frame, His Duty, and His Expectations*, 1749. The second English ed., 1791, contains the Eng. trans. of H. A. Pistorius's notes and additions to the German trans., and also a brief biography by his son (III, i-xv). Other biographical accounts are mostly drawn from this one: e.g., that of G. S. Bower, *Hartley and James Mill*, 1881, 1-7, which is also inaccurate in some details.

It is not entirely irrelevant to Hartley's character to note that he studied Mrs. Stephens's remedy for the stone, becoming convinced of its efficacy, that he then persuaded Parliament to award her the large prize offered for the discovery of a cure, and that it was this remedy, consisting largely of soap, which he consumed in large quantities in an ineffectual attempt to cure himself.

In the histories of psychology, see Brett, *op. cit.*, II, 278-286; Warren, *op. cit.*, 50-64; Th. Ribot, *English Psychology* (trans. from German, 1870), 35-43. For excerpts, see Rand, *Classical Psychologists*, 313-330. For writings of Hartley and concerning him, see Rand in Baldwin's *Dictionary*, III, 234 f.

If Hartley is the founder of associationism, then the date is 1746 and not 1749, for the theory is contained in his *Conjecturæ quædam de motu, sensus et idearum generatione*, 1746. See Bower, *op. cit.*, 5; Rand, *Psychol. Rev.*, 30, 1923, 306-311.

On Gay's earlier use of association as a fundamental principle, see Hartley's preface to the *Observations;* also Rand, *Classical Psychologists*, 311; *Psychol. Rev.*, 30, 311-313.

From Hartley to James Mill

Following the spirit of the text, we may content ourselves with the dates and important psychological writings of the men mentioned there.

1. *English associationists:*

Abraham Tucker (1705-1774), *Light of Nature Pursued*, 1768.

Joseph Priestley (1733-1804), an abridged edition of Hartley's *Observations on Man*, with three essays of his own added, 1775. Priestley was the chemist who discovered oxygen, but was also a student of Hartley.

Archibald Alison (1757-1839), *Essays on the Nature and Principles of Taste*, 1790.

Erasmus Darwin (1731-1802), *Zoonomia*, 1794.

In general, see Warren, *op. cit.*, 64-69.

2. *The Scottish school:*

Thomas Reid (1710-1796), *Inquiry into the Human Mind on the Principles of Common Sense*, 1764; etc. See Rand, *Classical Psychologists*, 361-373.

Dugald Stewart (1753-1828), *Elements of the Philosophy of the Human Mind*, 1792-1827.

Thomas Brown (1778-1820), *Lectures on the Philosophy of the Human Mind*, 1820. See Rand, *Classical Psychologists*, 374-394; cf. Warren, *op. cit.*, 70-80.

Sir William Hamilton's (1788-1856) writings were scattered and brought together by various editors later.

In general, see Brett, *op. cit.*, III, 14-18, 25-29.

3. *French psychology and associationism:*

M. C. de la Chambre (1594-1669), *Système de l'âme*, 1664.

N. Malebranche (1638-1715), *Recherche de la vérité*, 1674.

E. B. de Condillac (1715-1780), *Traité des sensations*, 1754. See Rand, *Classical Psychologists*, 341-360.

J. O. de la Mettrie (1709-1751), *L'homme machine*, 1748.

C. Bonnet (1720-1793), *Essai analytique sur les facultés de l'âme*, 1760. See Rand, *Classical Psychologists*, 331-340.

C. A. Helvétius (1715-1771), *De l'esprit*, 1758.

A. L. C. Destutt de Tracy (1754-1836), *Éléments d'idéologie*, 1801-1815.

P. J. G. Cabanis (1757-1808), *Rapports du physique et du morale de l'homme*, 1802.

F. P. G. Maine de Biran (1766-1824), *Essai sur les fondements de la psychologie*, 1812. See Rand, *Classical Psychologists*, 448-462.

In general, see Warren, *op. cit.*, 181-194; Brett, *op. cit.* II, 226-233, 290-300, 358-361, 375-382, III, 20-24; Dessoir, *op. cit.*, 221-230.

On the anticipation of the James-Lange theory of emotion by Malebranche, La Mettrie, de Tracy, and Cabanis, see E. B. Titchener, *Amer. J. Psychol.*, 25, 1914, 427-447.

Chapter 11

BRITISH ASSOCIATIONISM IN THE NINETEENTH CENTURY: THE MILLS AND BAIN

THE nineteenth century saw the culmination of associationism in James Mill, and its modification from a mental mechanics to a mental chemistry by John Stuart Mill. It saw associationism made over by Bain into the system that was to become the substructure for the new physiological psychology, and it saw the new theory of evolution first brought to bear upon psychology by another associationist, Herbert Spencer.

James Mill

In James Mill (1773-1836), associationism as a principle of mechanical compounding reached its climax. He may be said to stand for a 'mental mechanics,' just as his son, John Stuart Mill, represents 'mental chemistry.' The two Mills and Bain brought philosophical psychology to the point where scientific psychology could take it over.

Like his English predecessors whom we have studied in the last two chapters, James Mill was not a professional philosopher or psychologist. He was primarily interested in history and the theory of government, but he published in 1829 an important book, the *Analysis of the Phenomena of the Human Mind,* which, although second in importance to his history of British India, played a highly significant rôle in the history of psychology.

James Mill was a Scotchman by birth, and at about eighteen years of age came under the favorable notice of Sir John Stuart, whose wife arranged that Mill should receive a benefaction that enabled him to go to the University of Edinburgh. At this time Mill was 'destined for the church,' as the phrase was, and he prepared himself in theology, philosophy, and the classics, improving his insufficient income by tutoring. In philosophy he sat under the inspiring lecturer Dugald Stewart. He was licensed as a

preacher in 1798, but he was not successful for the excellent reason that the congregations that he addressed could not understand what he was saying. Faced with failure, and being forced to live in penurious circumstances, he finally gave up tutoring and sporadic preaching, and in 1802 went up to London in company with Sir John Stuart, then a member of Parliament.

In London he began a seventeen-year period in which writing and journalistic work were his sole source of revenue. He married in 1805 a young woman whose face was her fortune—an investment, so some biographers think, that was less happy than would have been an interest in philosophy and government. John Stuart Mill, namesake of James Mill's friend and patron, was born in 1806, and three brothers and five sisters followed him. James Mill's income was precarious and he had to use it to assist his father and sister as well as to support this large family. His natural disposition to be stern, uncompromising, and at times petulant, was not softened by these circumstances, nor by his wife's disappointment in the outcome of her marriage. However, he exhibited withal an amazing vigor. Several volumes of the *History of India,* incited in its writing by his pecuniary need, he composed at one end of a table, while John Stuart Mill went to school to his father at the other, learning among other things Greek and, in the absence of dictionaries, interrupting his father for the meaning of every new word! Such circumstances must have called for an unyielding nature.

James Mill's editorial connections were numerous, as were also the magazines to which he constantly contributed. The *Edinburgh Review* is the best known; his contributions to the *Westminster Review* are of a later date. His ten articles on government for the *Encyclopædia Britannica* were so important that they were later reprinted together. The *History of India,* begun in 1806, was finally published in 1817, and produced a change in his fortunes. It was an immediate success, and in 1819 he received, largely on its account, an appointment to the East India Company—a commercial post then not dissimilar to a government diplomatic position. He began at £800, but at the time of his death seventeen years later his annual stipend had increased to £2,000, a large salary for an academic man in those days.

Unlike the *History of India,* the *Analysis of the Human Mind* was not a response to financial need. It was the work of six

summer vacations. Mill began it in 1822, and it was finally published in 1829. In it we have to do, therefore, with the single psychological work of an important man of unusual intellectual vigor, who published it at the age of fifty-six, and who died seven years later (1836).

Of the twenty-five chapters that the two volumes of this work contain, only the first three need concern us seriously. James Mill followed Hartley (and Hume with a change of words) in making *sensations* and *ideas* the fundamental classes of elements, and the first two chapters deal respectively with these elements. The third chapter is all-important, for it is on the association of ideas. The remaining twenty-two chapters deal with consciousness, conception, imagination, classification, abstraction, memory, belief, ratiocination, evidence, reflection, pleasure, pain, will, intention, and kindred topics. The list is worth giving because it shows how the scope of psychology was becoming fixed in convention, and some such convention we still have with us. Ever since the establishment of experimental physiological psychology, systematic psychologists have been laying down premises that more or less commit them to a sensationistic psychology; and yet, when these same men have sought to write their systematic works, they have never been content to write as if psychology were nothing more than what their premises had yielded; they have always responded to this conventional pressure by an effort to deal in some fashion with some of these 'higher mental faculties.' It seems that almost everybody knows what psychology is about, though no one knows what it is. Such a state of affairs can be explained only historically, and the list of James Mill's chapters serves to make the point, although it could have been made equally well in connection with any of his predecessors or with his successor, Bain.

We have seen in the preceding chapter that the distinction between sensation and perception had been coming at the time of James Mill to be more and more clearly made. Sensation was held to be primary, and perception to be derived from it. In systematic classification one must, it was argued, begin with sensation, and hence psychology was thrown back to Aristotle's division of the senses into five for its fundamental principle of classification. Berkeley, for example, could argue that form is not a sensation, and that the visual and tactual perceptions of a sphere have, since they belong to different senses, nothing in common.

There is no sensation of sphericity. This point is clear in Berkeley, who made the differences of sensible qualities primary, and in Hume, who held that differences of sensory mode are elemental. As we have already noted, such a view was the natural outcome of empiricism.

In a 'mental chemistry,' however, it is important to know the number of elements; and, in accounting for complex phenomena, the more elements there are the better. Aristotle himself had hedged on the question of touch; it was for him a single sense, but nevertheless more complex than the others. Consequently we have had during the nineteenth century a search for more mental elements, and in particular a search for more sense-departments; and the new senses that have been 'discovered' have always come out of Aristotle's complex sense of touch, which, as we already know, at one time during this century had its complexity covered by the broad term *Gemeingefühl.*

James Mill's chapter on *sensation* shows this tendency when it was first becoming explicit. He subscribed to eight senses: Aristotle's five including touch, muscular sensations (which Thomas Brown and Charles Bell had also just added to the list as a sixth sense), sensations of disorganization in any part of the body (which, including itching and tickling, resembled E. H. Weber's later *Gemeingefühl*), and sensations in the alimentary canal (which we now know make up the larger part of the sensations of internal origin). What Mill said about these senses does not matter. It is enough to see that he felt the empiricist's need for making sensation primary, and the associationist's need for finding as many different elements as possible to enter into association. When psychology gave up the primacy of objects, denying that there are simple sensations of a *man* or of a *chair,* it had before it the positive task of making a very few elements account for all of the tremendous complexity of mental life.

For James Mill sensations were one of the "primary states of consciousness;" *ideas* were the other. Here he followed Hartley and Hume, although he did not, like Hume, place himself in the difficult position of maintaining that ideas are weaker than sensations. An idea for him was simply a copy of a sensation, though ordinarily distinguishable from it. The two are enough alike, he thought, sometimes to be confused. They differ, in spite of re-

semblance, in fundamental nature, in the fact that an idea never occurs unless its sensation has previously occurred, and in the further fact that the law of association applies to ideas, but not to sensations. The word *image* had not yet come into a technical meaning, but Mill used it as he used *copy*.

"After I have seen the sun, and by shutting my eyes see him no longer, I can still think of him. I still have a feeling, the consequence of the sensation, which, though I can distinguish it from the sensation, is yet more like the sensation than anything else can be; so like, that I call it a copy, an image, of the sensation, sometimes a representation or trace of the sensation."

James Mill's chapter on the *association of ideas* is classic because it represents at its culmination both the full power of associationism and its defects. One might read the chapter to-day either to understand how comprehensive and fundamental this principle of association can be, or else antagonistically because the chapter shows clearly the impossibilities to which this very universality leads.

Mill began by pointing out that the nature of consciousness itself is associative.

"Thought follows thought; idea follows idea, incessantly. If our senses are awake, we are continually receiving sensations, . . . but not sensations alone. After sensations, ideas are perpetually excited of sensations formerly received; after those ideas, other ideas; and during the whole of our lives, a series of those two states of consciousness, called sensations, and ideas, is constantly going on. I see a horse: that is a sensation. Immediately I think of his master: that is an idea. The idea of his master makes me think of his office; he is a minister of state: that is another idea. The idea of a minister of state makes me think of public affairs; and I am led into a train of political ideas; when I am summoned to dinner. This is a new sensation. . . ."

And thus the process continues throughout all waking life. This picture agrees essentially with the popular notion of association to-day. We have only to note in criticism that Mill, when he actually came to the discussion of association, illustrated by way of objects rather than sensible qualities. *Seeing the horse* is a sensation; *thinking of his master* is an idea. It is this reversion to Lockian terms that opens the way for Mill to progress to his comprehensive theory of compounding (*vide infra*).

The law of association does not (we are again following Mill) operate upon sensations. In a sense sensations are associated, but only in the sense that the nature of objects is such that certain sensations concur or habitually concur. They are found 'in association,' but the law is the law of the objects as the producers of sensations. This relationship is not at all what is meant by *association.* These concurrences are, however, very important, especially those that are often repeated. They may be, of course, either synchronous or successive, and the successive, Mill thought, are more frequent than the synchronous.

Out of these concurrences of sensations, concurrences depending upon the laws of objects, the law of the association of ideas arises.

"Our ideas spring up, or exist, in the order in which the sensations existed, of which they are the copies. This is the general law of the 'Association of Ideas:' by which term, let it be remembered, nothing is here meant to be expressed, but the order of occurrence."

Association is, therefore, not a power, nor a force, nor a cause; it is simply a matter of concurrence or contiguity. Sensory contiguities are copied in ideational contiguities.

Mill expressly denied the validity of Hume's three laws of association. Cause and effect Hume himself had reduced to priority and frequency—that is to say, to contiguity. Resemblance, Mill thought, is not effective except when the similar terms have been often together—as indeed they so often are. Contiguity is thus the only principle beneath the law.

Next we learn that the association of ideas, following of course the objective relationships of which they are counterparts, may be either *synchronous* or *successive.* The perceptions of objects are built up of synchronous associations: the sight and sound of a violin; the color, hardness, shape, size, and weight of a stone; a more complex group in the case of an animal or a man. The successive associations are, however, more numerous (so Mill believed), and their nature is best seen in the habitual sequence of words in thought. For example, in the Lord's Prayer, *Our* suggests *Father, Father* suggests *which,* and so on.

In James Mill we come at last to a clear recognition of the conditions of association. Associations, he noted, may vary in

strength; and there are three *criteria of strength.* One is 'permanence': the more permanent associations must be stronger. Another is 'certainty,' which plainly meant for him correctness and probably also the subjective assurance with which the association comes. The third is 'facility,' which may be equated to spontaneity or lack of effort in the formation of the association, and perhaps also to the readiness or speed of association. (One thinks at once of the later use of reaction times as a measure of associative strength.) These three factors are the observational criteria; but what causes the differences? Mill put forward frequency and vividness as the two *conditions of association,* and in so doing he anticipated what, in some quarters, is good modern doctrine. When impressional and associative tendencies have been distinguished, the differentiation has been by reference to frequency and vividness. *Vividness,* of course, is a vague term, and Mill expressly stated that he did not mean by it *intensity.* For illustration, he cited indissoluble associations induced by emotional situations. Perhaps to-day he would turn to attention for an explanation, though it is not clear that the one word is any less vague than the other.

James Mill might argue that successive associations are more frequent than synchronous associations; nevertheless, the latter class are equally important in a purely associational psychology like his. Thought, regarded in this way, seems to be made up of so much of each kind that it is hard to give either a predominance over the other. The subsumptive power of synchronous association is, however, very greatly increased by Mill's introduction of what amounts to a principle of fusion. Mill did not call it 'fusion,' or even recognize the inclusion of something new; but he was nevertheless quite explicit in his discussion of the property that synchronous associations have of combining or blending to form an apparently simple whole.

He first cited examples from sensation. The seven spectral colors rapidly rotated on a color-wheel appear to give one uniform color, white. "The several sensations cease to be distinguishable; they run, as it were together, and a new sensation, compounded of all seven, but apparently a simple one, is the result." This is not a case of association, for the colors are sensations; but, he argued, just the same thing may happen with ideas, or with sensations and ideas. At a simple level we find the phenomenon in

the ideational equivalents of the touch blends. For example, the idea of weight appears simple, but actually it involves both the idea of resistance (in turn including muscular ideas and ideas which constitute the will) and the idea of direction (involving at least the ideas of extension, place, and motion). Mill remarked that it is a "good metaphysician" who can trace the composition of the idea of weight, and indeed we now begin to see that his method may be as much metaphysical as psychological. In the same way, Mill taught that objects gain their objectivity by synchronous association: in the idea of a tree or a horse or a man there are a very great many simple ideas united by association, and—here is the crucial point—so intimately united that the object appears unitary. Words, too, get their meanings by association, and yet, because of the intimacy of the associative union, appear to be almost simple ideas.

It is plain that this discussion has landed us in an *associative theory of meaning,* very similar to the modern context theory (Titchener), if not identical with it. We met it first in Berkeley. It has kept turning up since. Now we find it in even clearer form in James Mill. There is no need to enter into the whole matter again; we should note only two specific additions that James Mill made. The one is that he appealed to attention in order to explain the familiar experiential fact that conscious meanings seem to be immediately and not associatively given. Mill thought that in an association the attention may be absorbed entirely by the consequent term of the association, so that the antecedent term is instantly forgotten. In such a case, the association would not appear to be an association. The other important new point is the statement that organic sensations are the most frequent antecedent terms that are thus forgotten. Thus James Mill recognized both the dilemma of the adequate observation of meaning (always difficult, whether one holds to an associative theory or not), and also the fact that organic sensations exhibit this difficulty in extreme degree.

Even so far it may have been possible for the reader to follow James Mill without vigorous dissent. It soon appears, however, that in admitting fusion in synchronous association, he has admitted to systematic psychology a rational principle capable of devouring observational fact. If we can discover several components, associatively combined, in the apparently, but not

actually, simple idea of a stone, why not go on? Mill did go on. We need not trace his entire progress, but may content ourselves with quoting the last paragraphs of the chapter on association of ideas, paragraphs which show where he came out.

"Brick is one complex idea, mortar is another complex idea; these ideas, with ideas of position and quantity, compose my idea of a wall. My idea of a plank is a complex idea, my idea of a rafter is a complex idea, my idea of a nail is a complex idea. These, united with the same ideas of position and quantity, compose my duplex idea of a floor. In the same manner my complex ideas of glass, wood, and others, compose my duplex idea of a window; and these duplex ideas, united together, compose my idea of a house, which is made up of various duplex ideas. How many complex, or duplex ideas, are all united in the idea of furniture? How many more in the idea of merchandise? How many more in the idea called Every Thing?"

In this *reductio ad absurdum* we see the persistent danger of philosophical psychology, unchecked by constant scientific observation. A rational principle is captured by the empirical method, and may then be turned loose to carry us even to the brink of absurdity. There is no logical reason to suppose that the idea of *everything* might not be an association of every idea of a thing, but there is not the least observational ground to believe, even with maximal telescoping, that a consciousness can contain a literally unlimited number of ideas at once. As a matter of fact, James Mill actually made no such assertion; he merely left us with a question. In the same manner we may leave him, and turn to the somewhat more satisfactory envisagement of the problem by his son, John Stuart Mill.

John Stuart Mill

John Stuart Mill (1806-1873) was an abler man than his father and had a much greater effect on the history of thought. This influence came, however, by way of his philosophy and his logic, and bore on psychology for the most part indirectly because psychology, still philosophically minded, could not fail to take account of the psychologically minded philosopher, Mill. It was, for instance, Mill's treatment of the psychological problem of the syllogism that Helmholtz had in mind when he discussed perception in the *Optik* in 1863. Yet Mill never wrote a psychology

like that of his English predecessors. His psychology is to be found in his *Logic* (1843), his *Examination of Sir William Hamilton's Philosophy* (1865), and in his father's *Analysis of the Human Mind* (1869). These dates show that in part his work comes after the formal date when experimental psychology is said to have been 'founded' (1860). He belongs at this point in our history because of the way in which he modified his father's doctrines of association, mental composition, and perception.

We have already seen something of John Stuart Mill's relation to his father, and his educational precocity is a matter of common knowledge. Mill, the son, never went to school except to his father. His entire youthful education was personally accomplished by careful, painstaking, and exacting paternal instruction. What he learned was further impressed by his becoming presently the tutor of his younger sisters and brothers. He began the study of Greek at the age of three. Before he was eight he had read Æsop's *Fables,* the *Anabasis,* all of Herodotus, some of Plato, and many other of the standard Greek texts. He had also read more of English history than most well-educated young men two or three times his age. At eight he began Latin, geometry, and algebra, and by the time he was twelve he had read more Latin than is usual in the education of youth. He kept on with his Greek and his history, and read in proof his father's *History of India,* which was published when the son was eleven. In the next two years his interest centered upon scholastic logic. He then went to France for a year, returned to England and to the study of psychology, and finally, when just seventeen, entered the employ of the East India House, in whose service his father then was. His education was stern and uncompromising. He had no boyhood friends, no child's play, and very little youthful reading. He did not realize that his education was unusual for his age, for he had no one with whom to compare himself, and his father always kept before him the degree to which he fell short of the possible ideal.

Mill remained with the East India Company thirty-five years, until the government, against his protest, took over its functions in 1858. Like his father he was successful in what was essentially governmental diplomatic work, and he obtained in this way a first-hand experience with politics that rendered him more than a merely academic writer on political subjects. His early writing was in journals and newspapers. The effect of his relation to his

father was marked. He admired and feared his father, and hesitated to differ with him openly; and his first book, the *Logic*, did not appear until seven years after his father's death (1836). There were several years of depression when Mill, brought up in an austere personal life to scorn all emotion, began to doubt the value of his political and social activities. The entire relation of this father to this son is an interesting psychological study.

John Stuart Mill's reading of Whewell's *History of the Inductive Sciences* stimulated his interest in logic, and resulted in 1843 in the publication of the *Logic*, still a very important book for those concerned with the logic of science. After this his interest and writing shifted to political economy. He married in 1851, when he was about forty-five years old, and during the seven years of married life before his wife's death he was greatly stimulated and aided by her in his political thought and productivity. After her death and the dissolution of India House (1858), he spent most of his time, except for three years in Parliament, writing at Avignon, where she died. The decade of the sixties was the period of his life in which his physiological interest came most to the fore. The *Examination of Sir William Hamilton's Philosophy* appeared in 1865. The new edition of James Mill's *Analysis* with notes by John Stuart Mill, Bain, and others was published in 1869. Most of his other writings were still, however, on political economy. He died, while still active and not quite yet sixty-seven years old, in 1873, the year before Wundt published the first edition of the *Physiologische Psychologie.*

John Stuart Mill accepted from his predecessors *sensation* (or impression) and *idea* as his systematic elements. He differed from his father in his reversion to Hume's belief that ideas are distinguishable from sensations in that they are weaker. Hume's view has proven untenable, but Mill had so little to say of sensation that the matter is unimportant.

In laying down the *laws of association*, also, Mill went back from his father's view to Hume's in adding 'similarity' to 'contiguity' as a principle of association. In 1843 he was, like his father, quite clear in showing that contiguity as an effective principle depends upon the frequency of concurrences, and in 1865 he named 'frequency' as a separate law. It is interesting to see that the importance of frequency, implicit even in Berkeley, was thus be-

coming recognized, even though, with contiguity having become a mental rather than an objective togetherness, it is almost impossible completely to separate contiguity and frequency: what we have in association is frequency of contiguities. In 1843 Mill named "intensity" as a third law of association, thus harking back to James Mill's vividness as a condition of association. In 1865, he omitted this law but created a new one which he called "inseparability." Inseparability, however, is simply the limiting case of frequency: when the contiguity admits of no exception and the frequency is great, the association becomes indissoluble. It is plain, then, that these laws of 1865 do not represent laws that operate in complete independence, but different aspects of the single principle of association, aspects which Mill wished to emphasize for systematic use later on. The formal statement is that Mill held to three laws in 1843: *similarity, contiguity,* and *intensity,* with frequency subsumed under the second; that in 1865 he stated as laws four principles of association: *similarity, contiguity, frequency,* and *inseparability,* omitting *intensity.* The crystallizing mind of James Mill, having separated these items, would have kept them rigidly distinct. The more subtle mind of his son separated them for expository purposes, and proceeded to build upon the central truth underlying the entire structure.

The really important thing, then, with John Stuart Mill is what he did with association after he had got it. James Mill had built mental mechanics, a theory of indefinitely complex mental mixtures, with the elements lost but nevertheless within the mixture. We left James Mill raising the question of the degree of complexity of the idea of Every Thing. John Stuart Mill saw the impossibility of this *reductio ad absurdum,* and for mental mechanics he substituted mental chemistry.

In the first place, we must note that John Stuart Mill admitted his father's notion of associative coalescence. Ideas may coalesce by forming so rapid an association that some of the ideas are not attended to and thus tend to drop out or even actually to disappear; or at the very least, being unattended to, to be immediately forgotten (cf. Berkeley). Thus total perceptions or ideas may be telescoped or short-circuited, a doctrine of the decay of mental formations that is not unfamiliar to-day (cf. Titchener).

This notion of fusion leads, however, at once into the chemical view. If an idea may actually disappear or even be diminished

in effectiveness, the associative whole is not merely a sum of its elemental parts; we have in it something new, just as water is more than hydrogen plus oxygen, and is also not all of hydrogen nor all of oxygen. Nor can the laws of the whole be predicted from the laws of the parts; they must be determined for themselves by experiment. The "compound" is not then a mere mixture with elements adhering; it is a generated product of combination, and thus the chemical analogy is fruitful. Here Mill in the *Logic* of 1843 is worth quoting.

"It is obvious that the complex laws of thought and feeling not only may, but must, be generated from these simple laws [of association]. And it is to be remarked, that the case is not always one of Composition of Causes: the effect of concurring causes is not always precisely the sum of the effects of those causes when separate, not even always an effect of the same kind with them. . . . The laws of the phenomena of the mind are sometimes analogous to mechanical, but sometimes also to chemical laws. When many impressions or ideas are operating in the mind together, there sometimes takes place a process of a similar kind to chemical combination. When impressions have been so often experienced in conjunction, that each of them calls up readily and instantaneously the ideas of the whole group, those ideas sometimes melt and coalesce into one another, and appear not several ideas but one; in the same manner as when the seven prismatic colors are presented to the eye in rapid succession, the sensation produced is that of white. But in this last case it is correct to say that the seven colors when they rapidly follow one another *generate* white, but not that they actually *are* white; so it appears to me that the Complex Idea, formed by the blending together of several simpler ones, should, when it really appears simple, (that is when the separate elements are not consciously distinguishable in it) be said to *result from,* or be *generated by,* the simple ideas, not to *consist of* them. . . . These are cases of mental chemistry: in which it is possible to say that the simple ideas generate, rather than that they compose, the complex ones."

The reader must grasp here the evolutionary idea if he is to understand the difference between the two Mills. He may be accustomed to think that ideally, if we knew *all* about hydrogen and oxygen, we should know all about water. He must realize that such a view is a matter of faith, for we never know that *all* about any element which we think ideally would enable us to predict the laws of the compound. We have always to study the compound directly, independently of its known or supposed elemental composi-

tion. Since we cannot prove the faith, Mill takes us to what we can prove: observational fact. If we dare not reason from the simple to the complex, we must go directly to both in experience and experiment. On this point Mill was quite explicit, for he added two cautions to the foregoing discussion of mental chemistry. The first is that we must go to experience to discover how any particular complex idea has been generated; it is not enough to reason that it may have been generated in such and such fashion. The second is that, even when we know the generative process, we cannot deduce the laws of the resultant: those laws must be found in every case from direct experiment.

Thus Mill, though not a scientist, thought clearly in the logic of science: and thus he rescued the doctrine of association and mental combination from the rationalism of his philosophical father and turned it over to experimentalism.

There remains for our consideration only John Stuart Mill's theory of *perception,* or perhaps we should say his psychological theory of matter. After all, the problem of perception as distinguished from sensation is originally the problem of the belief in objects or matter as lying behind experience. It was only because associationism sought to solve this problem by way of association that introspectional psychologists ever came to regard perception as a complex or an integration.

This problem takes us from the *Logic* to Mill's later work, the *Examination of Sir William Hamilton's Philosophy*. Here Mill undertook to face "the question of the reality of matter" by the formulation of a "psychological theory of the belief in an external world." He posited at the outset the notion that the mind is capable of expectation and that therefore, "after having had actual sensations, we are capable of forming the conception of Possible sensations," which we might feel though we are not at the moment feeling them. Now the thing that seems most definitely to distinguish matter from sensation, Mill thought, is the fact that sensations are fugitive and transitory, whereas matter is fixed and permanent. How, in view of this great difference, can Berkeley have been right in making matter sensory? Mill held that he was essentially right; that the sensations are not permanent like matter, but that the possibilities of sensation can be permanent; and that, when we perceive matter, we are actually conceiving of permanent possibilities of sensations which are not present. Mat-

ter is nothing other than these "Permanent Possibilities of Sensations"; nor its perception anything other than a belief in them.

"I see a piece of white paper on the table. I go into another room. If the phænomenon always followed me, or if, when it did not follow me, I believed it to disappear *è rerum natura,* I should not believe it to be an external object. I should consider it as a phantom—a mere affection of my senses: I should not believe that there had been any Body there. But, though I have ceased to see it, I am persuaded that the paper is still there. I no longer have the sensations which it gave me; but I believe that when I again place myself in the circumstances in which I had those sensations, that is, when I go again into the room, I shall again have them; and further that there has been no intervening moment at which this would not have been the case. Owing to this property of my mind, my conception of the world at any given instant consists, in only a small proportion, of present sensations. Of these I may at the time have none at all, and they are in any case a most insignificant portion of the whole which I apprehend. The conception I form of the world existing at any moment, comprises, along with the sensations I am feeling, a countless variety of possibilities of sensation: namely, the whole of those which past observation tells me that I could, under any supposable circumstances, experience at this moment, together with an indefinite and illimitable multitude of others which I do not know that I could, yet it is possible that I might, experience in circumstances not known to me. These various possibilities are the important thing to me in the world. My present sensations are generally of little importance, and moreover are fugitive: the possibilities, on the contrary, are permanent, which is the character that mainly distinguishes our idea of Substance or Matter from our notion of sensation."

While John Stuart Mill is not here following his own caution, given twenty-two years earlier, that we must stick to experience and experiment, his view is nevertheless historically important for two reasons.

In the first place, by linking it with the doctrine of association, he gave a psychological theory of the object that is essentially like Berkeley's or James Mill's. Association guarantees certain possibilities and joins them together in groups. The object comes into experiential being because, given one sensation, all the others, forming the group which constitute the object, are, under the associative law of inseparability, permanent possibilities of sensation. It is not quite so nearly the context theory of meaning as those we found in Berkeley and James Mill; but it is very near this theory.

It is not new, but it came in 1865 from the pen of a very influential man, when experimental psychology could already be said to have been born.

In the second place, we must note that this theory of the object as the permanent possibilities of sensation anticipates a modern view of perception. The crux of Mill's theory is that one may be conscious of the possibility of a sensation without being conscious of the sensation (or its idea). One may 'have' in mind the possibility without the sensation. In just the same way it has been argued that one may 'have' in consciousness a meaning without its representative content. The Würzburg school called such meanings 'imageless thoughts.' Ach (1905) of this school developed the concept of the 'determining tendency'—a tendency, because it is not necessarily realized in conscious content. Titchener in the context theory (1909) showed that the conscious content which is the context may drop off in a familiar perception and the meaning be 'carried by the determining tendency' for that contextual content. When one asks what such unconscious contexts actually are, one can only answer that they are potential contextual contents. The skilled musician playing in the key of *F*, 'knows unconsciously' that *B* is to be flat and not natural when he perceives a symbol that in itself might have either meaning. He has the meaning, but not the content. In such a case one can define the actual meaning in behavioral terms or as potential content. A potential content is, however, nothing else than a possibility of sensation or image. A similar relationship, so the author believes, can be demonstrated between Mill's notion of possibility, Ach's determining tendency, and McDougall's conception of purpose. This is not, however, the place to go into all these matters; we must content ourselves with this bare indication of Mill's modernity.

Alexander Bain

Bain (1818-1903) comes nearer to being a psychologist through and through than any person we have yet studied. Let us call the roll. Descartes, philosopher and physiologist; Leibnitz and Locke, philosophers and men of political affairs; Berkeley, philosopher, bishop, and educationalist; Hume, philosopher, historian, and politician; Hartley, learned physician; James Mill, historian and diplomatist; John Stuart Mill, philosopher, logician, and political

economist; Charles Bell, Flourens, Johannes Müller, E. H. Weber, physiologists all. There was as yet no formal place in the world for a psychologist, nor did Bain find such a place. The only post that he occupied for any great length of time was a chair of logic at Aberdeen, but he held this appointment *faute de mieux.*

Bain was a poor boy. His father was an Aberdeen weaver with five children and an income that he maintained only by working longer hours as the pay for piece-work persistently decreased. After a scanty schooling, Bain learned to work at the loom, earning money to pay for his board at home and to increase his education in an irregular fashion. He was precocious, but, having no easy access to books, was unable to utilize his intellectual ability to the full. Scientific and mathematical books, when he could borrow them, he soon assimilated. He was once allowed to examine the first volume of Newton's *Principia* for a half-hour, but it was several years before he succeeded in effecting its loan from a less cautious owner. By the time he was seventeen, he had mastered, largely by his own initiative, geometry, algebra, analytical and spherical trigonometry, and fluxions (the Newtonian equivalent of the Leibnitzian calculus); he had studied astronomy and had read considerable natural philosophy; he had become interested in metaphysics and had read Hume's *Treatise;* and he had begun Latin by way of a copy of the *Principia* and an English translation. No wonder that a minister of the church, on becoming acquainted with him, at once urged him to go to Marischal College, then separate from the university at Aberdeen.

At college this self-made student did excellently with poor instruction and was graduated after four years when he was twenty-two, dividing highest honors with another student. His interest and knowledge in natural philosophy had been increased, but the course in moral philosophy, the principal work of the last year, attracted his interest most, although pedagogically and inspirationally this course was about as poorly given as a college course could well be. When its professor, a year later, became an invalid, Bain was employed as a substitute to read the professor's notes and thus conduct the class. He added at times a very little of his own interpretation, only as much as he dared, and even thus he came under suspicion, a suspicion founded originally on the fact that he had never become a communicant of the church.

For the twenty years after his graduation, from 1840 to 1860, Bain remained an involuntary free-lance in London and Scotland. He applied several times for Scotch university chairs, but, though he had strong support, he was always, except once, finally defeated by academic politics and his taint of liberalism. Once he was appointed professor of mathematics and natural science at Glasgow, but the chair was ill-paid, the work was arduous and unsuited to Bain's increasing interest in psychology, and Bain resigned after a year. In London he had formed a friendship with John Stuart Mill, twelve years his senior, and with the intellectual circle in which Mill moved. He wrote for magazines, mostly on subjects that we should now call psychology and the philosophy of science. One gets some notion of the completeness of his psychological interest when one learns that in his article on toys in the *Westminster Review* of 1842 he developed his associative law of similarity. He helped Mill in the final revision of the *Logic* in that same year, and published a laudatory review of it immediately after its publication in 1843.

Meanwhile he was preparing for his great effort, a large systematic psychology. He began actual composition about 1851, and the work, for convenience of publication, was finally divided into two volumes. *The Senses and the Intellect* appeared in 1855, and *The Emotions and the Will,* after a delay by the publisher because the first volume had not sold well, in 1859. Bain was then forty-one years old. The two volumes are really one work and represent Bain's most important contributions to thought. They were successful, and Bain spent much time during the later half of his life in revising them. Revised editions of the first volume appeared in 1864, 1868, and 1894; of the second, in 1865, 1875, and 1899; that is to say, Bain remained the standard British psychological text for almost a half-century, until Stout replaced it.

In 1860 Marischal College was combined with the University of Aberdeen, which had previously consisted only of King's College, and a new chair of logic (and English) was created. For this chair Bain applied, and, after opposition to which he was now accustomed, he received the appointment. He held the post for twenty years, finally resigning on account of his health. His local success is indicated by the fact that he was then thrice (although still against opposition) elected rector of the university.

In neither English nor logic was Bain especially proficient, and

he set about remedying the defect by writing text-books. From 1863 to 1874 he published three manuals of grammar and rhetoric. In 1870 he published his *Logic,* based in part upon John Stuart Mill's, but also containing much original exposition.

Although for a decade Bain was thus diverted from his primary interest by the exigencies of his appointment, the diversion was only partial. Before 1870 he twice revised *The Senses and the Intellect* and once *The Emotions and the Will,* and in 1868 he published for instructional purposes an abridgment of these two large volumes, as *Mental and Moral Science.*

Moreover, in the next year there appeared the edition of James Mill's *Analysis* to which Bain and others added copious notes.

In 1872 Bain published his *Mind and Body.* Back in the '40's he had written on the conservation of energy and the mechanical equivalent of heat. Now he was seeking to resolve the inconsistency between the facts of natural and those of moral philosophy, the question: Is the open system of the will compatible with the closed system of energy? Bain's answer, presupposed in his psychological books, was what we now call psychophysical parallelism, although the phrase is not Bain's. This book is important in view of the extensive discussion of the problem of mind and body that flourished in the '80's and even more in the '90's.

It was in 1876 that Bain founded *Mind,* the first psychological journal in any country, although because both of its country and of its date of founding it has had a more philosophical cast than its younger cousins. Bain made his brilliant pupil, Croom Robertson, then professor of the philosophy of mind and logic at University College, London, the editor, and supported the journal financially until Robertson's early death in 1892.

Bain's last years were spent in retirement at Aberdeen. In spite of ill health he lived to be eighty-five, dying in 1903, which was also the year of the death of his contemporary, Herbert Spencer. His importance is partly due to his longevity. He had written his psychology before Fechner, Helmholtz, or Wundt, and he lived through, actively revising his works, the entire period of the formation and establishment of the new psychology.

Actually he was not a great man, in the sense that Descartes or Locke or Hume or Johannes Müller or Helmholtz or even Wundt was great. There was never any school or great theory that derived from him. He represented the culmination of association-

ism and the beginning of its absorption into physiological psychology. He is important mostly as a cardinal point in the historical orientation of psychology. His careful, scholarly work is a worthy monument to mark the turning point of psychology from empirical associationism to physiological experimentalism.

We cannot, of course, undertake to abstract these systematic psychological treatises of Bain's. We must content ourselves with noting his position on four important matters: psychophysical parallelism, physiological psychology, the doctrine of association, and the doctrine of the will.

1. Bain has been said to have originated the theory of psychophysical parallelism, but such a statement is not true. Bain gave the view concrete form in a distinctly psychological setting. Parallelism in its most general form goes back, as we have seen, to Leibnitz, who conceived of harmony in a pluralistic universe as preëstablished in the parallel courses of the monads. Parallelism of mind and body was for him simply a special case. His contemporary, Malebranche, also held a view of psychophysical parallelism; so did Hartley. The reason that we do not ordinarily cite Hartley in this connection is that Hartley was not interested in developing the view for its own sake. As a physician, he accepted materialism as applying to the body; to save the soul from materialism, he was forced into a dualism, and parallel accounts of mind and body were the easiest way out. For him Cartesian interaction would have been suspected of materializing the soul. So too Fechner, in his crusade against materialism, appealed to parallelism in 1851. Bain did not at first make a direct issue of the matter. His psychology of 1855-1859 simply took this point of view for granted. There are, he thought, two sides to every psychological question, the "physical side" and the "mental side"; and he frequently discussed a particular topic successively under these two heads. Later, as the psychological problems raised by the doctrine of the conservation of energy became more acute, he developed the view in *Mind and Body* of the "physical side" as a closed causal system with cause and effect quantitatively equivalent in terms of energy, and the "mental side" paralleling it without quantitative equivalence. Bain himself regarded the view as hardly more than a metaphysical makeshift. He was not clear himself as to whether he was dealing with two aspects of one

substance or two different substances. Of his facing of this problem he wrote later:

"It was necessary to classify the various alternative suppositions as to One or Two Substances, and to maintain the essential phenomenal distinctness of the psychical and the physical, while upholding the indissoluble union of the two. The expounders of the doctrine of the Trinity had formulated the mode of expressing the mystical union that we find in the Athanasian creed, as 'not confounding the persons nor dividing the substance,'—a not inapt rendering of the union of mind and body."

2. It is a necessary consequence of this parallelistic view that Bain should have written a physiological psychology, and in realizing this intention Bain adopted the form that later, after Wundt had put his seal upon it, was to become the standard pattern for textbooks of psychology. Physiological psychology, we now know, was not at all new with Bain. Descartes was a physiological psychologist, but we may follow Descartes's own practice and dismiss him as an "ancient." Hartley wrote a physiological psychology, but his physiology was the speculative physiology of Newton and not the result of scientific discovery. In the nineteenth century, however, scientific physiology developed rapidly and physiological psychology within it—a matter which we have already considered in detail in earlier chapters. It was pretty obvious whither psychology was tending. In 1852 Lotze wrote his *Medicinische Psychologie* with the subtitle *Physiologie der Seele*. We shall see in the next chapter that there is surprisingly little physiology and surprisingly much metaphysics in this soul physiology; nevertheless, the occurrence of the title at this date is significant. It is almost certain that Bain did not know this book of Lotze's; he never spoke German, and it is doubtful if he read it. It is also probable that he did not know Johannes Müller's psychological physiology. He rarely mentioned German sources. He had had, however, a postgraduate course in anatomy; he knew Dr. W. B. Carpenter personally and used his *Human Physiology*, and later Longet's *Physiology*, in working up his material on the senses. (This was long before Carpenter had published his *Mental Physiology*.) What Bain did was to devote nearly all of his introduction to a long chapter on the nervous system, then to plunge into movement, sense, and instinct, whence he passed to the higher functions. As we have noted, as far as possible he would discuss

both the physical and the mental aspects of every topic. Much of the introduction on the nervous system was not coördinated with the later psychology, but in this matter his books differ little from many recent texts.

The physiological approach led Bain to emphasize the senses. His discussion was full, and his classification conventional. To Aristotle's five senses he added only the rubric "organic." This last class, however, he recognized as very important, especially because it includes the muscular sensations that are involved in his theory of action.

Movement also, as a physiological datum, comes into Bain's psychology in its own right—not merely as a muscular sensation. As we have seen, the knowledge of reflex action as unconscious movement had greatly advanced within the preceding century. Thus Bain, like Hartley but with more reason, could consider *movement* as a psychological term, one that could properly become involved in associations.

3. Bain's discussion of the intellect was nothing more than his discussion of association. He held to two fundamental laws: contiguity and similarity.

As we should now expect, Bain held the law of contiguity to be a matter of the recurrence of previous concurrences of actions or sensations: they "tend to grow together, or cohere, in such a way that, when one of them is afterwards presented to the mind, the others are apt to be brought up in idea." Thus this law seemed to Bain primarily to explain retentiveness. It depends, he noted, upon the *repetition* of contiguities and also upon *attention*. It depends further upon the general retentiveness of the individual, for there are *individual differences* in the "aptitude for acquirement." To all this discussion there is quite a modern ring.

The law of *similarity* Bain, like John Stuart Mill, brought back into psychology after its banishment by James Mill. Bain's particular systematic purpose in this matter was to provide a psychological account of "constructive association," a phrase which he used for invention and mental creation in general. If contiguity explains only retention, i.e., the recurrence of past occurrences, how can there be constructive imagination as well as memory? Bain thought that a principle of "agreement" needed to be added in order to account for what is novel in thought. Most of Bain's illustrations of the associative operation of similarity are suscepti-

ble of reduction to contiguous partial identities, but there is no need for us to criticize Bain on this point, for his second law was never taken over by the new psychology as his first was.

It is important to mention that Bain discussed "compound association" at length. He thought of a realized association as a result of all the associative factors operating. "Associations that are individually too weak to operate the revival of a past idea, may succeed by acting together." This doctrine he developed through the various higher degrees of complication.

4. An intellectualistic psychology like associationism had not tended to raise the problem of the *will*. For several reasons, however, Bain had to face the problem. The growth of science had made materialism a general issue. The gradual establishment of the doctrine of the conservation of energy seemed to turn the physical world into a closed causal system. The doctrine of association was operating similarly in the mental world. One doctrine was materialistic and the other mechanistic; both tended to supersede the will, and in both Bain was personally interested. No wonder he could not avoid the problem.

Bain pointed out, in the first place, that the nervous system is capable of *spontaneous action*. Action is spontaneous, he thought, when it occurs independently of any external stimulation as in *reflex* and *instinctive* action. All action, however, involves actual movement and thus gives rise to muscular sensations or sensations of innervation (a theory which Bain later came to hold, though it is now discarded). Thus in movement, or even in intended movement, there is a definite sensory experience, which is the experience of effort and the experiential aspect of the will. Entering into association, the experience of effort may become an essential accompaniment of any part of the actional situation, even though there is no movement or intended movement; and thus the will may ultimately seem to pervade the mental life.

In this fashion Bain explained away that argument for freedom which is based upon the experience of volition. His use of the phrase *spontaneous action* seems to imply that freedom is somehow nevertheless left in, but actually it is not, nor could it be in such a system. Spontaneous action is free only of association; it is determined by the constitution of the nervous system. Probably Bain, who had met with such bitter defeats at the hands of religious orthodoxy, was willing to preserve a little ambiguity in the

word *spontaneous*, a word which Darwin used between idea and p[illegible] understand in this connection.

In brief, then, we see that Bain anticipated much of later psychology, just as he represented the culmination of the old. In regard to all of the four points which we have considered, he stands exactly at a corner in the development of psychology, with philosophical psychology stretching out behind, and experimental physiological psychology lying ahead in a new direction. The psychologist of the twentieth century can read much of Bain with hearty approval; perhaps John Locke could have done the same.

Evolutionary Associationism

After Bain, associationism may be said to have been carried further in the direct line of Spencer and Lewes, but this stage of the development we can dismiss briefly for the reason that the events occurred after the 'founding' of physiological psychology and thus could not form a preparation for it nor actually affect it greatly. There are some indirect effects of Spencer's psychology, it is true, and these we shall have an opportunity to note presently.

Herbert Spencer (1820-1903) wrote the first edition of his *Principles of Psychology* in 1855. He had thus published a complete psychology when Bain had published only the first volume of his. Nevertheless, Spencer in psychology comes after Bain. This first psychology of Spencer's never exercised great influence; it was simply another associational psychology, although by a very great man. Spencer's real influence upon psychology dates from the two volumes of the second edition published in 1870 and 1872. Here we find what has been called *evolutionary* associationism. It is true that Spencer claimed to have anticipated the evolutionary view in the first edition, and it is also true that he did more specifically anticipate Darwin (1859) by a year or so; but the completed psychological view must be studied in this second edition.

From the ideas of Locke to the sensations and ideas of Bain has been a long course of development. For Locke an idea might be *an elephant* or *drunkenness*, but for Bain sensations were no longer objects or meanings and the ideas were counterparts of sensations. Conscious data of the latter sort Spencer called *feelings*, and he distinguished both between centrally initiated feelings

peripherally initiated feelings (the organic and the external sensations), and between primary feelings (sensations and emotions proper) and secondary feelings (fainter ideational revivals of the primary). He felt, however, the inadequacy of the concept of analyzed experience, as history had developed it, and added a new class of elements, *relations between feelings.* There may be, he thought, relations of coexistence, of sequence, and of difference, occurring as data. In this way Spencer regained a little of what had been lost to the mind since Locke.

There are, however, two points of particular interest about these relations. The first is that, taking consciousness, Spencer reduced the task of association. Some things that would have been associations for his predecessors became simple elementary relations for Spencer. In this way the account of the mind was simplified and seemed a more precise picture of the relatively simple mind, which James Mill, for instance, had burdened with an impossible number of associated simple ideas. The other point is that a relation between feelings, given as a conscious element, is little different from a feeling of relation. We thus see in Spencer the origin of James's feelings of relation and of many other later attempts to add an imageless element to the inventory of consciousness.

Spencer's belief about *association* was that it held together both feelings and relations. The fundamental principle operating in it is, he thought, *similarity,* for association is between terms of the same kind. On the other hand, since associations are built up in experience, it is plain that Spencer could not abandon contiguity entirely. That he did not is plain when we see that he noted both vividness and repetition as conditions of association, and even went further in anticipating one of Ebbinghaus's laws when he observed that successive repetitions exhibit a diminishing effect.

Association in Spencer's hands was used further to accomplish what compounding was not already given in the relations at the start. He did not seek literally in this way to generate objects psychologically, because he thought that objects as such do not belong to the psychological world; but he undertook the corresponding task of distinguishing between subjective and objective mental states. For this differentiation he gave a list of differences that bears considerable resemblance to some more recent intro-

spectionists' accounts of the difference between idea and perception.

The really important novelty in Spencer's psychology is, however, his *evolutionary* doctrine, which amounts to his making the associative law of frequency operate phylogenetically. Association, when often repeated, entails an hereditary tendency which in successive generations becomes cumulative: such was Spencer's view of the inheritance of acquired associations and the formation of instincts. Racially, the doctrine runs, instincts are formed in this way out of reflex actions, which are at the bottom of psychic life. Volitions are formed in another way. Cognition and memory evolve from instinct. There is an evolutionary hierarchy, with simpler states giving rise to more complex.

Of the indirect influences of this evolutionary associationism upon subsequent psychology four should be mentioned. One we have already noted, viz., that Spencer, feeling the limitations of the ultimate elementarism of associationism, sought to extend the list. Associationism leads naturally to sensationism and the effort to transcend it has gone on ever since both within and without introspectional psychology.

Another influence was directed toward animal psychology. The crucial point in this development is the publication of Darwin's *Origin of Species* in 1859, but both Darwin and Spencer immediately extended the principle to include mind—Spencer in the way we have just seen, and Darwin primarily in his *Expression of the Emotions in Man and Animals* (1872). In the past it had been easy to deny animals souls and therefore minds (*cf.* Descartes); but the doctrine of evolution changed all this, and an animal psychology became necessary. It began by way of the anecdotal method, and presently became experimental (Thorndike, 1898, unless one presses certain earlier experiments on behavior).

A much more subtle influence, which the author is not yet ready to trace in detail, lies in the relation of evolution to American psychology. Both associationism and the doctrine of evolution affected James's thought markedly. In the same way, they seem to have been knit up with an American point of view, both in the so-called 'functional psychology' of the University of Chicago (Dewey, Angell) and the unnamed 'functional psychology' of Columbia University (Cattell, Thorndike). And then there was Ladd, a

'functionalist' at Yale. To this entire matter we return in a later chapter. Let it suffice here to say that, while German psychology was physiological, American psychology tended to be biological, as those two adjectives are distinguished by reference to evolution.

Finally, we must note that evolutionary psychology played into the hands of nativism and against geneticism. It is almost paradoxical that such should have been the case. Locke's empiricism led to associationism, and the genetic view of perception, for example (cf. Wundt), was the natural result. In fact, this view was as often called empiristic as genetic. Nativism, the opponent theory, seemed to go back through Kant to innate ideas in Descartes, the very view that Locke brought empiricism to combat. Spencer's theory was essentially a resolution of the two views, although because of the failure of science generally to accept the doctrine of the inheritance of acquired characters, his synthesis lacks the importance that it would otherwise have. Spencer simply substituted phylogenetic origin for ontogenetic origin in many cases; what is empiristically derived in the race may nevertheless be native in the individual, he might have said.

The other important author of evolutionary psychology at this time was *George Henry Lewes* (1817-1878). His influence, however, has not been nearly so great as that of Herbert Spencer, and we can afford to pass him by. In dealing with Spencer, we have already gone far beyond the middle of the nineteenth century, and our main purpose here has been to show the preparation in philosophical psychology for the new experimental psychology that we habitually think of as beginning about 1860. Now we must return to Herbart and Lotze in the corresponding period in Germany.

Notes

See the general remarks at the beginning of the notes in the preceding chapter.

James Mill

The *Analysis of the Phenomena of the Human Mind*, 1829, was reprinted in 1869 with many added notes by Bain, Findlater, and Grote, and a subsequent general editing and annotation by John Stuart Mill. This is the edition ordinarily available. For excerpts, see B. Rand, *Classical Psychologists*, 1912, 462-482.

For James Mill's life, see A. Bain, *James Mill*, 1882; G. S. Bower, *Hartley and James Mill*, 1881, 8-23; and references in John Stuart Mill, *Autobiography*, 1873, esp. 2-61.

On his psychology, see Th. Ribot, *English Psychology*, Eng. trans., 1874, 44-77; H. C. Warren, *History of the*

Association Psychology, 1921, 81-94; G. S. Brett, *History of Psychology*, III, 1921, 29-35.

The attempt to psychologize philosophy by reference to the lives of its authors must be made only with great caution; nevertheless, to the present writer, there appears to be a consistency between, on the one hand, the hard, inflexible persistence of James Mill, which brought him against great odds from penury to comfort, and, on the other hand, his clear, direct style and his courage in pressing a few simple principles to their ultimate psychological limits.

John Stuart Mill

For the psychology of the younger Mill, see his *System of Logic, Ratiocinative and Inductive*, 1843, esp. bk. vi, chap. 4 on the laws of the mind and mental chemistry; *Examination of Sir William Hamilton's Philosophy*, 1865, esp. chap. 11 on the psychological theory of the belief in an external world; and notes in James Mill, *Analysis of the Phenomena of the Human Mind*, 1869.

Mill's writings were extensive, and there is also a large critical literature upon his work. See Rand in Baldwin's *Dictionary of Philosophy and Psychology*, III, 372-376. The best biographical account is obtained by reading J. S. Mill's *Autobiography*, 1873, and then A. Bain, *John Stuart Mill*, 1882, which supplements it. In general brilliance J. S. Mill far surpassed the other men mentioned in this chapter. Because of his remarkable youth, Cox gives him the highest childhood 'intelligence' of any of the geniuses whom she studied (I Q = 190), and even as against the biographical record of young manhood she rates him as high as Berkeley. See C. M. Cox, *Early Mental Traits of Three Hundred Geniuses*, 1926; and previous mention of this matter in the notes of chaps. 1, 9, and 10. See also W. L. Courtney, *Life of John Stuart Mill*, 1889.

In the histories of psychology, see Ribot, *op. cit.*, 78-123; Warren, *op. cit.*, 95-103.

Alexander Bain

The two volumes of Bain's psychology are *The Senses and the Intellect*, 1855, and *The Emotions and the Will*, 1859. The text gives the dates of the three revisions of each. For excerpts on association, see Rand, *Classical Psychologists*, 483-504. The briefer textbook is *Mental and Moral Science; A Compendium of Psychology and Ethics*, 1868. In 1872, the mental and moral sciences were separated into two books, but the American edition of *Mental Science* is dated 1868.

For Bain on parallelism, see his *Mind and Body*, 1872. This book has been reprinted without change under many subsequent dates, and has also been translated into French, German, and Spanish. See also Bain's chapter on *The Correlation of Mental and Nervous Forces*, printed as an appendix to Balfour Stewart, *Conservation of Energy*, 1874. For a brief account of the history of psychophysical parallelism, see under *Parallelismus* in R. Eisler, *Wörterbuch der philosophischen Begriffe*, 1910, II, 975-983. For an indication of the degree of interest excited in this problem, see the bibliography on mind and body by Rand, in Baldwin's *Dictionary*, III, 1091-1099.

For Bain's life, see his *Autobiography*, with a supplementary chapter by W. L. Davidson, who also adds a complete bibliography of Bain's writings. We have referred above to Bain's biographies of James Mill and of John Stuart Mill. These books show their author's biographical interest immediately after his retirement from the Aberdeen chair, and the latter book also bears upon his own life.

In the histories of psychology, see Ribot, *op. cit.*, 194-254; Warren, *op. cit.*, 104-115.

Dr. W. B. Carpenter, mentioned in

the text, was long an acquaintance of Bain's, and was the originator of the phrase *unconscious cerebration* in his *Mental Physiology*, 1881.

Evolutionary Associationism

The important second edition of Herbert Spencer's *Principles of Psychology*, 1870-1872, is really simply the fourth and fifth volumes of the ten-volume *System of Synthetic Philosophy*, 1860-1897. The *Psychology* was several times revised; there was a fifth edition in 1890. For excerpts, see Rand, *Classical Psychologists*, 505-529. See, in general, Ribot, *op. cit.*, 124-193; Warren, *op. cit.*, 118-137. The literature on Herbert Spencer—expository, critical, and biographical—is very large.

George Henry Lewes's psychology is to be found in the five volumes of his *Problems of Life and Mind*, 1873-1879 (divided into three series; the two volumes of the third series were published posthumously). In general, see Ribot, *op. cit.*, 255-314; Warren, *op. cit.*, 137-153.

For a brief account of the growth of evolution in biology, see the last section of chap. 1. On animal psychology, see chaps. 19 and 21; on James and American functional psychology, see chaps. 20 and 21.

Chapter 12

GERMAN PSYCHOLOGY IN THE FIRST HALF OF THE NINETEENTH CENTURY: HERBART AND LOTZE

WE have completed our study of the philosophical preparation for scientific psychology that is British empiricism and associationism. We must now for a similar purpose return to Germany, the country of Leibnitz, whom we have considered in an earlier chapter; and we are thus brought to a consideration of the psychological influence of Herbart and of Lotze. Leibnitz was Locke's contemporary; Herbart was James Mill's; Lotze was Bain's. Was there, however, no German psychology of the eighteenth century, the century of Berkeley, Hume, and Hartley, of the Scottish school, of French associationism?

Of course there was, for this is the century of Kant. Philosophy had turned psychological with both Locke and Leibnitz. Christian Wolff (1679-1754), Berkeley's contemporary, was a disciple of Leibnitz. He systematized and popularized Leibnitz, and thus established the Germany psychology, under which Kant grew up and which he overthrew. Kant (1724-1804), while his philosophy was no less psychological than the philosophy of his immediate predecessors, shifted the psychological ground. Locke had said: "Nihil est in intellectu quod non prius fuerit in sensu." Leibnitz had added: "Nisi intellectus ipse." Now Kant, giving over Leibnitz's preëstablished harmony of monads, sought to determine the nature of this intellect that is prior to experience. Popularly Kant is known for his *a priori* intuitions and categories of the understanding, the innate 'givens' of the mind; but this emphasis throws him into the Cartesian tradition of innate ideas and places him in opposition to the Lockian empirical view. So fundamental is this difference that Helmholtz, more than seventy-five years later, in defending empiricism and attacking nativism for the sake of his theory of perception, began by attacking Kant. It is true that Kant provided some support for nativistic psychological theories,

but in general his effect upon modern psychology has been slight. This statement need not be thought to tell against Kant's greatness: it is more than a play on words to say that science is empirical and that thus empirical psychology influenced experimental psychology most. Kant's *a priori* method was not to be adapted to experiment, whereas empiricism by its very nature called for the controlled observation of the genetic process. Thus we may pass over Kantian philosophy, for the excellent reason that it does not bear with sufficient directness upon our subject-matter.

Kant's immediate successors in Germany were Fichte (1762-1814), Hegel (1770-1831), and Schelling (1775-1854). They were his successors in thought as well as in time, and, important as they are in the history of philosophy, they do not belong in this book. They were contemporaries of one another and also of Herbart (1776-1841), who thus, in returning to empiricism, placed himself in opposition to the tradition of his day. In fact, in German thought, Herbart's psychology derives more directly from Leibnitz than from any of the intervening philosophers. It is also more nearly compatible with the British development than is Kantianism. These reasons are enough to justify our omitting the eighteenth century in Germany and passing at once to Herbart.

Johann Friedrich Herbart

Johann Friedrich Herbart (1776-1841) was a philosopher and is best known as the 'father' of scientific pedagogy, which he founded upon psychology. His psychology is therefore of primary importance when Herbart is considered as an educational theorist, but it also in its own right occupies an important place in the history of psychology. Herbart's two psychological texts are his most important books, and, in spite of his denial of the possibility of psychological experiment, his work had a definite influence upon the later experimental psychology. It is, moreover, interesting to note that the dependence of scientific education upon psychology, a dependence which Herbart was the first to emphasize, has remained a tenet of educational theory until the present day, although opinion as to the exact manner in which psychology can be applied to education has varied greatly at different times. Within the present century, not only has psychology affected education,

but the demand of education upon psychology has had a notable effect upon the latter.

Herbart, owing to an accident in infancy, was a delicate child. He did not go to school until he was twelve, but was educated by his mother, a woman of unusual ability. He was somewhat precocious; his interest in logic is said to have begun during this early period. Altogether, his childhood has some resemblance to John Stuart Mill's, although there is no evidence that his early maturity was as great. At twelve he began attendance upon the *Gymnasium* in Oldenburg, his native city, and he continued there for six years. Kant had made a profound impression upon him at sixteen. At eighteen he went to Jena to study philosophy under Fichte, at a time when Kant at Königsberg was at the full height of his reputation. His own philosophy was, however, in process of formation, and he could not entirely accept Fichte's teaching. After three years he left Jena to become the private tutor of the sons of the governor of Interlaken. It was this chance event that shifted his interest from philosophy to education, and led to his first constructive thinking upon educational problems. Thus, after two years of tutoring and when he was still only twenty-three, he spent three years in Bremen in study and the development of his thought. Before leaving Switzerland he had visited Pestalozzi, the famous Swiss educational reformer. From Bremen he went for seven years (1802-1809) to Göttingen, where he first presented himself for the doctor's degree, and, after receiving it, became a *Dozent* in the university. In coming up for his degree he formally opposed the doctrine of Kant, and in the years following he continued with the maturation of his own philosophy and his theory of education. In both fields he published a considerable amount during these years.

The promise of genius in Herbart is shown by the fact that in 1809, when he was thirty-three, he was called to Kant's chair in Königsberg. Kant had died in 1804. Herbart stayed at Königsberg for twenty-four years, and these years form the important period of his life. He devoted them to the completion of his system of psychology and to the working-out of practical pedagogical problems. The latter do not concern us. In 1816 he published his *Lehrbuch zur Psychologie* and in 1824-1825 his *Psychologie als Wissenschaft.* He also wrote a *Metaphysik* (1828-1829), for psychology and metaphysics were not, in Herbart's view, distinct

subject-matters. Meanwhile his reputation increased, his classroom was crowded, and his fame spread throughout Germany.

In 1833, disturbed by Prussian antagonism to new educational experiments, he was glad to return to Göttingen as professor of philosophy, a chair now famous, for he was succeeded in it by Lotze, who in turn gave place to G. E. Müller. This appointment he held until his death, nine years later, in 1841.

We need now to consider the manner in which Herbart prepared the way for experimental physiological psychology. It may seem strange to say that he anticipated this movement at all, for he expressly denied the applicability of experiment to psychological problems, and he disbelieved in the relevance of physiology. Nevertheless, he had so positive an effect that there have been those who have dated the beginning of modern German psychology from him. What did Herbart think psychology should be?

1. The title of his second work practically gives the answer to this question: *Psychologie als Wissenschaft, neu gegründet auf Erfahrung, Metaphysik und Mathematik.* Psychology is a science, and it is grounded upon experience, metaphysics, and mathematics.

Psychology is *science.* This as an explicit contention is something new. Physiology is presumably a science, and we have seen philosophical psychologists citing physiology, and also physiologists (although mostly after Herbart) writing psychology. But to say that psychology is science and to go on to claim for it the mathematical method of science, as Herbart did, is new. Of course. Herbart was not saying that, as science, psychology is not also philosophy. This distinction comes later, when psychologies like Mach's could begin with 'antimetaphysical' chapters. Herbart founded psychology in part upon metaphysics. The present point is that, when psychology later was in whole or in part repudiating philosophy and thus arguing for its status as a science, there was Herbart to fall back upon, for the idea was not entirely new. As we shall see, this is not the only way in which Herbart sowed seed that brought fruit other than he had intended.

Psychology is *empirical* science, for it is grounded upon experience. This is not to say that it is experimental, for experiment is a method. Observation, not experiment, is the method of psychology, which thus grows out of experience. It is obviously an attribute of science to be empirical; science could hardly fail to be founded

upon experience. This definite appeal of Herbart's to observation had, however, the effect of separating him explicity from the *a priori* psychology of Kant and of uniting him potentially with the empiristic psychology of England, the school which provided the most immediate philosophical ground for the new psychology.

Psychology is *metaphysical* science. This is, of course, an Herbartian tenet that was not handed on to the new psychology. Its truth or falsity does not concern us. It was natural enough for Herbart to take this view, for he was a philosopher writing in the age when all philosophy had acquired a psychological cast. It seems, rather, that he believed that the metaphysical nature of psychology is one of the things that differentiate psychology from physical science. Psychology is metaphysical; physics is experimental. There is no doubt that it was this view, thoroughly exemplified in Herbart's psychological writings, that was largely responsible for the repudiation of Herbart by later scientific psychologists, like Wundt, who really owed much to him. For the same reason, the new psychology in Germany is said to have begun with Wundt and not with Herbart—nor with Lotze, who was equally metaphysical in his method.

Psychology is *mathematical* science. This is the primary distinction between psychology and physics. Physics uses both of the scientific methods: calculation and experiment. Psychology can use only the former. We shall see later just what was the nature of Herbart's metaphysical calculation, divorced from experiment. This use of mathematics Herbart fully exhibited in the *Psychologie als Wissenschaft,* and that fact means that Fechner's mental measurement was not a new idea in so far as it was measurement. Fechner's originality lies in his combination of Herbart's use of mathematics with Weber's use of experiment. There is, moreover, much else in Fechner that is of Herbartian origin.

Is psychology analytical? Here we must pause. Herbart declared that it is not. The mind is unitary and cannot be divided into parts. This is the familiar objection to analysis that we have encountered from Descartes to the present day. Herbart firmly took his stand against analysis, and thus seemed to separate himself widely from British empiricists. We may ask, however, whether science is not essentially analytical. Herbart answered this question in the negative. The experimental method is necessarily analytical, but science is not; and psychology is an unex-

perimental science. In fact, it is because of the unity of the soul that psychology cannot be experimental. Nevertheless, it is not clear, in spite of formal statement to the contrary, that Herbart avoided the analytical method. Even unexperimental mathematical science would seem to be analytical.

At any rate, Herbart's psychology is *mechanical,* with a statics and a dynamics of the soul. Ideas (in Locke's sense; *Vorstellungen*) interact and vary in strength as the result of the interaction. One idea, *a,* may arrest another idea, *b;* and the law of their relation is expressed in an equation where the terms are *a* and *b*. As quantities, *a* and *b* represent the strengths of the ideas. There is nothing of elementarism in saying that the ideas have degree or magnitude, but there is all of elementarism in separating one idea from another so that the two can interact. Kant had said that psychology could be neither experimental nor mathematical, because either method required the existence of two independent variables, and ideas could vary only in time. Herbart seems to have followed Kant in respect of experiment, but in the interests of the use of mathematics he pointed out that ideas had two variables, time and intensity. Actually, as Herbart's statics show, ideas have a third dimension, quality, which individualizes each idea and makes *a* different from *b*. Herbart thus of necessity met the analytical conditions for mathematical treatment by analyzing mind into separate ideas which vary independently in intensity. This analysis gave him the statics of the soul. The addition of time as a variable gave him the dynamics.

It is not just to blame Herbart for this confusion. In denying analysis, he was fighting the division of the mind into separate faculties. He was rejecting a kind of analysis to which psychology has never returned. Nevertheless, Herbart worked with ideational elements, elements that were so much more rigidly defined and so much less fluid than the elements of the associationists that they could even be fitted into mathematical formulæ. Titchener has remarked that mental analysis is one of the things that Fechner got from Herbart, and he is right in spite of Herbart's repudiation of elementarism.

We have seen what Herbart said psychology is. We have now to ask what, for him, it is *not*. The notion that it is not analytical we have refused to accept. What else is it not?

Psychology is not *experimental*. This contention of Kant's Her-

bart accepted. There is simply no obvious way, he thought, of experimenting upon the mind; and he thus expressed the view of the modern layman who is puzzled as to what it can be that the experimental psychologist does. We know now that both Kant and Herbart were wrong, that there is a sufficient number of independent mental variables to render experimentation possible. To have asked Herbart, however, to see this fact and act upon it would have been simply to ask him to do what Fechner and Wundt did later. We must not be surprised that Herbart missed a point of logic and failed fully to anticipate the future. It is wonder enough that he transcended his times as much as he did.

Psychology is not *descriptive.* Its business is not a mere description of the mind, but the working-out of its mathematical laws. In this point Herbart was reflecting the spirit of science, and there are many to-day who feel that a mere description of consciousness, without the formulation of laws and the usual accompaniment of quantification, is futile.

Psychology is *not physiological,* or at least not primarily so. Herbart was not interested in physiology, and he did not see that it could afford an approach to the problem of the mind. Since he was also not interested in experiment, he was not drawn into a consideration of the physiological technique for controlling the variables with which he dealt. It is no coincidence that physiological and experimental psychology began together; the one method requires the other.

Herbart did, however, recognize the relation between mind and body, and he laid down these three principles of connection. Bodily conditions may hinder the arousal of an idea (e.g., in sleep): this is repression (*Druck*). They may facilitate the arousal of an idea (e.g., in intoxication or passion): this is reinforcement (*Resonanz*). When the feelings or, through practice, the ideas cause movement (e.g., in emotion or simple action), there is cooperation between soul and body. But all this discussion constituted for Herbart simply a special chapter of a psychology which is not physiological in fundamental nature or method.

2. Let us now consider the nature of Herbart's systematic unit, the idea or *Vorstellung.* (We may use the word *idea* in Locke's sense, remembering that the German word includes both perceptions and ideas as the modern English usage goes.) Here we shall

discover why it is said that Herbart is the next step beyond Leibnitz.

We have already seen that these ideas are, according to Herbart, distinguishable from one another in respect of quality, and that every idea is invariable in quality. There is no shading-off of one idea into another; the differences are discrete. Every idea may, however, vary in intensity or force (*Kraft*), an attribute which is equivalent to clearness.

This force, which expresses itself in the clearness of the ideas, may be understood as a tendency of the ideas toward *self-preservation.* Each idea makes an effort to conserve itself as it enters into relation with others: the ideas are *active,* especially when there is opposition among them. Herbart thought of this tendency as the fundamental principle of mental mechanics, much as gravitation is the fundamental principle of physical mechanics. "Every movement of the ideas is confined between two fixed points: their state of complete inhibition, their state of complete liberty"; and there is a "natural and constant effort of all the ideas to revert to their state of complete liberty (absence of inhibition)."

The basis of the opposition lies in the quality of the ideas; *a* and *b* may be opposed to each other, *a* and *c* may not. The effect of opposition is mutual upon their intensities; if *a* and *b* are simultaneous and opposed, each diminishes the other. The "metaphysical reason why opposed ideas resist one another is the unity of the soul of which they are the self-preservations." "If they did not on account of their opposition inhibit one another, all ideas would compose but one act of the soul; and in so far as they are not divided into many by any kind of inhibitions whatever, they really constitute but one act." In other words, those ideas that can together constitute a single mental act (e.g., a color and a tone) do not resist each other; however, in general, when one considers the multiplicity of ideas, inhibitions resulting from mutual opposition are seen to be the rule of consciousness. Actually, Herbart was giving a mechanical explanation of the fundamental fact of the limited range of consciousness.

The next point—and it is of primary importance—is that Herbart believed that no idea was completely destroyed by inhibition. Under opposition, it merely "yields" as much as is necessary, loses in intensity or clearness, and passes from a state of reality to a state of tendency. Thus the suppressed ideas exist, but only as

tendencies. We must accept this statement as it is made. To say that something, no longer real, exists as a tendency is not necessarily a paradox, though superficially it may appear that what exists is real and that a tendency cannot do the one without being the other. To-day we still have with us the problem of the nature of the 'unconscious.' Literally, the 'unconscious' is unconscious consciousness, but the apparent paradoxical nature of this phrase does not exclude us from facing the many problems of mental tendencies or potentialities.

It was out of this mechanics that Herbart derived his notion of the threshold or limen of *consciousness*. "By the limen of consciousness," he wrote, "I mean those limits that an idea seems to overleap in passing from a state of complete inhibition to a state of real idea." Now we see why the strength of an idea is equivalent to its clearness, for the strong ideas in preserving themselves are above the limen and are therefore conscious; and the inherently weak ideas or those that have been weakened by inhibition may lie below the limen and be unconscious. Strength gives clarity.

It is plain that the composition of consciousness at any moment is the resultant of the mechanical interplay of many ideas. Of all those ideas below the limen, only those that fit in with the unity of consciousness find so little resistance that they can rise above. Thus we have the appearance of the conscious ideas 'selecting' from among those that are unconscious the ones that are consonant with themselves. There is, however, no free selection; everything depends upon the mechanics of resultants. It is in connection with this picture of the mind that the word *apperception* appears in Herbart's exposition. As with Leibnitz, any idea that rises into consciousness is apperceived, but Herbart meant more by the word, for no idea rises except to take its place in the unitary whole of the ideas already conscious. The apperceiving of an idea is therefore not only the making of it conscious, but also its assimilation to a totality of conscious ideas, which Herbart called the "apperceiving mass." Herbart is perhaps more famous for his doctrine of apperception than for anything else. The phrase *apperceptive mass* appears in common speech to-day. Yet Herbart made less of apperception in psychology than might be supposed. This view of his has become important because it is a psychological picture of the process of education and Herbart is famous, as he would have wished to be, as an educational theorist.

The details of this portion of Herbart's psychology derive, of course, directly from Leibnitz. *Apperception* was Leibnitz's word. The notion of activity in ideas was Leibnitz's, although the picture of their mechanical interaction is the exact antithesis of his doctrine of preëstablished harmony between completely independent monads. Leibnitz's *petites perceptions* have become in Herbart inhibited ideas. Both men use the same conceptions of degrees of perception and of the striving of ideas for self-realization. Herbart's limen of consciousness is only a step beyond Leibnitz. There are thus plenty of grounds for saying that Leibnitz, and not Kant, was Herbart's tutor.

We may also at this point look ahead. Leibnitz foreshadowed the entire doctrine of the unconscious, but Herbart actually began it. Wundt was to appeal first to unconscious inference in order to explain perception, and then to apperception. Fechner was to take from Herbart the notion of the measurement of the magnitude of conscious data, the notion of analysis (*vide supra*), and, most important of all, the notion of the limen. Moreover, this Herbartian concept of the limen was to lead Fechner to the degrees of intensity below the limen, his "negative sensations." The conception of active ideas striving for realization was to affect act psychology slightly and abnormal psychology greatly. Freud's early description of the unconscious might almost have come directly from Herbart, although it did not. There was still a use for some of Herbart's psychology fifty and even 100 years afterward.

3. It is quite true that Herbart's picture of apperception and the constitution of consciousness in general makes it appear that the principal mental thing that is going on all the while is inhibition. The activity of combination is mostly negative; the apperceiving mass of ideas selects new constituents merely by suppressing all but a few, which come up into consciousness of their own force. Nevertheless Herbart had something to say about the union of ideas when there is little or no conflict. There are three cases.

When there is no opposition between the ideas and they belong to the same "continuities" (modalities), they unite and we have *fusion*. The example is red and blue uniting to give violet. When there is no opposition and the ideas belong to different continuities, as with a sound and a color, they may still form a unity, and such a unity Herbart called a *complication*. It is this use of the words

fusion and *complication* that has been taken over by Wundt and those who have followed his lead. Thus the problem of the personal equation, which we studied in an earlier chapter, leads in psychology to the complication experiment, because it involves the union of impressions from the eye and the ear.

Inhibition also leads to laws of union when the inhibition is incomplete. Two ideas of equal strength completely inhibit each other—at least, Herbart thought that he had proved this to be a fact; but two ideas of unequal strength can never inhibit each other, and thus both remain, contributing their resultant to consciousness. (The mathematics of this case we shall examine in a moment.) Three or more unequal ideas may also yield a conscious resultant or may result in the complete inhibition of one, as the case may be. Herbart deals with these cases mathematically, but it will not profit us to follow him.

4. We have spoken repeatedly of Herbart's mathematical method, and yet the reader can scarcely be expected to say how, without experimental quantitative observation, it is possible to apply mathematics to psychology. There is no way to describe Herbart's procedure except by illustration. Let us therefore take the case of two simultaneous ideas of unequal intensity, a case which leads to one of Herbart's fundamental laws of the statics of the soul.

Let there be then, Herbart said, two simultaneous opposing ideas, a and b; and let a be greater than b. Each will have an inhibitory effect upon the other, so that each is diminished in force.

Herbart said further that the decrement in b will bear the same ratio to the total strength of b that a, which causes the decrement in b, bears to the total force of a and b together, i.e., $a + b$. Then Herbart immediately wrote a proportionality, but the argument may be clearer to the reader if we pause to call this decrement d. Thus the foregoing statement becomes:

$$a + b : a : : b : d;$$

but this is an equation, and

$$d = \frac{ab}{a + b}.$$

Hence the proportionality that Herbart wrote without this step:

$$a + b : a : : b : \frac{ab}{a + b}.$$

Since it is hardly likely that the reader will regard the formation of this proportionality as obviously correct, we may translate Herbart into a somewhat simpler statement. If d is the decrement of b, then the ratio, $\frac{d}{b}$ will be the proportional amount or fraction of itself by which b is diminished by a. This effect of a on b is, however, also dependent upon the relation of a to the total force of consciousness, $a + b$, for the larger a is with respect to b, the more effect does it have on b relative to b, i.e., the proportional amount that b is reduced is the proportion that a holds to the total force of consciousness. Thus:

$$\frac{d}{b} = \frac{a}{a+b}, \text{ and } d = \frac{ab}{a+b}.$$

If the reader is not now convinced, he is mistrusting Herbart's metaphysical rationalism, upon which these statements are founded, and not his mathematics, which merely proceeds from them. Let us go on.

After b is diminished by d, the residual strength of b is

$$b - d = b - \frac{ab}{a+b} = \frac{b^2}{a+b}.$$

But $\frac{b^2}{a+b} = 0$, only when $b = 0$ or $a = \infty$. Neither of these conditions can be true, for $b \neq 0$ by the hypothesis that b is a conscious idea; and $a \neq \infty$ because no idea can be of infinite strength. Hence it follows that

$$b - d = \frac{b^2}{a+b} \neq 0.$$

This is to say that, in the case we are discussing, the residual strength of b, after inhibition by a, can never be zero: a cannot completely inhibit b when there are only these two ideas interacting.

If the stronger idea cannot completely inhibit the weaker, one would hardly expect that the weaker could completely inhibit the stronger. We may, however, proceed exactly and apply the same process to show that b diminishes a by an amount $\frac{ab}{a+b}$ to a value $\frac{a^2}{a+b}$. Thus we find, not only that a cannot be completely inhibited by b, but also that in their interaction, action and

reaction are equal, for each is reduced by the other in the same amount, $\frac{ab}{a+b}$.

The final result is the general law: Of two simultaneous ideas of unequal strength, neither can suppress the other below the limen. The range of consciousness, Herbart might have said, is greater than two ideas.

Herbart went on to show that when three ideas interact, one may be completely inhibited; and thence he passed to other derivations in his mental statics, and thence to his mental dynamics. There is no need to follow him further, for we have seen, at an elementary level, the nature of his method.

What is the matter with it? The mathematics is sound; we all believe that consciousness is capable of supporting at least two ideas simultaneously; and yet this law and the others with it have never made a place for themselves in psychology. The trouble must be that the premises, though rationally plausible, are not convincing. The original statement that the relative inhibition of *b* would be in the proportion of *a* to the total strength of *a* and *b* is too simple, or at least too little grounded upon experience, to seem probable. Herbart, if he could not use experiment, at least needed some substitute for it. He exhibited the not uncommon case in science in which inadequate data are treated with elaborate mathematics, the precision of which creates the illusion that the original data are as exact as the method of treatment. It is rarely that the person who works well with mathematics has also the gift of criticism against experimental results or even against his assumed postulates.

5. History has criticized Herbart for us. His belief in a scientific psychology founded upon experience has persisted. His mathematical method proved vital only when united to experiment, and thus to physiology, both of which he rejected. His view of the relation of consciousness to unconsciousness is still of use, although with many modifications. His metaphysical foundation for psychology, however, has not survived. It is not that psychology is not founded upon metaphysics; psychologists have never succeeded in avoiding metaphysical discussion. What the history of psychology has shown is that there is an incompatibility between the empirical and the metaphysical bases of psychology. Herbart's metaphysics led him to substitute *a priori* generaliza-

tions for inductions based upon observation. Such a procedure might have attained some truth had he been willing afterward to appeal to experimental verification, but he was not willing. Herbart's *Psychologie als Wissenschaft* was thus only part of what a scientific psychology required.

Herbart represents, therefore, a transition from the pure speculation of Kant and Fichte and Hegel to the antimetaphysical experimentalism of Fechner and Wundt and Helmholtz. Hence it is natural that Herbart's school, the group of men who are called Herbartians, should contain the name of no experimentalist. Drobisch, the logician at Leipzig, was a prominent Herbartian. It was he who helped to bring Wundt, instead of Horwicz, to Leipzig in 1874, but he was not a psychologist. Waitz, Lazarus, and Steinthal were all Herbartians, but with ethnographic interests. W. F. Volkmann, Ritter von Volkmar, is perhaps the Herbartian who had the most direct influence upon modern psychology, for he wrote in 1856 a *Lehrbuch der Psychologie* which remained the only up-to-date textbook of psychology in German until Wundt published the *Physiologische Psychologie* in 1874. In general, Herbart's effect upon experimental psychology was not through the Herbartians at all. What happened was that his work directly influenced Fechner and Wundt, both in respect of what they borrowed from it, and also in respect of what they positively rejected.

Hermann Lotze

Lotze is less important in the history of psychology than Herbart. Herbart, for all his metaphysics, stands for the reaction in psychology away from the philosophy of Fichte, Hegel and Schelling toward a scientific psychology. Lotze remained more nearly in the direct tradition of these three post-Kantian philosophers. We can see the differences in the genetic course of the interests of the two men. Herbart, although he published his *Metaphysics* only a few years before his death, nevertheless tended to move from philosophy toward psychology. Lotze moved in the opposite direction. Frequently, though not always, it is a man's later work that determines his place in the history of thought—perhaps because his later work has upon it a stamp of maturity.

Primarily Lotze was a metaphysician. He enters into the history of experimental psychology for two reasons. First, there is

the bare fact that he published a "medical psychology, or physiology of the soul" in 1852, thus helping at a crucial moment to set the fashion for physiologizing psychology. The same book twenty years later would have had much less influence. Secondly, there was his theory of local signs, which more or less predetermined the fundamental nature of the problem of space-perception. To both of these points we shall return presently.

Rudolph Hermann Lotze (1817-1881) was the son of an army physician and was born at Bautzen in 1817, the year after the publication of Herbart's *Lehrbuch.* When Lotze was still an infant, however, his father's regiment moved to Zittau, and there Lotze spent his youth. He went first to the *Stadtschule,* and then at the age of eleven to the *Gymnasium,* which he attended for six years before he was ready to enter the university. His father died when he was twelve. The *Gymnasium* at Zittau was an excellent school and had several famous graduates. Not very much seems to be known about this period of Lotze's life; during it his interest in philosophy began and his especial interest lay in poetry.

At the age of seventeen (1834) Lotze went to the university at Leipzig and matriculated under the faculty of medicine. In thus selecting a profession he was following in his father's steps, but by temperament he was more disposed toward the arts and philosophy than toward science and medical practice. The result was a varied education. At Leipzig he began the writing of poems, of which he published a volume in 1840, his first publication after his dissertation. He was also drawn into philosophy, where he came under the inspiration of Christian Weisse, an Hegelian. On the scientific side he was thrown with E. H. Weber, A. W. Volkmann, and Fechner.

Weber, twenty-two years older than Lotze, had been professor of anatomy at Leipzig since 1818. Twelve years later, two years after Lotze had finally left Leipzig, he was to publish *Der Tastsinn und das Gemeingefühl* in Wagner's *Handwörterbuch.* A. W. Volkmann (not the Ritter von Volkmar, the Herbartian) had then just been promoted from *Dozent* to *ausserordentlicher Professor* of zoötomy at Leipzig, a post which he held for three years before he went to Dorpat. He was writing his *Physiologie des Gesichtssinnes* (1836), which Johannes Müller so frequently cited in portions of his *Handbuch,* also then in preparation. In this same year (1834) Fechner, with a reputation based upon his

research on the galvanic battery, had been appointed professor of physics, a post which he held until he resigned on account of ill health five years later. This was before Fechner's philosophical or psychophysical interests had occupied his attention, but nevertheless it was the same Fechner, and he must have given his science the cast that would attract a young, philosophically minded medical student. Weber, Volkmann, and Fechner were all within six years of each other in age, and were all in their thirties, young men for their positions, but twice the age of Lotze, who went to Leipzig at seventeen. He was a silent listener in Fechner's circle and corresponded with him and with Frau Fechner after he had left Leipzig a decade later. When he wrote his *Medicinische Psychologie*, he dedicated it to Volkmann and included in it a pre-Fechnerian discussion of Weber's law.

Lotze remained four years at Leipzig and then took his degree in medicine. He was still half a philosopher: his dissertation was on "the future biology according to philosophical principles." He spent, however, one year in the practice of medicine in Zittau, the home of his childhood, before it became plain to him that he was destined for an academic life. In 1839 he returned to Leipzig and was habilitated as *Dozent* in both the faculty of medicine and the faculty of philosophy, an unusual attainment. The poems of his student days were published in 1840, and he then began a period in which medical and philosophical publication were of about equal frequency. In 1841 his *Metaphysik* appeared, and also a critique of the theory of space of Weisse, his philosophical master. The next year there was his *Allgemeine Pathologie und Therapie als mechanische Naturwissenschaften*, by which he is said to have "lept into fame." There was even a second edition of this book six years later. In 1843 his *Logik* was published. Meanwhile Fechner had resigned, but was still living in Leipzig. Volkmann had gone to Dorpat and thence to Halle.

In 1844, when still only twenty-seven, Lotze accepted the offer of Herbart's chair at Göttingen, where he remained for thirty-seven years, almost until his death, being succeeded by G. E. Müller. For a while after his change to Göttingen, his interests seemed more physiological than philosophical. He wrote three chapters for Wagner's *Handwörterbuch der Physiologie: Leben und Lebenskraft* (1843), *Instinkt* (1844), and *Seele und Seelenleben* (1846). In 1851 he published his *Allgemeine Physiologie*

des körperlichen Lebens, and then in 1852 his famous *Medicinische Psychologie,* followed in the next year by *Physiologische Untersuchungen,* which completed his important psychological writing. After this, Lotze turned almost entirely to philosophy. His most important work was the three volumes that constituted the *Mikrokosmus,* which appeared from 1856 to 1864. His *System der Philosophie* in two volumes was published in 1874 and 1879. In 1881 he was persuaded by Zeller and Helmholtz, both his enthusiastic supporters, to accept the chair at Berlin, but he died of pneumonia three months after the change.

Lotze was a quiet, methodical man, with the sensitiveness of the esthete and the industry of the scientist. He was a meticulous and uninspiring lecturer, and he never had large classes. He was too much of a humanist to be a mechanist, and he spent his life in synthetizing these opposing views of the world. He came to no dogmatic conclusion and he founded no school, but his rare sympathy created for him, especially through his writings, a wide sphere of influence. His lectures, given extempore from carefully prepared notes, led to the posthumous publication of the dictata of his various courses, including psychology, which was the only subject upon which he lectured every year for the entire thirty-seven years at Göttingen. Opposed to pure materialism, he naturally influenced in psychology the antimechanical systems, and thus came less into contact with experimental psychology.

It is not strange that such a man should influence markedly his pupils and others who came within his circle of friends. Thus we must make particular mention, because they are psychologists, of Brentano, Stumpf, and G. E. Müller. Brentano, after being *Dozent* at Würzburg in 1866-1872, was promoted to a professorship. He was a priest and was appointed as a priest. In 1869 he had written the leading memoir in refutation of the dogma of the infallibility of the Pope, and, when this dogma, after violent controversy, was accepted by the Catholic Church, he felt it necessary to resign his priesthood and thus his professorship, after he had held it but a year. This was a situation that would naturally appeal to Lotze, who used his influence successfully to have Brentano appointed as a layman to the chair of philosophy at Vienna. Stumpf was a student of Lotze's in 1867-1868, received his doctorate at Göttingen in 1869, and was *Dozent* there from 1870 to 1873, when he went to Würzburg in Brentano's place.

G. E. Müller, after studying at Berlin and Leipzig, went to Göttingen at about this time, and received his doctorate there in 1873. He returned in 1876, and became *Dozent* for four years, writing *Zur Grundlegung der Psychophysik* (1878). The year 1880-1881 he spent at Czernowitz, but in 1881 he returned to Göttingen to succeed Lotze, who, as we have seen, went to Berlin a few months before his death. Brentano mentions Lotze in the preface of his *Psychologie* (1874); Stumpf dedicated the *Raumvorstellung* (1873) and Müller dedicated the *Psychophysik* (1878) to Lotze.

In this personal way Lotze exerted some influence upon experimental psychology, although it was not great, for it is hard to imagine two more different psychologists than Lotze and Müller, or more different psychologies. Primarily, however, it was Lotze's *Medicinische Psychologie oder Physiologie der Seele* in 1852 that leads to his inclusion in this history.

The title of this book of Lotze's reflects his dual interest in physiology and philosophy at the time of its writing. The content reveals further that Lotze was at heart a metaphysician. Just as his medical dissertation was founded upon "philosophical principles," so was his *Psychologie* a metaphysician's psychology. For this reason, the book as a whole never had great influence in a psychology that repudiated metaphysics. Herbart was overtly and thoroughly metaphysical, but it was not his metaphysical psychology that he contributed to experimental psychology; it was his empiricism, his mathematics, his analysis, his notion of activity, and his concept of the limen that were taken out of his psychology and built into a new structure. Lotze's psychology yielded fewer useful elements of this sort, little more, in fact, than the basal conception of a physiological psychology and the specific view of psychological space. Let us glance at its contents.

The first book of the *Medicinische Psychologie* is entitled "The General Fundamental Concepts of Physiological Psychology" (this phrase, *physiological psychology,* is explicitly used), with chapters on the existence of the soul, the psychophysical mechanism, and the essence and destiny of the soul. Physiology is brought into the second chapter in connection with the problems of mind and body, phrenology, and the seat of the soul, but the entire cast of the book, as these subject-matters would lead one to expect, is not physiological.

The second book has the title, "The Elements and the Physiological Mechanism of the Mental Life." The first chapter on sensation has little that is novel after Johannes Müller and Weber, though it is interesting to find, as we have already noted, a discussion of the proportions between stimulus and sensation eight years before Fechner immortalized Weber's law, although one year after Fechner's first discussion of this phase of the mind-body problem. The second chapter on feeling, and the third on movement and instinct, also contain little that is factually new or theoretically important. Lotze included Weber's *Gemeingefühle* under feeling. The final chapter is on space-perception and includes the famous doctrine of local signs to which we shall return presently.

The third and last book reflects Lotze's interest in therapy and pathology, and thus makes the book more nearly a medical than a physiological psychology. It bears the title, "Development of the Mental Life in Health and Disease"; and the chapters are on states of consciousness, the developmental conditions of the mental life, and the disturbance of the mental life. Of these chapters, the first discusses consciousness and unconsciousness, attention, and the course of ideas; the second, animal minds and instincts, and innate individual capacities (*Anlagen*); and the last, psychopathology.

If Herbart marked the transition from metaphysical to physiological psychology, Lotze marked a further stage of the same course of change. Herbart was full of argument, with few facts and no physiology. Lotze is full of physiological fact and is thus the more scientific of the two. Nevertheless, Herbart influenced factual scientific psychology the more, paradoxical as it may seem, because with a negligible factual basis he contributed methods and conceptions; whereas Lotze, with many facts, failed to add to the theoretical structure. If Lotze's facts had been new—but they were not. On the scientific side, Lotze was writing a textbook and not an opus.

His *theory of space-perception* was, however, an important and very influential contribution. Lotze began it by asserting that the mind is capable of the notion of space and that it is compelled by this notion to arrange sensory content spatially, even though that content has nothing in itself inherently spatial. This view contains two important points.

In the first place, we must note that Lotze believed that perceived space is derived from conscious data that are themselves non-spatial. He accused others of begging the question in their theories of space-perception. He complained that the view still persisted that somehow small copies of objects enter in the mind in modified form and, being disposed in respect of space, are thus immediately perceived spatially. Even Johannes Müller, who fought this view in his theory of the specific energies of nerves, did not wholly avoid its error, for he held that there is a spatial projection of objective images upon the nervous substance, and that the mind, in perceiving the state of the nerves rather than the object, directly perceives the spatial relations of this image. Such a view, Lotze thought, is not a theory of space, for it puts space in at the start and comes out with it at the end. A true theory of space is an account that goes behind space and shows how it is derived from what is non-spatial. This conclusion of Lotze's, we may object, hardly seems in itself to be a justification for his assumption that primary data of space are non-spatial. It was legitimate for him to say that the older beliefs begged the question and were therefore not theories at all; but such a conclusion did not prove that there must be a theory of space. Perhaps none was needed. It is probable that Lotze was arguing from empiristic intuition, that he felt sure that the raw data of experience have intensity and quality only, and that spatiality must therefore somehow be derived from these more primary other data.

In the second place, we must observe that even Lotze was not ready to try to conjure space out of something not spatial: the mind, he said, has an inherent capacity for arranging its content spatially. How else would it ever come to read space into what is not in itself spatial? It is quite obvious that some principle of this sort was needed, but it is also obvious that its admission robs of some of its cogency the argument that the older views begged the question. Lotze, like the others, found that he must put space into the mind before he was able to create it there. Nevertheless, Lotze cannot be accused of begging the question, of being guilty of the *petitio principii,* for, though he derived the perception of space in part from a capacity for spatial perception, space and the capacity for perceiving it are still two different things, and Lotze

did not fail to give us the account of the manner in which the one gives rise to the other. By his own criterion he had a theory.

The problem of nativism and empiricism in theories of space was not raised until a decade later, when Helmholtz attacked nativism. Nevertheless we can ask to which class Lotze's view belongs. His emphasis upon the innate capacity of the mind for spatial perception seems to place him with the nativists, until one realizes that the main force of his theory was to show that the perception of space is generated in experience from non-spatial materials. There is no doubt, therefore, that he belongs with the empiricists in this issue. In fact, it would seem that his own argument would be that the empirical view is the only theory, and that nativism is not a theory, but begs the question.

Now, the primary data of experience Lotze believed to be qualitative and intensive, and his theory held that it is from its intensive aspect that space evolves. Let us consider the space of touch first and of vision later.

Every tactual sensation (we are following Lotze) has its *local sign,* which is not a new attribute but a specific aggregation of intensities. A touch upon the skin has a diffuse effect because of the yielding and elasticity of the tissues, and it thus sets up a pattern of intensities. This pattern is different in every part of the skin that can be touched because of the differences in the tissues: some parts are hard and others soft, under some there are veins or tendons, and the pattern, depending on all these factors, varies in different places with the conformation of the body. It is exactly this intensive pattern that *is* the local sign: it is local in its dependence on bodily locality and it is local in its mental function as a sign; and yet, being only a sign of position, it is not in itself spatial, but only a pattern of intensities.

A very telling argument that was brought against this view of Lotze's lay in an appeal to bodily symmetry. The two halves of the body are alike in conformation: why do we not then, it was asked, confuse the right hand with the left? The fact is, of course, that the two hands are distinguished more readily than are very many more adjacent parts of the body of very different structure. Lotze replied to this objection by saying that the body is not perfectly symmetrical and that no two hands are ever exactly alike—an answer that leaves us still dissatisfied as to why the

two hands, so like in structure, are yet so very different in spatial reference.

The local signs thus give us different experiences for different localities, but they do not give us space, which is a continuous manifold. How does a consciousness of space arise? It comes, in Lotze's theory, by way of experience and *movement*. When, in movement of the body, a stimulus changes its region of stimulation, the local signs change, and successive local signs are the signs of adjacent localities. If we are equipped with a large number of local signs and we know all the signs that are adjacent, we can solve out, as it were, a kind of solid space. There is no reason why we should do this, except that the mind may tend to arrange all content spatially. Lotze believed that the mind possessed this tendency, and that, having all the local signs put into relation by movement, it created a psychological space out of them. Wundt and other empiricists of space have held a similar view, but Lotze was the originator of it.

With vision the case is less clear than with touch, because a photic stimulus does not mechanically create an intensive pattern. Here, however, Lotze appealed to the innate mechanism by which we tend to fixate what comes into attention. The fixation of an object that lies originally in the periphery of the visual field requires a movement of the eyes and, moreover, a different movement for every point of the periphery. Experientially these movements are intensive patterns and thus furnish the visual local signs. The theory, of course, fails to show how the eyes 'know' where to move in the first instance, but presumably Lotze felt that this matter could be dismissed to physiology and thus left out of account in psychology. At any rate, such movement is instinctive and thus is all ready to play its rôle in experience as soon as experience begins.

However, the problem of vision is not yet entirely solved. We can still tell where an object is without moving the eyes. How can we, if it is movement that creates the local sign? To this objection Lotze replied that we can experience a tendency to movement without the movement's taking place. It thus appears that the visual local sign is really only a tendency to movement, although its spatial meaning has been derived through actual movement. If this argument seems forced, nevertheless we have to remember that it reflects a real difficulty that is still with us.

Again and again introspection yields experience that is like the experience of movement when there is no movement observable. Quite generally against this dilemma psychologists have taken refuge in 'tendencies to movement,' 'incipient movements,' 'covert movements,' or 'implicit movements.'

Finally, we must note that Lotze so far recognized the fact that perceptions degenerate as to hold that the local signs might in the course of experience become actually unconscious. In the case of vision, not only do we no longer need, after sufficient experience, to have actual movement, but we may even have nothing experiential at all. This addition of unconscious local signs detracts from the beautiful simplicity of Lotze's theory, but the fact is obvious so far as ordinary introspection can reach. Lotze was never one to avoid such facts when he realized their existence.

In brief, then, Lotze's theory of space is this. Every visual or tactual stimulus sets up, or tends to set up, an experiential intensive pattern that is specific for the point stimulated. By movement these local signs can be related spatially and brought to mean locality in a single total space; and, because the mind tends to arrange its contents in space, it takes advantage of these local signs to create both space and locality for all the particular sensations. Physical locality is, of course, the starting point, and it yields intensive patterns which have in themselves nothing of space, except that they depend upon space. The mind, by way of movement, gets back the space which was lost in the purely intensive patterns. There is, however, no *petitio principii* here. That we should start with physical locality and end with mental localization is not to beg the question, but simply to treat of perception, which in its essence is the awareness of objects as they really are.

Notes

On Leibnitz, see chap. 9.

Herbart

There are two editions of J. F. Herbart's collected writings. The more familiar is G. Hartenstein's. Herbart's *Lehrbuch zur Psychologie*, 1816, 2d ed., 1834, has been translated into English by M. K. Smith, 1891. B. Rand, *Classical Psychologists*, 1912, 395-415, reprints excerpts from this translation. The *Psychologie als Wissenschaft*, 1824-1825, has not been translated. Herbart also published *Psychologische Untersuchungen* in 1839-1840, shortly before his death. For his other writings, see Rand in

Baldwin's *Dictionary of Philosophy and Psychology*, III, 1905, 253-257.

On Herbart's psychology, see, in the histories of psychology: Th. Ribot, *German Psychology of To-Day*, trans. from French, 1886, 24-48; M. Dessoir, *Outlines of the History of Psychology*, trans. from German, 1912, 210-221; O. Klemm, *History of Psychology*, trans. from German, 1914, esp. 103-111; J. M. Baldwin, *History of Psychology*, 1913, II, 76-82; G. S. Brett, *History of Psychology*, III, 1921, 76-82. See also O. Flügel, *Herbarts Lehren und Leben*, 1912; John Adams, *Herbartian Psychology Applied to Education*, 1897; and W. T. Harris's preface to Smith's translation of the *Lehrbuch*, *op. cit.*, v-xix.

For Herbart's life, see Flügel, *op. cit.*; A. M. Williams, *Johann Friedrich Herbart*, 1911, 8-22. The biographical charts in the front of B. C. Mulliner's translation of Herbart's *Application of Psychology to the Science of Education*, 1898, are more useful than the text.

For W. Wundt's comments on the Kantian and Herbartian psychologies, see his *Grundzüge der physiologischen Psychologie*, any of the six editions, the end of the very first section. The discussion differs in the last three editions.

The exposition of Herbart's psychology involves at every point the *Vorstellung*, a word that cannot be properly translated into English. Smith has translated it "concept"; Baldwin in translating Ribot has used "representation." Both words fail to carry the meaning. Another translation of letters and lectures (not cited here) has used "presentation," which is literally correct, but this English word has never gained the general connotation of *Vorstellung*. To keep the German word is awkward. The text has therefore kept to the word "idea," although this translation is only correct when *idea* means what Locke meant by it, i.e. both perception and idea.

Another word that gives difficulty is Herbart's *Hemmung*. Smith translates it by "resistance," "arrest," and "suppression"; Baldwin by "arrest." It has seemed better to the present author to give *Hemmung* its modern connotation and in general to say "inhibition."

Herbart's picture of the emergence of ideas above the limen of consciousness suggests at once the physiological parallel of J. Bernstein, in his *Untersuchungen über die Erregungsvorgang im Nerven- und Muskelsystem*, 1871, 170-202, and *Lehrbuch der Physiologie*, 1894, 568 ff. These books are rare, but cf. W. Nagel's *Handbuch der Physiologie*, 1905, III, 720 ff., and C. S. Myers, *Text-Book of Experimental Psychology*, 1911, I, 221 ff. The theory finds modern uses: cf. E. G. Boring, *Quart. J. Exper. Physiol.*, 10, 1916, 86-94. Bernstein pictures the central excitation as rising in a mound about the midpoint of its projection, extending above a given liminal base, and showing different amounts of "irradiation" or spread about the center. Herbart's view of the rise of a total idea was similar, for he spoke of the "arching" of an idea in consciousness and the way in which it becomes "pointed" when limited, with repetition, by apperception; cf. Herbart, *Lehrbuch*, sect. 26, d.

Herbart's use of the exact mathematical method to derive from an insecure basis results that have an illusory appearance of the mathematical certainty of the derivation, is a familiar phenomenon. It is often paralleled to-day in the use of elaborate statistical methods for the treatment of inadequate biological and psychological data. One may consult for instances the pages of *Biometrika* or the scattered literature of mental tests; nor is psychophysics free of this inconsistency. The work of the 'mathematicophiles' may perhaps be explained by the fact that the elaborate and arduous mathematical method distracts the attention from equal care in the selection or the experimental

attainment of data. In the author's opinion, however, the difficulty goes deeper. There is some evidence that mathematical ability is separated from other intellectual and esthetic abilities, more than these abilities are ever separated from one another. In the biological sciences, experimental and mathematical competence have almost never been combined in the same investigator so that both kinds of expertness can be brought to bear on the same problem. There may be some psychological reason, therefore, why Herbart, with no tradition in these matters upon him, and temperamentally disposed to favor the mathematical method, was predestined by this same temperament to reject the experimental method.

For Herbart's derivation of the law that two unequal ideas cannot inhibit each other, see *Psychologie als Wissenschaft*, sect. 44. This mathematically derived law is the one most usually cited (cf. the commentaries mentioned above); but Ribot, *op. cit.* 35, introduces a gross error in one of the equations.

In the text we have not mentioned Herbart's connection with associationism. Herbart cannot be called an associationist, for the meaning of that word has become too specific. Nevertheless, although his theoretical basis differs utterly from that of the English school, his treatment of the facts is almost (as James pointed out) identical. Hence it is not surprising that the English school should have had so much to do with the new school that was born in Germany with Wundt. Cf. also Herbart's discussion of the then recent history of psychology (Descartes, Leibnitz, Wolff, and Locke), *Psychologie als Wissenschaft*, sects. 17-22.

On the Herbartian school, see esp. Ribot, *op. cit.*, 49-67.

On Herbart's relation to physiological experimental psychology and also for an excellent account of his psychology, see Th. Ziehen, *Das Verhältnis der Herbartschen Psychologie zur physiologischexperimentellen Psychologie*, 1900, printed in Schiller and Ziehen, *Sammlung Abhandl. aus d. Gebiete d. päd. Psychol. u. Physiol.*, III, Heft 5. Much less satisfactory is F. Willers's dissertation, *Die psychologische Denkweise Herbart's im Verhältnis zu der modernen physiologischen Psychologie*, 1913.

Lotze

Beside R. H. Lotze's *Medicinische Psychologie*, 1852, there is his *Grundzüge der Psychologie*, 1881, which is the immediately posthumous publication of the summaries of his lectures on psychology in the winter of 1880-1881, summaries which Lotze dictated at the end of each lecture. There are two English translations of these notes under the title *Outlines of Psychology*, one by G. T. Ladd in 1886 (excerpts on theory of local signs in Rand, *Classical Psychologists*, 545-556) and the other by C. L. Herrick in 1885. To the German *Grundzüge* is appended a bibliography of Lotze's writings by E. Rhenisch. For bibliography, see also Rand in Baldwin's *Dictionary*, III, 347-350.

On Lotze's psychology, see especially Ribot, *op. cit.*, 68-95. For biographical accounts and discussion of his work in general, see Rhenisch, *Rev. philos.*, 12, 1881, 321-336; R. Falckenberg, *Hermann Lotze* (for he dropped his first name in later years), 1901; G. S. Hall, *Founders of Modern Psychology*, 1912, 65-121; M. Wentscher, *Hermann Lotze*, 1913, and, more briefly, *Fechner und Lotze*, 1925, 73-201. Less important are T. M. Lindsay, *Mind*, 1, 1876, 363-382; and L. Baerwald, *Die Entwicklung der Lotzeschen Psychologie*, 1905 (dissertation).

In the histories of psychology, beside Ribot, *loc. cit.*, see Baldwin, *op. cit.*, II, 82-86; Brett, *op. cit.*, 139-151.

On E. H. Weber and A. W. Volkmann, see chap. 6; on Fechner, see chap. 13.

The history of the Göttingen chair is this: Herbart, 1833-1841, eight years; Lotze, 1844-1881, thirty-seven years; G. E. Müller, 1881-1921, forty years, for it was about 1921 that Müller ceased to lecture, and Ach went to Göttingen.

Falckenberg, *op. cit.*, 193-203, gives indexes of letters of Lotze's, including correspondence with the Fechners and with Stumpf.

Lotze's philosophy was an *Ideal-Realismus*. He was a mediator between opponent views, between idealism and realism, between spiritualism and materialism. He thus, as we shall see in the next chapter, had much in common with Fechner.

His position on the problem of mind and body has not been discussed in the text because it enters into the history of thought more as a phase of his philosophy than as an influence upon experimental psychology. It is enough to say that he was antimaterialistic in his psychology in spite of his physiological interests. See Hall, *loc. cit.*; Wentscher, *Fechner und Lotze*, *op. cit.*, and more especially Th. Simon, *Leib und Seele bei Fechner und Lotze*, 1894, and A. Lichtenstein, *Lotze und Wundt*, 1900 (dissertation), 50-80.

For support of the author's view that Lotze's psychology was more metaphysical than scientific, see Ribot, *op. cit.*, 69-75; E. B. Titchener, *Experimental Psychology*, II, pt. ii, pp. cxi f., clix. The writers of the histories of psychology testify to the same fact by their scant mention of Lotze's name. We have said in the text that Lotze made his psychology physiological by giving the physiological facts, but bare facts did not interest Lotze greatly. He distinguished between *cognitio rei*, an intuitive knowledge of the essential nature of things, and *cognitio circa rem*, a knowledge of the more obvious external relations of things. The former is the metaphysician's truth, the latter is the scientist's fact, and it is plain, in this sense of the words, that Lotze preferred truth to fact, that he felt that scientific knowledge *about* things was but superficial in failing to penetrate to their true essence. See Lotze, *Medicinische Psychologie*, 3-65, esp. 55 ff.; Ribot, *loc. cit.*

THE FOUNDING OF EXPERIMENTAL PSYCHOLOGY

Chapter 13

GUSTAV THEODOR FECHNER

WE come at last to the formal beginning of experimental psychology, and we start with Fechner: not with Wundt, thirty-one years Fechner's junior, who published his first important but youthful psychological study two years after Fechner's epoch-making work; not with Helmholtz, twenty years younger, who was primarily a physiologist and a physicist, but whose great genius extended to include psychology; but with Fechner, who was not a great philosopher nor at all a physiologist, but who performed with scientific rigor those first experiments which laid the foundation for the new psychology and still lie at the basis of its methodology. There had been, as we have seen, a psychological physiology: Johannes Müller, E. H. Weber. There had been, as we have also seen, the development of the philosophical belief in a scientific or a physiological psychology: Herbart, Lotze; Hartley, Bain. Nothing is new at its birth. The embryo had been maturing and had already assumed, in all great essentials, its final form. With Fechner it was born, quite as old, and also quite as young, as a baby.

The Development of Fechner's Ideas

Gustav Theodor Fechner (1801-1887) was a versatile man. He first acquired modest fame as professor of physics at Leipzig, but in later life he was a physicist only as the spirit of the *Naturforscher* penetrated all his work. In intention and ambition he was a philosopher, especially in his last forty years of life, but he was never famous, or even successful, in this fundamental effort that is, nevertheless, the key to his other activities. He was a humanist, a satirist, and a poet in his incidental writings, and an estheticist during one decade of activity. He is famous, however, for his psychophysics, and this fame was rather forced upon him. He did not wish his name to go down to posterity as a

psychophysicist. He did not, like Wundt, seek to found experimental psychology. He might have been content to let experimental psychology as an independent science remain in the womb of time could he but have established his spiritualistic *Tagesansicht* as a substitute for the current materialistic *Nachtansicht* of the universe. The world, however, chose for him; it seized upon the psychophysical experiments which Fechner meant merely as contributory to his philosophy, and made them into an experimental psychology. A very interesting life to us, who are inquiring how psychologists are made!

Fechner was born in 1801 in the parsonage of a little village in southeastern Germany, near the border between Saxony and Silesia. His father had succeeded his grandfather as village pastor. His father was a man of independence of thought and of receptivity to new ideas. He shocked the villagers by having a lightning-rod placed upon the church tower, in the days when this precaution was regarded as a lack of faith in God's care of his own, and by preaching—as he urged that Jesus must also have done—without a wig. One can thus see in the father an anticipation of Fechner's own genius for bringing the brute facts of scientific materialism to the support of a higher spiritualism, but there can have been little, if any, direct influence of this sort, for the father died when Fechner was only five years old. Fechner, with his brother and mother, spent the next nine years with his uncle, also a preacher. Then he went for a short time to a *Gymnasium,* and then for a half-year to a medical and surgical academy. At the age of sixteen he was matriculated in medicine at the university in Leipzig, and at Leipzig he remained for the rest of his long life—for seventy years in all.

We are so accustomed to associating Fechner's name with the date 1860, the year of the publication of the *Elemente der Psychophysik,* and with the later years when he lived in Leipzig while Wundt's laboratory was being got under way, that we are apt to forget how old he was and how long ago he was beginning his academic life. In 1817, when Fechner went to Leipzig, Lotze was not even born. Herbart had just published his *Lehrbuch,* but his *Psychologie als Wissenschaft* was still seven years away in the future. In England, James Mill had barely completed the *History of India* and presumably had not even thought of writing a psychology. John Stuart Mill was eleven years old; Bain was not

born. Phrenology had only just passed its first climax, and Gall was still writing on the functions of the brain. Flourens had not yet begun his researches on the brain. Bell, but not Magendie, had discovered the Bell-Magendie law. It was really, as the history of psychology goes, a very long time ago that Fechner went as a student to Leipzig.

It happened that E. H. Weber, the Weber after whom Fechner named "Weber's Law," went to Leipzig in the same year as *Dozent* in the faculty of medicine, and was made in the following year *ausserordentlicher Professor* of comparative anatomy. After five years of study, Fechner took his degree in medicine in 1822. Already, however, the humanistic side of the man was beginning to show itself. His first publication (1821), *Beweiss, dass der Mond aus Jodine bestehe,* was a satire on the current use of iodine as a panacea. The next year he wrote a satirical panegyric on modern medicine and natural history. Both these papers appeared under the *nom de plume* 'Dr. Mises,' and 'Dr. Mises' was reincarnated in ironical bursts altogether fourteen times from 1821 to 1876. Meanwhile Fechner's association with A. W. Volkmann had begun. Volkmann came to Leipzig as a student in medicine in 1821 and remained, later as *Dozent* and professor, for sixteen years.

After he had taken his degree, Fechner's interest shifted from biological science to physics and mathematics, and he settled down in Leipzig, at first without official appointment, for study in these fields. His means were slender, and he undertook to supplement them by the translation into German of certain French handbooks of physics and chemistry. This work must have been very laborious, for by 1830 he had translated more than a dozen volumes and nearly 9,000 pages; but it was work that brought him into prominence as a physicist. He was also appointed in 1824 to give lectures in physics at the university, and in addition he undertook physical research of his own. It was a very productive period. By 1830 he had published, including the translations, over forty articles in physical science. At this time the properties of electric currents were just beginning to become known. Ohm in 1826 had laid down the famous law that bears his name, the law that states the relation between current, resistance, and electromotive force in a circuit. Fechner was drawn into the resulting problem, and in 1831 he published a paper of great importance on quantita-

tive measurements of the galvanic battery (*Massbestimmungen über die galvanische Kette*), a paper which made his reputation as a physicist.

The young Fechner in his thirties was a member of a delightful intellectual group in the university community at Leipzig. Volkmann, until he went to Dorpat in 1837, was also a member of this group, and it was Volkmann's sister that Fechner married in 1833. The year after his marriage, the year in which, as we have already seen, Lotze came to Leipzig as a student, Fechner was appointed professor of physics. It must have seemed that his career was already determined. He was professor of physics at only thirty-three, with a program of work ahead of him and settled in a congenial social setting at one of the most important universities. We shall see presently how far wrong the obvious prediction would have been. Fechner for the time being kept on with his physical research, throughout the still very fertile decade of his thirties. 'Dr. Mises,' the humanistic Fechner, appeared as an author more than half a dozen times. Toward the end of this period there is, in Fechner's research, the first indication of a quasi-psychological interest: two papers on complementary colors and subjective colors in 1838, and the famous paper on subjective after-images in 1840. In general, however, Fechner was a promising younger physicist with the broad intellectual interests of the *deutscher Gelehrter*.

Fechner, however, had overworked. He had developed, as James diagnosed the disease, a 'habit-neurosis.' He had also injured his eyes in the research on after-images by gazing at the sun through colored glasses. He was prostrated, and resigned, in 1839, his chair of physics. He suffered great pain and for three years cut himself off from every one. This event seemed like a sudden and incomprehensible ending to a career so vividly begun. Then Fechner unexpectedly began to recover, and, since his malady was so little understood, his recovery appeared miraculous. This period is spoken of as the 'crisis' in Fechner's life, and it had a profound effect upon his thought and after-life.

The primary result was a deepening of Fechner's religious consciousness and his interest in the problem of the soul. Thus Fechner, quite naturally for a man with such an intense intellectual life, turned to philosophy, bringing with him a vivification of the humanistic coloring that always had been one of his attributes.

His forties were, of course, a sterile decade as regards writing. 'Dr. Mises' published a book of poems in 1841, and several other papers later. The first book that showed Fechner's new tendency was *Nanna oder das Seelenleben der Pflanzen,* published in 1848. (Nanna was the Norse goddess of flowers.) For Fechner, in the materialistic age of science, to argue for the mental life of plants, even before Darwin had made the mental life of animals a crucial issue, was for him to court scientific unpopularity, but Fechner now felt himself possessed of a philosophic mission and he could not keep silence. He was troubled by materialism, as his *Büchlein vom Leben nach dem Tode* in 1836 had shown. His philosophical solution of the spiritual problem lay in his affirmation of the identity of mind and matter and in his assurance that the entire universe can be regarded as readily from the point of view of its consciousness, a view that he later called the *Tagesansicht,* as it can be viewed as inert matter, the *Nachtansicht.* Yet the demonstration of the consciousness of plants was but a step in a program.

Three years later (1851) a more important work of Fechner's appeared: *Zend-Avesta, oder über die Dinge des Himmels und des Jenseits.* Oddly enough this book contains Fechner's program of psychophysics and thus bears an ancestral relation to experimental psychology. We shall return to this matter in a moment. Fechner's general intent was that the book should be a new gospel. The title means practically "a revelation of the word." Consciousness, Fechner argued, is in all and through all. The earth, "our mother," is a being like ourselves, but very much more perfect than ourselves. The soul does not die, nor can it be exorcised by the priests of materialism when all being is conscious. Fechner's argument was not rational; he was intensely persuasive and developed his theme by way of plausible analogies, which, but for their seriousness, resemble somewhat the method of 'Dr. Mises's' satire, *Vergleichende Anatomie der Engel* (1825), where Fechner argued that the angels, as the most perfect beings, must be spherical, since the sphere is the most perfect form. Now, however, Fechner was in dead earnest. He said later in *Ueber die Seelenfrage* (1861) that he had then called four times to a sleeping public which had not yet been aroused from its bed. "I now," he went on, "say a fifth time, *'Steh' auf!'* and,

if I live, I shall yet call a sixth and a seventh time, *'Steh' auf!'* and always it will be but the same *'Steh' auf!'* "

We need not go further into Fechner's philosophy. He did call, or at least so Titchener thought, a sixth and a seventh time, and these seven books with their dates show the persistence and the extent of Fechner's belief in his own gospel. They are: *Das Büchlein vom Leben nach dem Tode,* 1836; *Nanna,* 1848; *Zend-Avesta,* 1851; *Professor Schleiden und der Mond,* 1856; *Ueber die Seelenfrage,* 1861; *Die drei Motive und Gründe des Glaubens,* 1863; *Die Tagesansicht gegenüber der Nachtansicht,* 1879. As it happened, the public never "sprang out of bed," not even at the seventh call, as Fechner had predicted it should. His philosophy received some attenion; many of these books of his have been reprinted in recent years; but Fechner's fame is as a psychophysicist and not as a philosopher with a mission.

His psychophysics, the sole reason for Fechner's inclusion in this book, was, as we have implied, a by-product of his philosophy. We must return to it.

It was one thing to philosophize about mind and matter as two alternative ways of regarding everything in the universe, and another thing to give the idea such concrete empirical form that it might carry weight with the materialistic intellectualism of the times or even be satisfactory to Fechner, the one-time physicist. This new philosophy, so Fechner thought, needed a solid scientific foundation. It was, as he tells us, on the morning of October 22, 1850, while he was lying in bed thinking about this problem, that the general outlines of the solution suggested themselves to him. He saw that the thing to be done was to make "the relative increase of bodily energy the measure of the increase of the corresponding mental intensity," and he had in mind just enough of the facts of this relationship to think that an arithmetic series of intensities might correspond to a geometric series of energies, that a given absolute increase of intensity might depend upon the ratio of the increase of bodily force to the total force. Fechner said that the idea was not suggested by a knowledge of Weber's results. This statement may seem strange, for Weber was in Leipzig and had published the *Tastsinn und Gemeingefühl* in 1846, and it was important enough to be separately reprinted in 1851. We must remember, however, that Weber himself had not pointed out the general significance of his law, and may have seen its

most general meaning only vaguely. He had hinted at generality in his manner of talking about ratios as if they were increments of stimulus, and in extending his finding for touch to visual extents and to tones. He had formulated no specific law. It was Fechner who realized later that his own principle was essentially what Weber's results showed, and it was Fechner who gave the empirical relationship mathematical form and called it "Weber's Law." In recent times there has been a tendency to correct Fechner's generosity, and to call the demonstrated relationship the "Weber-Fechner Law."

The immediate result of Fechner's idea was the formulation of the program of what he later called psychophysics. This program, as we have already observed, was worked out in the *Zend-Avesta* of 1851. There was still, however, the program to carry out, and Fechner set about it. The methods of measurement were developed, the methods that are still fundamental to much psychological research. The mathematical form both of the methods and of the exposition of the general problem of measurement was established. The classical experiments on lifted weights and on visual brightnesses, and on tactual and visual distances were performed. Fechner the philosopher proved to have lost none of the experimental care of Fechner the physicist. His friend and brother-in-law, A. W. Volkmann, then at Halle, helped with many of the experiments. Other data, notably the classification of the stars by magnitude, were brought forth to support the central thesis. For seven years Fechner published nothing of all this. Then in 1858 and 1859 two short anticipatory papers appeared, and then in 1860, full grown, the *Elemente der Psychophysik*, a text of the "exact science of the functional relations or relations of dependency between body and mind."

It would not be fair to say that the book burst upon a sleeping world. Fechner was not popular. *Nanna, Zend-Avesta,* and similar writings had caused the scientists to look askance at him, and he was never accepted as a philosopher. No one suspected at the time what importance the book would come to have. There was no furor; nevertheless the work was scholarly and well grounded on both the experimental and mathematical sides, and, in spite of philosophical prejudice, it commanded attention in the most important quarter of all, namely, with the other scientists who were concerned with related problems. Even be-

fore the book itself appeared, the paper of 1858 had attracted the attention of Helmholtz and of Mach. Helmholtz proposed a modification of Fechner's fundamental formula in 1859. Mach began in 1859 tests of Weber's law in the time-sense and published in 1863. Wundt, in his first psychological publications in 1862 and again in 1863, called attention to the importance of Fechner's work. Fechner's friend, Volkmann, published psychophysical papers in 1863. Aubert challenged Weber's law in 1864. Delbœuf, who later did so much for the development of psychophysics, began his experiments on brightness in 1864, inspired by Fechner. Vierordt similarly undertook in 1868 his study of the time-sense in the light of the *Elemente*. Bernstein, who had just divided with Volkmann the chair of anatomy and physiology at Halle, published in 1868 his irradiation theory, a theory that is based remotely on Herbart's law of the limen, but directly on Fechner's discussion. The *Elemente* did not take the world by the ears, but it got just the kind of attention that was necessary to give it a basal position in the new psychology.

Fechner, however, had accomplished his purpose. He had laid the scientific foundation for his philosophy and was ready to turn to other matters, keeping always in mind the central philosophical theme. Moreover, he had reached his sixties, the age when men begin to be dominated more by their interests and less by their careers. The next topic, then, that caught the attention of this versatile man was esthetics, and, just as he had spent ten years on psychophysics, so now he spent a decade (1865-1876) on esthetics, a decade that was terminated when Fechner was seventy-five years old.

If Fechner 'founded' psychophysics, he also 'founded' experimental esthetics. His first paper in this new field was on the golden section, and appeared in 1865. A dozen more papers came out from 1866 to 1872, and most of these had to do with the problem of the two Holbein Madonnas. Both Dresden and Darmstadt possessed Madonnas, very similar although different in detail, and both reputed to have been painted by Holbein. There was much controversy about them, and Fechner plunged into it. There were several mooted points. The Darmstadt Madonna showed the Christ-child. The Dresden Madonna showed instead a sick child, and might have been a votive picture, painted at the request of a family with the image of a child who had died.

There was this general question of the significance of the pictures into which Fechner entered. Then there was the question of authenticity. Which was Holbein's and which was not? Experts disagreed. Fechner, maintaining the judicial attitude, was inclined to believe that they might both be authentic, that if Holbein had sought to portray two similar but different ideas he would have painted two similar but different pictures. And finally, of course, there was the question as to which was the more beautiful. These two latter questions were related in human judgment, for almost every one would be likely to believe that the authentic Madonna must be the more beautiful. Some of these questions Fechner sought to have answered 'experimentally' by the consensus of public opinion on the auspicious occasion when the two Madonnas were exhibited together. He placed an album by the pictures and asked visitors to record their judgments; but the experiment was a failure. Out of over 11,000 visitors, only 113 recorded their opinions, and most of these answers had to be rejected because they did not follow the instructions or were made by art critics or others who knew about the pictures and had formed judgments. Nevertheless the idea had merit, and has been looked upon as the beginning of the use of the method of impression in the experimental study of feeling and esthetics.

In 1876 Fechner published the *Vorschule der Aesthetik,* a work that closed his active interest in the subject and laid the foundation for experimental esthetics. It goes into the various problems, methods, and principles with a thoroughness that rivals the psychophysics, but is too far afield for detailed consideration in this book.

There is little doubt that Fechner would never have returned either to psychophysics or to esthetics, after the publication of his major book in each subject-matter, had the world let him be. The psychophysics, however, had immediately stimulated both research and criticism, and, while Fechner was working on esthetics, was becoming important in the new psychology. In 1874, the year of the publication of Wundt's *Grundzüge der physiologischen Psychologie,* Fechner had been aroused to a brief criticism of Delboeuf's *Étude psychophysique* (1873). The next year Wundt came to Leipzig. The following year Fechner finished with esthetics and turned again to psychophysics, publishing in 1877 *In Sachen der Psychophysik,* a book which adds but little to the

doctrine of the *Elemente.* Fechner was getting to be an old man, and his philosophical mission was still in his mind. In 1879, the year of Wundt's founding of the psychological laboratory, he issued *Die Tagesansicht gegenüber der Nachtansicht,* his seventh and last call to the somnolent world. He was then seventy-eight years old. Finally, in 1882, he published the *Revision der Hauptpunkte der Psychophysik,* a very important book, in which he took account of his critics and sought to meet the unexpected demand of experimental psychology upon him. In the following years there were half a dozen psychophysical articles by him, but actually his work was done. He died in 1887 at the age of eighty-six in Leipzig, where for seventy years he had lived the quiet life of the learned man, faring forth, while keeping his house, on these many and varied great adventures of the mind.

This then was Fechner. He was for five years a physiologist; for eighteen a physicist; for five an invalid; persistently though recurrently for thirty-five, a philosopher; during this period, for a decade a psychophysicist, and for another decade an experimental estheticist; during the last ten years, an old man with his attention brought back again to psychophysics. If he founded experimental psychology, he did it incidentally and involuntarily, and yet it is hard to see how the new psychology could have progressed as it did without the *Elemente der Psychophysik* in 1860. It is to this book, therefore, that we must turn our attention.

Psychophysics

When Fechner began work on what was eventually to become the *Elemente der Psychophysik,* he had—beside his philosophical problem, his experience in physical research, and his habits of careful experimentation—Herbart's psychology as a background. From Herbart he obtained the conception that psychology should be science, the general idea of mental measurement and the related notion of the application of mathematics to the study of the mind, the concept of the limen (which Herbart got from Leibnitz), the idea of mental analysis by way of the facts of the limen, and probably also a sensationistic cast to all of his work, a cast which resembles Herbart's intellectualism. When Fechner wrote the *Zend-Avesta,* Lotze had not published his psychology. There was really no psychology at all except

the very influential psychology of Herbart, and the psychological physiology of Johannes Müller and E. H. Weber. Fechner was, however, too much of an experimentalist to accept Herbart's metaphysical approach or to admit the validity of his denial of the psychological experiment. Instead he set himself to correct Herbart by an experimental measurement of mind. All this, (we must not forget) was done in the interests of his philosophical attack upon materialism.

There is also to be mentioned Fechner's mathematical background. It will be recalled that Fechner had turned in part to the study of mathematics after he had obtained his doctor's degree. Fechner himself acknowledges debts to "Bernoulli (Laplace, Poisson), Euler (Herbart, Drobisch), Steinheil (Pogson)." He was thinking, however, more of the mathematical and experimental demonstration of Weber's Law. Steinheil had shown that stellar magnitudes follow this law; Euler, that tonal pitch follows it. It is plain, however, that Fechner placed the name of Daniel Bernoulli (1700-1782) first with reason. Bernoulli's interest in the theory of probabilities as applied to games of chance had led to his discussion of *fortune morale* and *fortune physique,* mental and physical values which he held (1783) are related to each other in such a way that the mental fortune always depends upon its ratio to the total fortune of its possessor. (Thus in gambling with even stakes, one stands to lose more than one gains, for a given loss after the event bears a larger ratio to the reduced total fortune than does the same physical gain to the increased total fortune—a conclusion with a moral!) Thus Bernoulli's *fortune morale* and *fortune physique* were mental and physical quantities, mathematically related, that corresponded exactly, both in kind and in relationship, to mind and body in general, and to sensation and bodily energy in particular, the terms that Fechner sought to relate, in the interests of his philosophy, by way of Weber's law.

On the purely mathematical side, Fechner is less clear as to his background, but it is plain that Bernoulli, Laplace, and Poisson were important. Nowadays we are apt to think especially of Fechner's use of the normal law of error as representing his mathematical interest. Fechner's method of constant stimuli makes use of this law, and the method has assumed importance because it is closely related to the biological and psychological statistical methods that also make use of normal distributions. Psychophysical

and statistical research are not always distinguishable at present. The method of constant stimuli was, however, only one of Fechner's three fundamental methods and it is no wonder that Fechner's evaluation of his mathematics was different from the modern interpretation.

Nevertheless, it is interesting to answer the question that arises about Fechner's use of the normal law. The principles were all contained in the earlier mathematicians' work on the theory of probabilities, work of which Bernoulli's is representative. Laplace, whom Fechner specially mentioned, developed the general law. Gauss gave it its more usual form, and the law ordinarily bears his name. Fechner refers to Gauss in his use of it, but Gauss seems to have been less important than Laplace. There was nothing new in making this practical application of the theory of probabilities. Since 1662 there had been attempts to apply it to the expectation of life, to the evaluation of human testimony and human innocence, to birth-rates and sex-ratios, to astronomical observations, to the facts of marriages, smallpox, and inoculation, to weather forecasts, to annuities, to elections, and finally (Laplace and Gauss) to errors of scientific observation in general. It was in 1835 that Quetelet first thought of using the law of error to describe the distribution of human traits, as if nature, in aiming at an ideal average man, *l'homme moyen,* missed the mark and thus created deviations on either side of the average. It was Quetelet who gave Francis Galton the idea of the mathematical treatment of the inheritance of genius (1869), but Fechner had nothing of this sort in mind. The older tradition, however, he must have known, at least in part, and it is from it that he took for the method of constant stimuli the normal law of error, now so important to psychologists.

Beside this general background and knowledge, Fechner brought to the problem of psychophysics several very definite things. First, there was the fact of the limen, made familiar by Herbart but also obvious enough in other ways, as, for example, in the invisibility of the stars in daylight. Second, there was Weber's law, a factual principle which, if not verified, could still be expected to persist in modified form. Third, there was the experimental method, which was equally fundamental and which derived from Fechner's own temperament in defiance of Herbart. Fourth, there was Fechner's clear conception of the nature

of psychophysics as "an exact science of the functional relations or the relations of dependency between body and mind." This conception was the *raison d'être* for the entire undertaking. Finally, there was Fechner's very wise conclusion that he could not attempt the entire program of psychophysics and that he would therefore limit himself, not only to sensation, but further to the intensity of sensation, so that a final proof of his view in one field might, because of its finality, have the weight to lead later to extensions in other fields.

We must pause here to note that Fechner's view of the relation of *mind and body* was not that of psychophysical parallelism, but what has been called the *identity hypothesis*. The writing of an equation between the mind and the body in terms of Weber's law seemed to him virtually a demonstration of their identity. Nevertheless, Fechner's psychophysics has played an important part in the history of psychophysical parallelism for the reason that mind and body, sensation and stimulus, have to be regarded as separate entities in order that each can be measured and the relation between the two determined. Fechner's psychology therefore, like so much of the psychology that has come after him, seems at first to be dualistic. It is true that he began with a dualism, but we must remember that he began in this way in order to show that the dualism was not real, but could be made to disappear by the writing of the true equation between the two terms.

It is so easy nowadays to think that the Weber-Fechner law represents the functional relation between the measured magnitude of stimulus and the measured magnitude of sensation, that it is hard to realize what difficulty the problem presented to Fechner. It seemed plain to him, however, that sensation, a mental magnitude, could not be measured directly and that his problem was therefore to get at its measure indirectly. He began by turning to *sensitivity*.

Sensation, Fechner argued, we cannot measure; all we can observe is that a sensation is present or absent, or that one sensation is greater than, equal to, or less than another sensation. Of the absolute magnitude of a sensation we know nothing directly. Fortunately, however, we can measure stimuli, and thus we can measure the amount of the stimulus necessary to give rise to a particular sensation or to a difference between two sensations; that is to say, we can measure threshold values of the stimulus.

When we do this we are also measuring sensitivity, which is the inverse of the threshold value. Fechner distinguished between absolute and differential sensitivity, two kinds that correspond respectively to the absolute and differential limens. He recognized the importance of variability in this subject-matter and the necessity of dealing with averages, extreme values, the laws of averages, and the laws of variability about the averages—in short, the necessity of using statistical methods. Now we see why the fundamental psychophysical methods are methods for measuring limens, methods defined by the experimental and mathematical technique necessary to yield limens.

Thus Fechner was led to believe that the stimulus, and hence sensitivity, can be measured directly, but that sensation cannot. Nevertheless, he saw that he could measure sensation itself indirectly by way of its differential increments. In determining the differential limen we have two sensations that are just noticeably different, and we may take the just noticeable difference (the *j.n.d.*) as the unit of sensation, counting up *j.n.d.*'s to determine the magnitude of a sensation. There was a long argument later as to whether every liminal increment of sensation (δS) equaled every other one, but Fechner assumed that $\delta S =$ the *j.n.d.*, and that the sensed differences, being all just noticeably different, were equal and therefore could constitute a proper unit.

One does not in practice count up units for large magnitudes. One works mathematically on the general case for the general function which can, perhaps, later be applied in measurement. Fechner went to work in the following manner. In expounding him we shall use the familiar English abbreviations instead of Fechner's symbols: S for the magnitude of the sensation and R for the magnitude of the stimulus (*Reiz*).

Weber's experimental finding may be expressed:

$$\frac{\delta R}{R} = \text{constant, for the } j.n.d. \qquad (1)$$

This fact ought to be called "Weber's law," since it is what Weber found. Fechner, however, used the phrase for his final result.

He assumed that, if (1) holds for the *j.n.d.*, it must also hold for any small increment of S, δS, and that he could thus express the functional relation between S and R by writing:

$$\delta S = c\frac{\delta R}{R} \qquad \textit{Fundamental formula (2)}$$

where c = a constant of proportionality. This was Fechner's *Fundamentalformel,* and we must note that the introduction of δS into the equation is the mathematical equivalent of Fechner's conclusion that all δS's are equal and can be treated as units. One has only to integrate to accomplish the mathematical counterpart of counting up units to perform a measurement. If we can write the fundamental formula, we can certainly measure sensation. Fechner, therefore, integrated the equation, arriving at the result

$$S = c \log_e R + C \qquad (3)$$

where C = the constant of integration and e = the base of natural logarithms. In formula (3) we really have the desired result, since it gives the magnitudes of S for any magnitude of R, when the two constants are known. Fechner had demonstrated the fundamental point of his philosophy. Nevertheless this formula was unsatisfactory because of the unknown constants, and Fechner undertook to eliminate C by reference to other known facts He let r = the threshold value of the stimulus, R, a value at which S, by definition, = 0. Thus:

When $R = r$, $S = 0$.

Substituting these values of S and R in (3), we get:

$$0 = c \log_e r + C$$
$$C = -c \log_e r$$

Now we can substitute for C in (3):

$$\begin{aligned} S &= c \log_e R - c \log_e r \\ &= c(\log_e R - \log_e r) \\ &= c \log_e \frac{R}{r} \end{aligned} \qquad (4)$$

We can shift to common logarithms from natural logarithms by an appropriate change of the constant from c to, let us say, k:

$$S = k \log \frac{R}{r} \qquad \textit{Measurement formula (5)}$$

This is the formula for measurement, the *Maasformel.* The scale of S is the number of *j.n.d.*'s that the sensation is above zero, its

value at the limen. Beyond this point Fechner went one more step. He suggested that we might measure R by its relation to its liminal value; that is to say, we might take r as the unit of R. If r be the unit of R, then:

$$S = k \log R. \quad \textit{Weber-Fechner Law} \quad (6)$$

This last formula, (6), Fechner called "Weber's Law." It is only as we view the matter now that we see that formula (1) is really Weber's law and that formula (6) should be called Fechner's law, or, as some writers have compromised the issue, the "Weber-Fechner Law." We must remember that $S = k \log R$ is true only when the unit of R is the liminal value of the stimulus and in so far as it is valid to integrate S and to assume that $S = 0$ at the limen. Furthermore, the entire conclusion depends on the validity of Weber's finding, formula (1), a generalization that further experimentation has verified approximately in some cases, but not exactly, nor for the entire range of stimuli.

About this claim of Fechner's that he had measured sensation vigorous controversy raged for forty years or more; and two of the fundamental objections are of sufficient interest to deserve brief mention here.

One argument was that Fechner had assumed the *equality of all j.n.d.*'s without sufficient warrant, and that he had thus in a sense begged the question, since there is no meaning to the statement that one δS equals another unless S is measurable. There is certainly some force to this criticism, but it can be met in two ways.

It was actually met in part by Delbœuf's notion of the sense-distance and the experiments on supraliminal sense-distances. Delbœuf pointed out that we can judge the size of the interval between two sensations immediately and directly. For example, we can say of three sensations A, B, and C, whether the distance AB is greater than, equal to, or less than the distance BC. Thus we perform a mental measurement immediately, and the question is not begged. Now suppose AB = BC psychologically, and suppose that we find that the stimulus for B is the geometric mean of the stimuli for A and C. Then we have shown that the *Fundamentalformel* holds for a large S like AB, and, if the same law holds for large distances judged equal and for *j.n.d.*'s, we may assume that *j.n.d.*'s must also be equal. As a matter of fact, the

experiments on the geometric means for large intervals are not sufficiently numerous to be determinative, but in general the conclusion from this line of attack favors the Weber-Fechner Law as an approximation of the truth.

The other way to meet the objection that all *j.n.d.*'s are not equal is to say frankly that the equality of units must be an assumption. Certainly one *j.n.d.* is equivalent to another in that

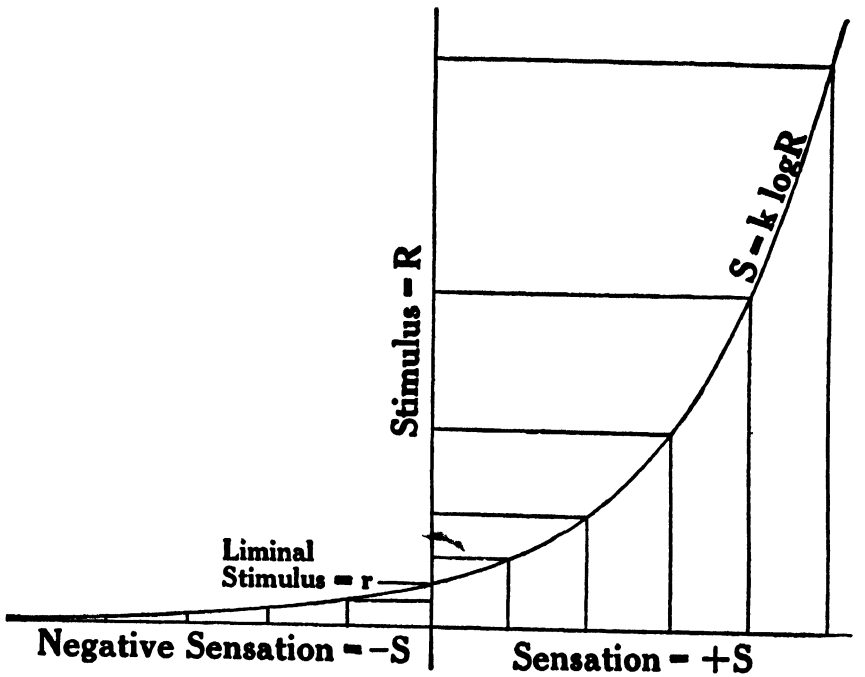

FIG. 5. WEBER-FECHNER LAW: S = k log R. The positions of the equally spaced vertical ordinates represent an arithmetic series of S; their successive heights the corresponding geometric series of R. Thus the curve shows how a logarithmic function represents a correlation between an arithmetic and a geometric series. It also shows why the function requires the theoretical existence of negative sensations, for, when S = 0, R = a finite value, r, the limen; and S passes through an infinite number of negative values when R varies between r and 0.

both are *j.n.d.*'s. The issue can be met thus on purely logical grounds, though this solution leaves open still the question of the exact sense in which *j.n.d.*'s as such are equal. So it is with all units, and even Delboeuf's sense-distance is not more satisfactory in this regard. The obvious fact is, nevertheless, that the Weber-Fechner Law states a relationship between two entities that are not identical. S must be something, and it cannot be R. Something other than the stimulus has been measured.

The other important criticism of Fechner has been called the *quantity objection.* It was argued that it is patent to introspection that sensations do not have magnitude. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow candle within itself." "This sensation of 'gray,'" Külpe remarked, "is not two or three of that other sensation of gray." Must not Fechner have tricked us when he proved by his figures something that we all can see is not true? The author does not believe that the criticism is valid; nevertheless it is clear to him that Fechner himself was to blame for this turn that criticism took. As we have seen, Fechner had said that stimuli can be measured directly and that sensations cannot, that sensations must be measured indirectly by reference to the stimulus and by way of sensitivity. No wonder the critics accused Fechner of measuring the stimulus and calling it sensation. No wonder they argued that his own statement that sensation cannot be measured directly is equivalent to saying that it cannot be measured at all.

The 'quantity objection' was never finally met. The experimentalists simply went on measuring sensation while the objectors complained, or at least they went on measuring whatever Fechner's S is. It was a curious state of affairs. The valid answer to the quantity objection is, in the author's belief, one that was not seen at all at the time. It lies in the fact that the measurement is always indirect, as much for stimulus as it is for sensation. Of course in immediate observation a scarlet is not so many pinks; but in immediate observation a yard is not a congeries of inches. A large magnitude is not more complex than a small one. Measurement is a method of establishing a relation and is never direct and immediate. A weight of 100 grams is not at all 100 one-gram weights, except in the sense that the two can be shown to be equivalent by the application of a method of comparison. The real error was Fechner's, in saying that stimuli can be measured directly, for they can no more be so measured than can sensation. Observation is not measurement. We think that we can see by looking at a yardstick that there are thirty-six inches in a yard, but any one who would take the trouble to construct faulty yardsticks would soon discover the fallacy of this belief. If Fechner, the physicist, had not taken the facts of physical measurement so

much for granted, we might never have had the almost interminable controversy about the 'quantity objection' at all.

We must now turn to certain matters that are connected with Fechner's name: Inner psychophysics, the limen of consciousness, negative sensations, and the psychophysical methods.

Fechner distinguished *inner psychophysics* from outer psychophysics. Outer psychophysics, he said, deals with the relation between mind and stimulus, and it is in outer psychophysics that the actual experiments are to be placed. Inner psychophysics, however, is the relation between mind and the excitation most immediate to it, and thus deals most immediately with the relationship in which Fechner was primarily interested. $S = k \log R$ is a relationship in outer psychophysics. Between R and S excitation, E, is interposed. Just where is the locus of this logarithmic relationship, between R and E or between E and S? It is possible that S is simply proportional to E and that the true law is $E = k \log R$, a statement which means that Weber's law does not solve the problem of mind and body as Fechner hoped it would. Fechner, however, maintained that E was probably proportional to R and that Weber's law is the fundamental law of inner psychophysics, $S = k \log E$.

This view Fechner supported with five arguments. (1) In the first place, he said in the *Elemente,* it would be inconceivable that a logarithmic relation should exist between R and E. Such a statement is hardly an argument, and Fechner took it back in the *Revision.* (2) Then he observed that the magnitude of S does not change when sensitivity is reduced, whereas it should if $S = kE$ and E is involved in the change of sensitivity. (3) Further he noted that Weber's law holds for tonal pitch, and that it would be impossible for the vibrations of E to have other than a proportional relation to the vibrations of R. (Of course he was in error in supposing that nervous excitation is vibratory.) (4) Next he pointed out that a subliminal S probably has an E, that the invisible stars in daytime probably give rise to excitation which is below the limen of consciousness. Such a fact could be true only if $S = k \log E$. (5) Finally, he appealed to the distinction between sleep and waking, and between inattention and attention, as indicating the existence of a limen of consciousness rather than a limen of excitation. This last argument is the most cogent. Certainly the mere fact of the selectivity of attention seems to mean

that there are many excitations, all prepotent for consciousness, of which only a few become conscious. However, Fechner's entire argument would not be taken very seriously at the present time. It is important for us merely to see why Fechner, working in outer psychophysics, thought he was solving the problem, all-important to him, of inner psychophysics.

From this discussion we see how important the fact of the *limen of consciousness* was to Fechner. Weber's law is based upon the limen, for, if $S = k \log R$, then, when $S = 0$, R is some finite quantity, a liminal value. Herbart's limen of consciousness is thus simply a corollary of Weber's law. In fact, Fechner was further consistent with Herbart in relating the limen to attention: when consciousness is already occupied with other sensations, a new sensation cannot enter until it overcomes the "mixture limen."

The psychology that depends upon Weber's law also requires the existence of *negative sensations*. Figure 5 shows graphically the logarithmic curve that gives the relationship of S to R for the Weber-Fechner Law. The function requires that $R = r$, the limen, when $S = 0$, and thus it gives negative sensations for subliminal values of R, for theoretically when $R = 0$, S is negative and infinite. Fechner believed that "the representation of unconscious psychical values by negative magnitude is a fundamental point for psychophysics," and by way of Weber's law he came to hold a doctrine of the unconscious not unlike that of his predecessors, Leibnitz and Herbart.

Fechner's claim to greatness within psychology does not, however, derive from these psychological conceptions of his, nor even from the formulation of the Weber-Fechner Law. The great thing that he accomplished was a new kind of measurement. The critics may debate the question as to what it was that he measured; the fact stands that he conceived, developed, and established new *methods of measurement*, and that, whatever interpretation may later be made of their products, these methods are essentially the first methods of mental measurement and thus the beginning of quantitative experimental psychology. Moreover, the methods have stood the test of time. They have proven applicable to all sorts of psychological problems and situations that Fechner never dreamed of, and they are all still used with only minor modifications in the greater part of quantitative work in the psychological laboratory to-day.

There were three fundamental methods: (1) the method of just noticeable differences, later called the method of limits; (2) the method of right and wrong cases, later called the method of constant stimuli, or simply the constant method; and (3) the method of average error. Each of these methods is both an experimental procedure and a mathematical treatment. Each has special forms. The constant method has been much further developed by G. E. Müller and F. M. Urban, and has thus become the most important. Changes and development, however, add to Fechner's distinction as the inventor. There are few other men who have done anything of equal importance for scientific psychology.

The storm of criticism that Fechner's work evoked was in general a compliment, but there were also those psychologists who were unable to see anything of value in psychophysics. But a few years after Fechner's death, James wrote: "Fechner's book was the starting point of a new department of literature, which it would perhaps be impossible to match for the qualities of thoroughness and subtlety, but of which, in the humble opinion of the present writer, the proper psychological outcome is just *nothing.*" Elsewhere he gave his picture of Fechner and his psychophysics:

"The Fechnerian *Maasformel* and the conception of it as the ultimate 'psychophysic law' will remain an 'idol of the den,' if ever there was one. Fechner himself indeed was a German *Gelehrter* of the ideal type, at once simple and shrewd, a mystic and an experimentalist, homely and daring, and as loyal to facts as to his theories. But it would be terrible if even such a dear old man as this could saddle our Science forever with his patient whimsies, and, in a world so full of more nutritious objects of attention, compel all future students to plough through the difficulties, not only of his own works, but of the still drier ones written in his refutation. Those who desire this dreadful literature can find it; it has a 'disciplinary value;' but I will not even enumerate it in a foot-note. The only amusing part of it is that Fechner's critics should always feel bound, after smiting his theories hip and thigh and leaving not a stick of them standing, to wind up by saying that nevertheless to him belongs the *imperishable glory,* of first formulating them and thereby turning psychology into an *exact science,*

> " 'And everybody praised the duke
> Who this great fight did win."
> "But what good came of it at last?"
> Quoth little Peterkin.
> "Why, that I cannot tell," said he,
> "But 'twas a famous victory!" '"

It is plain to the reader that the present author does not agree with James. Of course, it is true that, without Fechner or a substitute which the times would almost inevitably have raised up, there might still have been an experimental psychology. There would still have been Wundt—and Helmholtz. There would, however, have been little of the breath of science in the experimental body, for we hardly recognize a subject as scientific if measurement is not one of its tools. Fechner, because of what he did and the time at which he did it, set experimental quantitative psychology off upon the course which it has followed. One may call him the 'founder' of experimental psychology, or one may assign that title to Wundt. It does not matter. Fechner had a fertile idea which grew and brought forth fruit most abundantly—and the end of that growth is not yet.

Notes

The more important of Fechner's writings are mentioned in the text. The *Elemente der Psychophysik* was reprinted without change in 1889. To the end of the first volume of this edition Wundt has added a bibliography, originally compiled by R. Müller, of 175 of Fechner's publications. This bibliography has been reprinted by Kuntze, *op. cit. infra.* See also the bibliography of Fechner and about him, B. Rand, in Baldwin's *Dictionary of Philosophy and Psychology,* III, 1905, 199 f.

On Fechner's life, see J. E. Kuntze, *Gustav Theodor Fechner,* 1892; K. Lasswitz, *Gustav Theodor Fechner,* 1896, *et seq.;* G. S. Hall, *Founders of Modern Psychology,* 1912, 123-177; and very briefly Wundt and Titchener, *op. cit. infra.*

On Fechner's work and thought, see Kuntze, Lasswitz, and Hall, *op. cit.;* Th. Ribot, *German Psychology of To-Day,* trans. 1886, 134-187; W. Wundt, *Gustav Theodor Fechner,* 1901; E. B. Titchener, *Experimental Psychology,* 1905, II, pt. ii, pp. xx-cxvi. For a resumé of his philosophy, see W. James, *A Pluralistic Universe,* 1909, 133-177; and the very brief discussion in O. Külpe, *Die Philosophie der Gegenwart in Deutschland,* 1905, 72-78 (or earlier editions); but for James's evaluation of Fechner and psychophysics, see his *Principles of Psychology,* 1890, I, 533-549. On Fechner's philosophy, see also R. B. Perry, *Philosophy of the Recent Past,* 1927, 81-86. See also O. Klemm, *History of Psychology,* trans. 1914, 242-257; G. S. Brett, *History of Psychology,* 1921, III, 127-239.

The text makes some mention of Fechner's use of the normal law of error in the method of constant stimuli. The standard reference here is I. Todhunter, *History of the Mathematical Theory of Probability,* 1865, but this work does not include the nineteenth century. The use of the normal law in mental measurement is presented incidentally in E. G. Boring, *Amer. J. Psychol.,* 31, 1920, 1-33; see esp. 10 f.

The question of the equality of the *j.n.d.* units led to a tremendous controversy. It is not possible here to indicate the nature of it or to give the more important references. The entire matter is discussed very thoroughly and references are given in

Titchener, *op. cit.*, pp. lxviii-lxxxix. For what happens to the general problem of measurement when the ground is shifted from the *j.n.d.* to supraliminal sense-distances, see Titchener, *op. cit.*, II, i, pp. xxi-xxvii; ii, pp. cxvi-cxliv. Titchener's sense-distance is Delboeuf's *contraste sensible*, so Delboeuf receives credit for origination and Titchener for effective development. On the author's view, mentioned in the text, see Boring, *Amer. J. Psychol.*, 32, 1921, 455-462, esp. 455-458, and, less explicitly, *ibid.*, 31, 1920, 26-29. The real difficulty, at the level of this discussion, with $S = k \log R$ is that it has not been demonstrated convincingly enough with supraliminal sense-distances; cf. the historical discussion in Titchener, *op. cit.*, II, ii, 210-232.

On the 'quantity objection,' the controversy that it represents, and the ways of meeting it, see Titchener, *op. cit.*, II, ii, pp. xlviii-lxviii; Boring, *Amer. J. Psychol.*, 32, 451-460. Both passages give many references. The quotations of the text are from James, *Principles of Psychology*, I, 546; Külpe, *Outlines of Psychology*, 45.

The principal references to Fechner's *Elemente* itself (reprint of 1889) are: derivation of Weber's law, the *Fundamentalformel* and the *Maasformel*, II, 9-29; the three fundamental methods, I, 69-133, esp. 71-76; inner psychophysics, II, 377-547 in general, but 428-437 for Weber's law; limen of consciousness, II, 437-464; negative sensations, II, 39-46.

Chapter 14

HERMANN VON HELMHOLTZ

HERMANN LUDWIG FERDINAND von HELMHOLTZ (1821-1894) was a very great scientist, one of the greatest of the nineteenth century. By interest and temperament he was a physicist, though circumstances led him at first into physiological research. If we must classify him at all in accordance with the formal divisions of science, we must say that psychology ranks only third among his scientific contributions; nevertheless Helmholtz, with Fechner and Wundt, is first in importance in the establishment of experimental psychology. He was a man of wide interests, tremendous energy, and great technical and mechanical skill, and was always fundamentally the physicist in method and point of view in physiology and psychology as well as in physics. His contact with Johannes Müller had something to do with his interest in psychology, but it was not all. No man of such versatile genius and vigorous attack could have been working in physiological research in Germany in the '50's and '60's of the last century, when the problems of physiological psychology were so obvious, and have avoided contact with them. In physiological optics and physiological acoustics Helmholtz's genius found ready utilization. In the general problems of science he was forced to consider psychological problems. The times claimed his attention for psychology, though it is also true that Helmholtz determined the times. It was the times that set the problems, but it was Helmholtz's genius that saw the problems and advanced their solution. Unlike Wundt, Helmholtz held no brief for the formal establishment of psychology as an independent science; nevertheless the weight of his work and the influence of his greatness were such as to make him, with Wundt and Fechner, a 'founder' of the new science.

To the reader of this book Helmholtz's name is not new. We have already seen how he measured the rate of the nervous impulse after Müller had deemed the determination impossible.

We have examined Helmholtz's extension of Müller's doctrine of the specific energies of nerves. We have pictured the knowledge of the physiological psychology of sensation in the first half of the nineteenth century up to the time when Helmholtz's researches added so much to the knowledge of sight and hearing. We have now to say something about Helmholtz as a scientific personality, and to indicate the range and nature of his direct contributions to psychology.

Helmholtz was born in Potsdam, near Berlin, in 1821. His father, after a military experience, had become a teacher of philology and philosophy in a *Gymnasium*. His mother, Caroline Penne, was the daughter of an army officer and a descendant of William Penn. Helmholtz was a weak child with a very ordinary education. He did not do very well in his school studies, but his apparent mediocrity seems to have been due to his independence of thought rather than to any lack of ability. He had at home for play a large set of wooden blocks, and with these he learned many geometrical principles for himself before he encountered them at school. He read what scientific books his father's library afforded, and sometimes at school neglected the tiresome Cicero or Virgil to work out optical problems secretly beneath his desk, where his teacher could not see what he was doing. He had no gift for languages. His father tried to interest him in poetry and even to train him in writing poetry, but without much success. Neither the home environment nor the school curriculum was adapted to bring out the precocious mathematical and scientific capacity of the youth. At home he was present at many philosophical discussions between his father and his father's friends, discussions that centered for the most part about Kant and Fichte. It may be that Helmholtz's later reaction toward scientific empiricism and away from Kant's intuitionism began then.

At the age of seventeen, Helmholtz was quite clear in his own mind that he wanted to be a physicist. In fact, these interests were already well begun. There seemed, however, to be no prospect of a livelihood in pure science for this youth whose future was still so obscure. His father had been in the army. It was arranged therefore for him to enter a medico-chirurgical institute in Berlin where tuition was free for youths of promise who would undertake to train themselves to be surgeons in the Prussian army.

From 1838 to 1842 Helmholtz studied at this institute, and for

the next seven years, that is to say until he was twenty-eight years old, he was an army surgeon in Berlin. His passion for science was not, however, suppressed by these circumstances. He contrived to lead the academic life while still nominally a surgeon. Although he was never a student at the University of Berlin, he became attached to its academic circle. He became acquainted with Magnus, professor of physics, whom he was later in 1871 to succeed, and with Johannes Müller, professor of physiology. It was his natural bent for physics that took him to Magnus, and his training in medicine that threw him into physiology with the great Müller. Müller was then reaching the height of his influence: his *Handbuch* had been appearing from 1833 to 1838. Helmholtz's closest friendships were formed among Müller's students. There was Brücke, later professor of physiology at Vienna, and Virchow, later professor of pathology at Berlin, and, most important of all, Du Bois-Reymond, who succeeded Müller as professor of physiology at Berlin. The friendship with Du Bois-Reymond was always close; we have already seen how he aided Helmholtz in the exposition and publication of his results on the measurement of the rate of the nervous impulse.

In 1842 Helmholtz published his first paper, his medical dissertation. In it he showed that the nerve cells of ganglia are individually connected with separate nerve fibers that lead from them, a discovery that foreshadows the neurone theory. The compound microscope had been invented in the preceding decade, and this was the period in which many histological discoveries were being made. Helmholtz, however, had but a poor instrument with which to work. This paper is the beginning of the long series of his scientific publications; more than 200 articles and books had appeared under his authorship by the time of his death fifty-two years later.

In 1847, while still a surgeon in the army, Helmholtz read before the *Physikalische Gesellschaft* in Berlin his famous paper on the conservation of energy (*Ueber die Erhaltung der Kraft*). Nobody ever 'discovered' the law of the conservation of energy. The idea had been developing since Newton. Joule a few years earlier had demonstrated the fact that heat has a mechanical equivalent. Helmholtz brought together much of the previous work and gave the theory mathematical formulation. He was still being the physicist within physiology, for one of his motives was

to show that this principle works within the bodily machine, that the living organism is no exception to the laws of physics. The paper aroused vigorous discussion. Magnus declined to express an opinion, but many of the older physicists discounted the novelty of the view and thus the importance of the formulation. The younger physicists, on the contrary, seized upon the paper with avidity as marking a crucial advance in man's knowledge of the universe. Both groups were, of course, right. The idea was not new; ever since Descartes there had been those who thought of the body as a machine. Nevertheless, the theory needed formulation, clarification, and emphasis. This case has some resemblance to Müller's making the admitted facts of sensory specificity into the 'doctrine' of the specific energy of nerves.

By this time it was clear that the young man, who had entered the army as a surgeon in order to make a living, properly belonged in academic spheres. In 1849, at the age of twenty-eight, Helmholtz was called to Königsberg as professor of physiology and general pathology, and he remained in this position for seven years. His first scientific contribution of importance while at Königsberg was the measurement of the rate of conduction of the nervous impulse. We have already considered the significance of this research and seen the opposition it had to overcome before acceptance. It was at Königsberg, Kant's university, that Helmholtz's interest in problems of sensation began to take shape, and that his thought first directed itself to the scientific empiricism that he was later to oppose to Kant's intuitionism. He turned first to physiological optics. He invented the ophthalmoscope in 1851, and later the ophthalmometer. The ophthalmoscope was regarded as a wonderful instrument, for it enabled the investigator to look directly into the eye, and seemingly to bring the interior of the 'bodily machine' under direct observation. It was also in this period that Helmholtz embraced Thomas Young's theory of color vision and amplified it, thus implicitly and simply extending Müller's doctrine of specific nerve energies to the separate qualities of a single sense. The result of all this work was the undertaking of the writing of the *Handbuch der physiologischen Optik*, of which the first volume appeared in 1856. This volume shows how Helmholtz was working as a physicist within sense-physiology. His perceptual theory came later.

Meanwhile Helmholtz's fame was growing. In 1854 he paid his

first of many visits to England. His contact with English thought was much closer than was usual in Germany, and in certain limited respects Helmholtz belongs more in the British than in the German tradition.

In 1856 Helmholtz went to Bonn for three years as professor of physiology. Here his interest in sensation extended to include physiological acoustics.

In 1859 he was called as professor of physiology to Heidelberg, where he remained until 1871. The years at Heidelberg include the culmination of his genius in the investigation of the physical physiology of sensation. The second volume of the *Optik* appeared in 1860. Then, as if this classic work were not in itself enough for one man, the entire *Tonempfindungen,* including Helmholtz's resonance theory of hearing, was published in 1863. The last volume of the *Optik* came out in 1866, and the complete work was issued in the following year. Within sixteen years, between the ages of thirty and forty-six, Helmholtz completed the works that are still the classics for the experimental psychology of sight and hearing.

Magnus died in 1870, and in 1871 Helmholtz was called to Berlin as professor of physics, thus at last realizing in a distinguished appointment the desire of his youth. He had succeeded in being a physiologist as a surgeon, and a physicist as a physiologist. Now his natural genius was formally recognized. He was fifty years old, and remained at Berlin until his death twenty-three years later.

In 1887 he was made the first director of the new Physical-technisches Institut at Charlottenburg. In 1893 he visited America and the World's Fair at Chicago. During the return voyage to England, he was injured by a fall down the stairway of the ship. He died, partly as the result of these injuries, in 1894.

At Berlin Helmholtz occupied himself with the development of the theory of the conservation of energy, with problems of hydrodynamics, electrodynamics, and physical optics. It was his pupil Hertz who contributed to physical foundation of wireless telegraphy and the radio. Nevertheless Helmholtz maintained his interests in psychology and popular epistemology throughout his life. He revised the *Optik* once and the *Tonempfindungen* three times. He published *Die Thatsachen in der Wahrnehmung* in 1878. Earlier he had defended psychological empiricism against

psychological nativism by a series of papers on the geometrical axioms. His point in this matter was that the axioms are not innate ideas, but are arrived at through individual experience.

Helmholtz was a dramatic lecturer. At Berlin he developed impressive lecture-demonstrations. On four occasions he published collections of popular scientific lectures which present many of his general views.

In a word, Helmholtz was a physicist, but a physicist of such broad interests and activities that he became very important in both physiology and psychology. We shall survey briefly his contributions to psychology.

Sense-Physiology

Helmholtz's greatest contribution to the experimental psychology of sensation is the *Optik*. We have just seen that this very important work appeared in three parts in 1856, 1860, and 1866 respectively, and that it was then issued as a whole in 1867. It remains to-day the gospel in this field. Helmholtz undertook a revision of the work in his later years, and the second edition was published posthumously in 1896, with a huge bibliography of 7,833 titles appended by König, the editor. When a third edition was proposed a decade later, the editors after careful consideration came to the conclusion that, while the content of the first edition had stood the test of time in a most remarkable fashion, the additions and alterations in the second edition had not fared so well. It is as if the older Helmholtz, physicist, had been less immersed in this subject-matter than the younger Helmholtz, physiologist-psychologist. That these editors therefore reprinted (1909-1911) the first edition with extensive additional sections by themselves is a tribute to the insight of the Helmholtz of Königsberg and Heidelberg. Still later, at the American celebration of the centennial of Helmholtz's birth, an English translation of the third, and therefore the first, edition was decided upon, a translation which appeared with a few further additions in 1924-1925. After sixty years and more this treatise was still considered the fundamental text in psychophysiological optics, for it was translated and published for use, and not merely as an historical record.

It is not possible to indicate here the broad range of the con-

tent of this handbook. The three volumes have been characterized respectively as physical, physiological, and psychological in their treatment of vision, but the psychologist would be more likely to define their subject-matter as physiological, sensory, and perceptual, with the physical attack running throughout. The beginner in psychology knows, of course, most often about Helmholtz's theory of vision, the 'Young-Helmholtz' theory, yet this is but a small portion of the half million words that make up the *Optik,* as the seasoned investigator who uses the *Optik* as a *Handbuch* knows. In the last volume we find Helmholtz's more general psychology, the doctrines of empiricism and unconscious inference which we shall presently consider.

The *Tonempfindungen* of 1863, while a less extensive work than the *Optik,* occupies a similar scientific place. It is the fundamental, basic text on the psychology of tone. Helmholtz revised it in 1865 and again in 1870 and 1877, and in 1875 the first English translation, extensively supplemented by notes by Ellis, appeared. Students of both psychology and music still use the book, which is better known than the *Optik* in Great Britain and America because of its early translation.

The first of the three parts of the *Tonempfindungen* is the most important for psychologists. It contains Helmholtz's account of auditory stimuli and of Ohm's law of analysis, his discussion of the anatomy of the ear and the attendant resonance theory of hearing (now so definitely associated with Helmholtz's name), and the report of Helmholtz's researches on combination tones and on the nature of vowel qualities. The other two parts of the book deal with harmony and other musical relationships.

The historical importance of the publication of these two books in the early '60's can hardly be exaggerated. Fifty years before, physiologists had much to say about vision and hearing, the historic senses which Newton discussed, and little about the other senses. Then E. H. Weber wrote the first thoroughgoing psychophysiology of the cutaneous and other bodily senses. Now Helmholtz, like Newton, in having the physicist's point of view, but at a time when the meaning of psychology was becoming a little more obvious, was placing vision and hearing again in the van among those senses about which enough was known to permit the writing of scientific texts. Fechner showed that psychology could employ the scientific method of measurement. Helmholtz

showed what could be done in research and in the accumulation of facts in the two leading sense-departments. The movement toward a psychological science was already under way. With a method of measurement available, with an exhibit of what could actually be done if the experimenter would but get to work, and with the general notion already explicit, it only remained for Wundt to seize the opportunity, to cry the slogan, and psychology as 'an independent science' would be 'founded.'

Helmholtz did not, of course, 'found' experimental psychology, for he thought of psychology as essentially physiological, and physiology as essentially physical. Had the question been raised, he would have opposed a 'mentalism,' just as he did oppose a vitalism. Psychology was for him an exact science, dependent upon the use of mathematics, as Herbart had tried to show, and upon experiment, which Herbart denied. Johannes Müller exerted a very great influence upon Helmholtz's thought in psychological matters, and here we should note the importance that Helmholtz ascribed to the doctrine of the specific energies of nerves. Helmholtz once likened Müller's formulation of this theory to Newton's formulation of the law of gravitation, so great and general a value did he attach to it. After all, quality then seemed to be the fundamental dimension of experience, and to say that difference of quality meant difference of place, and perhaps of structure, in the nervous system was to enunciate a great generalization. Unfortunately history has not entirely realized Müller's anticipations.

Helmholtz's extension of the theory of specific energies to the different qualities within the single sense-department is extremely important as influencing later thought. He seems to have made the extension unconsciously, as a matter of course, or else to have assumed that Thomas Young originated it, long before Müller or even Bell. It was, however, Müller's doctrine, thus conceived, that gave Helmholtz's theories of vision and of hearing their fundamental form. About both these theories controversy still waxes and wanes. Nevertheless, even should they both be abandoned—and it would be a bold prophet who would suggest that they will be—we should find that much of our knowledge of the psychology of vision and hearing had originated in research inspired, positively or negatively, by Helmholtz's theories or their consequences.

Empiricism

Helmholtz was not a systematic psychologist, but his work in vision brought him to the problem of visual perception and thus of perception in general. In many ways, perception has been the central problem of systematic psychology, and it thus comes about that Helmholtz occupies an important position in the history of systematic psychological thought. That Helmholtz was primarily an experimentalist simply goes to show that systematization and experimentation can not be separated in the history of experimental psychology.

Helmholtz stood for psychological empiricism. He belongs thus systematically more with British thought than with German, in the tradition of John Locke down to the Mills, rather than in the tradition of Leibnitz, Kant, and Fichte. German philosophical psychology had stressed intuitionism—that is to say, the doctrine of innate ideas, of *a priori* judgments, of native categories of the understanding. British psychology was built about empiricism, the doctrine of the genesis of the mind through individual experience. Helmholtz took his stand with the latter group against the reigning German philosophy of Kant and Fichte. Later within psychology the opposition became that between geneticism and nativism in perception, with Helmholtz and Wundt representing geneticism and Hering nativism.

Kant had laid down the twelve categories of the understanding, the forms of mind which condition judgments, and had shown that judgments may be *a priori* (given in the understanding) or else *a posteriori* (dependent upon experience). Examples of *a priori* judgments were supposed to be the geometrical axioms, such physical axioms as the indestructibility of matter, causality, and the nature of time and space, including the three-dimensionality of space. Fichte had centered his philosophy about the notion that time and space are *a priori* intuitions. It was this doctrine that Helmholtz undertook to combat. He had already formulated the empiristic view in 1855 while he was still at Königsberg, Kant's university, and somewhat after the appearance of Lotze's genetic theory of space in 1852. He defended it vigorously in the introduction to the third volume of the *Optik* (1866). His papers on the geometrical axioms came later (1866-1894). *Die Thatsachen in der Wahrnehmung* (1878) presents the view. We may turn to

Helmholtz's own account of his grounds for the empiristic theory.

"The *empiristic theory* seeks to demonstrate that at least no other forces are necessary for their origin beyond the known faculties of our minds, even though these forces themselves may remain entirely unexplained. As it is in general a useful rule for scientific investigation to make no new hypotheses so long as the known facts appear to be sufficient for explanation, I have thought it necessary to prefer the empiristic view in its essentials. The *nativistic* theory gives even less explanation of the origin of our perceptual images, for it simply plunges into the midst of the matter by assuming that certain perceptual images of space would be produced directly by an inborn mechanism provided certain nerve fibers were stimulated. In the earlier forms of this theory some sort of self-observation of the retina was presupposed, for we are assumed to have an inborn knowledge of the form of this membrane and of the position of the separate nerve endings in it. In the recent form of this view, especially as developed by E. HERING, there is an ideated subjective visual space wherein the sensations of the separate nerve fibers are supposed to be registered according to certain innate laws. Thus in this theory, not only is KANT's assertion adopted, that the general perception of space is an original form of our ideation, but there are laid down as innate certain special space perceptions."

Helmholtz did not believe that the nativistic theory could be disproved. He held rather, as Lotze had, that it was no theory at all, for it says nothing about space except that space is not generated in experience and must therefore, *faute de mieux,* be native. In general he believed that the nativistic view was unncessary and gratuitous, that the development of perceptions in experience is to a certain extent demonstrable, and that there is no need of hypothesizing in addition another ground of perception unless positive evidence can be adduced for it.

Since Kant and Fichte had used the *geometrical axioms* as examples of *a priori* intuitions, Helmholtz undertook to show that they too are the products of experience. In all he published seven papers on this matter in 1866, 1870, 1876-1877, and 1894. The fundamental ground of all geometrical proof lies, he argued, in the demonstration of the congruence of figures, and this demonstration can be achieved only by the superposition of the one figure upon another. Superposition, however, involves movement (and presumably also the fact that objects do not change in size or

form when moved), and these facts of movement are known only by experience. The telling part of Helmholtz's discussion was, however, his pictures of non-Euclidean space.

He raised the question as to what geometry would be developed by beings who lived in another kind of space than ours. There might, for example, be "sphere-dwellers," who lived entirely in a spherical surface; for them the axiom of parallels would not hold, for any two straight lines, if sufficiently produced, would intersect in two points. Beings who lived in an egg-shaped surface would find that circles of equal radii at different places would have different circumferences. Dwellers in a pseudosphere or in other non-Euclidean spaces would have still different axioms and different geometries. So too, we can conceive, though not imagine, hyper-spaces of four or more dimensions—space, for example, where there are forms that bear the same relation to the sphere as the sphere does to the circle.

So effectively were these pictures drawn that in certain limited circles they became the vogue of the times. Zöllner, the astronomer at Leipzig, came out with the theory that space must be curved and finite (a theory that has a modern ring), or else, he said, since time is infinite, all matter would already have been volatilized. This theory is interesting, but thereafter Zöllner was drawn into a consideration of the performances of the great American medium, Slade. Zöllner—and in part he had the support of Weber and Fechner—suggested that many of Slade's phenomena were accomplished by the use of a fourth dimension; in a fourth dimension objects could be taken out of a closed box as readily as, in three dimensions, they can be lifted out of a square. As has always been the case when psychic research was in question, the result was a violently emotional and personal controversy. Helmholtz was criticized as having laid the ground for a mystical view and thus for a great scientific scandal. One of Helmholtz's accusers was even dismissed from Berlin on account of the nature of his accusations. Helmholtz had done his task so well that the argument for empiricism was now used as an argument for the reality of a kind of space that had never been experienced.

Before we leave this topic, it will be interesting to contrast Helmholtz with Johannes Müller in respect of it. Müller, as we have seen, had a great influence upon Helmholtz, and Helmholtz acknowledged his greatness and especially the importance of the

theory of the *specific energies of nerves*. Yet Müller was a nativist. His doctrine of specific energies has been called a naïve Kantian physiology. Space, he thought, was native to the mind, which can thus perceive spatially the image upon the retina. Helmholtz, while acknowledging the theory of specific energies as a very great discovery, nevertheless took the opposite view about space. Here he followed and extended Lotze's treatment. He admitted for the different nerve fibers the equivalent of non-spatial local signs, and then proceeded to show that it could be only by experience that these characteristics come to generate the space that is known in adult experience. We are observing one of those curious turns in scientific thought: Helmholtz accepted Müller's theory and used it in the interests of an opposite view. The change came about with the general development of thought. In the '30's it seemed sufficient to get the stimulus-object as an image on the retina near enough to the brain, the seat of the mind, for the mind directly to perceive it. In the '60's, science was not satisfied with the attribution of vague powers to the mind; it wanted some positive account of how the mind perceived.

In this connection Helmholtz also took up the facts of the singleness of perception in binocular vision. He laid down the doctrine of corresponding retinal points for the two retinas, a doctrine consonant with the theory of specific nerve energies. Yet the theory of corresponding points works only for the horopter: from all other places a point should appear double. Helmholtz refused to accept Hering's sensations of retinal disparity; they were to him another nativistic mystery. He argued that certain double images come through experience to be perceived as single, a genetic process in which unconscious inference plays a rôle. To the place of "unconscious inference" in Helmholtz's psychology we shall turn in a moment.

We ought to observe that Helmholtz admitted the existence of instincts, briefly, as a mystery which he was not prepared to explain. He felt bound to admit that some animals possess at birth a great deal of specific knowledge which they cannot have acquired by way of individual experience. This point is a concession to the enemy, and Helmholtz did not develop it. He was conversant with Darwin's and Lamarck's views and sympathetic with them. Spencer had not yet shown that nativism may become a positive theory when made over into a phylogeneticism.

Unconscious Inference

The doctrine of unconscious inference (*unbewusste Schluss*) is historically a very important part of Helmholtz's theory of perception and thus of his systematic psychology. The doctrine is really a corollary of the empiristic position. It is most familiar to the psychologist in connection with Helmholtz's theory of color contrast. Red and verdigris are complementaries: they contrast. A gray stimulus appears on a red ground. It contrasts with the red and by unconscious inference we see it as the opposite, that is to say, greenish. The theory has never commanded general acceptance, and stated baldly in this way it seems to violate those excellent principles which Helmholtz urged against nativism; it would seem to be simply a verbal cloak for scientific ignorance. No such casual statement, however, begins to do justice to Helmholtz's views. He was arguing, in the first place, that perception may contain many experiential data that are not immediately represented in the stimulus, a view which ought to have the support of every psychologist who has ever studied an illusion, as well as the support of the modern *Gestalt* psychologists. He was arguing, in the second place, that these aspects of the perception that do not immediately represent the stimulus are, in a sense, additions which accrue to the perception in accordance with its development in past experience. Here *Gestaltpsychologie* would desert Helmholtz on the ground that *experience* is too vague a term and on the further ground that some of these phenomena are conditioned, not upon the past, but upon the present structure of the psychophysical system in which they occur. Nevertheless, some of them are dependent upon the past, and what seems vague in the twentieth century may have been relatively precise in the middle of the nineteenth. It was only as a last resort that Helmholtz decided to call these unconsciously determined phenomena "inferences," in an attempt to characterize their nature in a single word, and even then he sought both to affirm and to deny their inferential nature by the use of a paradoxical phrase: *unconscious inference*. In spite of misgivings about the term, we must therefore take this doctrine of the great empiricist seriously.

Helmholtz adopted this theory while he was at Königsberg, for it is an essential phase of his empiricism. He expounded its essentials in a lecture that was published in 1855. He used it in the

second volume of the *Optik* in 1860. In the third volume in 1866 he gave the full exposition which we follow here. He revised it somewhat in *Die Thatsachen in der Wahrnehmung* of 1878. In the meantime, however, the doctrine had received additional support, in its espousal by Wundt in the *Beiträge zur Theorie der Sinneswahrnehmung,* in the years 1858 to 1862, while Wundt was Helmholtz's assistant at Heidelberg. There is no doubt, however, that the theory belongs more to Helmholtz; Wundt admitted that it does. Helmholtz was the senior, had the theory first, and kept it; Wundt was the younger, took up the view later, and presently abandoned it. Nevertheless, there may have been mutual interaction: Helmholtz's thorough discussion of 1866 is four years after Wundt's in the *Beiträge.*

We may begin our account by quoting from the *Optik* one of Helmholtz's general statements about unconscious inference.

"The psychic activities, by which we arrive at the judgment that a certain object of a certain character exists before us at a certain place, are generally not conscious activities but unconscious ones. In their results they are equivalent to an *inference,* in so far as we achieve, by way of the observed effect upon our senses, the idea of the cause of this effect, even though in fact it is invariably only the nervous excitations, the effects, that we can perceive directly, and never the external objects. Nevertheless, they thus appear to be differentiated from an inference, in the ordinary sense of this word, in that an inference is an act of conscious thinking. There are, for example, actual conscious inferences of this sort when an astronomer computes the positions of the stars in space, their distances from the earth, etc., from the perspective images he has had of them at different times and at different points in the earth's orbit. The astronomer bases his conclusions upon a conscious knowledge of the laws of optics. In the ordinary acts of seeing such a knowledge of optics is lacking; still it may be permissible to designate the psychic acts of ordinary perception as *unconscious inferences,* as this name distinguishes them sufficiently from the ordinary, so-called conscious inferences. While the similarity of the psychic activities in the two cases has been doubted and will perhaps always be doubted, still no doubt can remain of the similarity of the results of such unconscious inferences and of the conscious inferences."

Helmholtz had three positive statements to make about these unconscious inferences.

1. *Unconscious inferences are normally irresistible.* In the typi-

cal case, they appear to be irresistible, but Helmholtz's account is so full of instances of the way in which they can be modified that it seems better to qualify his general statement slightly. He means, of course, that experiences like contrast colors and the singleness of binocular vision come immediately and universally under given conditions and cannot be modified by taking thought —at least that they cannot readily or ordinarily be modified by taking thought. They are "irresistible," he said, because they are unconscious and cannot therefore be corrected by conscious reasoning. The exact degree of irresistibility that he had in mind will appear more plainly in our consideration of the next point. It is enough to note here that irresistibility is Helmholtz's ground for considering unconscious inferences as facts of sufficient precision for inclusion under scientific law.

2. *Unconscious inferences are formed by experience.* Thus the doctrine of unconscious inference becomes the tool of empiricism. These inferences, Helmholtz thought, are at first conscious (unless the instincts be an exception) and develop by *association* and *repetition* into unconscious inferences. In this way Helmholtz comes directly into accord with English associationism, where association is always the servant, and often the only servant, of empiricism. Nevertheless, this view should present no difficulty to any modern psychologist. It is the commonplace of introspective psychology that conscious states, by repetition and under the law of habit, are telescoped and reduced until the given process is largely or entirely unconscious, as far as introspection can reveal, and as against what at first seemed to be the logically necessary essentials. Helmholtz's view is merely the principle of conscious decay under habituation.

It is now plain just what Helmholtz meant when he called the unconscious inferences "irresistible." He meant that well-established associations are virtually inevitable, but that, being associations, they may occur with all degrees of inevitability, and that, being learned, they may also be unlearned. A word gives rise to its meaning 'inevitably,' we should say, except for the fact that the meaning can be changed or, under certain conditions, prevented from occurring. Delayed perceptions also throw light on this matter. The stereoscopic perception of the form of a crystal from an outline stereogram may not give the perception of a solid crystal at once, yet the perception of a solid object becomes irresistible as

soon as the 'idea' of the crystal is realized. The perception of any strange sensory pattern, originally unintelligible, is similar; now it is one thing, now another, until some perception finally remains fixed as embodying the 'true' meaning. In a sense each false completion of the perception is at the moment 'irresistible,' if we remember that this word does not mean that several incompatible completions cannot all exist potentially at the same time and yet all be 'irresistible.'

Helmholtz made an especial appeal to optical *illusions* as supporting the empiristic argument. Many of the optical illusions are practically compulsory. On the other hand, as we now know more definitely than did Helmholtz, they can nearly all be reduced, or even abolished, by the assumption of a certain point of view toward them, like the 'analytic attitude.' Helmholtz said that we can learn to correct them, and on this ground he laid down a general principle:

"Nothing in our sense-perceptions can be recognized as sensation which can be overcome in the perceptual image and converted into its opposite by factors that are demonstrably due to experience."

That which is alterable in experience must have come about by way of experience. In a strict sense, then, it is only sensations that are 'irresistible.' The perceptual additions are introduced by no conscious process and are therefore normally immediate. The real test that they are perceptual, and not sensory, is that they can be 'resisted' by indirect means.

3. *Unconscious inferences are, in their results, like conscious inferences from analogy and are thus inductive.* Helmholtz began his discussion with this point, which explains the name *unconscious inference;* but it is obviously the least important and the most disputable. Helmholtz referred to J. S. Mill's discussion of the syllogism: All men are mortal; Caius is a man; therefore Caius is mortal. It can be argued that the syllogism is a circle and accomplishes nothing, since, if Caius is a man, the statement, "all men are mortal," already includes Caius. We must know the conclusion in order to state the major premise. In this matter Helmholtz followed Mill in saying that we are really dealing with a case of analogical induction. We cannot, of course, observe all men in order to establish the major premise. The best we can do

is to observe many men and then by analogy to conclude on the humanity of Caius and thus to induce his mortality. This process is conscious inference, whereby past experience is able to take care of an entirely new case that has never been included within it.

In the same way, perception works in accordance with definite laws under entirely novel conditions. From many, but not all, cases, the law of color contrast or of the stereoscopic combination of crystal-forms becomes established in experience; then the novel color or the novel form produces its proper result, even though it has never been experienced before. This is Helmholtz's argument, and its validity ought not to be disregarded merely because some of his instances, like color contrast, seem ill chosen. Had there been no problem of the novel perception that is more than sensation, mere association would have been enough for Helmholtz. As it is, he concluded that the results of the perceptual process are like the results of inductive inference, but that the process itself is obviously not conscious as it is in ordinary inference. Hence he invented the term *unconscious inference.*

Perception

Helmholtz's fundamental theory of perception is very simple. The bare sensory pattern, as directly dependent upon the stimulus-object, he called a *Perzeption.* A pure *Perzeption* is, however, comparatively rare; it is nearly always supplemented and modified by an imaginal increment, dependent upon memory and induced by unconscious inference, which makes it over into what may be called an *Anschauung,* which is also literally the *Wahrnehmung,* since it is by *Anschauungen* that objects are correctly perceived and truly identified (*wahr* = 'true'). If the sense-impressions are entirely lacking and we have only the imaginal equivalent of the *Anschauung,* then the experience may be called a *Vorstellung,* a use of this German word more nearly like the English *idea* than is usually the case. *Perzeptionen* are rare. *Vorstellungen* lie outside the universe of discourse. The key to perception lies in the *Anschauungen,* which involve both sensation and imagery, both stimulation and unconscious inference. This view of perception is really the historical link between the problem of perceptual complexity in English associationism and the same problem in the German tradition which Wundt began.

Helmholtz also, like Mill and all those before him since Locke, undertook to say something of the nature of the *object.* To say how experience becomes differentiated into objects is to attack the problem of perception in what is historically the older, and alternative, way. The object, according to Helmholtz, is nothing more than an aggregate of sensations, an aggregate that has been formed in experience because the sensations habitually occur together, and that is not analyzed into its sensory constituents except by a special act of attention. This is the empiristic theory of the object, now familiar to us. Helmholtz, however, went further to say that the object is built up in experience by a sort of "mental experimentation"; we discover by trial and error which sensations can be changed by the will and attribute to the object those that cannot.

Thus the *properties of objects* are merely their effects upon our senses, the relations of the objects to the organs of sense. This is the view to which we were led by Locke's doctrine of secondary qualities and by that portion of Johannes Müller's doctrine of the specific energies of nerves which anticipates the modern distinction between adequate and inadequate stimulation.

The *permanence of objects,* in Helmholtz's discussion, results from the mental experimentation. They cannot be changed by the will except to be made to disappear. When they disappear, they can be brought back by the will, simply by bringing the object again into relation to the sense-organs. We turn the head and the object goes; we turn it back and the object comes again. It must have been there all along. Helmholtz in all these matters was influenced by John Stuart Mill, and we seem to have in his theory of objective permanence only an incomplete discussion that is consonant with Mill's view that objectivity depends upon the conception of the permanent possibilities of sensation.

Scientific Observation

The consideration of the laws of perception led Helmholtz to a very interesting discussion of the nature and limitations of scientific observation, a discussion that is relevant to all psychological observation, but perhaps in particular to what later came to be called introspective observation (*Selbstbeobachtung*). He noted that we have in scientific observation ordinarily to deal with

Anschauungen and not *Perzeptionen;* that is to say, the observation depends upon the past experience of the observer, his unconscious inferences, and the resulting modification of the sensory core. For this reason two different observations of the stimulus-situation may both be 'correct' in so far as they are accurate records of the experience. There may be a personal equation in any observation, and Helmholtz even went so far as to say that most of the observations in the *Physiologische Optik,* being his own, may be subject to this personal error. He had written, as it were, a book about himself. Only the way in which these observations have stood the test of repetition now shows that Helmholtz was being overcautious for the sake of making a point, that his facts of physiological optics were based more upon *Perzeptionen* and less upon *Anschauungen* than he may have supposed. This distinction is, however, not the entire business. Some data, which Helmholtz thought to be unconscious inferences, may be almost universal. If it is an unconscious inference that a given mixture of primary colors appears to be an unanalyzable white, then the personal error disappears entirely. On the other hand, Helmholtz's remarks are obviously in point as against much modern research. All the talk about the influence of a 'laboratory atmosphere' upon observational results means merely that investigators may observe what they are trained to observe, and that it is also true that observers need to be trained.

How, then, are psychological observers to be trained? Helmholtz noted that attention is normally to objects and not to sensations, and that the correction of the objective *Anschauungen* is very difficult. Some observers, like Purkinje, were, he thought, peculiarly gifted in observing sensations and in abstracting from the imaginal supplements that unconscious inference adds. More often, however, the attention has to be especially directed toward the phenomenon before the observation can be made. The right direction of attention may happen by chance; it may be indicated by previous observation or by a theoretical implication; it may require special conditions. Helmholtz gave numerous instances. He mentioned, seemingly as cases of specially directed attention, Purkinje's remarkable observational power, the perception of double images in binocular vision, and the perception of the luminous dust of the eyes, which is always present in all people but is generally first noted by persons with eye-trouble who attribute it

to the trouble. He shows the necessity for auxiliary aids by citing Mariotte and the discovery of the blind-spot, the existence of which can only be shown experimentally; the perception of partials and some combination tones in clangs and dyads, tones which can only be perceived when attention is directed specifically toward them in advance; and the appearance of a landscape when the head is inverted (as when viewed between the legs), a case where the colors come out vividly instead of being toned down by the unconscious modifications of habitual perception.

Fundamental Laws of Mind

Before completing this outline of Helmholtz's more important systematic psychological views, brief mention should be made of certain of his general laws of mind, laws that are significant for scientific thinking.

1. In the first place, he admitted *causation* to the status of a law that is given prior to all experience. For all of Hume, for all his antagonism to intuitionism, he here swung into the Kantian tradition. The shift does not seem to have been logically necessary, but it was natural for a scientist. Causation is the scientist's creed, the backbone of his faith. It is almost impossible for him to admit without emotion that it may be only a mental artifact, alterable in experience. The attribution of freedom to the universe always arouses scientific resistance. Perhaps here we have the reason for Helmholtz's abandonment of empiricism in the interests of causation as an inborn law.

2. Then there is Helmholtz's *law of sufficient reason,* which "is nothing more than the urgent desire to comprehend everything." This specifically is the scientist's drive, but it is also the intellectual urge in all mental life.

3. Finally Helmholtz noted the tendency to generalize, to form *general conceptions,* a tendency which operates in all mental life as in the objectification of perceptions, but which is also at the very basis of scientific thought.

After all, Helmholtz had a great deal that was psychological to say. No wonder, then, that the history of psychology claims him.

Notes

For Helmholtz and the measurement of the rate of the nervous impulse, see chap. 2. For Helmholtz and the doctrine of the specific energies of nerves, see chap. 5. For the physiological psychology of the nineteenth century up to Helmholtz's epoch-making attack upon the problems of sight and hearing, see chap. 6.

The best biography of Helmholtz is L. Koenigsberger, *Hermann von Helmholtz*, 3 vols., 1902-1903, and Eng. trans. J. G. McKendrick, *Hermann Ludwig Ferdinand von Helmholtz*, 1899, is briefer but excellent. There is also G. S. Hall, *Founders of Modern Psychology*, 1912, 247-308; and J. Reiner, *Hermann von Helmholtz* [1905].

Helmholtz's two handbooks are: *Handbuch der physiologischen Optik* (1856-1866), 1867; 2d ed. (1885-1894), 1896; 3d ed., 1st ed. with additions by A. Gullstrand, J. von Kries and W. Nagel, 1909-1911; French trans. of 1st ed., 1867; Eng. trans. of 3d ed. with further additions, 1924-1925; *Die Lehre von den Tonempfindungen als physiologischer Grundlage für die Theorie der Musik*, 1863; later editions, 1865, 1870, 1877; Eng. trans. with additions by A. J. Ellis, 1875, 1885, *et seq.*

Especially relevant to this chapter is *Die Thatsachen in der Wahrnehmung*, 1878 (more usually 1879), an address in Berlin in 1878, reprinted in *Vorträge und Reden*, 1884, II, 217-271.

Helmholtz's writings are very extensive. Many of his more popular writings have been reprinted as *Populäre wissenschaftliche Vorträge* (Eng. trans., 1881), 1865-1871, 1876, and *Vorträge und Reden*, 1884. Many of the technical and less accessible papers are reprinted in the three volumes of his *Wissenschaftliche Abhandlungen*, 1882-1883, 1895. The last and posthumous volume contains A. König's bibliography of 217 titles, pp. 607-636, which is also published separately.

Beside the discussions of Helmholtz's psychology in the biographies, see V. Heyfelder, *Ueber den Begriff der Erfahrung bei Helmholtz*, 1897; F. Conrat, *Helmholtz' Verhältnis zur Psychologie*, 1903, and *Hermann von Helmholtz' psychologische Anschauungen*, 1904; P. Hertz and M. Schlick, *Hermann v. Helmholtz' Schriften zur Erkenntnistheorie*, 1921, which again reprints *Die Thatsachen in der Wahrnehmung*.

Helmholtz's medical thesis and first publication on the relation of nerve cells and nerve fibers of ganglia is *De fabrica systematis nervosi evertebratorum*, 1842, reprinted in *Wissenschl. Abhandl.*, II, 663-679.

Helmholtz's famous paper on the conservation of energy, *Ueber die Erhaltung der Kraft*, 1847, was only the sixth publication that he had ever made. It is reprinted in *Wissenschl. Abhandl.*, I, 12-75.

Helmholtz never discussed his 'extension' of the theory of specific energies of nerves as if it were a novel contribution. In the *Optik* (1860) he seems to take it as a matter of course, but this treatment may reflect the fact that he had already dealt with the matter in 1852. This early paper is *Ueber die Theorie der zusammengesetzen Farben* (reprinted in *Wissenschl. Abhandl.*, II, 3-23), and in it Helmholtz gave his color theory, discussed Müller's doctrine as if it applied to the separate visual qualities, and attributed the entire view to Thomas Young (1802).

The section of the text on Helmholtz's empiricism and the following sections draw primarily upon the general introductory section to the third volume of the *Optik*, "Von den Wahrnehmungen im allgemeinen" (1866). *Die Thatsachen in der Wahrnehmung* is really more epistemological than

psychological. For popular accounts of Helmholtz and the geometrical axioms, see Hall, *op. cit.*, 256-269; McKendrick, *op. cit.*, 250-267.

For Helmholtz's espousal of empiricism while he was still at Königsberg, see his *Ueber das Sehen des Menschen*, 1855, reprinted in *Vorträge und Reden*, 1884, I, 365-396.

The doctrine of unconscious inference is implicit in the paper just cited, but it was not so named. Neither does the term occur in the discussion of visual contrast of 1860. Here the phrase is "illusion of judgment": see the *Optik*, II, sect. 24, the very end of the discussion. Wundt really used the phrase first in 1858 (*Beiträge zur Theorie der Sinneswahrnehmung*, p. 65) and stressed it in 1862 (*ibid.*, 422-451). For Wundt's relation to Helmholtz, see W. Wundt, *Erlebtes und Erkanntes*, 1920, 155-169. Hall on this point is inaccurate.

'Unconscious inference' is the best translation of *unbewusste Schluss*. The English *Optik* says "unconscious conclusion." Baldwin in translating Ribot says "unconscious reasoning." Others have used *inference*, which seems to the author best as denoting the inductive nature of a process. (Here and elsewhere the author has departed at will from the Eng. trans. of the *Optik*, usually in the interests of a more literal translation.)

Chapter 15

WILHELM WUNDT

WUNDT is the senior psychologist in the history of psychology. He is the first man who without reservation is properly called a psychologist. Before him there had been psychology enough, but no psychologists. Johannes Müller was a physiologist. John Stuart Mill was a logician and economist. Lotze was a metaphysician. Helmholtz was a physiologist and physicist. Bain was really a psychologist, but formally a logician. Fechner, whom psychology claims, was first a physicist and then by his own major intent a philosopher. Wundt held a chair of philosophy, as the German psychologists still do, and wrote voluminously on philosophy; but in his own eyes as in the eyes of the world he was, first and foremost, a psychologist. When we call him the 'founder' of experimental psychology, we mean both that he promoted the idea of psychology as an independent science and that he is the senior among 'psychologists.'

Wilhelm Wundt was born in Baden in 1832 in Neckarau, a suburb of Mannheim. His father was a Lutheran pastor. Wundt led the life of an only child, although he had three brothers and sisters; two of them, however, died so early that Wundt did not remember them; one brother, eight years older, had left home for school when Wundt was very young. Wundt and his parents moved to Heidensheim, a village in central Baden, when Wundt was about six. After two years in the *Volkschule,* his education was undertaken by a vicar, Friedrich Müller, presumably Wundt's father's assistant, who shared his room with Wundt. This vicar soon came to command the affection and admiration of the young Wundt to a degree which Wundt's parents had never done, and, when the vicar shortly was called to the neighboring village of Münzesheim, Wundt was so disconsolate that his parents allowed him to go to live with the vicar and thus continue his private education. Wundt seems to have had no boyish friends, no habit of play. Except on Sundays, he spent his time in vicar Müller's

house working on the tasks that Müller had left, indulging often in flights of fancy, and longing always for the vicar's return from his parish duties. When Wundt was thirteen, he entered the *Gymnasium* at Bruschal, a school in which the Catholic influence predominated. Partly for this latter reason, his parents decided to send him elsewhere, and he went the next year to the *Gymnasium* at Heidelberg. Here he made friends, developed an intensive habit of reading, and in general entered upon the life of learning in which he was to become so distinguished. He was ready for the university when he was nineteen.

The picture of Wundt's youth resembles that of some other boys who became great scholars in manhood. It seems to have been almost entirely a life of study, with few friendships and social activities except those that came in adolescence in connection with the intellectual life at the *Gymnasium*. Wundt ought to have been precocious, at least in a bookish way, but there is no way of telling, since his own modest reminiscences are the only record of his youth. His one intense childish attachment was not even within his own family, but to his beloved teacher, Müller. It is possible that this attachment and the lack of normal outlets for boyish energy fixed habits of thought and study upon Wundt that in part account for his later great accomplishment.

In 1851 Wundt went to Tübingen. His parents chose it for him from among the smaller universities in or near Baden: Heidelberg, Freiburg, and Tübingen. However, he remained but a year, for the next autumn he went to Heidelberg for three and a half years. It was at Tübingen that he made the decision to become a physiologist. His father had died, and his mother's means were small. Wundt had some doubts as to whether he was suited to be a physician, but the medical training offered an indeterminate compromise between preparing to become a doctor, a profession which would earn him a livelihood, and studying the sciences, a task more congenial to Wundt's scholarly temperament. So Wundt, like Lotze and Helmholtz before him, went into medicine because of the necessity for earning a living. It is thus, in a sense, because young men have to be self-supporting and because the medical faculties of the German universities gave a truly academic training, which could nevertheless be made profitable later in the practice of a physician, that modern psychology began as physiological psychology.

Wundt's first year at Heidelberg was devoted to the study of anatomy, physiology, physics, chemistry, and some practical medicine. Out of this came his first publication on the sodium chloride content of urine (1853). In his second year he was more definitely introduced to the practice of medicine, and his interest in the physiological work of Johannes Müller at Berlin and of Ludwig at Leipzig increased. In his third year he became an assistant in a medical clinic in Heidelberg, and also published a paper on the effect of the section of the vagus on respiration (1855). By this time he had received a thorough practical training in the medical art, a training from which he had not failed to extract the purely scientific content. Moreover, he had made a start in research.

Whatever had been his doubts four years before, it was now quite clear to him that he was not directed toward the medical profession. In the spring of 1856, he went for the second semester to Berlin, to Johannes Müller's institute of physiology, two years before Müller's death, to study physiology under the man who was then the world's greatest physiologist—the 'father of experimental physiology,' as he has since been called, by a title similar to Wundt's in experimental psychology. Wundt said that he found "the character of German science at Berlin purer in its depth and in its many-sidedness than in the universities of southern Germany." The training at Heidelberg had been too practical for his academic temperament. At Berlin he met, not only the best science of his day, but also the greatest minds. Beside Müller in physiology and Magnus in physics, he seems to have seen most of Du Bois-Reymond, who was then engaged in trying to settle a controversy concerning muscular contractions that had arisen between Eduard Weber in Leipzig and A. W. Volkmann in Halle (Fechner's friend). If anything remained to determine Wundt for the academic life, this stimulating experience at Berlin was decisive.

Wundt returned to Heidelberg, still in 1856, took his doctorate in medicine, and was then habilitated as *Dozent* in physiology, an appointment that he held from 1857 to 1864. He was already beginning to show signs of his future productivity. He published three purely physiological papers in 1856-1857, and then, in 1858, his first book, *Lehre von der Muskelbewegungen*. An earlier interest in pathological anatomy had already passed; his interest

in physiology was paramount; but his bent toward psychology had begun. In 1858 he published the first section of the *Beiträge zur Theorie der Sinneswahrnehmung,* the section on touch, drawing largely upon Weber, Johannes Müller, and Lotze. Wundt already was conceiving of perception as something psychologically more than the physiologist's sensation. The last paragraph of this section contains a brief discussion of unconscious inference (*unbewusste Schluss:* the phrase is used) as the mechanism of perception; and this paragraph must have been written, perhaps published, before Helmholtz came to Heidelberg.

It was in the fall of 1858—altogether an important year for Wundt—that Helmholtz came from Bonn to the physiological institute. Helmholtz's coming coincided with a curricular change that required all persons coming up for the state examination in medicine in Baden to have had a semester's laboratory course in physiology, and Wundt was appointed assistant to drill the sudden inroad of students in the standard experiments on muscle twitches and conduction of the nervous impulse. Wundt stood this dreary routine for a while, without being convinced of its value for potential doctors, and then after several years resigned to resume his docentship. He did not, as one text has it, resign because he knew too little mathematics to satisfy Helmholtz; of this statement Wundt himself remarks that Helmholtz needed no assistance with his mathematics. Altogether there seems to have been respect and mutual admiration between Helmholtz, eleven years the senior, and Wundt, the junior, but no great personal intimacy. Perhaps, as Titchener has observed, their temperaments were too different. Nevertheless, the fact remains that they were both at Heidelberg in the same department for thirteen years until Helmholtz went to Berlin in 1871.

It was in these seventeen years at Heidelberg (1857-1874) that Wundt changed from a physiologist to a psychologist—the first psychologist, we have said. He was *Dozent* in physiology or assistant in the physiological practicum until 1864; then he was made *ausserordentlicher Professor.* In 1871, when Helmholtz went to Berlin, Wundt was his natural successor, but he was not appointed. The chair fell to Kühne, and Wundt stayed on until 1874, when he was appointed to the chair of inductive philosophy at Zürich, a title that signalized his shift of interest. At first at Heidelberg he lectured in alternate semesters on experimental

physiology and medical physics, lectures that resulted later in two books, which he began when he resigned the assistantship. These books are the *Lehrbuch der Physiologie des Menschen* (1864, with new editions in 1868 and 1873) and the *Handbuch der medicinischen Physik* (1867).

It is worth noting that Wundt's emphasis on medical physics means, for all that Johannes Müller had done, that it was still necessary to keep saying that physiology is a science in the sense of physical science. It is quite reasonable, then, that Wundt should also have felt the need for stressing the fact that psychology, as physiological psychology, is a science. Even to-day psychologists have not ceased to be self-conscious about the scientific nature of psychology. In 1858-1859 Wundt began to formalize his epistemological notions by offering lectures on an introduction to the study of the natural sciences. The next year he lectured on anthropology, defined as the natural history of man. He introduced other physiological topics as replacements for the old lectures.

Meanwhile he was working on the *Beiträge zur Theorie der Sinneswahrnehmung*. The first section of 1858 was followed by two more in the following year, one more in 1861, and the last two sections in 1862. The entire book, with its introduction that Titchener says outlined his life's program, was also published in 1862. The correspondence of this program with subsequent events cannot, however, mean that Wundt clearly foresaw, even by intention, his future. How could he? He was still a *Dozent* in physiology with physiological projects under way; there were in the world no experimental psychologists to show that the world could provide a place for the working-out of such a program. Nevertheless Wundt had an idea, and its value was to be attested by the later use that he was able to make of it.

We must pause a moment over the *Beiträge*. Here we have a book that has some claim to making the beginning of experimental psychology, partly because it is in content experimental psychology; partly because it presents a formal plea for experimental psychology, called by that name; and partly because, for all its shortcomings, it is Wundt's first book in experimental psychology.

It was not, of course, the very first book in experimental psychology nor the greatest of its date. Fechner's *Elemente* had

appeared two years before the completed *Beiträge,* although two years after its first portion. Still Fechner, thirty-one years older than Wundt, had begun work while Wundt was yet a student in physiology. The experimental contents of the *Beiträge* were new, but they were hardly so psychologically original as Weber's work on touch had been. No, the actual experimental content of the book is of primary psychological importance only because it shows Wundt, while still immersed in physiology, beginning to think and work in experimental psychology, and moreover in the crucial field of perception.

The book, however, made a claim for experimental psychology. Wundt spoke of an *experimentelle Psychologie.* He was not as specific as he was later. He argued merely that "all psychology begins with introspection [*Selbstbeobachtung*]," and that there are two *Hülfsmittel:* experiment and *Geschichte,* the natural history of mankind. One works, he said, inductively by these means. Thus Titchener was able to say that Wundt throughout his life carried on the three phases of the psychological program of the *Beiträge:* experimental psychology, social psychology (*Geschichte*), and a scientific metaphysics (referring here to Wundt's discussion of induction). The prognostic significance of this tripartite scheme need not concern us; it is important to see that Wundt took the method of experiment, even though he did not think it the only psychological method, so seriously that he spoke readily of experimental psychology. Elsewhere Wundt has told us more of his own thinking. It was about 1858 that Herbart's *Psychologie als Wissenschaft* especially engaged him in the days when he was first beginning to lecture on the general principles of natural science. It was then that he came to the conclusion that psychology must be *Wissenschaft,* but that, *als Wissenschaft,* it must be dependent upon experiment as Herbart had said it is not. For many years Wundt had to fight the Herbartian tradition, but nevertheless it was Herbart that gave to him, as well as to Fechner, the notion of a scientific psychology, though to these men *scientific* meant *experimental.* These matters were not clear to Wundt when he published the first part of the *Beiträge* in 1858, but when he came to add the general introduction to the entire book in 1862, he had the benefit of much thought and lecturing and of Fechner's *Elemente,* and could speak easily of an "experimental psychology."

In the years of the *Beiträge,* Wundt's other interests in psychology were rapidly expanding. In 1861 we find him speaking before the astronomical section of the *Naturforscherversammlung* in Speyer on the psychophysiological explanation of the personal equation. In 1862 he offered at Heidelberg a course of lectures on "psychology from the standpoint of natural science," and in the following year, he published them as the *Vorlesungen über die Menschen- und Thierseele,* a book that was important enough to have a revised edition almost thirty years later, an English translation of the revision, and then repeated reprintings until after Wundt's death. It has been described as the naïve psychology of the physiologist, but actually, whatever its structure, it contained an indication of many problems that were to make up the body of experimental psychology for many years. The personal equation was there and with it the reaction experiment; the perceptual problems of the *Beiträge* were treated more popularly; the psychophysical methods, only three years old, were summarized; and many other more systematic matters, all foretastes of Wundt's later expository method, were included. The Heidelberg course continued under its first title every year until 1867; then the title changed to "Physiological Psychology": perhaps physiological psychology as a formal subject-matter with this name can be said to have begun in 1867. The later '60's were, however, as we have said, taken up with physiological work and publication, the work upon which Wundt embarked when he gave up the assistantship in the physiological practicum for doctors preparing for the state examinations. The psychological work was indirectly all a preparation for the great *Physiologische Psychologie* of 1873-1874, but it is doubtful if Wundt was actually working at the book in the '60's. His capacity for intensive work and immense production was too great to have made so long a period of preparation necessary.

Wundt thought and wrote at an unusually productive rate all his life. It has been said humorously that Cattell's presentation of an American typewriter to Wundt in 1883 may have had something to do with the remarkable volume of Wundt's published writings. Nothing could be farther from the truth. In 1857 he had only just become *Dozent* in physiology. Six years later he had performed experiments and written the *Beiträge* with its systematic introduction; he had worked up lectures in psychology

and had published them in a second book; he had got hold of various fundamental ideas, like the notions of experimental psychology and of historical psychology, and the fact that the experiments on the personal equation opened the way to the psychology of the temporal course and association of ideas. All this was in psychology only. Meanwhile he was publishing on physiological subjects; he was lecturing on microscopic anatomy with demonstrations, on medical physics, on the physiology of generation and development, on anthropology, and so forth, with the topics constantly changing; and he was engaged in the uninteresting mass instruction of the doctors in the practicum. In default of an adequate biography, one can study his bibliography and list of lectures for these years, and then one begins to realize the measure of Wundt's capacity.

Wundt's lectures on physiological psychology, begun in 1867, were to become a book, the most important book in the history of modern psychology, his *Grundzüge der physiologischen Psychologie*. The first half was published in 1873, and the second in 1874, both while Wundt was still at Heidelberg. This book was, on the one hand, the concrete result of Wundt's intellectual development at Heidelberg and the symbol of his metamorphosis from physiologist to psychologist; and, on the other hand, it was the beginning of the new 'independent' science. It was a systematic handbook in both senses of the word; it was built about a system of psychology, and it attempted systematically to cover the range of psychological fact. It was much more sophisticated than the two earlier books on psychology. The doctrine of unconscious inference was gone; the notion of apperception had appeared, although it did not reach its full vigor until later editions. It is easy for the critical reader to stress the changes in the successive six editions of the *Physiologische Psychologie* (1873-1911); however, the great changes, like those in the doctrines of apperception and of feeling, and the broad expansion of the text, are significant only when one realizes that the essential structure of the system was predetermined in 1874 and held to ever after. Wundt did not write another, more mature system of psychology: he modified, improved, and expanded the original. It was called a "physiological psychology," and it was his great argument for an experimental psychology.

Wundt stayed at Zürich only a year (1874-1875). In 1875 he

accepted the call to Leipzig to a chair of philosophy. This change is significant. It brought Wundt formally into the field where psychology was supposed to belong, and it brought him there from physiology. Thus began that paradoxical situation, which still obtains, whereby experimental laboratories grew up as adjuncts to German chairs of philosophy. It was not the Herbartians at Leipzig who called Wundt in preference to his competitor, Horwicz, but Zöllner, who arranged to have a chair divided and to get Wundt as one of two where there was but one before. It is doubtful, however, if any of them fully realized how much of an experimentalist they were getting. They may have thought of Wundt as a misfit scientist converted to philosophy—and indeed there was some truth about the conversion to philosophy. They did, nevertheless, also get an experimentalist in Wundt. With the system published, Wundt in his new chair was able to start out directly upon its extension, by way both of its internal logic and of external experiment.

The experimental work was not long in appearing. In 1879, four years after he had come to Leipzig, Wundt founded, as almost every psychologist knows, the first psychological laboratory in the world. Of course, as we have so often remarked in other connections, nothing that is called 'first' is ever literally first; there always turn out to have been 'anticipations.' William James had a room formally set apart for psychological experimentation at Harvard University about 1874-1876, and Stumpf is said to have had an acoustic laboratory of tuning-forks in a cigar-box in the middle '70's. Such laboratories, however, were not 'founded'; they simply occurred and existed. Besides, the phrase for a psychological laboratory in German is *Psychologisches Institut,* and institutes are recognized administrative units, though laboratories may be merely places for work. This first *Psychologisches Institut* at Leipzig was a primitive affair of a few rooms, soon increased to eleven, in an old building since torn down. It gave way to much better quarters in 1897, but it was in this first building that experimental psychology actually got its firm foothold upon the world in its independently administrative existence. There, more than in any other place, were trained the men who furnish the important names in the subsequent history of experimental psychology, not only Germans, like Kraepelin, Lehmann, Külpe, and Meumann, but also almost all of the first generation of experi-

mentalists in America, like Stanley Hall, Cattell, Scripture, Frank Angell, Titchener, Witmer, Warren, Stratton, and Judd. Hall's contact was slight. Cattell was Wundt's first assistant, self-appointed with genuine American intrepidity. Wundt relates that Cattell simply came to him and said: "Herr Professor, you need an assistant, and I will be your assistant!" In this connection we may remark that Cattell must be one of the few students who brought to Wundt, accustomed to assign problems arbitrarily to students, his own problem, the problem of individual differences, and who succeeded in working upon it, for all that Wundt called it *"ganz amerikanisch"*—as indeed the problem has turned out to be.

The productivity of the new laboratory required a medium of publication. Thus in 1881 Wundt founded the *Philosophische Studien* essentially as the organ of the laboratory and of the new experimental psychology. The first psychological journal must be said to be *Mind,* which Bain founded in 1876, but the 'new psychology' did not immediately flourish in England, and this journal never became permanently its organ. Except for *Mind* then, the *Philosophische Studien* was the first journal of experimental psychology, and the output of Wundt's laboratory was its main source of supply. Nowadays the title sounds strange, but we must remember not only that Wundt was professor of philosophy, but also that Wundt believed that philosophy should be psychological and that he was then well started upon the philosophical decade of his life, a decade in which his important books were a logic, an ethics, and a system of philosophy. After all, the philosophers who called Wundt to Leipzig were not paid in such bad coin.

The '80's formed Wundt's philosophical decade. In 1880-1883 he published the two large volumes of his *Logik,* a book that contains many of his views on psychology and psychological method. In 1886 there appeared the *Ethik.* In 1889 he issued the *System der Philosophie,* a book that Titchener describes as a complete program of a scientific philosophy, the outcome (so Titchener thought) of the plan for a scientific metaphysics outlined in the *Beiträge* twenty-seven years earlier. These three books together contain about 2,500 pages of text, and might well occupy a scholar for ten years. They did not, however, exhaust Wundt's energy. In 1880 he also published the second edition of the

Physiologische Psychologie, much revised, and enlarged into two volumes. In 1887 the third edition appeared. Beside minor articles, he was writing at length in the *Studien.* And all the while he was directing the work of this new, successful, and highly productive venture, the Leipzig laboratory. There are few men who can accomplish so much at so effective a level in so brief a time.

The remainder of the story of Wundt's life is too recent to be of as great concern to us. In 1890 Wundt had already got experimental psychology permanently established in the world of science. He had christened the new psychology "physiological psychology." He had made the argument for a scientific psychology and had begun experimental psychology. He had founded the first laboratory or institute of psychology and had proved that it could be productive. The researches were culminating. He had begun a journal of theoretical and experimental psychology and had maintained it. Other laboratories, patterned upon Leipzig and founded by men trained at Leipzig, were beginning to be established in Germany and America. The character of the new psychology in Germany was already determined by Wundt. To some extent he had also predetermined American psychology, at least as far as the laboratories were concerned, although America, except for Titchener, was destined to have a psychology more or less its own from the very start.

In 1889 Wundt received the honor of being made rector of the university at Leipzig. The measure of his work is, however, to be found in his writings. Every major enterprise that he had begun he continued with revised editions. The fourth edition of the *Physiologische Psychologie* appeared in 1893; the fifth, expanded into three volumes, contained the radical change necessitated by the adoption of the tri-dimensional theory of feeling, and came out in 1902-1903; the sixth, much like the fifth, was issued in 1908-1911. The new theory of feeling first appeared in the *Grundriss der Psychologie* in 1896, and stimulated a very great deal of experimental work in its support and refutation. By 1911 this popular book had passed into the tenth revised edition, followed by five more unaltered reprintings. The *Vorlesungen der Menschen- und Thierseele* of 1863 Wundt revised in 1892, and then twice more, publishing the last revision in 1919. He wrote a little *Einführung in die Psychologie* in 1911. All this mass of publication was psychological in the strict sense of the word,

but Wundt also continued to revise and publish his philosophical texts. There were three revised editions of the *Ethik* between 1892 and 1912; three of the *Logik*, which expanded into three large volumes, between 1893 and 1921 (for the last was published posthumously); three of the *System der Philosophie* between 1897 and 1919.

Still we have not told the entire story. The '90's were a period of activity in which he rounded out his system of psychology and altered it almost completely to accord with his changed views upon feeling. The new century, however, brought him the leisure to return to the unfulfilled task, outlined in the *Beiträge* of 1862, the writing of the *Völkerpsychologie*, the natural history of man, which only, Wundt thought, could give the scientific answer to the problem of the higher mental processes. The first volume of this work appeared in 1900, was later revised, and finally became two volumes in a second revision. The second volume was published in 1905-1906 and became two volumes on revision. Then, from 1914 to 1920, six more volumes appeared, making ten in all. When the reader has comprehended the magnitude of this work, he should then consult Wundt's bibliography in order to see how actively he was engaged in the writing of articles besides his work on the books. Without a break, the enormous productivity and fertility of 1857-1862 was continued for sixty-three years, until Wundt's death in 1920. Even his death seems to have accorded with his systematic habits. All the revisions had been completed. The *Völkerpsychologie* was at last finished. He wrote his life's psychological reminiscences in 1920, and died, on August 31 at the age of eighty-eight, shortly thereafter.

At first the new psychology seemed to center in Wundt and Leipzig. Later, of course, psychology passed beyond Wundt. There were other experimental schools and controversy within Germany and in America. In the last two editions of the *Physiologische Psychologie*, Wundt stressed his system of psychology more, and abandoned the attempt to make the work a complete handbook of psychological knowledge. The *Philosophische Studien* ceased in 1903, except for a *Festschrift* in Wundt's honor, and Wundt sought to use the other journals, especially the *Archiv für die gesamte Psychologie* which replaced the *Philosophische Studien*. He had been too long independent, however, to work

through the medium of others, and he soon began the *Psychologische Studien* as the organ of the Leipzig school.

Wundt was an encyclopedist and a systematist. He had an almost unrivaled capacity for bringing together a tremendous array of facts into a systematic structure. The parts of such a structure tend to become theses, so that systematic writing of this sort takes on the nature of the demonstration of a proof. Thus Wundt was also an effective polemicist. In all this work his method resembles more the method of the philosopher than that of the scientist. At bottom his temperament seems to have been philosophical. It is not that he wrote philosophically and also on philosophy because he had come to hold a chair of philosophy instead of physiology; he was appointed as a philosopher because, even when a physiologist, his bent had been philosophical toward the theory of science.

Was Wundt an experimentalist as well as a philosopher? This question is a very difficult one to answer. It is clear that Wundt came by a rational philosopher's method to his convictions about experimental psychology, that he founded a laboratory, began an experimental journal, conducted experimental research, and always held his theories in the light of all the available experimental fact and subject to revision on the basis of new experimentation, and that he did all this, not because he was by nature an experimentalist, but as the result of a philosophical conviction. He was an experimentalist; but his experimentalism was the by-product of his philosophical views. Wundt never held that the experimental method is adequate to the whole of psychology: the higher processes, he thought, must be got at by the study of the history of human nature, his *Völkerpsychologie*. Thus he could come at and formulate and promulgate an elaborate theory of feeling, that vitally affected his entire system, all before there was much experimentation to support or refute it. Thus also in his psychology conceptual matters like the doctrines of analysis and apperception could seem to others to be his important contributions. With less vitality, he might have held the belief in the experimental method for psychology without ever putting it into practice; but Wundt was Wundt: he could found a laboratory, direct a mass of experimental research, and issue a new experimental journal, all in the decade when his primary concern

lay in the writing of three huge encyclopedic books on logic, ethics, and scientific metaphysics.

Wundt's System

It is hard to say how much of Wundt's system of psychology is essentially part of the history of experimental psychology. At first glance the *Physiologische Psychologie* gives the impression of being experimental through and through. In so far as it is a handbook, it is an experimental handbook; but the system is not the handbook, though both lie within the same covers and on the same pages. The appeal of Wundt's argument is constantly to experiment, and the uncertain points within the argument constantly led to the setting of problems within the Leipzig laboratory. However, all these matters are details within the system, and not of the system itself. The general truth is that the system in its broad outlines is of the order of a classificatory scheme, incapable of experimental proof or disproof. Even such a detail as the tenet that "feeling is the mark of the reaction of apperception upon sensory content" is not so formulated that its truth or falsity can be demonstrated by experiment. Nevertheless the divorce is not complete. Wundt, by his definition of psychology, made introspection for the time being the primary method of the psychological laboratory. The tridimensional theory of feeling stimulated a great mass of experimentation and was perhaps disproved by the experiments—at least it failed to be proved where it should have been. In general, the author believes that the relation of experiment to many of Wundt's important systematic conceptions is less the relation of proof to conclusion than that of illustration to principle. The principles stand as plausible and in accord with personal experience; the experiments illustrate them more precisely. The same experiments might fit another system.

For purposes of exposition, we may distinguish roughly four periods in the development of Wundt's system of psychology, and it will be useful for us to keep these periods in mind, because in part they represent the changing influence of Wundt's psychology upon experiment, and because in part they are indicative of the psychology of the times.

1. The '60's, as we have already seen, were the *presystematic,*

formative period in Wundt's psychological thinking. Both the theory of perception and the distinction between feeling and sensation were then founded upon the doctrine of unconscious inference.

2. With the writing of the *Physiologische Psychologie,* the primary principle of Wundt's system became clear, and we find the doctrine of *psychological compounding* explicit. Unconscious inference had gone, although we find now that "cognitive signs" enter into the theory of perception as marking off the objective from the subjective. In general it appears that mind is to be described in terms of formal elements, like sensation, which have attributes of their own, and which are connected by association—the fundamental psychological principle borrowed from England, and, as far as history has progressed, necessary as the synthetic principle in an analytic system of psychology. Apperception appeared, but it was not yet very important. Feeling was but an attribute of sensation. All this holds essentially for the first three editions of the *Physiologische Psychologie* (1874-1887) and thus overlaps the founding of the laboratory and of the *Philosophische Studien.*

The elementarism involved in psychological compounding by association has had a very great effect upon psychological work, even though, as we shall see later, Wundt seems not to have meant it in quite so mechanical a sense as his critics have thought. Until phenomenological observation recently came into fashion in the laboratory, practically all introspection was analytical; and introspective analysis meant the resolution of experience into compounds of sensations or other elements like them. It is true that Külpe's school of imageless thought was opposed to the sensory nature of certain conscious data; nevertheless it was motivated by the effort to find new elements for thought. Even the modern movement of *Gestalt* psychology, with its great experimental vitality, might be said paradoxically to owe something to Wundt's elementarism, for it is the opposition of this movement to Wundt's analysis and compounding that has given it its dynamic urge for productivity.

3. In the *Grundriss* of 1896 Wundt promulgated the famous *tridimensional theory of feeling.* The systematic importance of this view is tremendous. At one bold stroke, it adds to the elements already available for compounding at least as many again. First, feeling had been but an attribute of sensation; then there

had been just the various intensities of pleasantness and unpleasantness; now, within the dimensions of pleasantness-unpleasantness, strain-relaxation, and excitement-calm, there were not only as many new simple feelings as there had been elements before, but, with the possibility of feelings compounding and compounding again into total feelings, there were very many more. The change seems in part to be an admission that sensationism and associationism were alone inadequate for a satisfactory picture of the mind. Moreover, the sanction for this multiplicity of feelings was empirical but not experimental—if we may use the word *empirical* to designate the method by which the psychologizing philosopher consults his own experience and the casual experience of others without rigorous experimental control. Such an origin left Wundt free to employ the newly created feelings for many purposes where the older, more limited concepts were inadequate. It is true that Wundt did not, like Külpe later, seek to make these new elements into thought processes; nevertheless it is impossible to resist the impression that these feelings, responsible to nothing but the will of their master, prevented many problems from coming clearly to the fore, like the problem of meaning, which Wundt had in part in earlier days met by the hypothesizing of cognitive signs.

Although this multitude of feelings began life as a partially irresponsible invention, Wundt could not rest without an experimental foundation for them. If experiment did not lead to the feelings, at least the feelings led to experiment—as was so often the case with Wundt. Wundt himself, in Lehmann's published curves of pulse and breathing, sought, and thought he found, bodily correlates for each of the six terms of the tridimensional system. Then there followed a period of great experimental activity. There was, especially in Germany, much testing of these correlations. There was some work in America also. In Germany, where Wundt now held in psychology the position of authority that Johannes Müller had once held in physiology, the general tendency of experimenters may be said to have favored the theory, even when their experimental results showed contradictions or gave but equivocal support. In America, Titchener pressed the method of impression into service, and sought, first by his own experiments and then by way of papers from his laboratory at Cornell, to refute the doctrine by showing an identity

between the two new paired dimensions and the orthodox dimension of pleasantness-unpleasantness. This is not the place to enter into the history of the experimental work upon feeling that Wundt aroused; it is enough for us to see that the state of psychology at the end of the last century and the beginning of the present was such that a purely systematic venture of this sort could excite endless work in the psychological laboratories of the two countries that led the way in the new science.

4. The final period in the life of Wundt's system dates approximately from the beginning of the present century. The fifth edition of the *Physiologische Psychologie* (1902-1903) presents the full argument for the new theory of feeling, and it also marks the increased importance of *apperception* as a systematic concept. Feeling and apperception are not unrelated, for feeling is the experiential symptom of apperception. "Feeling is the mark of the reaction of apperception upon sensory content." The trouble with apperception had been that it was an activity and did not lend itself readily to the observational method that Wundt's experimentalism demanded. The new theory of feeling provided a way out of the difficulty, a way that was not possible with the older parsimonious theory of pleasantness-unpleasantness. As we have already seen, these twenty years of the twentieth century spanned Wundt's writings of the ten volumes of the *Völkerpsychologie,* a work that was undertaken in part because Wundt thought it provided the proper approach to the 'higher mental processes.' It is possible that, if Wundt had been younger, we should have had a seventh edition of the *Physiologische Psychologie* with the results of this labor carefully integrated with the rest of the system.

Systematic Fundamentals

Wundt held that psychology is *Erfahrungswissenschaft,* the science of experience. It is not metaphysics, and must develop itself without recourse to metaphysics. Herbart had ordained psychology a science, but one founded on experience, metaphysics, and mathematics, and not upon experiment. To this conception Fechner not only admitted experiment, but made it fundamental. Wundt ruled out the metaphysics, which Fechner, with his dominant philosophical interest, had not avoided, and which

Lotze in his 'physiology of the soul' had cultivated. German psychology had always been metaphysical. Wundt, almost a philosopher, began the antimetaphysical tradition which still persists.

Thus Wundt argued that psychology is not the science of 'inner experience,' because the distinction between inner and outer experience is not valid; feeling is 'inner' in that it is consciously subjective, and perception is 'outer' in that it refers to objects, but psychology deals with both alike. Nor is there, Wundt thought, an 'inner sense.' The data of experience are merely themselves; a perception does not have to be perceived in order to be a perception; it has only to occur. There is, he admitted, a distinction between physics and psychology, but this difference lies in the point of view with which experience is regarded and is thus not within experience.

The valid differentiæ of psychology come then in marking it off from physics. Psychology deals, not with inner experience, but with *immediate experience,* and its data are *anschaulich,* a word that in this context may be translated as 'phenomenal,' although it actually goes further in indicating the palpable nature of the stuff of experience. Physics takes experience mediately and its data are conceptual. In fact, it is because its data are conceptual that its method is mediate, for its elements are inferred, and are not given immediately as phenomena in experience. The permanence of matter is conceptual, for no experience in itself is permanent.

The subject-matter and the method of psychology cannot be discussed separately. If the subject-matter is immediate experience, it is plain that the method is immediate experiencing. For want of a better name, we may call the method *Selbstbeobachtung,* without meaning anything about the self, or introspection without meaning anything about a mental eye that turns about and looks into the mind. These words are names only; they mean nothing more than that having an experience is the same as observing it. We shall see later how Avenarius and Mach led Külpe and Titchener later to modify the concept slightly. The important fact for us is that Wundt, when he said in the *Beiträge* in 1862 that *Selbstbeobachtung* is the method of psychology, laid down the law that psychology is introspective, a

law which, in spite of 'objective psychologies,' withstood serious attack until behaviorism came into vogue in America.

Physics may be conceptual; nevertheless it represents a very real world. Mind and matter, or mind and body, cannot, however —so Wundt thought—be compared. They are totally different universes. Thus Wundt was a dualist, and, as a dualist, he was a psychophysical parallelist. The theory of interaction he rejected because natural science is organized about a closed system of causality which cannot affect the mind or be affected by it. One may get, it is true, the appearance of interaction, as in the case of sensation where nervous stimulation seems to give rise to sensory experience, but the case is only one of appearance. It is a case where identical conditions give rise to both physical and psychical processes, which are thus concurrent but neither identical nor causally related to each other. Moreover, sensation is an exceptional case. Psychophysical parallelism is not universal nor a general metaphysical principle. It holds only in those cases where it has been demonstrated to apply, where we actually find concurrence. In this manner Wundt, for all that he founded 'physiological psychology' and wrote chapters on the nervous system, really went far toward dismissing the body from psychology. It is only in the latter half of his intellectual life that other psychologists have been insisting upon bringing the body back.

Finally we must note that Wundt outlined the problem of psychology as (1) the *analysis* of conscious processes into *elements*, (2) the determination of the manner of *connection* of these elements, and (3) the determination of their laws of connection. Physics abstracts properties; psychology isolates part-contents, which still maintain their phenomenal actuality. The goal of psychology is the analysis of mind into simple qualities and the determination of the form of their ordered multiplicity. The method, Wundt thought, is adequate to the problem except in the case of the higher processes, where analysis fails and we are reduced to the comparative observation of social phenomena, as when we use the study of language as the key to the psychology of thought.

Unfortunately, *multiplicity* is an equivocal word. As we have seen, Wundt fixed the notion of elementarism upon psychology. On the other hand, it must be said that Wundt was no more

interested in analysis than in synthesis, in elements than in manifolds. Which is the conscious reality, the element or the manifold? Wundt seemed to say that the manifold was the phenomenal reality, but that the element is also real and not an artifact of the method. There is an ambiguity here. When we come in a moment to Wundt's theory of actuality, we shall find it hard to believe that he ever meant the element as anything more than an analytical abstraction posited in the interests of description. Nevertheless it is quite clear that he thought of the elements as experienced as such, and in fact his conception of introspection almost demands such a view. If introspection is the mere having of experience immediately, then it cannot be an inferential process of abstraction, and the elements must stand up under attention immediately in the given. It is only fair to Wundt to remark here that the conceptual nature of elements was not so clearly seen in the nineteenth century as in the twentieth. In chemistry atoms were very real things, and, being real, seemed also to be actually phenomenal.

Mental Process

The obvious objection to psychological elementarism is the fact that phenomenal experience is a constant flux. It is not even a kaleidoscopic change of parts, for there are no separate parts. It is, as James made clear, like the flow of a stream that cannot properly be thought of as a grouping of elements. Wundt sought to emphasize this fact by naming the element a "mental process." The force of this term is that it persistently asserts that experience is *active* in the sense of changing process, although not in the sense of an activity that requires an agent.

This view leads over into Wundt's *theory of actuality*. Primarily this theory is nothing more than that the mind, as actual, is immediately phenomenal and is thus not substantial. But essentially the mind is active, and we thus find the original and the modern meaning of the word *actual* combined in its application to the mind. Man has, we might say, a 'real, live mind.'

The term *mental process* might have been expected to secure psychology against the introduction of substantial states of mind, but the word has not always kept its meaning. In the hands of introspectional psychologists, such mental processes as sensations,

images, and simple feelings have often been treated as static bits of consciousness, and have thus given rise to a false elementarism for which Wundt is held responsible and against which the new movements of *Gestalt* psychology and of behaviorism have reacted. Of course Wundt was partly responsible, for to hold that an element is a process is to present a difficult and somewhat ambiguous concept.

Wundt developed his notion of mental content by a statement of oppositions. Mind is actual; it is therefore not substantial. It is activity and not passive being. It is process and not object. Hence it proceeds by way of lawful development and not by way of fixations. These statements summarize the total theory of mental actuality.

Mental Law

Wundt's fundamental principle of law is that of *psychic causality*. Under this principle he meant to include all laws of the interdependence of conscious data. It is a principle that holds in the purely phenomenal realm, and should not be confused with psychophysical causality, which involves the dependence of mind upon body.

It has been argued that cause and effect are physical concepts that cannot apply to mind. Wundt's answer to this argument was that psychic causality is different from physical causality, but the word *cause* is applicable to mental events if we understand what it means in that sphere. In the first place, physical causality is a conception that is bound up with the nature of interdependent substances; there is (as we have just seen) no mental substance, for mind is 'actual.' We must therefore dismiss the notion that psychical causality deals with the interrelation of separate, substantial, and permanent mental things. In the second place, we find that physical causality is understood in terms of the quantitative equivalence of cause and effect in terms of energy; the two are not only correlated events in which the cause is prior to the effect, but they are so related that the cause can be translated into the effect by reducing the relation to the transfer of a determinable amount of energy. There is no mental energy, nor any other all-pervading concept to which everything psychical can be reduced. Therefore we must understand that there is no equivalence intended when we speak of psychic causal-

ity. Psychic causality is simply the principle of growth or development of the mind, where lawful change is the natural process of an active mind. The reader who refers here to Hume's discussion of causality will see that Wundt's doctrine is consistent with it, and that in physics the principle of quantitative equivalence has been grafted on to the general notion of cause as the doctrine of the conservation of energy developed. Wundt held, not only that the phenomenal mind is always in change, but that the changes are lawful. Psychic causality as a principle is merely an assertion that the course and pattern of the constantly flowing, conscious stream depend upon definite laws of sequence, that 'this' regularly follows 'that,' even though 'this' and 'that' are themselves processes and not fixed substantial things.

Under the general law of psychic causality may be subsumed all the other laws. One of the most important of these is the law of *psychic resultants* or the principle of *creative synthesis*. In this principle we have Wundt's 'mental chemistry.' It is not strikingly different from John Stuart Mill's. The important change in the doctrine of synthesis came between James Mill and John Stuart Mill. James Mill asserted that very many ideas can be knit together by association into a complex idea, and that the nature of the complex resultant is to be understood by the fact that all of the components are still actually present in it. John Stuart Mill, however, used the chemical analogy, pointing out that the combination of elements gives rise to resultants that have properties which were not properties of the elements. This view is creative synthesis, which may be thought of as lawfully and causally determined. When the objection is raised nowadays to Wundt's associationism as being a 'mental chemistry,' the objector is more often thinking of the strict atomic-molecular mental chemistry of James Mill than of the more reasonable 'mental chemistry' of John Stuart Mill and Wundt. Such an argument is always confusing, for it was John Stuart Mill who used the term *mental chemistry* in opposition to the very doctrine that is now often intended by the phrase.

Then there is Wundt's law of *psychic relations:* a psychic content acquires significance from the other contents with which it stands in relation. Such a view includes plainly the associationistic theory of meaning or of the object, a theory which we have already examined in detail, and also its modern equivalent,

Titchener's context theory of meaning. Wundt's thought, however, can be illustrated at a much more concrete and specific level, for he uses the principle in accounting for the facts of the Weber-Fechner Law. Fechner had said that this law is psychophysical, that it expresses a measured relationship between mental and bodily process. Others had held that it is purely physiological, a relationship between some peripheral and some more central nervous process. Wundt declared that it is purely psychological. Sensation, nervous excitation, and stimulus are all proportional in respect of their intensities, but a judgment of the amount of difference between two sensations is proportional to the magnitudes of the sensations. Such a statement is identical with the statement that the judged difference is directly proportional to the logarithm of the sensory magnitudes judged. Thus it is plain that a law of psychic relations would be operating. The significance of the difference between sensations depends on the relation of their absolute magnitudes. Wundt's psychological law of relativity grew out of this argument.

There was also for Wundt a law of psychic contrast, which seems to be a special case of the law of psychic relations. Opposites mutually reinforce each other. Wundt based the law on the facts of affective contrast where opposition within the pairs of feelings is most obvious.

Association for Wundt was one of the most important cases of the operation of the law of psychic resultants. It is natural, since he drew so much upon English psychology, that he should develop it at length. Association is a fundamental principle of connection, and in its primitive form it is simultaneous, although it readily becomes successive. It occurs passively, for active association is apperception. Wundt distinguished several cases of association.

1. First, there is *fusion,* which may be the intensive fusions of tones or feelings, or the extensive fusion of sight or touch. A fusion, he held, always involves a blending of the elements with a consequent loss of independence among them, a dominance of one element over the others, which thus play a contributory rôle, but a recoverability of independence of any element by apperceptive isolation. The typical instances are tonal clangs and their analysis into partials, visual localization analyzable into

the visual content and the kinesthetic localizing content, and complex feelings.

2. Second, there is *assimilation,* by similarity or by contrast. Here the examples are the optical illusions. When the phenomenal extension of a line is increased by the addition of a geometrical extension, assimilation by similarity is taking place; when the extension is diminished by an extended motive, the assimilation is by contrast. A very great many illusions come under this class because the dichotomy 'similar-contrasting' is exhaustive wherever the effect of the added motive is of the same nature as the motive.

3. Finally, there are the *complications* where the association is between different sense-departments. The term, as we have seen, comes from Herbart. Its importance is partly derived from the cultivation of the 'complication experiment,' the classical psychological experiment which grew out of the astronomer's discovery of the personal equation in the 'eye and ear' method of observation. Nevertheless, the complication, once defined, comes for Wundt to include almost all complex perceptions: the visual perception of a body as hard or cold, the reaction of the visual image of an orchestral instrument to its sound, most mechanisms of localization, and most linguistic associations between ideas, the sound of the words that stand for them, or the look of the words.

4. Beside these immediate perceptual associations, there is the whole class of memorial associations, a class which became more important after Ebbinghaus had invented the experimental method of working upon association and memory (1885).

Apperception

Wundt's doctrine of apperception has been the occasion for a great deal of discussion in psychological writing and also for some criticism of Wundt, but it seems that Wundt attached rather less importance to it than did some of his followers and critics. We may, therefore, content ourselves with the bare mention of the three aspects of Wundt's doctrine: apperception as phenomenon, as cognition, and as activity.

1. Wundt's faith required that he should deal only in those systematic terms the meaning of which in terms of phenomenal

experience is clear. Thus apperception, although neither an element nor a complex of elements, has its *phenomenal* side. It is a phenomenon that there are two degrees of consciousness, that all processes within the range of consciousness lie, as we may say, within the field of consciousness (*Blickfeld*), but that of these processes few are brought within the focus of consciousness (*Blickpunkt*). These processes within the *Blickpunkt* are apperceived. The range of the *Blickpunkt* is the range of attention, which is always less than the total range of consciousness and thus measures apperception. In this way the phenomena of apperception came under experimentation. The range of the *Blickpunkt* was determined under various conditions to be about six items or groups. The discovery that the so-called 'muscular reaction time' was generally about one tenth of a second less than the sensorial reaction time was taken to mean that the latter involved apperception of the sensory impression, whereas the former did not, and that apperception thus required about one tenth of a second. Experimental findings of this sort naturally seemed to confirm the scientific status of apperception.

There is also, in Wundt's doctrine, the phenomenal relation of apperception to feeling. Association is passive, but apperception is active, Wundt asserted. Is this activity of apperception given in immediate experience? In a sense Wundt thought that it is, for apperception is ordinarily accompanied by a feeling of activity, which is what marks it off phenomenally as active.

After Wundt had developed the tridimensional theory of feeling, the relation between feeling and apperception became important. The feelings come about, Wundt believed, as apperception operates; feeling is normally the mark of the reaction of apperception upon sensory content. On this view, feeling is the sign of apperception and almost its phenomenal representative. This phase of the theory is, however, less important because of the failure of Wundt's extended theory of feeling to establish itself experimentally.

2. Wundt further distinguished apperception from association by saying that apperception occurs in logical connections between mental contents, whereas associative connections are not logical. Thus apperceptions may be analytic or synthetic. Judgment is analytic apperception, for it isolates a content. The synthetic apperceptions may be of various degrees of intimacy, from the

bare *agglutination* up through the typical *apperceptive synthesis* all the way to the *concept*. These three stages of synthesis might be represented by the apperceptions of $2 + 3$, $\sqrt{2}$, $\sqrt{-1}$, although these simple examples are not Wundt's. It is plain that apperception was held by Wundt to have a *cognitive* function. Is this function exhibited on the phenomenal level? Wundt doubtless thought it is. His primary contribution to psychology had been to exclude the logically conceptual. For some such reason he had abandoned the unconscious inference. Followers of Wundt (like Titchener, who continued the course laid down by Wundt further than Wundt ever went) have never accepted the concept as phenomenal. Külpe rejected it at first and then admitted it. This controversy is still going on to-day, and there is no final verdict. The important things to note are that Wundt did not go all of the way upon the path that leads away from knowledge toward phenomenal experience, and that this cognitive function of apperception was never sufficiently defined by experimental settings to gain a clear scientific meaning.

3. Finally we remark that Wundt held that apperception is *active*. This is a view consonant with the theory of actuality; apperception is a constant current in the stream of consciousness. Moreover, as long as apperception means flux and change, there is nothing in it inconsistent with Wundt's system. Difficulty arises only when activity is taken as implying an active agent. To the author of this book many of Wundt's statements seem to connote a free apperception that accomplishes this or that in the psychic field. Wundt would of course have denied this imputation, as he was bound by his systematic position to do. He is protected by his own caveats. If there be any argument at all on this point, it must hinge on the degree with which the ordinary connotation of words can be arbitrarily altered by a redefinition that precedes the argument.

The Work of the Leipzig Laboratory

Wundt's laboratory did more than set the fashion for the new psychology; it defined experimental psychology for the time being, because the work of this first laboratory was really the practical demonstration that there could be an experimental psychology, and thus an example of what an experimental psy-

chology would be like in fact. It is important, therefore, for us to examine into the nature of the work of the Leipzig institute in its earlier years in order to see what effective methods and experimental subject-matters formed the foundation of the new scientific psychology.

Practically all the work from the Leipzig laboratory was published in the *Philosophische Studien* (1881-1903) and there is not very much in this journal that did not come either directly from Leipzig, or from Wundt's students so soon after leaving Leipzig that they still represented the intentions of Wundt. If we omit theoretical papers, many of which Wundt wrote himself, and the propædeutic experimental work on apparatus and method, especially the psychophysical methods, we have left about 100 experimental researches that characterize the laboratory's first twenty years.

About a third of these researches represent problems on topics that would ordinarily be labeled 'sensation,' and more than half of the total deal with matters of sensation and perception. Such papers were always in the majority, and the relative proportion of them became greater as the period advanced and the total output increased. When Wundt is called a 'sensationist,' we must mean not only his systematic pronouncements, but also the dominant character of the experimental work which he determined. A sixth of his laboratory's output dealt with the problem of action, the reaction experiment, and the mental chronometry that came out of the 'subtractive procedure' as applied to reaction times. This work was, next to sensation and perception, the most important topic of the period 1881-1895, and was at its height in the middle quinquennium of these three. As the interest in the reaction experiment began to wane, research on attention and on feeling got under way. These two topics received their maximal emphasis in the '90's, and each represents about a tenth of the work of these twenty-odd years. Scattered all the way through this total period there were occasional studies of association, which increased in number after Ebbinghaus had experimentalized the problem of memory in 1885. Wundt and Leipzig were, however, never important in the development of the psychology of memory; Wundt went his own way, and his laboratory with him, and outside discoveries did not greatly deflect his course.

Within the field of *sensation and perception*, most of the work

was on *vision.* There had been more to say, or at least more was known, about vision than about any other sense, all the historical way from Newton to Helmholtz. To the classical knowledge Wundt added his new conviction about experimental psychology, his genius in formulating significant problems, and Fechner's new method of measurement. There were half a dozen papers on the psychophysics of light and the excitation of the retina (1884-1902). There were three papers on the psychophysics of color (1891-1898). Kirschmann and Hellpach investigated peripheral vision (1889-1900). Kirschmann made his classical studies on visual contrast (1890) and on color-blindness (1892). There were also studies of the Purkinje phenomenon and of negative after-images. In the field of visual perception, there were the well-known half-dozen studies by Titchener, Kirschmann, and Arrer on binocular vision (1892-1901). Beside two unimportant studies of the preception of form, there was Martius's work on apparent visual size (1889) and Thiéry's study of optical illusions (1895). Marbe and Dürr worked on seen movement with Külpe at Würzburg (1898-1899). Altogether these visual studies represent about a quarter of the total work of the Wundtian school during its first two decades.

In the field of *auditory sensation,* Tischer, Lorenz, Merkel, Luft, and Frank Angell published psychophysical papers (1883-1891). Scripture and Krueger investigated beats and combination tones (1892-1901); and less well-known authors, the fusion and analysis of clangs. Lorenz's paper on tonal interval was famous because of the controversy it involved (1890).

Touch, ever since E. H. Weber's *Tastsinn und Gemeingefühl,* has been for psychologists the third most important sense. Kiesow, Stratton, and Bader published research from Leipzig on tactual sensation (1895-1902). The perceptual problems of tactual localization and the two-point limen found their place in the *Studien,* although some of these, like Washburn's, were not accomplished at Leipzig (1895-1902).

Kiesow's classical studies of *taste* (1894-1898) were begun at Leipzig, but the problem of smell was not attacked directly.

There should also be mentioned here the classical work on the *time-sense,* the perception or estimation of temporal intervals, by Kollert, Estel, and Meumann (1881-1896). Here, as in the preceding instances, the range of dates shows how a problem sel-

dom completely disappeared, but would be reinvestigated later in the light of criticism and the advance of knowledge.

Next to sensation and perception, the topics connected with the *reaction experiment* made the greatest claim upon the attention of the Leipzig laboratory. This work seemed for a while to be the great discovery of the 'new' psychology, because it appeared to give rise to a chronometry of the mind. If the 'muscular reaction,' it was argued, involves perception of the stimulus but not its apperception, and if the 'sensorial reaction' is like the muscular except that the stimulus is apperceived, then the additional time required by the sensorial reaction must be the time that apperception takes. Since the sensorial reaction with practised subjects nearly always takes just about one tenth of a second longer than the muscular, there seemed to be considerable precision in the statement that apperception in such a case occupies this tenth of a second. This 'subtractive procedure' was then applied to other cases, where the reaction was further complicated, with the result that times for cognition, discrimination, will, and association were made out, although these times were less precisely defined than the time of apperception. The method seemed to possess almost unlimited possibilities, and it must have seemed a wonderful thing to be able to measure, in thousandths of a second, mental processes, links in the mental chain of events, when such functions of the mind had always resisted experimentation. What an answer to Herbart, who said that mind could not be experimented upon! Unfortunately the promise of the method was not realized, for the constancy of the measured times was not great, and introspection showed that in a more complicated reaction the entire conscious pattern is changed, and that the alteration is not merely the insertion of another link in a chain.

It is, however, no disgrace to the Wundtian laboratory that it investigated this method completely, for the result might have been different. There were important papers on the times of different mental processes by Friedrich, Merkel, Cattell, L. Lange, Martius, Titchener, and Kraepelin (1881-1894). Lange's classical paper showing that the difference between the sensorial reaction and the muscular reaction (and therefore the explanation of the old problem of the absolute personal equation) is to be laid at the door of the predisposing attention (1887), did much to stimulate the movement, and also to direct interest from reaction to atten-

tion. Naturally there was work upon the variation of reaction time with different sense-departments, and with different sensory intensities. The later significance of the problem was shown by Alechsieff's study of 1900.

In the field of *attention,* there were researches on the complication experiment, the range of attention, and the fluctuation of attention, all classical topics which appeared to represent the yielding of vague mental functions to the rigor of experimentation. The complication experiment was the oldest; as we have seen, the name came from Herbart and the experiment from the astronomers' troubles with the relative personal equation. The facts of attentional accommodation and of prior entry were the results of the work accomplished by von Tschisch, Pflaum, and Geiger (1885-1902). Dietze's study of the range of auditory attention (1884) is important because it was at the basis of Wundt's conception of attention as bidimensional, as embracing, not only simultaneous, but also successive events. Later Eckener, Pace, Meumann, and Marbe described the fluctuation of weak stimuli (1892-1896), results that were interpreted by Wundt as meaning that attention fluctuates.

The experimental study of *feeling* at Leipzig was entirely the work of the '90's, the decade in which Wundt developed his new tridimensional theory which the laboratory was called upon to support. On the introspective side, the most important research was Cohn's development of Fechner's method of impression as the well-known method of paired comparisons (1894). Later there were half a dozen researches on the method of expression, relating changes of pulse, breathing, muscular strength, and so on, to correlated feelings (1895-1903). Most of these papers sought to support Wundt's new theory, and are now seen, with the theory, to have failed.

While there was almost as much work done at Leipzig on *association* as on feeling or on attention, the results were less significant. None of the Leipzig studies of association is 'classical' in the sense that it fixed another form of investigation for some time to come or represented the first work on an important section of the psychological subject-matter. Trautscholdt's study of the statistics of association (1882) is perhaps best known because there have been so many later classifications of association and because it was cited by Wundt in the succeeding four editions of the

Physiologische Psychologie. The other studies on tonal memory, recognition, practice, mediate association, and the course of association (1886-1901) made little impression as compared with the effective research upon memory by Ebbinghaus and G. E. Müller that belongs to the same period.

It is fashionable nowadays for psychologists to complain about the narrowness of the Wundtian psychology, and even at times to regret their heritage. Almost all the new schools have been founded as a protest against some one or other characteristic of Wundt's psychology, but we may welcome the new schools without condoning the complaint. At any one time a science is simply what its researches yield, and, in the author's view, the researches can be nothing more than those problems for which effective methods have been found. Those who declare that a scientific psychology must undertake the solution of such problems as emotion, thought, will, intelligence, and personality, and that an experimental psychology which does not deal with these problems is not a complete psychology at all—those persons lose sight of the fact that the research in any science must arise out of available methods. They have gone either to common sense or to the classical philosophical psychology for their intuition of the scope of psychology and have then sought to say that scientific method must be applicable to tasks that are thus imposed from without. In the first days of the Wundtian laboratory, experimental psychology was no more nor less than the results that the laboratory yielded; and that is our heritage, whether we like it or not. Since then, new methods have been discovered, research has ensued, and the scope and meaning of psychology have been changed. Intelligence, for example, proves to have scientific meaning, because there is a method for it. Will, on the other hand, still lacks scientific significance because there has never been any experimental research that sufficiently defined the concept.

For all this, there is some evidence that the historical weight of the Wundtian psychology has, because of its priority, been more influential than the mere mass of its discovered facts would require. It is not many years since the chapters of any ordinary textbook of psychology were very largely the same as the fields of research reported in the *Philosophische Studien.* Only recently has the composition of the psychological text tended to change. It might have changed more rapidly had not the 'founder of

experimental psychology' made the line between orthodoxy and heterodoxy so pronounced.

Notes

For Wundt's life, see E. B. Titchener, Wilhelm Wundt, *Amer. J. Psychol.*, 32, 1921, 161-178, 575-580; W. Wundt, *Erlebtes und Erkanntes*, 1920. Except for the first two pages, Titchener writes of Wundt's intellectual development as a carrying-out of the program laid down in his *Beiträge* of 1862. Wundt himself gives psychological reminiscences, dealing more with the psychology of his life than with the objective fact; nevertheless, by careful reading much of his biography up to the end of the century can be pieced together. Perhaps the accounts are not so incomplete as they seem: Wundt's life was quiet and uneventful as against the affairs of the world, the life of a *Gelehrter* where all the noteworthy occurrences are mental. There is also G. S. Hall's account in *Founders of Modern Psychology*, 1912, 311-458; it is clear that this biography is inaccurate in some details, and Wundt himself has characterized it as "von Anfang bis zu Ende erfunden" (*Erlebtes und Erkanntes*, 155). Some very vivid and anecdotal reminiscences of Wundt by seventeen of his American students are given in *Psychol. Rev.*, 28, 1921, 153-188.

The importance of Wundt's discussion of the personal equation with the astronomers in 1861 at Speyer has not been worked out. See Titchener, *Amer. J. Psychol.*, 34, 1923, 311. Titchener here refers to the two-line note on p. xxi of Wundt's *Beiträge* (*op. cit., infra*) as highly significant, but the relationship is not obvious. Titchener does not mention the fact that Wundt himself noted the importance of the event in the preface of the first edition of the *Physiologische Psychologie*, 1874, p. v.

For Wundt's own statement as to why he called a predominantly experimental journal of psychology *Philosophische Studien*, see that journal, 1, 1883, 615-617.

Wundt's writings were much too extensive for a list of all the more important ones to be given here. There is a complete bibliography, by his daughter, Eleonore Wundt, *Wilhelm Wundts Werk*, 1927, published as No. 28 of the "Forschungsinstitut für Psychologie," in *Abhandl. d. sächs. staatl. Forschungsinstitut*. This bibliography includes even obscure minor articles and contains over 500 titles. It gives also the titles of Wundt's lecture courses in every semester from 1857 to his last lecture in 1917. More accessible but less complete is the bibliography by Titchener *et al.*, *Amer. J. Psychol.*, 19, 1908, 541-556, with seven supplementary lists in *ibid.*, vols. 20-25 incl., and 33.

The important psychological and philosophical books (not physiological), mentioned in the text, are in their various editions as follows:

Beiträge zur Theorie der Sinneswahrnehmung (1858-1862), 1862.

Vorlesungen über die Menschen- und Thierseele, 1863; 2d to 6th editions, all revised but the fifth, and the second greatly revised, 1892, 1897, 1906, 1911, 1919. English translation of second edition by J. E. Creighton and E. B. Titchener, 1894.

Grundzüge der physiologischen Psychologie (1873-1874), 1874; 2d ed., 1880; 3d ed., 1887; 4th ed., 1893; 5th ed., 1902-1903; 6th ed., 1908-1911. All editions were revised and expanded. The first was a single volume; the second, third, and fourth were two volumes; the fifth and sixth were three volumes.

Logik, 1880-1883; revised editions in 1893-1895, 1906-1908, 1919-1921.

Ethik, 1886; revised editions in 1892, 1903, 1912.

System der Philosophie, 1889; revised editions in 1897, 1907, 1919.

Grundriss der Psychologie, 1896; revised editions in 1897, 1898, 1901, 1902, 1904, 1905, 1907, 1909, and 1911, and five unaltered editions later. English translation of first edition by C. H. Judd, 1896; also for some of the revisions.

Völkerpsychologie; vol. I, 1900, revised, 1904, revised and expanded into vols. I and II, 1911 and 1912 respectively; vol. II, 1905-1906, revised and expanded into vols. III and IV, 1908 and 1910 respectively; vols. V to X, 1914, 1915, 1917, 1917, 1918, and 1920, respectively.

Einleitung in die Philosophie, 1901; eight subsequent impressions up to 1922.

Einführung in die Psychologie, 1911; three later impressions up to 1918. There is an Eng. trans. by R. Pintner, 1912.

Wundt's penchant for writing can be statisticized, though one must not lose one's sense of humor in so doing. His daughter's bibliography cites 491 items, where an 'item' is taken as any writing, from one of less than a single page up to the entire 2,353 pages of the last edition of the *Physiologische Psychologie*. If we exclude mere reprinted editions, but include all the pages of every revised edition, the adding-machine shows that Wundt in these 491 items wrote about 53,735 pages in the sixty-eight years between 1853 and 1920 inclusive. In spite of all the many one-page items, Wundt's average adventure into print was about 110 pages long, with over seven such adventures in the average year. If there are 24,836 days in sixty-eight years, then Wundt wrote or revised at the average rate of 2.2 pages a day from 1853 to 1920.

A very great deal has been written about Wundt's systematic views, psychological and philosophical. Any large library is likely to contain a dozen or more German dissertations about him. The most useful books are E. König, *W. Wundt, seine Philosophie und Psychologie*, 1901, and R. Eisler, *W. Wundts Philosophie und Psychologie*, 1902. O. Passkönig, *Die Psychologie Wilhelm Wundts*, 1912, is a less satisfactory resumé in the form of an abstract of his entire psychology. Th. Ribot in *German Psychology of To-Day*, 1886, dealt with Wundt, but that was before half of Wundt's life as a psychologist had been lived. Hall, *loc. cit.*, has a long account of Wundt's views, the histories of psychology mention him repeatedly, and there is also H. Höffding, *Moderne Philosophen*, 1905, Eng. trans., 1915, 3-37, considering Wundt thus briefly; none of these accounts is, however, satisfactory. Wundt resisted epitomization, partly by the mere bulk of his writing, and further by his capacity for integration, which results in the fact that the complete doctrine for any single concept is to be got only from a variety of books and a greater variety of chapters, i.e., from all the various loci where Wundt thought the concept was in context. Without considering articles at all, the last editions of the important books listed above total more than 13,000 pages. Yet these books are of different dates, and, if the summarizer attempts a genetic account of Wundt's thought, he must go to the successive editions, and not only to the last. It is no wonder then that Wundt's psychology and philosophy still lack an adequate summary.

On the nature of Wundt's system and the thesis that his life may be regarded as the carrying-out of the program that he laid down in the *Beiträge*, see E. B. Titchener, *Amer. J. Psychol.*, 32, 1921, 161-178. On Wundt's system as contrasted with Brentano's, see Titchener, *ibid.*, 108-120.

On the psychological systematic fundamentals, the nature of mind, the distinction between psychology and physics, and in particular the theory of ac-

tuality, see Wundt, *Physiologische Psychologie*, 6th ed., 1911, III, 733-738; *Logik*, 4th ed., 1921, III, 257-265. On the theory of actuality, cf. Eisler, *op. cit.*, 40-49; O. Külpe, *Einleitung in die Philosophie*, 1895, or Eng. trans., sect. 23. In general, see Wundt, *Philos. Stud.*, 4, 1887, 292-309; 12, 1894, 149-182; Eisler, 29-35. On Wundt's doctrine of analysis and elements, see Wundt, *Philos. Stud.*, 1, 1883, 473-494; E. H. Hollands, *Amer. J. Psychol.*, 16, 1905, 499-518; 17, 1906, 206-226.

On psychophysical parallelism, see Wundt, *Physiologische Psychologie*, 1911, III, 739-754; *Logik*, 1921, III, 249-257; cf. Eisler, *op. cit.*, 46-51; König, *op. cit.*, 110-119.

On psychic causality and the specific laws, see Wundt, *Philos. Stud.*, 10, 1894, 1-124; *Physiologische Psychologie*, 1911, III, 755-770; *Logik*, 1921, III, 266-288; cf. Eisler, *op. cit.*, 51-58; König, *op. cit.*, 103-107, 120-127, 156-160. At this point the reader may do well to refer again to the discussion of Hume's consideration of the nature of causality in chap. 10. On the Mills and mental chemistry, see chap. 11.

On association and apperception, see Wundt, *Physiologische Psychologie*, 1911, III, 492-554; cf. Eisler, *op. cit.*, 58-69. On the doctrine of association, see also Wundt, *Philos. Stud.*, 7, 1891, 329-361. On the doctrine of apperception, see also König, *op. cit.*, 127-134; W. B. Pillsbury, *Amer. J. Psychol.*, 8, 1897, 315-392.

The development of Wundt's theory of feeling is seen in the *Physiologische Psychologie*, chap. 10 of the first four editions, chap. 11 of the fifth and sixth editions. It is these last two editions that give the new tridimensional theory, which first appeared in the *Grundriss der Psychologie*, 1896, or Eng. trans., sects. 12-13. On Wundt's doctrine of feeling and its development, see Titchener, *Amer. J. Psychol.*, 19, 1908, 213-231. On the tridimensional theory, see Hollands, *ibid.*, 17, 1906, 206-226.

The student of Wundt's system will not have to stop his reading with the references of the five preceding paragraphs; these citations are merely keys to the topics mentioned, and every one of them leads on elsewhere in Wundt.

The discussion in the text of the work of the Leipzig laboratory is based upon an analysis of 109 papers in the *Philosophische Studien*. Nearly all of these studies were made in Leipzig under Wundt's personal direction. A few are by Wundt's students after they had gone out from Leipzig to other universities, but these papers are obviously inspired by the Leipzig atmosphere. There are just a few other exceptions. Martius was at Bonn, but his research is integrated with Leipzig. M. F. Washburn was Titchener's first doctoral candidate at Cornell, after Titchener had gone from Leipzig to Cornell. Külpe went from Leipzig to Würzburg and Dürr was his student there; Marbe came later to Würzburg from Leipzig.

The list of names in the text gives some idea of how many important psychologists were trained with Wundt in the early days of the Leipzig laboratory. I do not know where there is a record of all of Wundt's students, but the following list gives the names of most of the important European psychologists who worked with Wundt up to and including 1900. I cite the names approximately in the chronological order of their first association with Wundt: E. Kraepelin (d. 1926, Munich), H. Münsterberg (d. 1916, Harvard), A. Lehmann (d. 1921, Copenhagen), O. Külpe (d. 1915, Munich), A. Kirschmann (Leipzig), E. Meumann (d. 1915, Hamburg), K. Marbe (Würzburg), F. Kiesow (Turin), G. F. Lipps (Zürich), G. W. Störring (Bonn), F. Krueger (Leipzig), W. Wirth (Leipzig).

America was close behind Germany in adopting the new psychology, and it also took its cue from Wundt. Stanley Hall visited Leipzig in the first year of Wundt's new laboratory, and

founded in America, six years after Wundt began the *Philosophische Studien*, the *American Journal of Psychology*, thus the second journal of experimental psychology in history. The proportion of Wundt's students from America was very large. Cattell was his first assistant. The following list, arranged chronologically like the other, is, I think, almost complete for Wundt's American students before 1900: G. S. Hall (d. 1924, Clark), J. McK. Cattell (New York), H. K. Wolfe (d. 1918, Nebraska), E. A. Pace (Catholic University), E. W. Scripture (Vienna), F. Angell (Stanford), E. B. Titchener (d. 1927, Cornell), L. Witmer (Pennsylvania), H. C. Warren (Princeton), H. Gale (Minnesota), G. T. W. Patrick (Iowa), G. M. Stratton (California), C. H. Judd (Chicago), G. A. Tawney (Cincinnati). Lest the comparison of the nearly complete American list with the highly selected European list imply an incorrect ratio between the two, it may be well to mention that, of the 122 authors who contributed original articles to the *Philosophische Studien*, only thirteen were Americans.

Chapter 16

BRENTANO, STUMPF, AND G. E. MÜLLER

IT is much too simple a picture to suppose that Wundt's promotion of the new psychology means that all the lines of experimental psychology have diverged from Wundt. It is true that they all come from Germany, but within the German circle there were other men who did not acknowledge descent from Wundt, and other schools which did not come out of Leipzig. First and foremost there was Brentano, the father of the modern school of act psychology. The Aristotelian Brentano might almost be omitted from a history of experimental psychology, were it not for the fact that many experimental movements are marked by his influence, and for the further fact that the act psychology of Brentano represents the opposition to the content psychology of Wundt, and that each thus influenced the other, as antagonists do. Brentano was closer to Lotze than to Wundt. Stumpf was a pupil of Lotze and Brentano, and showed Brentano's influence in his systematic views. Nevertheless Stumpf belongs within experimental psychology. Ultimately at Berlin he became identified with the experimental school that was, perhaps, Leipzig's chief rival. There was, however, also G. E. Müller, Stumpf's contemporary and another pupil of Lotze's, who succeeded Lotze at Göttingen. In point of view and in the nature of his experimental work, Müller resembles Wundt; still he was not a Wundtian. The Göttingen laboratory thus forms an old experimental tradition, an alternative, as it were, to Leipzig within the school of content psychology. If we devote the present chapter to these three men, we shall have established the general pattern of the new psychology in its youth in Germany.

Franz Brentano

Franz Brentano (1838-1917), a descendant of an ancient Italian family, was born at Marienberg on the Rhine, where a branch of

the family had settled long before. From early youth he was destined for the priesthood, for which his training began when he was about sixteen years old. He went first to Berlin to study philosophy, and there Trendelenburg taught him to appreciate Aristotle, who came thus to be the dominating influence throughout the rest of his life. A year or so later, in 1856, Brentano went to Munich, where he came under the influence of Döllinger, a Catholic historian and theologian, who was subsequently excommunicated because of his criticism of the Church. This latter fact is interesting in view of Brentano's later difficulties with the Church. Finally he went to Tübingen where he took his degree in philosophy in 1864, completing a decade of preparatory study. In the same year he was ordained a priest at Graz and entered a Dominican convent. He had already published a paper (1862) on the manifold meanings of Being in Aristotle, and had dedicated it to Trendelenberg.

In 1866 Brentano left the convent and became *Dozent* at Würzburg, where he was to remain for seven years and throughout the first of the two crises of his life. Here he lectured on philosophy and published half a dozen papers dealing with the history of science within the Church and with the philosophy of Aristotle. For two years (1865-1867) Stumpf, ten years the junior, was Brentano's pupil at Würzburg. Brentano then sent Stumpf to Lotze at Göttingen for his doctor's degree, but Stumpf thereafter returned to Würzburg for another year of study (1869-1870). It was thus by way of Brentano that the indirect influence of Aristotle finally turned up in the Berlin laboratory.

While Brentano was *Dozent* at Würzburg, there arose within the Catholic Church a controversy concerning the dogma of the infallibility of the Pope. Brentano published in 1869 a memoir in refutation of the proposed doctrine, a memoir so able that he became practically the intellectual leader of the liberal party within the Church. In 1872 Brentano, the priest, was promoted to be professor extraordinary of philosophy, but unfortunately at about the same time the dogma of infallibility was accepted by the Church. The liberal party had lost, and Brentano was placed in an untenable position, for he now came to see clearly a matter that had long troubled him, namely, that allegiance to the Church could come to be incompatible with the integrity of intellectual research. In this dilemma of conscience, Brentano

acted with scrupulousness and courage; in March, 1873, he resigned his professorship at Würzburg for the reason that he had been appointed as a priest; he then in April resigned his priesthood, in so far as he could resign it, and put off the clerical garb.

During the following year, with neither church nor university to occupy him, Brentano wrote the book by which he is best known, *Psychologie vom empirischen Standpunkte,* which was published in 1874. It is a single volume of 350 pages, intended to be but the first volume of two, although the second never appeared. This book is 'empirical' in the sense that Brentano said, "Experience alone influences me as a mistress"; it represents a philosopher's reaction toward experience and away from dogma. It is not an experimental psychology, but seeks a systematic picture in the interest of gaining one psychology to replace the many that were current. Brentano thus took account of Mill, Bain, Fechner, Lotze, and Helmholtz, all of whom he mentioned in his preface, but he often wrote of them negatively, respecting their work, yet arguing against their conclusions. The first half of Wundt's *Physiologische Psychologie* had been published in 1873 and Brentano cited it more than once. He did not agree that scientific psychology is physiological psychology; he thought that Wundt had stressed method in the term only. Psychology, Brentano argued, is a science, but one that ought not to be further qualified.

The year 1874 is important in psychology because it is the date of Brentano's book and of the completed first edition of Wundt's handbook. These two books both represent attempts to formulate the 'new' psychology, and to formulate it as a science. The contrasts, however, interest us more than the similarities. Brentano's psychology was empirical but not experimental; Wundt's was experimental. Hence Brentano's method was argumentative and Wundt's was descriptive in intention, although he dropped more into argument, as we have seen in the preceding chapter, than might have been expected. Brentano organized his system about the psychical act; Wundt built his about sensory contents. We shall have more to say of the act presently.

Although Brentano used the leisure gained by his twofold resignation to write his *Psychologie,* it was naturally not agreeable to him, a man thirty-six years old and conscious of his intellectual power, to remain without connection with either church

or university, without a formal medium for his service of "the higher interests of mankind," as he once described his mission. Lotze, who was just the sort of person to be concerned about Brentano's predicament, interested himself on Brentano's behalf, and presently, still in 1874 and with Lotze's aid, Brentano secured appointment as a layman to the professorship of philosophy at Vienna. This successful resolution of the first crisis in Brentano's life was, however, terminated only six years later by the second crisis.

Brentano fell in love with a Catholic, who could not in Austria contract a marriage with a man who had been formerly a priest. Once again, then, Brentano resigned his professorship, this time in order that he might assume Saxon citizenship. Then he was married in Leipzig, and returned immediately to Vienna, where he was appointed to lecture in the university. All this happened in 1880.

In 1894 his wife died. Disheartened by this event and by the onset of ill health, he resigned his university post. A disease of the eyes threatened the loss of his sight. For more than a year he tried first one place of residence and then another in Switzerland and in Italy. Finally in 1896 he settled in Florence. During the next nineteen years of retirement he was writing, mostly on philosophy and less on psychology. His eyes were growing worse, and an operation on them in 1903 failed. However, ill health never weakened his conscience nor his power to act upon scruples: in 1915 he left Florence for Zürich because he was a pacifist and Italy had entered the World War. His health declined further at Zürich, and he died there of appendicitis in 1917 at the age of seventy-nine.

It is plain that Brentano was primarily a person. Wundt was, in a sense, an institution that prevailed in part by the vigor and mass of its production. Wundt was the first professional psychologist. Brentano was not a professional psychologist. He was a courageous idealist with a mission to seek truth by way of untrammeled research. The volume of his productivity was not great. His bibliography lists but thirty-eight publications during his life, and only eight of these are important for psychology. The first was the famous *Psychologie* written in the interval between Würzburg and Vienna. It was almost two decades before he again published strictly psychological matter. Then, shortly before he

finally left Vienna, he wrote three articles on optical illusions, which attracted attention because this was the period when interest in illusions was at its height. After he had settled in Florence, he read a paper on the doctrine of sensation at the Munich Congress in 1896 and published an article on the quality of tones in 1905. Then in 1907 there appeared a small, but important, book on *Sinnespsychologie,* Brentano's second psychological book. Finally, in 1911, he published *Von der Klassifikation der psychischen Phänomene,* an equally important book, a supplement to the *Psychologie* or, as it may be regarded, a substitute for the missing second volume of the *Psychologie,* then thirty-seven years belated.

Brentano's influence is to be explained in part by his personality, in part by the remarkably effective and trenchant character of the little writing that he produced, and in part by the historical accident that it was he who deflected the light of Aristotle into the realm of modern psychology. He thus stood in a sense at the beginning of that full half of modern German psychology that is empirical, but not wholeheartedly experimental, and that we call the act school.

Since Brentano, living in the days when experimental psychology had arrived, was nevertheless not an experimentalist, we need further only to characterize his act psychology, in order that we may later be able to understand those compromises which experimentalists made with it.

We have already seen that 'empirical' psychology is not 'experimental' psychology. Brentano had respect for the results of experimentation, but he believed that all this stressing of experimentation led to an overemphasis upon method, and a failure to see the psychological wood for the methodological trees. In this view he resembled William James and, to a lesser extent, all the philosophers who seek to interpret experience. The difference here involved is presumably one of temperament: the philosopher is so keenly interested in the broad interpretation of nature that he lacks the patience for the labor of experimentation which limits his vision for the time being to details. Thus Brentano, in arguing about the optical illusions, was quite ready to draw new forms of old illusions and so pictorially to submit his case on the printed page to the experience of the reader: this is the empirical method in concrete form, the *experimentum crucis.* But

Brentano never undertook to measure the amounts of illusions under different conditions by the psychophysical methods; this course would have been the experimental method, and would have yielded more precise information about the points in question. The *experimentum crucis* belongs in an argument and is thus apt to be part of the empirical method. Systematic experimentation yields precise description and is the *sine qua non* of the experimental method.

A fundamental test of a system of psychology is the manner in which its author distinguishes psychology from physics. Brentano defined psychical phenomena by their possession of *immanent objectivity*. Phenomena possess immanent objectivity when they refer to a content—are directed upon an object, have that object 'inexisting intentionally' within them. These phrases become intelligible only when it is realized that psychical phenomena are to be thought of as *acts*. When one sees a color, the color itself is not mental. It is the seeing, the act, that is mental. There is, however, no meaning to *seeing* unless something is seen. The act always implies an object, refers to a content. The color as content of the act, 'seeing,' thus 'inexists' by intention within the act. A psychical act is therefore not self-contained, but contains its object within itself intentionally; that is to say, it is characterized by immanent objectivity. Physical phenomena, on the other hand, are self-contained because they do not refer extrinsically to objects. Superficially, the difference between psychology and physics seems to be that between act and object; fundamentally, however, this difference lies between the possession of intention or reference by the psychical act and of intrinsic completeness by the physical phenomenon.

It is important to note that physics and psychology are thus related, because it is to the contents of physics that the psychical acts refer. Moreover, Brentano transfers to physics the contents that Wundt, for example, treated as psychological. 'I see a color.' The color is not mental as sensation, but is in itself physical, though it exists in intention within the act of 'seeing.'

We have seen in earlier chapters that with the associationists the problem of matter became the problem of perception. So with Brentano the problem of meaning disappears as a separate problem, because all psychical phenomena refer to content as part of their ultimate nature.

Brentano divided the acts into three fundamental classes: the acts of *ideating* (sensing, imagining), the acts of *judging* (like acknowledging, rejecting, perceiving, recalling), and the psychic phenomena of *loving* and *hating* (like feeling, wishing, resolving, intending, desiring). The object of an act may be another act, and it does not change the other act for it to become an object of an act. Thus the argument produces an involved system, but the details do not matter to us. We have here enough of Brentano to explain Stumpf's systematic views, to justify Külpe's beliefs in imageless thoughts, and to account for the psychologies of Witasek and Messer that compromise between Brentano and Wundt by letting acts and contents live together in the same system.

Carl Stumpf

Carl Stumpf (1848-) was born in the village of Wiesenthied in that province of Bavaria which contains Würzburg. His father was the provincial court physician and his mother's father was also professionally engaged in medical jurisprudence. This grandfather spent the years after his retirement in Stumpf's home, and was responsible for much of Stumpf's education before and even after he entered the *Gymnasium*. He grounded Stumpf in Latin. Physicians visited the house, and three of them, whom Stumpf came to know, were university professors. Thus the boy came in constant contact with medicine and natural science. Music, however, was his great love. He began the violin at the age of seven, and in the course of the next ten years learned five other instruments. At ten he began to compose, even writing the words and music for an oratorio for three male voices; and when eleven years old he went away to a *Gymnasium* for four years. Then, when his family moved to Aschaffenburg, he attended the *Gymnasium* there for two years more. Thus the first seventeen years of his life were spent in a normal educational development, supported by a home background of science and music—especially music.

In 1865 Stumpf went to the neighboring university of Würzburg. His dominating passion was for music, but he could not take a degree in music. In the first semester he tried esthetics, which led him toward philosophy; in the second semester he took up law in order to have a profession that would leave him time

for music. Then, at the end of this semester, Brentano was habilitated as *Dozent* at Würzburg. Immediately Stumpf was captured by the vigor of Brentano's personality and the keenness of his thought, and from then on Stumpf became his disciple. Brentano was merciless in the rigor of his thinking, and Stumpf for the first time found himself being disciplined in careful thought. At that time Brentano was absorbed by his aspiration for a philosophical and religious renaissance of Christianity, and this idea became Stumpf's dominant motive for four years. Brentano held that the scientific method is the true philosophical method, so his disciple, Stumpf, undertook to learn science in the chemical laboratory, even to the extent of causing a fire in the building, for as Stumpf remarked, "manual dexterity did not belong to my capacities." (The scientific nineteenth century bred a philosophy that turned to science for its method, and it is an historical commonplace to find its young philosophers attempting to practise science, not because they were experimentally minded—they were not; they were philosophers—but because they wished to put into practice the philosophical belief about method.)

Stumpf had one year with Brentano; then his new master sent him to Lotze at Göttingen for his degree. It was to Lotze, the philosopher, not Lotze the psychologist, that Stumpf went. The *Medicinische Psychologie* was then fifteen years old, and Lotze had by that time finished the *Mikrokosmos*. Lotze took a fatherly interest in Stumpf and grounded him more thoroughly in the theory of knowledge. Stumpf also pursued his science, working in physiology with Meissner, and in physics with Wilhelm Weber, the brother of E. H. Weber. He received his doctor's degree in the summer of 1868.

Then Stumpf returned to Würzburg for two more years of study. He was still under the dominion of Brentano's conception of the new Christianity; just at the time, it was, that Brentano was writing the famous memoir against the dogma of the infallibility of the Pope. Stumpf schooled himself in theology and scholastic philosophy, until in 1870 he returned to Göttingen to be habilitated as *Dozent* with a dissertation on the mathematical axioms—a topic to which Helmholtz had already appealed in his defense of empiricism directed against Kant.

Stumpf was *Dozent* at Göttingen for three years. He learned to know Lotze well. He became acquainted with E. H. Weber,

when Weber was visiting Göttingen from Leipzig, and Weber demonstrated the sensory circles on Stumpf's own skin. He met Fechner in Leipzig in the decade of Fechner's interest in esthetics, and served as an observer for him in the experiments upon the golden section. He played the cello in a little private orchestra. He began work on a critical history of the concept of substance, but laid the task by as fruitless when in 1872 the idea of a study of the origin of space-perception struck him. This idea proved fertile, and Stumpf published his first psychological book, *Ueber den psychologischen Ursprung der Raumvorstellung,* in the spring of 1873. It was a nativistic theory (Hering's nativistic views had been public property since 1864), and argued that both color and extension are equally primitive part-contents of visual sensation. There was a practical timeliness about this publication, for five chairs of philosophy were becoming vacant, and Stumpf used the work as a basis for his application for a better post. He just missed appointment at Vienna, but he was successful at Würzburg, where Brentano had just resigned, and where he was supported by both Brentano and Lotze. Thus in 1873 for the third time he went to Würzburg, but this time he went as professor. During his last year at Göttingen, G. E. Müller was also one of Lotze's students, but there seems to have been little contact between Stumpf and Müller.

The next two decades of Stumpf's life were to bring many geographical changes. He spent six years at Würzburg, five at Prague, five at Halle, and five at Munich, and then finally went to Berlin in 1894.

The years at Würzburg saw Stumpf turning seriously to psychology, but not from philosophy. Philosophy was still "mistress of the house," but, if science were to furnish the method for philosophy, Stumpf conceived that scientific psychology might well be cultivated in the interests of philosophy. In this view he was still following Brentano, whose *Psychologie* had now appeared. He thus became interested in the psychology of association, especially in Mill, for he thought he could use this doctrine in the interest of the concept of substance. Presently, however, it was his love of music that determined him. For a decade philosophy had outrivaled music in Stumpf's life, but now it occurred to him that he could serve philosophy by way of the psychology of music. In 1875 he began work on the *Tonpsycholo-*

gie, turning for the first time to psychological experimentation. Stumpf then expected to spend but a few years with the psychology of tone and music and not to devote his life to it, as eventually he did. What really happened was that Stumpf had now found a way of integrating his early inborn love of music with his acquired love of philosophy, and, with the conflict resolved, was on the way to becoming effectively productive. He published very little while at Würzburg, but his great work was already begun.

In 1879 he went, again with Brentano's support, to the chair of philosophy at Prague, replacing the Volkmann who had written the *Lehrbuch der Psychologie* in 1875-1876 (not Fechner's physiological friend, but the Herbartian). At Prague were both Mach and Hering, and Stumpf formed an intellectual contact with the former and a friendship with the latter. William James visited Stumpf at Prague in 1882 (as he did again later in Munich), and a friendly correspondence between the two men began. James had been attracted by Stumpf's book on space-perception. Although worried by the lack of laboratory equipment, Stumpf continued work on the *Tonpsychologie* and finally published the first volume in 1883.

In 1884 Stumpf, anxious to return to Germany, was appointed professor at Halle. A little later Husserl, recommended to him by Brentano, became first his pupil and then *Dozent.* At Halle Stumpf began his investigations of primitive music, but his principal task was the completion of the *Tonpsychologie.* The second volume of this work deals with the combinations of tones, and thus reports the results of Stumpf's famous experiments on tonal fusion, historically the most important outcome of the two volumes. These experiments Stumpf, still lacking university equipment, was able to perform on the cathedral organ. He was publishing little but working persistently.

Then in 1889 Stumpf went to Munich, the fifth university at which he had held an appointment. In a sense, he had here at last a laboratory, although a very small one. It consisted of the attic floor of a high tower and a cabinet in the hall, where he kept tuning-forks that he could use in the lecture-room on Sunday. He got a continuous series of forks by purchasing an old tuning-fork piano from a janitor in the physical institute, and disassembling it. These forks helped to complete the *Tonpsychologie,* and the second volume appeared in 1890. The next year Stumpf

criticized at length the work of Lorenz on tonal distances. Lorenz had worked with Wundt at Leipzig, and Wundt had made the results his own in the latest edition of his *Physiologische Psychologie.* A violent and acrimonious controversy ensued between Wundt and Stumpf, in the course of which each published thrice. Stumpf of course began it by his criticism of Lorenz, but it was Wundt who made it personal. The clash seems to have arisen because Stumpf leaned heavily upon his own musical sophistication, while Wundt relied on the laboratory results with apparatus and the psychophysical methods. Whatever is obtained under unprejudiced, carefully controlled experimental conditions must be right, Wundt virtually said. If the laboratory yields results that are obviously contrary to expert musical experience, they must be wrong, was Stumpf's rejoinder. Stumpf retorted reluctantly to Wundt's personal invective, and always remembered the experience bitterly.

In 1894 Stumpf's academic wanderings were ended by his appointment to the chair at Berlin. Ebbinghaus had been at Berlin as *ausserordentlicher Professor* and had started a laboratory there. For some reason or other Ebbinghaus was not advanced and went to Breslau shortly after Stumpf came. Stumpf therefore had the most distinguished appointment that Germany could offer. As the world sees these things, Wundt was undoubtedly then the foremost, as well as the senior, psychologist. His writings were already enormous; he had founded the first and leading laboratory; the *Philosophische Studien* was past maturity, whereas the psychologists outside of Wundt's dominion had only just begun, under Ebbinghaus's leadership, the *Zeitschrift für Psychologie.* It has been said that the great and influential Helmholtz opposed Wundt's appointment at Berlin. However this may be, the chair fell to Stumpf, and he now had a small laboratory and a large future. He also found himself in what he regarded as the most musical city in the world and the most intellectually stimulating university in Germany.

With this change, Stumpf's psychological capacities burst into productivity. The next thirty years saw more than five times as much publication as the previous thirty. He was drawn into many activities—"often all too many," he said. The laboratory expanded from three dingy rooms to a large and important institute. With Lipps he was joint president of the International Congress

of Psychology at Munich in 1896, the only one of the nine congresses that has been held in Germany. In 1897 he was persuaded to undertake a study of a four-year-old prodigy, a research similar to those on musical prodigies that he undertook some years later. In 1900 he was the joint founder of the Berlin *Verein für Kinderpsychologie.* He had considered the writing of a third volume of the *Tonpsychologie,* but instead he began the *Beiträge zur Akustik und Musikwissenschaft* (1898 *et seq.*). In 1900 he founded the phonogram archives for records of primitive music, an enterprise for the development of which much of the credit belongs to von Hornbostel. In 1904 he was diverted into the investigation of "der kluge Hans," the educated horse that turned out to be a mind-reader, responding to the small unconscious movements of his mentor. In 1907-1908 he was honored by the rectorship of the university. He retired from active work in 1921, when Wolfgang Köhler was appointed to succeed him, and discontinued lecturing in 1923. His eightieth birthday in 1928 was celebrated by the presentation of his bust to the university.

An examination of Stumpf's writings shows that they were nearly all on the problems of tone and of music. His technical contribution to psychology is thus within a narrow and limited field, and the debt of psychology to him on this account is less than it is to many other men. His greatest importance to psychology comes in a more general way.

It is apparent that Stumpf's position enhanced his influence, but it is also true that it was his ability that secured for him the position. Stumpf (for all that Wundt had said to the contrary) never forgot the lesson in keen and thorough thinking that Brentano taught him. Stumpf played an important rôle in the revision of the concepts of psychophysics, though no writing of his bears a psychophysical title. His psychophysics is to be found mostly in the first volume of the *Tonpsychologie.* In this volume also is his doctrine of attention, and his discussion of other general topics like practice, fatigue, analysis, comparison, and surrogation. His erudition is shown in his excellent article on the theory of mathematical probability which he published in 1892. He had an important and influential theory of the feelings as sensations concomitant with the other sensations to which the feeling seems to be attached, and he promulgated and defended it ably in 1907

and in 1916. He had placed himself in opposition to the James-Lange theory of emotions somewhat earlier (1899).

More important than any of these interests, except of course his persistent attack upon the problem of music and tone, are Stumpf's systematic contributions. In 1907 he published *Erscheinungen und psychische Funktionen* and *Zur Einteilung der Wissenschaften.* The content of these papers is of course far from matters of experimentation; nevertheless Stumpf affected experimental psychology by them in that he put the stamp of Brentano for the time being upon the Berlin laboratory. We cannot yet divorce experimental psychology from systematic psychology.

First, before we discuss these papers, it must be said that the new gospel of phenomenology was in the air. There can be no doubt that Husserl, Stumpf's student and *Dozent* at Halle, started phenomenology with his *Logische Untersuchungen* (1900 *et seq.*), which he dedicated "in honor and friendship" to Stumpf. Husserl had been Brentano's pupil before he came to Stumpf, and his life had been devoted to the development of phenomenology, a discipline that deals with pure consciousness by a method of immanent inspection. Neither Husserl nor any one else has thought that this phenomenology should be any one's psychology. In his psychology Husserl followed Brentano in defining psychology as the empirical science of mental facts, the science of experiences or acts, which intend the material facts, the non-experiences, the objects with which physics deals. Husserl's main interest was not psychology, but he worked out differently the view that had been Brentano's, and sometimes, in very un-Husserlian fashion, this view has been called phenomenology. The point is that the word *phenomenology* was in the air, and so were Husserl's views. They were current in Berlin. Later Külpe at Würzburg incorporated some of them into his 'bipartite' psychology of act and content. Külpe may have obtained them directly from Husserl, but it has been said that Bühler took them to him when he went from Berlin to Würzburg about the time that Stumpf was publishing these papers.

In this milieu Stumpf set about a classification of experience, of the immediately given; and he distinguished three primary classes:

1. First there are the *phenomena,* sensory and imaginal data,

like tones, colors, and images, which constitute the subject-matter of *phenomenology*. The phenomena are not the data of physics, and phenomenology is a propædeutic science (*Vorwissenschaft*) to physics and psychology, for it studies this antecedent experimental material.

2. Then there are the *psychical functions*, like perceiving, grouping, conceiving, desiring, willing. These functions are the equivalents of Brentano's acts; the two words are almost interchangeable: Brentano and Husserl said *Akt*, Stumpf and Külpe said *Funktion*. The psychical functions, of course, are dealt with by *psychology*, to which phenomenology is propædeutic.

3. The third class is the *relations*, which belong to *logology*, another propædeutic science. Relations have always made trouble in psychology. They seem to come quite immediately into experience and yet not in the same sense that sensations come; thus it was never clear to the elementarists whether they could speak of relational elements or not. Stumpf therefore left the relations in experience, and gave them a compartment of their own. It is plain that, like phenomena, they have epistemological priority over the functions, so that logology must also be propædeutic to psychology.

4. There remains finally the problem of immanent objectivity. Brentano had made the phenomena the object of the acts. In the situation *I see red*, it seems as if the red were both a phenomenon and also existent intentionally within the act. In the case of *I like red*, it is not, however, so clear that the red is a phenomenon. The independent existence of the phenomena appears to exclude their intentional 'inexistence,' not merely for formal reasons but as an actual matter of experience. For this reason Stumpf created a special class for the immanent object of the functions, and called these objects *formations* (*Gebilde*) with a special cognitive, propædeutic science *eidology*, to take care of them.

The two last groups are chiefly advantageous in getting rid of difficulties; the first two groups are of interest in themselves. According to this classification, Wundt might be caricatured as a phenomenologist, Brentano as a psychologist, and Stumpf, if judged by the content of the *Tonpsychologie*, as both. As a *Tonpsychologe* Stumpf was certainly more of a phenomenologist than a psychologist, paradoxical as the words now become. The distinction, however, had the advantage of leaving room for both

act and content somewhere, and is thus at the basis of the 'bipartite' psychologies of Külpe, Witasek, and Messer, who sought later to leave both kinds of data within psychology.

In all this discussion Stumpf by no means lost his empirical habit of mind. If the reader does not understand the difference between a psychical function and a phenomenon, Stumpf gives him examples of their independent variability. The function changes without the phenomenon, when an unnoticed phenomenon becomes noticed without change in itself, as when a musical chord or a touch blend or a taste blend is analyzed. The phenomenon changes without a change in the function, when the room gets darker at twilight without the change being noticed, or when sensations change continuously but we notice the change abruptly only at intervals of the just noticeable difference.

Formally, then, Stumpf took his stand with the act school, ruling the sensationistic phenomena out of psychology into phenomenology. Actually, however, what he did was the opposite of what he said: he brought phenomenology into psychology. In the first place he legitimatized it as an alternative to the act as a subject-matter for study. In the second place, he never got rid of it, for the reason that he was too much interested in it himself, and, say what he might, the world took Stumpf's interest to be in psychology, as he himself did in 1883 and 1890. It is not surprising then to find Stumpf's pupils at Frankfurt and later at Berlin beginning an experimental phenomenology that formed the basis for the new *Gestaltpsychologie*.

There remains for remark but one other way by which Stumpf's importance in experimental psychology is to be explained. He exerted an influence upon psychology by way of his students, some of whom have been more experimentally active than he. In this manner he has had, however, not nearly so much effect as Wundt, because Wundt was established at Leipzig almost twenty years before Stumpf terminated his wanderings at Berlin, and because Wundt had the beginnings of a formal laboratory fifteen years earlier than Stumpf. Wundt's students were already numerous and going out into positions before Stumpf had left Munich. America took its lead from Wundt because Leipzig, advertised as the new, unphilosophical laboratory of psychology, was well established when Americans had to go abroad to become psychologists. By the time Stumpf came to Berlin Americans

were just beginning to take their doctorates in philosophy at home—from Wundtians.

Of course Stumpf had pupils, but none from Berlin of the seniority of Wundt's because he settled down there so late. Max Meyer, just before he came to America, must have been one of Stumpf's earliest students for the doctorate at Berlin. Stumpf himself especially mentions his assistants, Schumann and Rupp. Schumann, who took over the *Zeitschrift für Psychologie* on Ebbinghaus's death in 1909, is an important figure. He went to Göttingen when Müller went there in 1881, took his degree, and stayed until 1894; then he went to Berlin when Stumpf went there and was Stumpf's right-hand man for eleven years until he left for Zürich and later for Frankfurt, where he now is. Rupp is best known to psychologists for his technical skill with apparatus. Nowadays Berlin is known as the primary focus of the new *Gestaltpsychologie.* The theorist can find some relation of *Gestaltpsychologie* to Stumpf, for the psychology of *Gestalt* is in a sense an attempt to resolve within experimentalism the conflict between act and content, just as Stumpf was the most phenomenological of the act psychologists. This connecting thread is, however, but slender. Köhler, who succeeded Stumpf at Berlin and is one of the leaders of this new school, came to psychology with a training in physics and, although Stumpf's pupil, seems to owe but little to his master, except perhaps a training in rigorous thought which Stumpf got from Brentano and passed on to all his pupils. There are also others of Köhler's academic generation who were Stumpf's pupils and who are playing an important rôle in present-day research.

In a word, Stumpf was primarily a philosopher; that is to say, he was a psychologist in the interests of philosophy. He was a musician by native endowment, and brought this interest to the service of his psychology. He was an experimentalist by philosophical conviction but not by temperament. Thus for a quarter of a century he regretted his lack of laboratory facilities and then, when he got them, he turned over the technical side, in which he ardently believed, to Schumann and Rupp, although he experimented himself long and patiently with the tones. His position as director of the laboratory at Berlin led him into many administrative acts that benefited experimental psychology, like the founding of the archives for phonograms and the *Verein* for child

psychology. Personally, however, he remained a psychological theorist who formulated a system of act psychology friendly to experimentalism, and a student of the psychology of music.

Georg Elias Müller

Georg Elias Müller (1850-) was born in Grimma in Saxony, not far from Leipzig. As a boy he received at the *Fürstenschule* at Grimma a humanistic education which included but little mathematics or science. However, he must have had a philosophical bent, for from this training he extracted a primary interest in philosophy when he was still but fifteen years old. This interest of Müller's was begun by his reading Goethe's *Faust,* the poetry of Byron and Shelley, and Edward Young's *Night Thoughts,* which had been so popular a century before that they had been translated into German. In this way Müller became a youthful philosopher and also somewhat of a mystic. From mysticism, however, he was rescued by the writings of Lessing, the philosophically minded poet, dramatist, and critic. Müller learned from him the value of careful, rigorous thought, as Stumpf had learned it from Brentano, and thus initiated the habits of thinking that have been one of his outstanding characteristics throughout his life.

After half a year at the *Gymnasium* at Leipzig, Müller entered the University of Leipzig to study philosophy and history, philosophy because he liked it, history because it might later gain him a post as teacher. This was in 1868, while Wundt was still at Heidelberg beginning his lectures on physiological psychology. At Leipzig Müller came most under the influence of Drobisch, who inducted him into Herbartian philosophy. In 1869 he went to Berlin to study history with the great historians there, archæology and the history of art in the museums, and the philosophy of Aristotle with Bonitz and with Trendelenburg, who had made an Aristotelian of Brentano fourteen years before. For the moment Müller's affections had swung toward history; nevertheless a study of Lotze's writings decided him for philosophy as his *Hauptfach.* Stumpf had been torn between two loves, music and philosophy, and had only much later discovered how to make the latter serve the former. Müller would have liked to make history propædeutic to philosophy, but he was prevented

from this decision by the belief of the times, which we have already met, that natural science and mathematics are the proper basis for philosophizing. The struggle between science and history was prolonged in Müller's mind, often, as he said, "until late in the night." The issue was resolved, strangely enough, by the Franco-Prussian War. Müller left his studies and enlisted in a regiment of volunteers. To the young man of twenty years, the life in the army proved an exhilarating vacation from studious routine. Then, after his military year, history seemed to him too narrow a subject to provide the perspective that he desired and he turned readily to the study of natural science, and in particular to Helmholtz's *Physiologische Optik,* which furnished to him the idea of his dissertation.

He therefore in 1871 returned to Leipzig, and in the spring of 1872 he went to Lotze at Göttingen for a year. Lotze, whose writings had decided him for philosophy when he was still at Berlin, now became a close personal friend and exercised a great influence over Müller's thought. He completed the training in precise thinking that Lessing's writings had begun, and he emphasized still further the necessity for grounding philosophy upon science. We have already said that Müller had little contact with Stumpf, two years his senior, although this was Stumpf's last year at Göttingen. At the end of it, Müller received his doctorate on his dissertation, *Zur Theorie der sinnlichen Aufmerksamkeit.*

In tracing the history of experimental psychology, we have to remember (if, for example, we say that it began with Fechner in 1860) that it did not grow up at once. There was in the '60's some psychological experimenting going on, mostly in the hands of physiologists. The philosophers, however, owned psychology; they believed that philosophy and thus psychology must depend upon scientific method, but for all of this conviction they could not make themselves into experimentalists. They could, however, be good empiricists; they could use the results of science when the results were available, and they could emphasize experience at the expense of pure reason. Thus the path from philosophy to experimental psychology lay through empirical psychology. Stumpf in 1873 published his nativistic theory of space-perception and Müller in the same year his dissertation on sensory attention. Both monographs were empirical, but not experimental, theses. Müller's discussion was keen and exhaustive (setting the standard

for his subsequent style), and it concerned a topic that has ever since resisted any generally accepted method of experimentation. So it came about that, thirty-five years later, books on attention were still citing Müller's dissertation profusely.

After receiving his degree, Müller became a tutor in Rötha near Leipzig, and later in Berlin. Then he suffered from a severe illness which obliged him to return to the home of his parents. When at Leipzig he had become acquainted with Fechner, and had since been carrying on a scientific correspondence with him. Thus it happened that, while he was convalescing from this illness, he turned his attention to psychophysics and, with his keen critical sense, to the criticism of Fechner. The result was a revision and extension of psychophysical methodology, which he presented as an *Habilitationsschrift* at Göttingen; thus in 1876 he became *Dozent* there after three years' absence. This dissertation was published as *Grundlegung der Psychophysik* in 1878 and was supplemented in the following year by an article on the method of right and wrong cases (constant stimuli). These two publications together contained many innovations in method that have since become standard in psychophysical procedure. The book was largely taken up with a discussion of the facts of Weber's law. The later paper contains the table for Müller's weights for the observed frequencies of right and wrong cases, and of course the reasons and rules for their use.

Müller remained at Göttingen as *Dozent* for four years. In 1880 he went for a year to the chair of philosophy at Czernowitz. Then in 1881 Lotze was persuaded to go to Berlin, where he died a few months later, and Müller succeeded Lotze at Göttingen. Here he was to remain for the rest of his life, retiring after forty years of continuous service. Stumpf held appointments at six universities and did not settle down until half his maturity had passed. Except for the year at Czernowitz, Müller never held a university post at any place other than Göttingen. There he added further distinction to an already distinguished chair. In succession, Herbart held it for eight years, Lotze for thirty-seven, and Müller for forty.

Thus Müller at Göttingen became in a fashion an institution, as Wundt did at Leipzig, but as Stumpf never did at Berlin. He had an excellent laboratory. Visitors have said that he was apologetic for it, but an American surveyor of the German psy-

chological laboratories in 1892 said that it was "in many respects the best for research work in all Germany." Presumably it was then second only to Leipzig. However, it is not the space and apparatus that count, but the results. Müller, with a critical acumen equal to Stumpf's, was as a psychologist less of a specialist (although as a philosopher more specialized). In the early days he kept on with psychophysics, becoming on Fechner's death the greatest psychophysicist. In the '90's he made the psychophysics of both vision and memory his subjects. He contributed little to general psychophysics after 1903, but the other two topics have remained with him throughout his life. In all these fields he took over problems from their originators, criticized them, corrected them, extended them, and centered research about them. Psychophysics he inherited from Fechner, the problems of vision from Hering, memory from Ebbinghaus. The men who worked under him at Göttingen furnish the second most distinguished group of names of German psychologists, for the students of Wundt must certainly rank first. Müller, trained in criticism by Lotze, was always theoretically minded, although he never wrote a systematic handbook, as did Wundt, nor published epistemological papers, as did Stumpf. He was purely a psychologist. Stumpf claimed always to be a philosopher who psychologized in the interests of philosophy. Wundt claimed to be a psychologist, but we have seen that the philosopher's mind dominated him. More than either of these men, Müller succeeded in leaving philosophy, his first love, behind him and in sticking to psychology. He may not have been entirely successful, but for his time he was relatively successful. As he grew older, systematic questions came to interest him more and more, as is the case with old men. In respect of system, it is plain that Müller belonged with Wundt as a psychologist of content. With a background similar to Stumpf's he ought to have been an act psychologist in belief, if not in practice. The distinction between belief and practice is, however, just the point. Psychologists do not know how to experiment upon acts: they can experiment with contents. In so far as Müller succeeded in being an experimental psychologist, he was bound to move toward Wundt and away from Brentano.

Schumann was Müller's unofficial assistant at Göttingen from the beginning in 1881 until Schumann went to Stumpf in 1894. It is not clear just what was going on in Göttingen in the '80's.

Külpe was there for a couple of years about 1884, after having been at Leipzig and Berlin for a year each and before he returned to Leipzig to take his degree and to be Wundt's assistant. Alfons Pilzecker came in 1886 and stayed at least fourteen years. He began work on attention and published in 1889, as his dissertation for the doctorate with Müller, *Die Lehre der sinnlichen Aufmerksamkeit*, a work that took its inception from Müller's dissertation of 1873 with a similar title. Müller himself was still concerned with psychophysics, for he published with Schumann on lifted weights in 1889 the research from which he concluded that the judgments of 'heavier' and 'lighter' are dependent upon the muscular preparation of the subject. It is plain that he was occupied with the problem of muscle, for in the same year his *Theorie der Muskelcontraction* saw the light. It was in 1885 that Ebbinghaus published his classical experiments on the measurement of memory, and in 1887 Müller and Schumann started in to work together on memory with Ebbinghaus's *Erlernungsmethode* (method of complete mastery). Müller was always keen to pounce upon a new method. He and Schumann continued these experiments until 1892 and published them the next year in the classical paper which includes the rules for forming nonsense syllables.

After the publication of this work, Müller immediately undertook its continuation with Pilzecker. This time he worked on the *Treffermethode* (method of right associates). Adolph Jost, another student of Müller's, in the middle of the decade was, however, the first to publish this method. His work resulted in 'Jost's law,' the law that, when two associations are of equal strength, a repetition strengthens the older more than the younger. This was Jost's theory for explaining the advantage of distributing the repetitions in time. Müller and Pilzecker went into print in 1900 in a joint monograph, developing the *Treffermethode* and showing the significance of reaction times as indicating the strength of associations.

Müller had not, however, dropped psychophysics. Lillien J. Martin was a student with him from 1894 to 1898, and they undertook the experiments that appeared later (1899) under their joint authorship as *Zur Analyse der Unterschiedsempfindlichkeit*. After Fechner's *Elemente*, this book is the classical study of the psychophysics of lifted weights, that most thoroughly investigated

psychophysical function. At the same time Victor Henri, Binet's pupil from Paris, came to Göttingen, and there performed the experiments that resulted in his *Ueber die Raumwahrnehmungen des Tastsinnes* of 1898. This book is, after E. H. Weber, the classical work on the error of localization and the two-point limen on the skin, and falls in line with Müller's study in 1879 of the method of right and wrong cases as applied to this sensory problem. Müller did more than open his laboratory to these students. Henri, although he dedicated his book to Binet, was explicit in his appreciation of Müller's ready assistance.

However, psychophysics and memory were not enough for the indefatigable Müller. At the same time that these studies were going on, he was interesting himself in the problems of vision. In 1896-1897 he published four articles called *Zur Psychophysik der Gesichtsempfindungen,* using the term *psychophysics* here in the physiological, rather than the mathematico-methodological sense. These articles present and defend Müller's theory of color vision, which is so well known now as often to be called Hering's theory, which it includes. Müller adopted Hering's theory of the three reversible photochemical substances (he held that the processes were chemical rather than metabolic as Hering had thought), and added his concept of cortical gray as the zero-point from which all color sensations diverge. On Hering's theory, when the black-white, blue-yellow, and red-green excitations are all in equilibrium, one should see nothing, a visual 'silence,' but actually one sees gray. Müller supposed, and brought some empirical evidence to bear upon his supposition, that there is a constant gray aroused by the molecular action of the cortex, a view that is much more reasonable than Hering's attempt to get around the difficulty by way of experienced composites of sensations of equal weights, in which there is no predominant component to characterize the totality in respect of color. Here also Müller dealt extensively with the problem of qualitative and intensive series, and further, in the first paper, laid down five psychophysical axioms which must underlie, he thought, the hypothesizing of physiological processes for the purpose of explaining conscious processes.

In the first decade of the new century, Müller's most important contribution to experimental psychology was undoubtedly his *Gesichtspunkte und Tatsachen der psychophysischen Methodik,*

first published in 1903. It was this book that delayed the publication of the second volume of Titchener's *Experimental Psychology,* for Titchener had finished the first volume in 1900-1901 and was ready with both parts of the second, which is concerned entirely with psychophysics, when Müller's new handbook appeared. Titchener had to take account of Müller again all through the *Instructor's Manual,* but finally published in 1905. This book of Müller's was his last word on everything in psychophysics, for he did not publish again in this field. It did not change the face of psychophysics or present any large new view; it was simply a thorough revision of the entire field and a summing-up of whither, in Müller's view, psychophysics had come.

There is little else from Müller's own hand that is of importance during this decade. He returned to his color theory in 1904 with a discussion of its bearing upon color-blindness, and in the same year (as again in 1913) to the problem of memory with his account of the mathematical prodigy, Rückle. He must toward the end of the decade have been beginning his large work on *Gedächtnistätigkeit.*

Meanwhile his laboratory was growing in influence. Narziss Ach, who was finally to succeed Müller, was his first official assistant from 1901 to 1904. In 1904 Hans Rupp, with a new doctorate in philosophy from Innsbruck, became assistant for three years, before he went to be Stumpf's assistant for fourteen. We have already seen that most of Stumpf's assistants came from Müller. Eleanor A. McC. Gamble from Wellesley was Müller's student in 1906-1907, and published afterward the classic monograph on the method of reconstruction for the measurement of memory, for which most of the research, however, was done at Wellesley before she went to Göttingen. David Katz became assistant in 1907 and held the post until 1918, almost until Müller's retirement. He had received his doctorate at Göttingen in 1906. In 1909 he published a very important paper, *Die Erscheinungsweisen der Farben,* distinguishing between the characteristics and conditions of volumic colors, surface colors, and film colors which are the more primitive and from which the others are derived. This paper was really a bit of experimental phenomenology in that it attempted description in a realm where analysis into sensational elements was inadequate. Appearing in the year before Wertheimer's inauguration of what eventually

proved to be the school of *Gestaltpsychologie,* it becomes one of the many instances in the history of science which go to show that formally new ideas are never actually new: a school cannot be founded until its principles are already in existence. Katz went to Rostock in 1919. Then there was also E. R. Jaensch, who took his doctorate at Göttingen in 1908 and stayed for two years. He published from Göttingen *Zur Analyse der Gesichtswahrnehmungen,* with a dedication to Ebbinghaus and Müller. Since 1913 he has been at Marburg, and his name is now associated with the discovery of eidetic imagery and the related classification of persons into types. Thus the problems of perception were coming to concern Müller through his students, even before *Gestaltpsychologie* had forced perception to the center of the stage.

The next decade was noteworthy because of the publication of Müller's *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes.* There were three volumes, issued in 1911, 1913, and 1917, the second volume last. The work, although of a different date in the history of experimental psychology and by an author who relied more upon formal experimentation, may be said to be for memory what Stumpf's *Tonpsychologie* was for tone. The volumes give a mass of new information, especially by way of the prodigy, Rückle, and sum up their author's views in respect of the field. Naturally Müller's thoroughness brought in theoretical topics; almost a third of the first volume is taken up with a discussion of the method of introspection. Yet Müller was not writing a system of psychology; he was holding himself to experimental work and doing only as much theorizing as the experiments required.

Phenomenology and perception would still not down at Göttingen. Müller himself in 1912 published briefly on visual localization and at length in 1915 on an explanation of the Aubert phenomenon in visual localization, a paper which later brought him into conflict with the youthful *Gestaltpsychologen* from Berlin. Edgar Rubin came in 1912 from Copenhagen for a couple of years with Müller to get his degree. He published from Göttingen his *Visuell warhgenommene Figuren,* a book that was accepted bodily as part of *Gestaltpsychologie* in spite of its source, because it was phenomenological and analyzed visual perceptions into such elements as figure, ground, and contour, instead of the more conventional sensory ultimates.

In 1921 Müller retired, apparently to devote himself to the problems of vision for a while. He has published frequently within this field during the last decade, as has also Oswald Kroh, who was his assistant in the few years remaining after Katz left Göttingen. In fact, Müller is said to be writing a *Psychophysik der Gesichtsempfindungen* now.

In 1923 *Gestaltpsychologie* called forth Müller's criticism in his *Komplextheorie und Gestalttheorie,* a book on methodology in perception that takes the position that *Gestaltpsychologie* is not new, nor does it seem to have been entirely new in view of the nature of the research on perception that had issued from Göttingen. In 1924 Müller published a little *Abriss der Psychologie,* his only attempt to treat the entire field of psychology as a whole.

In a word, then, Müller is the first experimental psychologist, among the men whom we have considered, who was little else than an experimental psychologist. He brought his philosophical acumen into his work in his logical precision and his trenchant criticism, and by avoiding philosophy and becoming a scientist, he lived up approximately to the teaching of the philosophy of his youth that science must precede philosophizing. Within experimental psychology he had a broad interest and a fertile mind. His students received from him more than their meed of inspiration and help, and through his own work and through theirs he exerted a great influence upon experimental psychology in its formative years. As a power and an institution he is second only to Wundt.

Notes

The academic wanderings of the men mentioned in this chapter will become much clearer to the reader if he refers to the map of German and Austrian universities inside the covers of this book.

Brentano

The full titles of Franz Brentano's three psychological books are: *Psychologie vom empirischen Standpunkte,* 1874; *Untersuchungen zur Sinnespsychologie,* 1907; *Von der Klassifikation der psychischen Phänomene, neue, durch Nachträge stark vermehrte Ausgabe der betreffenden Kapital der Psychologie vom empirischen Standpunkte,* 1911. The three short articles on optical illusions, to which the text refers, are to be found in *Zsch. f. Psychol.,* 3, 1892, 349-358; 5, 1893, 61-82; 6, 1893, 1-7.

On Brentano's life and work, see O Kraus, *Franz Brentano, zur Kenntnis seines Lebens und seiner Lehre,* 1919. In this book Stumpf contributes his reminiscences of Brentano, which throw light on the relation of the two men, pp. 85-149; Husserl also contributes his reminiscences, pp. 151-167. There is also an excellent brief account of Brentano's life: M. Puglisi, *Amer. J.*

Psychol., 35, 1924, 414-419. Both Kraus and Puglisi give the bibliography of Brentano's writings, lists differing only in small details.

Titchener has done more than any other writer to introduce Brentano to Americans. On the modern systems of act psychology, see E. B. Titchener, *Amer. J. Psychol.*, 33, 1922, 43-83, and on Brentano in particular, 43-47. For more about Brentano and the contrast between Brentano and Wundt, see Titchener, *ibid.*, 32, 1921, 108-120. Titchener makes this contrast between the two men identical with the contrast between empirical and experimental psychology, and again echoes this distinction in a retrospect of experimental psychology, *ibid.*, 36, 1925, 313-323, esp. 316 ff.

L. Carmichael, *Amer. J. Psychol.*, 37, 1926, 521-527, criticized Titchener for confusing *empirical* with *rational* in this contrast of Brentano with Wundt. The difficulty seems to be that the empirical method, which strictly speaking includes the experimental, is so unmethodical that its rationalism stands out and relatively it appears to be rationalistic, except when one compares this kind of empiricism with real 'a priorism.' See the author's remarks, *ibid.*, 38, 1927, 475-477. Titchener also made the same point, *J. Gen. Psychol.*, 1, 1928, 176 f.; and he noted further the danger of confusion that arises from translating both *empirisch* and *empiristisch* by the English "empirical." Brentano's psychology was empirical, but not empiristic, as it would have been had it been derived from the empiricists and had it described the mind as derived from experience, instead of merely deriving the description of the mind from experience.

Stumpf

Carl Stumpf's important book is called simply *Tonpsychologie*, I, 1883, II, 1890. His *Beiträge zur Akustik und Musikwissenschaft*, beginning in 1898, had run to nine *Hefte* in 1924. *Heft* 1, 1898, is Stumpf's text on consonance and dissonance, and is thus in a sense the beginning of a third volume of the *Tonpsychologie*. As a secondary source for this theory, cf. H. T. Moore, *Psychol. Monog.*, 17 (no. 73), 1914, 11-18. Stumpf also wrote a brief popular summary of his work on primitive music and the origin of music: *Die Anfänge der Musik*, 1911.

Stumpf's first psychological work, that got him the chair at Würzburg, is *Ueber den psychologischen Ursprung der Raumvorstellung*, 1873. His two very important theoretical papers are: *Erscheinungen und psychische Funktionen, Abhl. d. kgl. preuss. Akad. d. Wiss. zu Berlin (philos.-hist. Kl.)*, 1906, no. 4, 40 pp.; and *Zur Einteilung der Wissenschaften, ibid.*, 1906, no. 5, 94 pp. Both are separately paged, and the reprints are dated 1907. For Stumpf's position on the question of mind and body, see his opening address, *III internat. Kongr. f. Psychol.*, 1897, 3-16; reprinted as *Leib und Seele* in 1903 and 1909.

Stumpf has written his own account of his life and work in R. Schmidt, *Die Philosophie der Gegenwart in Selbstdarstellungen*, V, 1924, 205-265 (also printed and paged separately), and to it is attached a bibliography of his writings. This list (to 1924) contains fifty-four entries that concern music or tones, twenty-seven entries for other psychological topics, and fourteen entries for philosophical subjects. In this sense, Stumpf was more of a *Tonpsychologe* than a general psychologist, and more of a psychologist than a general philosopher, but he thought of his psychology as philosophy. For Stumpf's relation to Brentano, see both Schmidt, *loc. cit.*, and Kraus, *loc. cit.*

For Stumpf's work in his special field on tones, on fusion, on consonance and dissonance, on combination tones, on primitive music, on vowels, and on the analysis of speech sounds, the reader must see, beside the sources mentioned above, Stumpf's bibliogra-

phy, and thence the original studies. This work of Stumpf's has a wide range within a special field.

Stumpf's interest in music led him to the theory of emotion and feeling. On this topic he published three important papers: *Zsch. f. Psychol.*, 21, 1899, 47-99; 44, 1907, 1-49; 75, 1916, 1-38. For a discussion of this theory, its motivation, its background, and the various criticisms of it, see Titchener, *Amer. J. Psychol.*, 28, 1917, 263-277.

Beside everything that has been mentioned, there are two other theoretical papers of Stumpf's that have attracted especial attention. One deals with the attributes of visual sensation and the much-discussed problem of intensity as a visual attribute: *Die Attribute der Gesichtsempfindungen, Abhl. d. kgl. preuss. Akad. d. Wiss. zu Berlin* (*philos.-hist. Kl.*), 1917, no. 8, 88 pp.; the other is a discussion of the relation between sensation and image and of their criteria: *Empfindung und Vorstellung, ibid.*, 1918, no. 1, 116 pp.; both are separately reprinted and paged. With respect to the first, cf. Titchener, *Amer. J. Psychol.*, 34, 1923, 310 f.

Of Stumpf's psychophysics only the psychophysical expert ever hears, and yet scattered through the *Tonpsychologie* is a positive psychophysical theory which contributed to the psychophysical epoch; see Titchener, *Experimental Psychology*, II, ii, 1905, esp. pp. clxi-clxiii, but also the many references in the index. See also Stumpf's papers on the concept of mathematical probability, *Sitzber. d. kgl. bayr. Akad. d. Wiss. zu München* (*philos.-philol. Cl.*), 1892, 37-120, 681-691.

The controversy on tonal distances with Wundt is to be found as follows: for the results criticized, W. Wundt, *Physiologische Psychologie*, I, 1887, 428 f.; C. Lorenz, *Philos. Stud.*, 6, 1890, 26-103; for Stumpf's critique of Lorenz, *Zsch. f. Psychol.*, 1, 1890, 419-485; for Wundt's three attacks, *Philos. Stud.*, 6, 1891, 605-640; 7, 1891, 298-327; 7, 1892, 633-636; for Stumpf's interspersed replies, *Zsch. f. Psychol.*, 2, 1891, 266-293, 438-443. Cf. the author's comments on these polemics: *Psychol. Rev.*, 36, 1929, 107-113.

The text makes it clear why it is not possible to compile a list of the influential psychologists who were Stumpf's students before 1900, as we did for Wundt in the last chapter. There is no such list. Stumpf himself mentions (Schmidt, *op. cit.*, 220 f.) as his most important students, first F. Schumann and H. Rupp; then, as especially concerned with the acoustics, O. Abraham, K. L. Schaefer, M. Meyer, O. Pfungst, E. M. von Hornbostel, and G. J. von Allesch. He mentions W. Köhler in another connection. Certainly among his more prominent students we should add A. Gelb, K. Koffka, H. S. Langfeld, and W. Poppelreuter.

Stumpf himself wrote the history of the Berlin laboratory in M. Lenz, *Geschichte der kgl. Friedrich-Wilhelms-Universität zu Berlin*, 1910, III, 202-207. Ebbinghaus, who had preceded Stumpf at Berlin, had completed his famous experiments on memory there and may be said to have founded the Berlin laboratory. It is plain that Stumpf thought of the actual beginning of the laboratory, although it was called the *Psychologisches Seminar*, as occurring with his coming in 1894. The more formal *Psychologisches Institut* began in 1900. Schumann, coming from Müller, was assistant and *Dozent* from 1894 to 1905. Then von Hornbostel was assistant for a year, and Ach for another year. Ach, like Schumann, came to Berlin from Göttingen. Then Rupp, also from Göttingen, took over the work until Stumpf's retirement.

Müller

There is available no biographical sketch of Georg Elias Müller. The author has constructed the picture of Müller at Göttingen from many scattered sources. For the earlier picture

he is indebted to a letter on the subject from Professor Müller.

No bibliography of Müller's writings has ever been published. For this reason it seems best to list below the articles cited in the text in their chronological order. The list is by no means a bibliography, and opinions will differ as to whether it contains even all of the most important writings. In connection with some of these items it is possible to cite secondary sources for the student who is just about to become acquainted with Müller. Here is the list:

1873. *Zur Theorie der sinnlichen Aufmerksamkeit*, 136 pp. Cf. E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, esp. 188-206, 356-359.

1878. *Zur Grundlegung der Psychophysik*, 424 pp. Cf. numerous references in Titchener, *Experimental Psychology*, II, ii, 1905.

1879. Ueber die Maasbestimmung des Ortsinnes der Haut mittels der Methode der richtigen und falschen Fälle [*Pflüger's*] *Arch. f. d. ges. Physiol.*, 19, 191-235.

1889. With F. Schumann: Ueber die psychologischen Grundlagen für die Vergleichung der gehobener Gewichte, *ibid.*, 45, 37-112. Cf. *Amer. J. Psychol.*, 2, 1889, 650 f.

1889. Theorie der Muskelcontraction, *Nachtrichten d. kgl. Gesellsch. d. Wiss. zu Göttingen*, 1889, 132-179. Cf. E. C. A., *Amer. J. Psychol.*, 2, 1889, 490-492.

1893. With F. Schumann: Experimentelle Beiträge zur Untersuchungen des Gedächtnisses, *Zsch. f. Psychol.*, 6, 81-190, 257-339. Cf. J. A. Bergström, *Amer. J. Psychol.*, 6, 1894, 301-303.

1896. Zur Psychophysik der Gesichtsempfindungen, *Zsch. f. Psychol.*, 10, 1-82, 321-413.

1897. *Idem, ibid.*, 14, 1-76, 161-196.

1899. With L. J. Martin: *Zur Analyse der Unterschiedsempfindlichkeit*, 233 pp. Cf. F. Angell, *Amer. J. Psychol.*, 11, 1899, 266-271; Titchener, *Experimental Psychology*, II, ii, 1905, 300-310.

1900. With A. Pilzecker: Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Ergbd. 1, 300 pp.

1903. Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik, in L. Asher and K. Spiro, *Ergebnisse der Physiologie*, Jhrg. II, Abth. ii, 267-516. Reprinted with separate paging in 1904. Cf. Titchener, *Experimental Psychology*, II, ii, 1905, esp. 310-313.

1904. Die Theorie der Gegenfarben und die Farbenblindheit, *Ber. ü. d. I Kongr. f. exper. Psychol.*, 6-10.

1904. Bericht über Untersuchungen an einem ungewöhnlichen Gedächtnis [Rückle], *ibid.*, 46-48.

1911. Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, I, *Zsch. f. Psychol.*, Ergbd. 5, 403 pp. Cf. Titchener, *Amer. J. Psychol.*, 23, 1912, 490-494 (introspection); J. W. Baird, *Psychol. Bull.*, 13, 1916, 373-375.

1912. Ueber die Lokalisation der visuellen Vorstellungsbilder, *Ber. ü. d. V. Kongr. f. exper. Psychol.*, 118-122.

1913. Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, III, *Zsch. f. Psychol.*, Ergbd. 8, 567 pp. Cf. Baird, *loc. cit.*

1913. Neue Versuche mit Rückle, *Zsch. f. Psychol.*, 67, 193-213.

1915. Ueber das Aubertsche Phänomen, *Zsch. f. Sninesphysiol.*, 49, 109-246. Cf. K. Koffka, *Psychol. Bull.*, 19, 1922, 572-576.

1917. Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, II, *Zsch. f. Psychol.*, Ergbd. 9, 682 pp.

1923. *Komplextheorie und Gestalttheorie: ein Beitrag zur Wahrnehmungspsychologie*, 108 pp. Cf.

W. Köhler's reply, *Psychol. Forsch.*, 6, 1925, 358-416; and Müller's rejoinder, *Zsch. f. Psychol.*, 99, 1926, 1-15.

1924. *Abriss der Psychologie*, 124 pp.

The following are all very important researches done at Göttingen with Müller or under his personal direction, which the authors warmly acknowledge:

A. Pilzecker, *Die Lehre der sinnlichen Aufmerksamkeit*, 1889, 84 pp.

Müller and Schumann, 1889, *op. cit.*

Müller and Schumann, 1893, *op. cit.*

A. Jost, Die Associationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen, *Zsch. f. Psychol.*, 14, 1897, 436-472.

V. Henri, *Die Raumwahrnehmungen des Tastsinnes*, 1898, 228 pp.

Martin and Müller, 1899, *op. cit.*

Müller and Pilzecker, 1900, *op. cit.*

E. R. Jaensch, Zur Analyse der Gesichtswahrehmungen, *Zsch. f. Psychol.*, Ergbd. 4, 1909, 388 pp.

Idem, Ueber die Wahrnehmung des Raumes, *ibid.*, Ergbd. 6, 1911, 488 pp.

D. Katz, Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung, *ibid.*, Ergbd. 7, 1911, 425 pp.

E. Rubin, *Synsoplevede Figurer*, 1915; trans. as *Visuell wahrgenommene Figuren*, 1921, 244 pp.

E. A. McC. Gamble's monograph on the method of reconstruction, *Psychol. Monog.*, 10 (no. 43), 1909, does not belong in this list because most of the experiments were completed at Wellesley before she went to Göttingen.

For a description of the Göttingen laboratory in 1892, see O. Krohn, *Amer. J. Psychol.*, 5, 1893, 282-284.

MODERN EXPERIMENTAL PSYCHOLOGY

Chapter 17

THE 'NEW' PSYCHOLOGY

ORTHODOX experimental psychology is the psychology of Wundt. Such is the historical fact. New canons might later become orthodoxy against still newer heterodoxies, but Wundt delineated that separate department of thought and activity which he called experimental psychology. We know that Wundt prevailed: the psychology that he put forth to the world by precept and practice came into being as the type for experimental psychology.

Now Wundt's psychology, as we have seen, was not entirely the result of experimentation. Primarily it was an introspective psychology of sensory elements that enter into associations and other combinations. Its data were habitually called the contents (*Inhalte*) of consciousness, and, for want of a better name, Wundt's systematic position may be characterized as the psychology of content. In these terms, the fundamental dichotomy in Germany is formed by the distinction between *content* and *act*, a distinction that reflects the difference between Wundt and Brentano.

The psychology of content, originating with Wundt, tended to be all that we have just said it was: introspective, elementaristic, sensationistic, associationistic. This is the psychology that was being called at the end of the last century the 'new' psychology. The new thing about it was that it was experimental. It was 'experimental psychology' or 'physiological psychology,' for so definitely did it derive from Wundt that Wundt's practical identification of the two terms *experimental* and *physiological* led to their interchange by others. However, the 'new' psychology was something more than experimental and physiological: it was also in its systematic aspect some variant of this psychology of content.

An experimental, physiological psychology of content represents the norm for the 'new' psychology; nevertheless the 'new' psychologists stressed one aspect or another according to indi-

vidual temperament and training. Külpe, with philosophical interests, was much concerned with systematic issues and was, in this period, a leader in the school of content. He was also a pioneer in experimentation, but his own work could hardly have lain within physiology. For Ebbinghaus the experiment was the primary consideration, and systematization, while it concerned him, did not determine him. Ebbinghaus was captured by psychological experiments which were hardly good form for a psychology of content, and yet, if we had to classify Ebbinghaus in respect of the systematic dichotomy of the times, it is plain that we should have to place him with the psychologists of content, as a somewhat delinquent member of that school. At the other extreme from the systematists were the physiologists. The times created a group of psychological physiologists, like Hering and von Kries, who contributed many valuable experimental results that fitted into the psychology of content, although not motivated by the larger systematic concern. The 'new' psychology had therefore a right wing of systematists, a center of experimentalists, and a left wing of physiologists. The lines of division were not at all sharply drawn, for every man belonged to the whole; but in general Wundt tended toward the right, Ebbinghaus toward the center, and von Kries toward the left.

The psychologists of the act also thought of themselves as contributing to the 'new' psychology. The historical fact, however, seems to be that act psychology has never lent itself to experimentation as readily as the psychology of content, which was created for the sake of experimental psychology. We shall therefore ignore act psychology for the time being, but shall undertake in the next chapter to see just what it contributed, in spite of itself, to experimental psychology.

In the present chapter we shall examine the contribution of some of the psychologists who worked approximately within the frame that Wundt manufactured for experimental psychology. To be a 'new' psychologist at the close of the nineteenth century was not to be a Wundtian in any immediately personal sense. G. E. Müller, as we have seen, stood consistently and very influentially for this 'new' psychology without ever having gone to school to Wundt, except as Wundt had had more to do than any one else with the existence and form of the 'new' psychology. Ebbinghaus, like Müller, was not a Wundtian and yet

did much for the 'new' psychology. Nevertheless, there was also the direct influence of Leipzig that was very important. Külpe, Meumann, and Titchener all carried on, with the necessary genetic changes, the tradition of Wundt, their teacher, and contributed, as much as those of their generation who had never been at Leipzig, to the development of experimental psychology.

It is the purpose of the present chapter to continue the story of the last by showing the development of experimental psychology in the hands of those psychologists who were most closely related in thought to Wundt and Müller, the pioneers in the psychology of content. For this purpose we can stress especially the contributions of Ebbinghaus, Külpe, and Titchener to the 'new' psychology.

Hermann Ebbinghaus

Hermann Ebbinghaus (1850-1909), the son of a merchant, was born in Barmen, not very far from Bonn, the seat of the first university that he attended. He was two years younger than Stumpf and six months older than G. E. Müller. After attending the *Gymnasium* at Barmen, he went, when he was seventeen years old, to Bonn to study history and philology. Thence he migrated, after the custom of German students, first to Halle and then to Berlin. Altogether he spent at these universities three years (1867-1870), and in their course he was won from his earlier interests to philosophy. Trendelenberg, at Berlin, who captured Brentano and influenced Müller, may have had something to do with the change. Then came the Franco-Prussian War, and Ebbinghaus, like Müller, joined the army. After the war he returned to Bonn, and there in 1873 received his doctor's degree in philosophy on a dissertation on von Hartmann's philosophy of the unconscious.

The next seven years Ebbinghaus spent in an independent life of study. First he returned to Berlin for a couple of years, and there his reading shifted towards science in accordance with the philosophical tradition of the day. (Brentano had put Stumpf to studying science; and Lotze, Müller.) The three years following 1875 he spent in France and England, studying and engaging in tutoring. It was during this period that he found a copy of Fechner's *Elemente* at a second-hand book-shop in Paris. The significance of this work for a scientific psychology caught his

attention at once, and in considering the problem Ebbinghaus saw that experimental psychology had made great advances by Fechner's method in the domain of sensation, but that it had as yet been unable to attack the 'higher mental processes' that should make up so large a part of psychology. He must surely also have known Wundt's *Physiologische Psychologie,* then a few years old, and, if he did, he would have been confirmed in this conviction.

Ebbinghaus then, alone, without the stimulus of a university environment, without personal acquaintance with Fechner or Wundt or Lotze or with such promising younger men as Stumpf or Müller—with his sole reliance on Fechner's book and his own temperament—set about adapting Fechner's method to the problem of the measurement of memory. It seems quite probable that his thought in these years followed actually the exposition of *Ueber das Gedächtnis.* There, in the first chapter, he worked out the conditions that must be fulfilled for measurement to be possible. Then, picking up frequency of repetition as the essential condition of association, he showed how repetitions could be used as a measure of memory. For the principles of method, he relied on Fechner. The problem of memory he got from the British associationists, a very natural acquisition for a young philosopher spending time in study in England. We have seen in earlier chapters how the laws of association had gradually converged upon frequency as the primary condition of association. It was Ebbinghaus who fixed this outcome of British associationism, by seizing upon repetition and making it the basis for the experimental measurement of memory. Although later, in dedicating his *Psychologie* to Fechner, he could modestly say of his own work, "ich hab' es nur von Euch," the fact remains that he was in this research highly original. While he got mental measurement from Fechner, he did not take over bodily the psychophysical methods; probably he realized, as later research has shown, that they were unduly laborious when applied directly to mnemonic materials. He invented the nonsense syllable, it would seem, out of nothing at all in the way of ancestry. If he was to measure the formation of associations, he had to have a material, uniformly unassociated, with which to begin; so he took two consonants and a vowel at random and put them together, like *zat, bok, sid,* and thus found himself in possession of about

2,300 nonsense syllables, which could be associated in lists for learning. Such lists are much more homogeneous than lists of words, for with them the habits of language do not represent associations of unknown degree already formed. With nonsense syllables and poetry as materials, with himself as the sole subject, with the *Erlernungsmethode* and the *Ersparnismethode* (methods of complete mastery and of savings) as instruments, he began his experimental measurement of memory, and some of the experiments were completed before he came out of his academic isolation and into the university world.

In 1880, however, he went to Berlin and was habilitated there as *Dozent*. He kept on with his experiments upon memory, and, unwilling to trust the results that he already had, he repeated the old experiments and verified them. Finally, in 1885, he published the epoch-making report of this work under the title *Ueber das Gedächtnis*. Except for his dissertation, it was his first publication. Beside the matters just mentioned, the book contains the results of measuring the effect upon learning of varying the length of the material learned, of measuring retention as a function of different amounts of repetition, of measuring forgetting as a function of time (the famous 'forgetting curve'), and of measuring the strength of direct and remote, forward and backward associations within a given material. The study is a model of clarity, precision, and interesting exposition in scientific writing. It was epoch-making, not merely because of its scope and style, although these features must have helped, but because it was seen at once to be a breach by experimental psychology in the barrier about the 'higher mental processes.' Ebbinghaus had opened up a new field which the patience of G. E. Müller and his associates was soon to develop, and in this way experimental psychology became again vitalized by a sense of progress and of its destiny.

In 1886 Ebbinghaus was made *ausserordentlicher Professor* at Berlin. Undoubtedly his new fame aided his promotion. In this position he remained at Berlin for eight years. He did not continue his work on memory; he was original and an originator, but, with the first task accomplished, he was content to let others, like Müller, work out the method to the end. In the '80's he published experiments upon brightness contrast and upon Weber's law applied to brightnesses.

Then in 1890 he founded with Arthur König the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.* In England Bain had founded *Mind* in 1876, but Great Britain was not to be the medium for the new psychology. Then in 1881 Wundt had begun the *Philosophische Studien,* which remained, however, primarily the organ of the Leipzig laboratory; and in 1887 Stanley Hall had started the *American Journal of Psychology,* thus showing that America was not far behind Germany in the development of the new psychology. In this case it was really ahead of the rest of the world, because the *American Journal of Psychology* was not biased in the direction of any one school. Germany needed a general journal for its psychology. By 1890 there was too much going on outside the sphere of Leipzig for Wundt's *Studien* to suffice. So Ebbinghaus, with König's aid, founded the *Zeitschrift.* He was able to enlist in this venture the help, as coöperating editors, of Helmholtz, of such psychological physiologists as Aubert, Exner, Hering, von Kries, and Preyer, and of such psychologists as Theodor Lipps (who had just gone from Bonn to a full chair at Breslau), G. E. Müller, and Stumpf. Thus the *Zeitschrift* represented in a way a coalition of independents outside of the Wundtian school.

Early in the '90's Ebbinghaus published his theory of color vision. On the whole, however, he had become a leader without much publication. Although his memory experiments must always remain an historical event, marked by unusual originality, Ebbinghaus's success in general must be ascribed to other factors than the significance and quantity of his scientific publication. We may reasonably assume that it was for this reason that he was not promoted at Berlin, and that Stumpf came in over him to the chair of philosophy in 1894. We have already repeated the rumor that Helmholtz threw his influence against Wundt's call to Berlin. Berlin may have thought that Ebbinghaus lacked the caliber required by its chair, or it may have wanted for the chair of philosophy some one like Stumpf who had less definitely repudiated philosophy for psychology than had Ebbinghaus, or it may simply have had to make its choice between two men of equal age and similar attainments and have chosen Stumpf. However all this may be, the fact is that in 1894, a few months before Helmholtz's death, Stumpf came from Munich to the full

chair at Berlin, Lipps went from Breslau to Stumpf's post at Munich, and Ebbinghaus moved into Lipps's chair at Breslau.

Ebbinghaus remained at Breslau until 1905. He was still susceptible to new ideas. In 1897 he published a new method for testing mental capacity in school-children, a study undertaken at the behest of the aldermen of Breslau, who came to him with an inquiry about the distribution of the hours of study of the school-children. This new method was what we now call the 'Ebbinghaus completion test,' a method of mental testing that is utilized in a great many modern intelligence tests and that is to-day one of the most successful 'mental tests,' for getting at the intellectual attainments of students in college courses. Binet had, of course, already begun his researches on the child mind, a fact which may account for Ebbinghaus's publishing a brief account of his work in French, but Binet's *L'étude expérimentale de l'intelligence* did not appear until 1903. A claim can certainly be made that Ebbinghaus's originality penetrated the field of the higher processes, not only in respect of memory, but also in respect of intelligence as the mental tests now test it.

In 1897 Ebbinghaus also published half of the first volume of his *Grundzüge der Psychologie*. The second half of this volume, as well as the complete volume, was issued in 1902. The book was a tremendous success. Within two years there was a demand for a revised edition, which Ebbinghaus published in 1905 without having written the second volume. The first ninety-six pages of the second volume he put out in 1908, but within a few months he was asked to prepare a third edition of the first volume. He began this revision, but was prevented from completing it by his untimely death. Nevertheless, the demand for the book was so great that the first volume was revised and the second volume completed by Dürr after Ebbinghaus's death, and then the first volume was again revised by Bühler after Dürr's death.

The success of this work was largely the result of Ebbinghaus's clear and interesting style as an expositor. He was a very effective writer, lucid and precise, scientifically rigorous, but with his human personality showing through withal. As an expositor he was the antithesis of the armored Müller, who marshaled arguments with a general's strategy, or of the host that was Wundt, who overwhelmed his reader with a horde of facts, arguments and dicta. Ebbinghaus was for Germany what William

James was for America, the author of the only readable, kindly handbook of psychology, that nevertheless fell not a whit short in scientific care and exactitude.

When Ebbinghaus had completed the first revision of the *Psychologie* in 1905, he went from Breslau to Halle. Here his productive time seems mostly to have been occupied with further revisions. He also undertook another venture in textbook writing. He was asked to contribute the article on psychology to a large compendium on *Die Kultur der Gegenwart,* and this section, appearing in 1907, was reprinted as his *Abriss der Psychologie* in 1908. The *Abriss* was even more popular than the *Grundzüge,* for it went into a second edition in 1909, and then posthumously in the hands of Dürr and Bühler into numerous editions, as well as French and English translations.

Ebbinghaus died of pneumonia suddenly in 1909, when he was just a little over fifty-nine years old. He had been to the end active in his writing of these books, in his participation in psychological congresses and meetings, in his editorial work, and in the many ways in which a keen mind and a sympathetic personality could affect his colleagues and contemporaries. The comment of the time reflected the belief that psychology had suffered an irreparable loss, so sudden and unexpected was his death. Everybody remembered the crucial importance of his work upon memory. Nevertheless, the comment was colored by personal admiration. Except for the memory work, Ebbinghaus's influence on psychological thought by way of systematic or experimental contribution was very little, much less than the influence of Stumpf or Müller, his contemporaries, or of Külpe or Titchener, his juniors.

We have here, then, the picture of a man who was important in experimental psychology in spite of a paucity of experimental work. In his list of writings there are only a little over a dozen important publications. The key to his place in the history of psychology is his personality. He had a keen intellect and an unusual sense of relations. He was original in thought, and he preferred to start new things and let others carry them on. Thus we find him working on memory, founding the *Zeitschrift,* experimenting on brightness, theorizing on color, inventing an intelligence test, and writing a textbook. It was the success of the textbook that forced him to continue it in successive revisions;

but for the external demand, he would probably have been at something else shortly. His dynamic personality was combined with great tolerance and a vivid sense of humor, and all these qualities together made him a leader in the meetings of the *Gesellschaft für experimentelle Psychologie.* His wider reputation was due to his expository style. He was an extremely careful experimenter, and a superficial acquaintance with him at close range made him seem like a precise technician. But he was not a technician at heart; he was almost an artist. His care in experimental work was the enthusiasm of the mathematician for the neat demonstration or of the physicist for the perfect experiment. In all this he was self-made. He had no teachers who determined the course of his thought, but formed his life at the start with no contact other than that with the spirit of the times. He also had no pupils of importance in psychology. He neither founded a school nor wanted to found one. He was content to do well without great ambition. Moreover, he lacked the intolerance that most great psychologists have had, and thus, while personally very influential, he left no deep imprint on the psychological world after his death.

He is especially important to experimental psychology because he helped to make articulate and effective the spirit of the times that called for an emancipation of psychology from philosophy. The subtitle of *Ueber das Gedächtnis* was *Untersuchungen zur experimentellen Psychologie,* and the title-page carried in Latin the quotation, "From the most ancient subject we shall produce the newest science." This thought he echoed over twenty years later in the often quoted opening sentence of the *Abriss:* "Psychology has a long past, but only a short history." In personality and literary style, in his effectiveness by way of these media, and in his dynamic urge to undertake new things, he resembles William James, but he differs from James in that he used his powers mainly on behalf of precise experimentation and thus to further the cleavage between philosophy and psychology.

Ebbinghaus's systematic views are not important. Here his tolerance within the experimental field became eclecticism. He was primarily a psychologist of content because he accepted the psychology that had proved itself amenable to experimentation, but he had no fervid convictions on anything but experimentalism. In the *Psychologie* he rather avoided the problem of elementarism

and wrote of the simplest mental "formations" and the general laws of the mental life. He had, without knowing it, a cast toward the sort of psychology of capacity that has characterized America: his memory experiments were more like mental tests than introspective descriptions, and he invented the completion test. Had the behaviorists been in existence, they might even have sought to claim him as their companion. The fact remains, however, that one cannot take Ebbinghaus's psychology as other than a psychology of the Wundtian tincture, somewhat diluted by tolerance and by a dislike of philosophical argument.

Külpe, Mach, and Avenarius

Külpe was a very different person from Ebbinghaus, in part because Külpe never gave up the philosophy of his student days for a complete devotion to the new experimental psychology. He was an important figure in the 'new' psychology. As its perspective becomes defined with the lapse of time, it begins to appear that he and Titchener were the most influential of Wundt's immediate students. Külpe was a Wundtian even in the way in which he challenged the Wundtian doctrine by insisting that thought, like other mental processes, must yield its laws to the experimental method. Moreover, he was like Wundt in being half a philosopher, although the significance of philosophy in the lives of the two men is, as we shall see, quite different. Külpe was of the next academic generation to Ebbinghaus and Müller and Stumpf, and thus of the next generation but one to Wundt. His influence was not great until after the beginning of the present century, but the 'new' psychology was still young enough then not to have passed its formative period.

Oswald Külpe (1862-1915) was born in Candau in Courland in what was then Russia and is now Latvia. The region is very near East Prussia, and, in spite of an ancestor who had been master of the chase to Catherine II, the family was German in thought and interest. His father was a notary and his uncle a clergyman. He attended the *Gymnasium* at Libau on the Baltic coast of Courland. Then he spent two years in private tutoring, beginning his university studies thereafter when he was nineteen.

He went in 1881, not to Königsberg, the nearest university, but to Leipzig, in order to study history. At Leipzig, however, he came

into contact with Wundt, who diverted him, for the time being, to philosophy and, of course, to experimental psychology, which was still an infant science, for the Leipzig laboratory had been in existence only two years. He was not, however, entirely weaned from his desire to become an historian. He stayed with Wundt a year, and then went to Berlin for a semester to study history again. Perhaps he was weighing history against psychology in this turning of his back on Wundt and going to the great Berlin historians like Mommsen, Kirchhoff, and Diels. The experience at Berlin did not, however, settle the matter in favor of history. After a single semester he went in 1883, not back to Wundt, but to G. E. Müller at Göttingen, two years after Müller had succeeded Lotze. It would be interesting to know whether Külpe had heard Ebbinghaus lecture at Berlin and whether he went to Müller with Ebbinghaus's new experiments in mind. He spent three very important semesters with Müller. He began at Göttingen his dissertation on feeling, a theoretical study of the kind that Müller himself wrote on attention and Stumpf on space-perception. We lack Külpe's direct word concerning his thought at this time or the way in which he felt Müller's influence, but we have it indirectly that Külpe always regarded Müller as second only to Wundt in the determination of his early life. When his dissertation was published later at Leipzig, Külpe expressed his appreciation of Müller's aid and did not mention Wundt.

Still Külpe was undecided between history and psychology as a life-work. He left Müller to go to Dorpat to study history for a year. Müller had had the same choice to make, and could readily have sympathized with him. After a year at Dorpat, for the third time the pendulum swung back to psychology, and Külpe then in 1886 returned to Wundt at Leipzig, where he was to remain for eight years.

In 1887 he took his degree on the dissertation *Zur Theorie der sinnlichen Gefühle,* which Müller had started him upon. In the next year he became *Dozent.* For this advancement his *Habilitationsschrift* was another theoretical study, *Die Lehre vom Willen in der neueren Philosophie,* which Wundt published in the *Philosophische Studien.* At about the same time he became Wundt's second assistant, when Cattell, the self-appointed first assistant, left to return to America. Now Külpe found himself immersed in experimental work of his own and of others. The enthusiasm

about mental chronometry, the method of measuring the temporal relations of mental processes by the reaction experiment and the subtractive procedure, was then at its height. Külpe contributed to this movement an important paper (1891) on bimanual reaction times, showing that that hand is favored toward which the attention is directed, and thus supporting the attentional theory of reaction time that had come out of Lange's psychological analysis of the personal equation. In his experimental work he proved himself a careful technician and thus a worthy servant of the 'new' psychology.

In these days Külpe was turning over in his mind the project of a textbook. Wundt's *Physiologische Psychologie,* the only handbook of the new psychology, was in its third edition and about to pass into the fourth, always expanding. Wundt was becoming more and more complicated. There are certain limitations in the progress of thought which an individual cannot readily overcome. He may modify and revise with the utmost honesty, but, the farther he goes on, the less able is he to change direction radically or to check the weightier lines of his development. It is a psychological law of inertia, as against both change of direction and change of speed. What would happen to science if its great men did not eventually die, no one can guess. What does happen is that a new man takes up the work of an older man without the constraint of inertia from his past, that he thinks, works, and writes more simply and directly, and that thus from the old he creates something new that gradually itself accumulates inertia. Wundt was becoming too involved, too much at the mercy of his convictions. For instance, he had created and energized experimental psychology, but he had admitted its ineffectiveness against the problems of the higher processes and was turning to *Völkerpsychologie* for their solution. It seems as if Külpe must have said to himself: 'Experimental psychology is experimental psychology, and I shall write a book of the new psychology that gives the report of experiments about the mind, and when there are no experiments I will not write.' It is by such naïve judgments of younger men that science is constantly revitalized. So Külpe undertook (as so many after him have undertaken!) to write a clear, simple, direct text of experimental psychology.

At this point we must consider for a moment an important systematic matter. Külpe at Leipzig in the early '90's was plan-

ning and writing what was to become his *Grundriss der Psychologie*. As far as the record goes, he would seem to have been closest in the laboratory to Meumann, Frank Angell, and Titchener. Of course, Külpe was *Dozent* and assistant, and these men were but students. Titchener's British brusqueness offended Külpe at first. Nevertheless, there was amongst them much talk of Külpe's book, so much in fact that, when it did appear, Titchener felt a part ownership in it, translated it at once, and then wrote a similar book of his own. One of the topics that these men discussed was Wundt's definition of psychology as the science of immediate experience, and of physics as dealing with mediate experience. Wundt's view is easier to understand nowadays with all that has been written since in this vein, but then it seemed unsatisfactory. Moreover, there was the new epistemology of Mach and of Avenarius. Titchener seized especially upon Mach and was ever after greatly influenced by him. Külpe, more given to philosophical intricacies, favored the difficult Avenarius. There is no real difference here, for the two men later agreed that they were both saying the same thing though in very different words. The point is that Mach and Avenarius affected, on the systematic side, the new psychology.

We can digress only briefly from the story of Külpe to tell what it was that these two men were saying.

Ernst Mach (1838-1916) was a physicist, who after being a *Dozent* at Vienna and a professor at Graz, became professor of physics at Prague from 1867 to 1895 and at Vienna from 1895 to 1916. He was a brilliant, versatile man, who was interested in the 'new' psychology and contributed to it by way both of actual experimental work and of his epistemological theory of the relation of psychology to physics. He has even been called a psychologist. To the Austrian school of the form-qualities he contributed the notion of sensations of space just as Brentano contributed the act, but we are not to think that there was a personal relation between the two men. Mach went to Vienna just after Brentano had finally left Vienna.

While Mach was at Prague, he published his *Lehre von den Bewegungsempfindungen* (1875), in which he reported the results of the experiments on the perception of rotation, a description of his rotation apparatus (a standard piece in the early psychological laboratories), and his theory of the functioning of the semicircular

canals in this perception, a theory which, for want of a better, persists to the present day with but slight modifications, although there are serious objections to it. This book alone would have brought him into the body of psychologists. His most important work, also published from Prague, is his *Analyse der Empfindungen* (1886), which went into five editions and was translated into English. The epistemological views of this book he later elaborated in his *Erkenntnis und Irrtum* (1905), but the first book is historically the more influential. More than any one else he determined the view of science that Karl Pearson puts forth in his *Grammar of Science*. Both of these men uphold the conception of a scientific law as a correlation between observed data, a view that is the practical application to science of Hume's theory of causality, which we discussed in an earlier chapter. This conception was especially useful to the new physiological psychology, because its laws always involved the statement of relationships between conscious data and physiological data, terms observed in correlation by different methods.

The main thesis of the *Analyse* is, however, Mach's insistence that sensations are the data of all science. He was writing a practical epistemology for scientists, not for philosophers, for his introductory chapter was entitled "Antimetaphysical." Poor metaphysics! Herbart had left it in psychology and had ruled experiment out. Lotze had not avoided it. The 'new' psychologists, even those with philosophical interests like Wundt and Külpe, were never tired of trying to free psychology of philosophy. Thus Mach, adopting the spirit of the times, was an uncritical idealist in his view of science. It is not necessary to go into the intricacies of his views. It is easy to show that all science is observational and that the primary data of observation are sensory. Like Wundt with his immediate and mediate experience, Mach justified introspection, by establishing the observational status of conscious data beyond a doubt, and throwing upon physics the burden of giving an account of itself. This was before the days of Husserl and Stumpf, who tackled the same problem by setting up phenomenology as a propædeutic science.

Mach quoted Krause, who had framed this paradigm: "Problem: To carry out the self-inspection of the Ego. Solution: it is carried out immediately." That is to say, there is no problem. Sensations are not observed; they are given. Being given, they

cannot be shown to be in error. Illusions are 'illusory': there are none, or rather, the straight rod thrust into water is bent, and, if there be any illusion, it is that the rod is still straight. There is no ego; there are only sensory data. If we say, "It lightens," we ought also to say, "It thinks"; *cogitat,* not *cogito.* "The world consists only of our sensations." Dreams are as valid knowledge as perception.

Mach was a clear, vivid writer, like Flourens, Ebbinghaus, and William James, and some of his influence was due to his style. To Külpe and Titchener he seemed to establish the validity of introspection as a scientific method. He was so engrossed with emphasizing the community of psychology and physics that he was somewhat obscure in indicating the distinction, but this deficiency Avenarius, with a similar view, supplied.

Richard Avenarius (1843-1896), professor of philosophy at Zürich from 1877 to 1896, was as difficult, uninspiring, and involved a thinker as Mach was simple, dramatic, and clear. He worked without knowledge of Mach, though at the same time, but both men later agreed that their theories were essentially the same. He too sought to work out a theory of science, to avoid metaphysics, and to eliminate the ego from his theory. The two big volumes of his *Kritik der reinen Erfahrung* came out in 1888 and 1890, and were the only work of importance that their author accomplished. They were even more difficult to write than to read, for they broke Avenarius's health and he died not long after their publication.

Avenarius began by hypothesizing a "System C," a bodily system upon which consciousness depends. The System C is practically the central nervous system, but Avenarius avoided the difficulty of defining the limits of the part of the nervous system essential to mind by the unimpeachable circular definition that the System C is whatever is essential to mind. In psychology we have to do with "R-values," the stimuli, and "E-values," statements of experience. The E-values depend directly upon the System C and are direct consequences of its constitution. The System C is constantly undergoing metabolic changes between catabolism and anabolism, but it tends always to maintain a "vital balance" between these two opposing influences. The R-values work toward catabolism and the tendency toward a balance is preserved by opposing S-values. Thus the formula for

the vital balance is $f(R) + f(S) = 0$, and an inequality means that the balance is destroyed and that there is a "vital difference." The genetic course by which a vital difference approaches balance is a "vital series," and Avenarius gave a very plausible account of the waxing and waning of attention in these terms. We have, however—so the theory goes on—to consider two kinds of vital series. There are *independent* vital series which occur in the System C and are physical. There are *dependent* vital series, which parallel the independent vital series of the System C, dependent upon these latter; they are psychological. The two are covariants, but one can be understood as independent, whereas the other cannot be fully described except as dependent upon the first.

It is a travesty on Avenarius to condense his theory into a couple of hundred words, but the important thing for us is to see where Külpe and Titchener got the words *dependent* and *independent* as applied to experience. We can now return to Külpe.

Külpe published his *Grundriss der Psychologie* in 1893, dedicating it to Wundt. He defined psychology as the science of "the facts of experience," and he further pointed out that it is characterized by "the dependency of facts on experiencing individuals." This is the idea that he got from Avenarius, and it had for him the advantage of allowing physics also to deal with experience taken as independent of the experiencing individual. Mediate experience, which Wundt assigned to physics, seems, being mediate, not to be experience at all. The new formula was better.

Külpe then proceeded to write his textbook of experimental psychology, omitting the parts where speculation in Wundt had had to take the place of scientific fact. As compared with Wundt, he succeeded pretty well. The first third of the book dealt with the more or less positive knowledge about sensation. A tenth dealt with memory, treating briefly of Ebbinghaus, and more liberally of the British associationism. Perhaps Külpe was not yet impressed by the importance of Ebbinghaus's work, although later he gave Ebbinghaus credit for initiating the third great period in the history of experimental psychology. Another tenth of the book covered feeling, somewhat in the older Wundtian manner, for this was before most of the experimental work on feeling that Wundt's new tridimensional theory set off. He had, however, Lehmann's work with the expressive method to which to refer.

(It was upon Lehmann's work that Wundt presently founded his new theory.) Then Külpe gave to fusion and colligation as much space as to feeling. Under fusion could go Stumpf's finding on tonal fusion, and something about colors, touch blends, and emotions. *Colligation* was Külpe's well-known term for the spatial and temporal forms of combination, and he had all the facts and theories of space-perception to deal with, as well as the work on the time-sense, some of which had been done by Meumann in the Leipzig laboratory. Külpe also included the work on reaction under the temporal colligations, and made there his criticism of the subtractive procedure in mental chronometry that is supposed to have been its death-knell. He concluded the book with the shortest section of all, on attention. There were five pages on the will and self-consciousness. There was not a word on thought, and thereby hangs the tale of Külpe's life.

Külpe and the Würzburg School

No sooner was the *Grundriss* published than Külpe was advanced to be *ausserordentlicher Professor* at Leipzig, but before the year was out (1894) he went to the full chair at Würzburg, where the famous Würzburg school of imageless thought was to develop under his direction. The psychology of thought was to become Külpe's primary concern, but before we turn to this topic we must note that Külpe's interest was also shifting toward philosophy and esthetics.

In 1895 he published his *Einleitung in die Philosophie,* a text similar in style to the *Psychologie* and a very successful one. It had reached the seventh revised edition at the time of Külpe's death, and was also translated into English by Pillsbury and Titchener jointly. In 1902 Külpe published *Die Philosophie der Gegenwart in Deutschland,* which ultimately went into six editions and an English translation. In 1907 there appeared his treatise on *Immanuel Kant,* which had two more editions. After 1910 there were at least two important articles and two important books on philosophical subjects, which we shall mention later. We are prone to think of Külpe at Würzburg as immersed in the new systematic experimental introspection; but even more he was cultivating his love of philosophy.

Külpe had always been a great lover of music, and at Würz-

burg he turned also to writing on esthetics. His first paper (1899) had to do with the objectivity of esthetic laws and their dependence upon stimuli. In the same year he wrote on association as a factor in the esthetic impression. In 1903, in the Stanley Hall commemorative number of the *American Journal of Psychology*, Külpe published a paper on experimental esthetics. The anonymous author of a very complete biographical sketch of Külpe seems to feel that esthetics was one of the dominant interests in Külpe's life.

Meanwhile, however, Külpe was continuing his strictly psychological work. He wrote on attention in 1897 and on psychophysics in 1902. In the summer of 1902, he performed certain experiments on abstraction with W. L. Bryan of Indiana. These experiments raised seriously the question as to whether all the attributes of a sensory impression are simultaneously present, since the attentive predisposition for one may lead to a complete failure of introspection with regard to the others. Külpe may not have thought this work important, for he did not report it until 1904; as a matter of fact, C. Rahn made later use of it in his criticism of Titchener, and Titchener, in the present author's opinion, ultimately revised his views about the observational status of the sensation and the attribute. There was also a related paper of Külpe's in 1902 on the objectification and subjectification of sensory impressions.

As this record goes, it would seem that Külpe was more concerned, during the first decade of the present century, with philosophy and esthetics than with psychology. His only books were philosophical. It is therefore probable that a great deal of his personal leisure was given to philosophy. On the other hand, publication of the greatest importance was issuing from his laboratory, and Külpe was the inspiration of it all. He was also an observer in most of the experiments, and those who are familiar with the laborious method of introspection can guess how much time he spent upon the mere routine of these researches. However, Külpe was not at this time ready to bring the results together in a theory of his own, and his psychological work of the decade must therefore be regarded as primarily the work of the students of the 'Würzburg school.'

We have said above that the omission of a section on thought from the *Grundriss* was highly significant in Külpe's later life.

In 1893 Külpe could not handle thought in an experimental psychology. He was unwilling to follow Wundt in ruling it out of the laboratory; but since it had never been in the laboratory, there were no experiments to cite. It is clear that Külpe determined to remedy this deficiency. Ebbinghaus had brought memory, a 'higher mental process,' under the experimental method; why not bring thought into the laboratory too, and round out the new psychology?

The Würzburg school begins technically with the paper by Mayer and Orth (1901) on the qualitative nature of association. Thought seems to be a course of association; if it is, then the introspective method ought to yield a description of thought. In the same year Marbe, then *Dozent* at Würzburg, published his much more important experimental study of judgment. Marbe discovered a very surprising thing. A subject lifts two weights and judges which is the heavier. There is plenty of conscious content, like sensations and images in an associative temporal course, but it provides "no psychological conditions of the judgment." That is to say, the judgments come, they are usually right, and the judger does not know how they got into his mind! Such a view of judgment seemed to contradict the accepted belief of centuries; the laws of logic were supposed to be the laws of thought, and the process of thought was definite, like the process of the syllogism. Now mind was turning out, by the introspective method, to be an irrational associative train of mental contents that nevertheless reaches a rational conclusion. Marbe had good observers, among them Mayer, Orth, and Külpe himself; nevertheless, it was possible that all of consciousness did not get into their reports.

This question therefore arose: What other contents can there be in consciousness to account for thought, when the images and sensations which introspection always yields prove inadequate? The answer to the question was *Bewusstseinslagen,* or 'conscious attitudes,' as the German has been translated. Marbe had spoken of conscious attitudes, but it remained for Orth in a new paper (1903) to make them focal. Orth was not working on thought, but on feeling. He had in 1903 either to accept Wundt's new theory of a multiplicity of feelings, and a mind with more affective than sensory content, or else to advance some other hypothesis. His view was that many of these Wundtian feelings and many

other contents of the mind are really what may be called conscious attitudes, obscure, intangible, unanalyzable, indescribable contents that are neither sensations nor ideas. He sought under this classification to include many of Wundt's feelings, James's fringes of consciousness, and Höffding's quality of familiarity. The *Bewusstseinslage* was thus a new imageless element of mind, and might prove useful for the psychology of thought.

The next research in this series was Watt's. He undertook in 1904 a direct attack upon the problem of thought itself. His observers were asked to form partially constrained associations, like naming a superordinate for a subordinate or a part for a whole. He did not solve the problem of thought by this means, for he came out noting the paucity of consciousness in relation to its thoughtful accomplishment. He contributed, however, three changes to the picture of these researches.

In the first place, he introduced the Hipp chronoscope and its accessories for the securing of accurate reaction times for the associations. In the end, the times did not do him much good, but the presence of the chronoscope must have kept the ghost of the metaphysical Herbart from influencing the results, and moreover it was an evidence of good intentions. If he did not succeed in measuring thought, his failure was not due to indolence.

In the second place, he initiated the introspective method of fractionation. A conscientious observer may use hundreds of words in describing the consciousness of a few seconds, and his memory fades while he is getting the experience into words. Watt divided the consciousness, therefore, into four periods: the preparatory period, the event of the appearance of the stimulus-word, the period of the search for the reaction-word, and the event of the occurrence of the reaction-word. He had his observers confine themselves first to one period and then to another, and thus eventually got a more accurate and complete account of what had taken place.

In the third place, Watt placed the stress upon the *Aufgabe,* where it has ever since remained. Every one had been expecting to find the key to thought in Watt's third period, the period of the search for the word that would satisfy the conditions, but it was the third period that presented inadequate content. Watt discovered that the thought-process would run itself off at the presentation of the stimulus-word, provided the task or *Aufgabe* had

been adequately accepted by the observer in the preparatory period. This was really a remarkable result. So far as consciousness goes, one does one's thinking before one knows what he is to think about; that is to say, with the proper preparation the thought runs off automatically, when released, with very little content. Watt's *Aufgaben* were the various tasks that he set his observers, but so important did this initial preparation become that the word *Aufgabe* has slipped into psychology as a fundamental introspective concept. It is used loosely for any potentiality of consciousness. Strictly speaking, the *Aufgabe* is the conscious task or purpose that precedes a conscious course. The *Aufgabe* may therefore be thought of as setting up in the subject an *Einstellung* or 'set'; and the subject, in accepting an *Aufgabe,* as becoming *eingestellt.*

Ach's work on action and thought (1905) is much better known than Watt's, but its apparently greater importance is due to the fact that he gave definite formulation to many of the results of Watt's work. Ach used the Hipp chronoscope also and verified much of the older work on reaction times. With him it became clear that the problems of thought and action are essentially the same. In both cases one has some specific end to achieve, and the psychophysical process, released by a stimulus, runs its course to that end. To name a rime for a stimulus-word is psychologically no different from pressing a given finger when a given letter appears.

There are three things of importance that Ach did. In the first place, he invented the term *systematic experimental introspection,* which became the slogan of this school, as it was later of J. W. Baird's laboratory at Clark University. *Systematic* refers most definitely to fractionation, Watt's method which Ach used and so named. *Experimental* undoubtedly refers to the chronoscope among other things. The formula was an insistence on careful scientific technique, even when the method used was introspection.

In the second place Ach invented—or did he merely name?—the *determining tendency.* Watt had tried to account, by way of Müller's 'perseverative tendencies,' for the way in which the initial *Aufgabe* carried over unconsciously to the intended end. Ach preferred a new name to fit the new situation. The concept of the determining tendency implies that the tendency operates superiorly to reinforce associative tendencies. Thus, given a *5* with

a *2* below it, printed on paper, the most usual associates would be *7, 3,* and *10.* If, however, the subject has been instructed to add, one association will be strengthened so that *7* will almost inevitably occur; or, if the *Aufgabe* has been to subtract, then another association is reinforced and becomes the strongest. This notion of effective predetermination had of course been prepared by Lange and the Wundtians when they referred the differences in personal equation to the effect of attention. Külpe had taken a hand in the matter when he argued in the *Grundriss* against the subtractive procedure and said that a change in the preparation might change the whole consciousness, and not merely add or subtract a factor. However, Ach gave this relation between preparation and end a reality by naming it. It is quite possible that he overdid the matter, for the determining tendency was accepted by opponents of the Würzburg school, and has even been called a physiological process, although the only thing that makes it physiological is the fact that it is not conscious.

The third point that we must note about Ach's work is his invention of the *Bewusstheit* ('awareness'). A *Bewusstheit* is, like the *Bewusstseinslage,* a vague, intangible, conscious content that is not image or sensation. The descriptive word for it is *unanschaulich,* which Titchener has translated as "impalpable." The action or thought consciousness lacks enough content adequately to clothe itself, but nevertheless systematic experimental introspection reveals something more than palpable contents; the consciousness has impalpable moments, *Bewusstheiten.* Such was Ach's view. It is not clear that there is any real difference between Orth's conscious attitude and Ach's awareness, although Ach identified the conscious attitude with an awareness of relation, thus making *awareness* the broader term. However, both were imageless elements in thought, and there was little more that could be said about them.

It ought here to be said of Ach that his research was begun in 1900 with Müller at Göttingen and finished at Würzburg in 1904. The book is dedicated to both Müller and Külpe. Moreover, the work was finished before Watt's printed paper was available, so that Ach's apparent crystallization of Watt's ideas was practically an independent discovery.

After Ach came Messer of Giessen, whose experimental study of thought was carried through at Würzburg in the summer semes-

ter of 1905 In a sense Messer continued Watt's method of constrained associations. The resultant paper is replete with introspective results, and the general conclusion consists of an attempt to classify the conscious data. It served by reiteration to emphasize the general thesis of the Würzburg school, but, viewed from the perspective of many years, it seems not to have developed the position in any radical way.

The last person whom we need to mention in this school is Bühler, who came to Külpe from Berlin in 1907, and shortly thereafter published three papers on the psychology of the thought-processes. Bühler's work is memorable for his use of the *Ausfragemethode* (a very different method from the questionary form of the so-called *Aussagemethode*). In the *Ausfragemethode,* the experimenter questions the observer and the observer replies; there is free, sympathetic communication between them. It is plain that some such method as this is necessary for the psychoanalyst, but the guardians of 'systematic experimental introspection' have generally criticized a method that is so friendly to suggestion, and that makes so little distinction between rigorous description of mind (*Beschreibung*) and the giving of interpretative statements about mind (*Kundgabe*). Bühler was severely criticized by Wundt, by Dürr, who was one of his observers, and by von Aster. Titchener, of course, criticized the entire movement. Bühler, however, like Messer, left the total picture of thought without important change, and we may thus close our account of the Würzburg school and return once again to Külpe.

It is not easy to place Külpe in his own school. The Würzburg period was terminated by his transfer to Bonn in 1909, to Benno Erdmann's chair. He was becoming interested in the relation of psychology to medicine. He published on this topic in 1912, and he had been given an honorary medical degree by the medical faculty of Giessen in 1907. Philosophical problems were continuing to concern him. From Bonn he published papers on epistemology and natural science, on the concept of reality, and on the doctrine of the categories; and his most important book was *Die Realisierung* (1912), a philosophical study of reality, which of necessity dealt with psychological problems. It is plain that he was not yet ready to sum up the status of the psychology of thought.

It is true that in 1912 he published a short article on the modern psychology of thought. In this paper he summarized the work at Würzburg and attempted thus to illustrate the nature of thought. He did scarcely more, and while summary and illustration are useful, the world was expecting from him some more positive construction. He was lecturing on psychology and preparing his lectures as the basis of a book to replace the *Grundriss*. He died without publishing them, but Bühler published them posthumously in so far as they were complete. They represent what is, in view of human inertia, an astonishing change in Külpe's outlook since he wrote the *Grundriss* twenty years before, and they contain a pretty complete system of psychology. But the chapter on thought was missing! Bühler said that Külpe had not been lecturing on the topic.

What had happened?

In the first place, the work of the school had failed of its positive purpose. It had yielded determining tendencies and imageless thought. The hypothesizing of the determining tendency was in part a negative and in part a positive result. It was negative in so far as it asserted that the essential conditions of the course of conscious events are not conscious. It was positive in so far as it definitely placed the problem outside of introspection, and in so far as it emphasized the discovery that the key to thought, as well as to action, is to be found in the preparation of the subject. The finding of imageless thoughts is, however, now regarded as a purely negative discovery. The conscious attitudes and the awarenesses were never characterized except in terms of what they are not, and it became evident that scientific introspection could not simply admit any statement of an observer about his mind and still keep clear the meaning of conscious actuality. However, Külpe never came to share this view of the critics of the Würzburg school. His letters to friends (*ca.* 1910-1912) show that he felt that the psychology of thought was steadily developing from, though beyond, these researches which we have mentioned. Apparently he did not stop to sum up the subject because the work was still progressing and was not yet ready for recapitulation, and because he had more important work to do, especially the writing of *Die Realisierung*, which he expected to be his greatest book.

It thus seems that Külpe hoped eventually to bring thought into order both by further work upon the problem and by its clarifica-

tion by philosophical insight. He did not believe that there was any incompatibility between his philosophical and psychological interests, and it appears that Husserl indirectly led him away from Wundt and toward Brentano. Husserl's *Logische Untersuchungen* had appeared in 1900-1901, but the work seems not immediately to have been taken at Würzburg with the seriousness with which Külpe later regarded it. Messer was the first of the school to mention Husserl, and he mentioned him but once, although very favorably. Bühler, however, had read Husserl at Berlin and his paper showed the influence. Bühler may even have introduced Husserl to Külpe. Now we have already seen in our account of Stumpf that the principal effect of Husserl upon psychology was to justify phenomenology, and it is also plain that phenomenology —not exactly Husserl's kind, but the kind that Stumpf and other psychologists could accept—blithely admits awarenesses into the fold when dour introspectionism keeps them out. Here then was an opening, but the way beyond was not yet clear. Another effect of Husserl upon psychology was to reinforce Brentano and thus the act. There is room in phenomenology for acts as well as contents; it is a tolerant discipline. If we do not know certainly the exact course of Külpe's thought, at least we know that he leaned more and more toward Brentano and that he came eventually to a bipartite psychology of palpable contents and impalpable acts (or functions, as he preferred to call them.) The posthumous *Vorlesungen* contain much of the contents of the old *Grundriss* under "Contents," whereas the new materials of mind come under "Functions." Had the chapter on thought been written, it would certainly have appealed to the functions, or perhaps to both contents and functions, for Külpe once wrote in a letter: "I distinguish between *Gedanken* and *Denken;* the former are contents, the latter, in their different forms, acts or functions." We must mention this psychology of Külpe's again in the next chapter, but the present account goes to show how it was possible for a man to pass by imperceptible degrees from the psychology of Wundt to the psychology of Brentano.

In our attempt to understand Külpe we must not make the mistake of thinking that he thought he was deserting psychology for philosophy. Psychology was philosophy for Külpe just as much as it was for Stumpf. Külpe's experimentalism was not thought by him to take him outside of philosophy. Thus his

eventual search for the key to thought in a more philosophical and a less experimental manner represented for him merely the normal process of research on a single topic, thought, with one method failing and a new method being tried. The course Külpe's views took parallels his philosophical development from the positivism of Mach and Avenarius of his youth to the realism of his maturity.

There is little more to be said of Külpe's life. He was called from Bonn to Munich in 1913, and died there suddenly of influenza two years later, leaving his psychological work incomplete.

Külpe belongs to the school of content in his early period. He passed imperceptibly to the middle ground which includes both content and act. Between 1893 and 1912 we see the interesting transitional stage of the Würzburg school. The impalpable functions were there, but they were masquerading as mental elements; for all their impalpability they were, being infected with Wundtian elementarism, less fluid than Brentano's acts.

Külpe is quite properly to be considered as one of the chief representatives of the 'new' psychology. He accepted experimentalism. He wrote a book in which he refused to depart widely from experimental results. He could not write in that book on thought, but he undertook the experimental investigation of thought, seeking to extend the experimental method to the last stronghold of speculation, as Ebbinghaus had done for memory. He learned much, and it is still too soon to say whether he succeeded in his purpose. At the dramatic moment in this work he died, too early, at the age of fifty-three, without having yet convincingly demonstrated to the world that Wundt was wrong, or that Wundt was right, when he said that one cannot experiment upon thought.

Edward Bradford Titchener

Titchener was an Englishman who represented the German psychological tradition in America. We have already seen how close he was to Külpe at Leipzig, and in the early days there was a parallel between these two men. Both were Wundtians; both went out to new laboratories in the beginning of the '90's and undertook, with modifications, to continue the Leipzig tradition; Külpe wrote his *Grundriss* in 1893, and Titchener translated it in 1895 and wrote a similar book in 1896; both in the new century built schools about themselves by concentrating the publica-

tions of their students about their own processes of thinking. On the other hand, Külpe was more of a philosopher in his psychology than was Wundt and definitely opposed himself to his master in the fundamental thesis of the Würzburg school. Titchener avoided philosophy more than Wundt and never opposed him on so crucial an issue. For this reason Titchener really resembles G. E. Müller more than he does Külpe, and he always had for Müller a respect and admiration that rivaled only his feeling for Wundt. Both Müller and Titchener refused to philosophize in spite of an early training in philosophy; both championed the new experimentalism in everything but personal experimentation; both devoted themselves primarily to the theoretical discussion, criticism, and interpretation of experimental results; and both built up an elaborate and penetrating polemical method and attitude. Of this last point it may be said that Titchener was always extremely flexible in his theorizing and extremely rigid in his polemizing, an inconsistency that often goes with greatness.

Edward Bradford Titchener (1867-1927) was born in Chichester, in the south of England, of an old family in which he took much pride. The family had in his generation little money, and he was forced to rely on his exceptional ability in order to obtain scholarships for his education. One of these scholarships took him to Malvern College, where he must have done well, for it is related that James Russell Lowell, who was distributing the prizes one year and who had already presented several prizes to Titchener, remarked, when the same youth appeared again for still another prize, "I am tired of seeing you, Mr. Titchener." After Malvern he went in 1885 to Oxford, to Brasenose College, where he had obtained a scholarship. His family had arranged for him to go to Cambridge, but he himself had his heart set upon Oxford and managed to have his way. Thus early one gets a glimpse of the insistent independence of thought and action that characterized his whole life.

He remained five years at Oxford. During the first four he was a student of philosophy. We have seen that British empiricism and associationism were the philosophical ancestors of the modern psychology, and it is therefore not unnatural that Titchener should have become interested in Wundt's new physiological psychology. He did not find much sympathy for this interest at Oxford, but Titchener never swam in the social current. He translated into

English all of the third edition of Wundt's *Physiologische Psychologie*, which had only just come out. He never published the translation, but took it with him later to Leipzig, where he learned from Wundt that the fourth edition was almost complete (which s later he translated, only to be forestalled by the fifth). The las year at Oxford Titchener spent as a research student in physiology with Burdon-Sanderson, who made a great impression upon him. Whether Titchener turned to physiology because of physiological psychology, or whether the process worked the other way, it is impossible to say. At any rate he gained a lifelong respect for the British biology of this time and did his first published research in this field; at the same time he translated Wundt ar d went, with no encouragement at all from his Oxford friends, o Leipzig to study under him.

At Leipzig in 1890 Titchener found Külpe, Meumann, Kirs mann, Kämpfe, Pace, Scripture, and Frank Angell. The next year the last three returned to America and Warren and Witmer s American students replaced them. Titchener roomed with Meumann, planned with Külpe the *Grundriss*, and formed a clo personal friendship with Frank Angell. Like Külpe he fell in wit the enthusiasm about mental chronometry, and at Wundt's behest completed and published his first psychological research on the reaction times for what was then called 'cognition.' His dissert tion was on the binocular effects of monocular stimulation, and he had completed both studies and taken his doctorate in 18 after but two years with Wundt. Nevertheless, Wundt made great impression upon him, an impression that was never obliter ated.

Titchener would have liked to return then to Oxford, but ther was no place and little sympathy at Oxford for physiologica psychology or for a physiological psychologist. He lectured there on biology in the summer, and then came (1892) to Cornell in America, to the new laboratory which Frank Angell had opened the year before and which he was now leaving to go to the newly founded Stanford University. Titchener spent the rest of his life at Cornell, thirty-five years in all, almost, but not quite, as lo as Lotze and Müller had successively been active at Götti Only once did he return to Europe: to the International Co of Psychology at Munich in 1896. As the years went on, he eve left Ithaca less and less frequently, in part because, while he w

the names, like Washburn, Bentley, Gamble, Whipple, and Baird, have already become prominent in the history of American psychology. The volume of Titchener's publication was large. Beside books, his bibliography lists 216 of his own articles and notes and 176 publications from the Cornell laboratory. His professional correspondence was also large. With his personal influence, with many students afield who had once come under it, with all this publication, with the contrast that he presented against the American background, he could not but be the most influential minority in America.

The Psychological Physiologists

Thus far we have sought to catch for ourselves the spirit of the 'new' psychology as it came to life in the minds of certain men: Fechner, Wundt, Müller, Stumpf, Ebbinghaus, Külpe, Titchener. For these men it was not merely a profession, but also a purpose, no more a fact than an aspiration. In spite of very different temperaments, beliefs, and forms of thought, they were all alike in their consciousness of assisting earnestly in the renaissance of psychology. Research might have led on to other research and an accumulating body of fact and there might yet have been no psychology as we know it to-day, had it not been for this unifying purpose, this conviction, with integrative force, that there was to be simply more physiology and more philosophy, but a new scientific entity. The 'new' psychology was intensely self-conscious and personal, and it is for this reason that the biographical method of its exposition has seemed best.

Nevertheless it is clear that, while these separate biographical currents show the quality and the course of the stream, they do not truly describe its breadth. There were not a few men but many, not several lines of research but a multitude of experiments, that contributed to the new science and fixed its course. In later chapters we shall have something to say about the cross-sections of research that belong with these longisections of men's lives, but here we must pause to say something about the other men who contributed, though in many cases less abundantly, to the common

In the first place we must note that when Wundt named the new psychology "physiological psychology," he was not merely

giving expression to an epistemological conviction, but was describing the actual nature of the new psychology as the child of philosophy and physiology. At the start there were more physiologists than philosophers-turned-psychologist engaged in the new endeavor. The *Zeitschrift für Psychologie,* which began in 1890, had added to its title *und Physiologie der Sinnesorgane,* and its board of editors, picked as the most representative group outside of Leipzig, included seven physiologists (Aubert, Exner, Helmholtz, Hering, König, von Kries, Preyer) and only four psychologists proper (Ebbinghaus, Lipps, Müller, Stumpf). Let us therefore call the roll of the physiologists who were also of the 'new' psychology.

The list of course begins before Wundt. The early chapters of this book have dealt with that period. Johannes Müller, E. H. Weber, and Helmholtz were psychological physiologists and, for the time being at least, made it respectable for a physiologist to concern himself with problems of mind. Weber would hardly be known to-day except for his psychological work.

Of Fechner's generation there had been *A. W. Volkmann* (1800-1876), who wrote the physiology of vision (1836) that Johannes Müller referred to in his *Handbuch,* who was professor of physiology at Halle for thirty-nine years (1837-1876), who wrote the section of vision in Wagner's *Handwörterbuch* (1846), who aided Fechner in his experiments on the method of average error (1856-1857), and who wrote a physiological optics (1863).

A little later there was *Karl von Vierordt* (1818-1884), the physiologist at Tübingen, who is known for his work on the time-sense (1868). Titchener credits him with having formulated the method of right and wrong cases before Fechner. Vierordt comes into the picture more because of his research than because of his personality.

In the same way the Dutch oculist, *F. C. Donders* (1818-1889) requires mention because he worked with reaction times (1865-1866) and gave his name to one of the methods of compound reactions, and because he formulated Donder's law of eye-movement (1875).

Hermann Aubert (1826-1892) is more nearly of a piece with the 'new' psychology. He was professor of physiology at Breslau and then at Rostock (1862-1892). At Breslau he wrote a *Physiologie der Netzhaut* (1865) and at Rostock a *Grungzüge der*

physiologischen Optik (1876). Both books are still cited in the psychology of vision. In his later years he turned to other problems, like the physiology of circulation. He was one of the first editors of the *Zeitschrift für Psychologie.*

It requires an effort to remember that *Ewald Hering* (1834-1918) was really a physiologist and not a psychologist. He was first at Prague (1870-1895), and then at Leipzig (1895-1918) after the crucial period when Wundt was getting the new physiological psychology under way. His name is associated, of course, with the psychology of vision, and especially with the color theory that has shared with Helmholtz's the greatest vogue. Like Purkinje, he was an excellent observer, and fitted particularly well into the picture of the new psychology because he relied in his researches upon observation of the nature of the visual experience. The four parts of his *Beiträge zur Physiologie* (1861-1864) all had to do with the physiology of vision. He followed them up with *Die Lehre vom binocularen Sehen* (1868) and *Zur Lehre vom Lichtsinne* (1872-74). His final work is *Grundzüge der Lehre vom Lichtsinn,* which was published posthumously in its complete form (1920). His psychological interests were not, however, limited solely to the subject-matter of vision. For instance, his essays on memory (1870) and the specific energies of nerves (1899) are well known and were translated into English. Like Aubert, he was one of the first editors of the *Zeitschrift.*

Sigmund Exner (1846-1926) spent his academic life (1870-1926) at Vienna, where he was ultimately given the chair in physiology (1891). Beside extensive research on purely physiological problems, he published numerous articles on the problems of vision and on the localization of cerebral function. He is also known for his general discussion of the physiological explanation of psychical phenomena. He too was one of the editors of the *Zeitschrift.*

Julius Richard Ewald (1855-1921) was a physiologist who spent his academic life (1880-1921) at Strassburg, where he came presently to be the professor of physiology (1900). His special interest was the physiology of the end-organs, and thus indirectly of sensation. He gave his name to the 'pressure-pattern' theory of hearing, a theory that is opposed to Helmholtz's, since it avoids the conception of resonating elements in the inner ear. He was another editor of the *Zeitschrift.*

Except for Hering, *Johannes von Kries* (1853-) has probably had more influence on psychology than any other person thus far mentioned in this list. In his early days he was associated briefly with Helmholtz at Berlin (1877) and with the famous physiologist, Ludwig, at Leipzig (1878-1880). After that he went to Freiburg (1880-1895), then to Berlin (1895-1897), then to Munich (1897-1908), and then back to Freiburg again (1908-). He is best known for his contributions to the physiology of vision, especially his *Duplicitätstheorie* that relates the retinal rods to twilight vision and the retinal cones to daylight vision (1894). He wrote *Die Gesichts-empfindungen und ihre Analyse* (1882), more than a third of the sections on vision in Nagel's *Handbuch der Physiologie* (1905), many of the extensive additions to the third and posthumous edition of Helmholtz's *Physiologische Optik* (1910-1911), and finally, when he was seventy years old, an *Allgemeine Sinnesphysiologie* (1923). In spite of being a physiologist, he had a bit of the philosopher in him. As a young man he wrote an excellent but little-known book on the theory of probabilities (1886), and as an old man a logic (1916). He too was an original editor of the *Zeitschrift* and he really belongs more to the 'new' psychology than to physiology.

Arthur König (1856-1901) was the physiologist at Berlin who joined with Ebbinghaus in the foundation of the *Zeitschrift*. Like von Kries and Hering, he was a physiologist who dealt in the problems of vision. In the '90's he published many careful measurements of color vision, and his researches were collected and printed together posthumously (1903). He was especially concerned with the determination and measurement of color-blindness. He determined the quantitative form of the spectral color triangle, with Dieterici the form of the curve of the distribution of brightnesses for different wave-lengths of light at many intensities of illumination. He also published a color theory, which has been of little importance. Immediately after Helmholtz's death he undertook the publication of the second edition of Helmholtz's *Physiologische Optik* (1896) and added to it his famous bibliography of vision, consisting of almost 8,000 titles. The range of this subject, even at that time, shows why so many of the psychological physiologists were primarily interested in problems of vision. König died when he was but forty-five years old, before the full promise of his early work had been realized.

König was succeeded as the physiological editor-in-chief of the *Zeitschrift* by *Wilibald A. Nagel* (1870-1910), who was then at Berlin, but who later went to Rostock. He too was primarily interested in the topic of vision, as his many articles in the *Zeitschrift* attest, but he also accomplished considerable research in the sense-departments of taste, smell, and touch. He edited an important *Handbuch der Physiologie,* of which the third volume (1905) is the standard handbook for the psychophysiology of sensation for that period. In it he himself wrote a small portion of the section on vision, and the sections on taste, smell, organic sensation, and the specific energies of nerves. With von Kries and Gullstrand he was an editor of the third edition of Helmholtz's *Optik,* and wrote some of the additions to the second volume (1911). He died when he was only forty.

The authority on smell was the Dutch physiologist, *Hendrik Zwaardemaker* (1857-1930) who has spent practically his entire academic life at Utrecht. He published the classic work on smell, *Die Physiologie des Geruchs* (1895). As we saw in Chapter 6, there had up to that time been almost nothing scientific to say about smell. Now there was something, and Zwaardemaker, except for Hans Henning, has remained the authority on olfaction practically until the present. He published again on the subject thirty years later: *L'odorat* (1925). He was always in close touch with the 'new' psychology and became an editor of the *Zeitschrift für Sinnesphysiologie,* when that began as the second division of the *Zeitschrift für Psychologie.*

We should also mention here *Armin von Tschermak* (1870-), who, after being *Dozent* at Leipzig and Halle, became *ausserordentlicher Professor* of physiology at Vienna (1906-1913) and professor at Prague (1913-). He was also on the board of the *Zeitschrift für Sinnesphysiologie.* He has contributed mostly to the psychology of vision, but also to some extent to other psychophysiological problems.

It is plain that the 'new' psychology was really physiological psychology. The physiologists were having almost as much to do with it for a time as the psychologists proper. We have mentioned only those physiologists who were seriously involved in the new movement. There were many others less closely connected with it. For instance, in the volume of Hermann's *Handbuch der Physiologie* that deals with sensation (1879-1880), A. Fick

of Würzburg and W. Kühne of Heidelberg supplemented Hering in the chapters on vision; V. Hensen of Kiel wrote the chapters on hearing and suggested a theory of hearing that is still cited; M. von Vintschgau of Innsbruck wrote the portion on taste and smell and is thus one of the classical sources for these modalities; and O. Funke of Freiburg wrote all of the section on touch except temperature, which Hering took.

We have until now not mentioned the physiologist *William Preyer* (1842-1897), another original editor of the *Zeitschrift*, because in his research he was more of a psychologist than a physiologist. He was, of course, one of the older men, younger than Wundt, older than Stumpf, Müller, and Ebbinghaus. As a student he had worked at Bonn and then at Paris with Claude Bernard (1862-1865). He was *Dozent* at Jena (1866-1869) and then became professor of physiology there (1869-1888). Then he did an unusual thing; he resigned his chair at Jena because he preferred the intellectual atmosphere at Berlin, and became *Dozent* there (1888-1893). Five years later he was overtaken by ill health and he died after four years of illness. His most important book is *Die Seele des Kindes* (1882, and later editions), and thereafter he occupied himself primarily with child psychology. In the earlier years he published research on color vision (especially 1868, 1881) and on hearing (1876, 1879). His determinations of the lower limits of hearing are classic. He was a friend of Fechner, and the correspondence between the two men from 1873 to 1882 has been published (1890).

The Periphery of the 'New' Psychology

Beside the physiologists, there were a number of psychologists who may be described as belonging more or less on the periphery of the new experimental psychology. There was Münsterberg, who began at the core but was lured to other interests in America. There were others of Wundt's students, who, for the most part, came after the newness of this movement had begun to wear off, or who were less important, or who settled down in some special field. There were also others like Lipps and Ziehen who were not experimental psychologists, but were infected by the spirit of the times. And then there were the French psychologists, like Ribot and Binet, who belong properly in the French tradition,

but who nevertheless were on the outskirts of the German movement. Let us call this roll, as we have called the roll of the physiologists.

Next to Fechner and Müller, the Belgian, *J. L. R. Delbœuf* (1831-1896) of Liège played the most important rôle in psychophysics. His important books were *Étude psychophysique,* 1873, and *Théorie générale de la sensibilité,* 1876. These two monographs were reprinted together as *Éléments de psychophysique,* 1883, and followed by *Examen critique de la loi psychophysique,* 1883. Psychophysics is a special field and we cannot enter into Delbœuf's work here. Perhaps the most important thing that came out of it was the new conception of the sense-distance, a conception that disposed of the objection to Fechner's measurement of sensation. Fechner had thought of sensations as magnitudes, quantities definitely related to a zero-point; and much of the resistance to Fechner, and thus to quantitative experimental psychology, was based upon the introspective fact that sensations are not given in consciousness as large or small. Delbœuf's notion, that sensations, without being magnitudes, can nevertheless be arranged in a continuum so that there are observable degrees of distance between them, met this objection; and Titchener made much of the point when he came to write the *Experimental Psychology.*

Theodor Lipps (1851-1914) was the only psychologist of the act school (unless we are to count Stumpf) who was included in the original editorial board of the *Zeitschrift,* the organ of the 'new' psychology outside of the sphere of Leipzig. Actually, then, Lipps does not belong in this chapter, but in the next. It is hardly proper, however, to omit him, because his *Grundtatsachen des Seelenlebens* (1883) was a very important book that took account of all the 'new' psychology of that date and because his *Raumaesthetik* (1897) played into all the other work on optical illusions. He is best known for his esthetics. He was by temperament more of a logician than an experimentalist, and he wrote a logic (1893). He was very prolific. He was first at Bonn (1877-1890), then at Breslau (1890-1894), and then at Munich (1894-1914).

Theodor Ziehen (1862-) was a philosopher by predilection and for many years a psychiatrist by training and occupation. After studying philosophy at Würzburg (1881-1883) he turned

to medicine at Berlin (1883-1885) and was appointed on the basis of his doctor's dissertation assistant in the psychiatric clinic at Jena, where he was also *Dozent*. He was at Jena for fourteen years as *Dozent* and *ausserordentlicher Professor* (1886-1900), and then became professor of psychiatry successively at Utrecht (1900-1903), Halle (1903-1904), and Berlin (1904-1912). During all this time he tried to lead the double life of a psychiatrist and a philosopher, but finally retired to Wiesbaden (1912-1917) to devote himself entirely to philosophy. He is now professor of philosophy at Halle (1917-). In the early days at Jena, he published a *Leitfaden der physiologischen Psychologie* (1891), a textbook of physiological psychology that, because of its clear and forceful style, reached a twelfth edition (1924). He signalized his return to philosophy by writing *Die Grundlagen der Psychologie,* 1915, a book which deals with the philosophical and epistemological bases of psychology. As a psychologist he was not a Wundtian, though he did much to popularize physiological psychology. He might be called an associationist, but not an elementarist. He has written much in psychiatry and much in philosophy. He wrote these two books about psychology, and published considerable experimental work in his psychiatric days.

Hugo Münsterberg (1863-1916), however, began his academic life as if he were to be one of the leaders of the new movement. Actually he was Wundt's student (1882-1885), although he showed the impress less than many others. After Leipzig he went to Heidelberg for study (1885-1887), and then selected Freiburg for his habilitation as *Dozent* (1887-1892). At Freiburg he published his *Beiträge zur experimentellen Psychologie* (1889-1892). He had developed a laboratory there, and his experiments were highly original and attracted considerable attention at the time. Much of the criticism was negative, and Titchener, then at Leipzig, took him to task for his misunderstanding of Wundt. G. E. Müller attacked him vigorously. On the other hand, William James, who had just published his famous *Principles of Psychology,* wrote Münsterberg congratulating him on his "sense for the perspective and proportion of things" which his critics lacked. James was so well impressed with Münsterberg that he finally succeeded in arranging to have Münsterberg come to Harvard for three years (1892-1895), with the hope that the appointment could be made permanent. The plan worked well; Münsterberg

was offered a permanent professorship, he took two years in Germany to think it over, and finally he came to Harvard for the rest of his life (1897-1916). At Münsterberg's coming, James had his own title changed from "professor of psychology" back to "professor of philosophy," to give Münsterberg a clear field. Thus Münsterberg became the exponent of the new psychology at Harvard. As it happened, however, the original plan never worked out. There is almost nothing of importance in experimental psychology connected with Münsterberg's name except some of the little experiments in the *Beiträge* of the Freiburg days. What happened was that Münsterberg was too original; his dynamic mind went on at once to still newer psychologies. He broke ground in psychotherapeutics, in juristic psychology, and in industrial psychology; in a sense he 'founded' applied psychology; he was at times deep in the investigation of psychic research. Partly because of this and partly in addition to it, he became a public character, widely known and quoted. He was even sent abroad for a year (1910-1911) to help in founding an American Institute in Berlin, a quasi-diplomatic mission. It is plain that such a life is not the life of a scientist, and the promise of Freiburg was never realized, although many other achievements were. Münsterberg died during the World War, broken in spirit by the shattering of his dreams of *rapprochement* between Germany and America and by the hostility of Americans to him in that period of fear of Germany.

None of Wundt's students played so important a rôle in establishing the new psychology as Külpe and Titchener. There was *Emil Kraepelin* (1856-1926) of Heidelberg (1890-1903) and Munich (1903-1926), but he was a psychiatrist. In fact, he had written a psychiatry (1883) when he was only twenty-seven years old, one that went into many editions. He was as distinguished as any of Wundt's pupils, but not an experimental psychologist as the phrase is used.

There was *Ernst Meumann* (1862-1915), who after moving from Leipzig to Zürich, to Königsberg, to Münster, to Halle, to Leipzig, finally settled down at Hamburg (1911-1915); but he was claimed by educational psychology. His *Oekonomie und Technik des Lernens* (1903, with two revised editions and an English translation) was a classic in its field and showed the Wundtian tradition throughout. Nevertheless he never was thoroughly as-

similated by pedagogy. He founded the *Archiv für die gesamte Psychologie* in 1903, when Wundt gave up the *Philosophische Studien.* With Wundt he had worked on the time-sense, and an important piece of apparatus from this experiment bears his name. In his later years he published on esthetics. His death from influenza at the age of fifty-two was unexpected and cut short a life that promised much more than had been realized.

Then also among Wundt's pupils there were *Alfred Lehmann* (1858-1921) of Copenhagen, distinguished for his work on the method of expression; *August Kirschmann* (1860-), for a long time at Toronto, but now at Leipzig, and still remembered for his work with Wundt on color contrast; *Gustav Störring* (1860-), after Leipzig at Zürich, Leipzig, and Bonn, known to psychologists best for his psychopathology, but much more of a philosopher than anything else; and *Friedrich Kiesow,* for many years at Turin, known originally for his work with Wundt on taste, but since then continuing experimental work in the field of touch.

Besides the Wundtians, there was *Friedrich Schumann* (1863-), whom we have already mentioned as Müller's and Stumpf's assistant. He is best known for his work on memory with Müller and for his studies of visual space-perception (1900-1904). He has now long been at Frankfurt (1910-), and has there a very influential laboratory. There was also *William Stern* (1871-), who did not come from Leipzig, but studied at Berlin successively with Ebbinghaus and Stumpf. He is noted for his differential psychology and his educational psychology, but experimentalists know his psychophysical work and his invention of the tone-variator that bears his name (1898).

Outside of Germany, the new psychology was welcomed in America, and that story we shall leave to a later chapter. In England it has never been welcomed, although it now 'has its foot in the door.' In France it has always also been somewhat out of place, for France plays a part in the history of psychology only in the more strictly physiological psychology and in psychopathology, where she assumes a leading rôle. France had *Théodule Armand Ribot* (1839-1916), who undertook to interpret the new psychologies of England (1870) and Germany (1879) to his country, and who wrote many books on psychology thereafter. He was not an experimentalist, however. He knew

Charcot, and Janet was his pupil. *Alfred Binet* (1857-1911), director of the psychological laboratory at the Sorbonne (1892-1911), came a little nearer the German tradition in some of his experimental work. On the whole, however, he is rightly placed by his early concern in the psychology of reasoning (1886), which led him to his famous study of intelligence (1903) and thus on to the Binet scale for measuring intelligence, an achievement that has made his name familiar to laymen all over the world.

This ends our lists. There is little in the factual content of these lists that is of value to the reader, except as hints to further reading. Taken as wholes, they should indicate, however, the range and the ramifications of this 'new' psychology. It was a great new thing. Some men were immersed in it. Others were drawn partly into it. Still others, indisposed for it by temperament or training or environment, were forced to take account of it. No one could write psychology and ignore it. Brentano harked back to Aristotle, but drew pictures of optical illusions. Empiricism had become experimentalism, and the world had to go along, for the times do not stand still, and a man writes more or less of the age in which he writes.

Notes

The text encounters a difficulty of terminology. The 'new' psychology was experimental, physiological psychology, but it was in a great degree linked with a systematic position that has never been given a generally accepted name. This position is typified by the systematic position of Wundt, but in the hands of others it became a much broader thing than Wundt alone made it. To call it 'sensationistic' or 'associationistic' psychology is to limit it too narrowly; to call it 'elementaristic' is to include too much and also to give it the wrong emphasis. The author has chosen the word *content*, because content (*Inhalt*) has historically been opposed to act. Külpe and Messer finally tried to resolve the division within psychology by placing content and act (or function) side by side within psychology. If the reader prefers another name for this position, the author will have no quarrel with him, and he therefore undertakes to list the possible choices.

G. E. Müller has recently placed the word *Komplextheorie* in opposition to *Gestalttheorie*, but the word *complexes* has a meaning preëmpted by psychoanalysis, and lacks the general sanction of *contents*. Titchener similarly once used for this position the term *'structural' psychology;* unfortunately, however, this term has become ambiguous, because *Gestaltpsychologie* has been called *Strukturpsychologie*, and (alas for clarity!) *Gestaltpsychologie* has even been translated into English as "structural psychology," thus adopting the name of its chief enemy. *Verbindungspsychologie* stresses Wundt's fundamental principle of mental connections, but the term does not pass well into English. The psycholo-

gist of this school usually talks about his data as 'conscious processes,' and the school might be called 'process' psychology. The author dislikes this term because the word *process* (*Vorgang*) was invented as a persistent assertion of the fluidity of the mental elements, that is to say, as an answer to the criticism of elementarism; and yet the conscious data in the hands of this school are always becoming more or less fixed and stable, in spite of being called 'processes.' In America the adjectives *introspective* and *introspectional* are frequently used by behavioristically minded psychologists for their opponents, but it does not seem desirable to the author to go to America for a name when America, except for Titchener, has held so inconstantly to the German tradition; there is no similar stress upon the word *Selbstbeobachtung* in Germany.

The whole difficulty arises because this school, that takes its origin from Wundt, has never regarded itself as a school of psychology, but simply as one with experimental psychology. All the heretics need names in order to set themselves off from orthodoxy; but the conservatives need no name for themselves and have never recognized the imminence of a revolution by adopting one. It is impracticable to name a group which will not accept the name. This very fact is, however, an important historical datum. Orthodox experimental psychology, even in its systematic tenets, is a self-conscious school only as against philosophy. It has therefore never felt the need of any other name than "psychology," which it felt entitled to carry away from philosophy when the two separated. The enemies of this orthodox psychology name it, but always in accordance with what they most dislike in it.

In connection with the academic appointments of the men mentioned in this chapter, see the map of universities inside the covers of this book.

Ebbinghaus

On Hermann Ebbinghaus's life, see the memorial in *Chronik d. kgl. vereinigten Universitäts Halle-Wittenberg*, 1908-1909, 21-24; E. R. Jaensch, *Zsch. f. Psychol.*, 51, 1909, iii-viii; R. S. Woodworth, *J. Philos.*, 6, 1909, 253-256. Woodworth gives the essential bibliography.

Ebbinghaus's dissertation is *Ueber die Hartmannsche Philosophie des Unbewussten*, 1873, the year of Stumpf's *Raumvorstellung* and G. E. Müller's *Aufmerksamkeit*.

Ueber das Gedächtnis, 1885, was translated in 1913 into English under the title *Memory*. The two pioneers in the experimental investigation of memory are Ebbinghaus and G. E. Müller. Ebbinghaus got his inspiration from Fechner; Müller inherited the leadership in psychophysics from Fechner. It has been a matter of comment, therefore, that neither of these men transposed the psychophysical methods intact upon the problem of memory, determining by them mnemonic limens and thus securing much greater mathematical precision for their measurements. Recent attempts to use the psychophysical *Konstanzmethode* for memory have, however, shown that the work involved is tedious and thus often impracticable; perhaps both Ebbinghaus and Müller knew this fact. Cf. H. D. Williams, *Amer. J. Psychol.*, 29, 1918, 219-226.

The two experimental papers on brightness and the paper on color theory are: Die Gesetzmässigkeit des Helligskeitscontrastes, *Sitzungsber. d. kgl. preuss. Akad. d. Wiss. zu Berlin*, 1887, 995-1009; Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen [*Pflüger's*] *Arch. f.d. ges. Physiol.*, 45, 1889, 113-133; Theorie des Farbensehens, *Zsch. f. Psychol.*, 5, 1893, 145-238.

The paper on the completion test is Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre

Anwendung bei Schulkindern, *Zsch. f. Psychol.*, 13, 1897, 401-459. For the brief French account, see *Rev. sci.*, 4 sér., 8, 1897, 424-430.

The big textbook is *Grundzüge der Psychologie*. The first half of vol. I appeared in 1897; the whole of vol. I in 1902, the 2d ed. of vol. I in 1905; the first *Lieferung* of vol. II in 1908; the 3d ed. of vol. I, ed. by E. Dürr, in 1911; what is really the 1st ed. of vol. II, completed by Dürr, in 1913; the 4th ed. of vol. I, ed. by K. Bühler, in 1919.

The little textbook is *Abriss der Psychologie*. It was first published as the section on psychology in P. Hinneberg's *Die Kultur der Gegenwart*, I, vi, 1907, 173-246. The 1st ed. of the *Abriss*, so entitled, is 1908; 2d ed., 1909; Eng. trans., 1908; French trans., 1910. Dürr published 3d, 4th, and 5th editions (1910-1914), and the series has kept on, e.g., Bühler published the 8th ed. in 1922.

The high esteem in which Ebbinghaus was held by contemporary psychologists is shown by the eulogistic remarks of the conservative E. B. Titchener, *Amer. J. Psychol.*, 21, 1910, 405, in a page which is not a necrological notice and where praise could have been spared had the writer so desired.

Külpe

For biographical accounts of Oswald Külpe, see C. Bäumker, *Jahrbuch d. bayr. Akad. d. Wiss.*, 1916, 73-102, which contains a bibliography of sixty titles compiled by Bühler; K. Bühler, *Lebensläufe aus Franken*, II, 1922, 243-255; and an anonymous memorial, presumably by a colleague, in the *Jahrbuch d. Ludwig-Maximillians Universitäts München*, 1914-1919 (publ. 1927), 25-29. Unfortunately all these sources are relatively inaccessible. More accessible, but less complete, is A. Fischer, *Zsch. f. päd. Psychol.*, 17, 1916, 96-99. Külpe's doctoral dissertation, when published as a separate, has a *Vita*.

Certainly there is no readily available bibliography (except for the six books and eleven articles listed in *Amer. J. Psychol.*, 27, 1916, 296). The author therefore gives below a list of twenty-seven publications, which are mentioned directly or indirectly in the text or are items that seem generally to be considered important.

1887. Zur Theorie der sinnlichen Gefühle (dissertation), *Vtljsch. f. wiss. Philos.*, 11, 424-482; also separate with *Vita*.

1888-1889. Die Lehre vom Willen in der neueren Philosophie (Habilitationsschrift), *Philos. Stud.*, 5, 179-244, 381-446.

1891. Ueber die Gleichzeitigkeit und Ungleichzeitigkeit von Bewegungen, *Philos. Stud.*, 6, 514-535; 7, 147-168.

1893. *Grundriss der Psychologie;* Eng. trans., 1895.

1894. Aussichten der experimentellen Psychologie, *Philos. Monatshefte*, 30, 281-294.

1895. *Einleitung in die Philosophie;* successive editions in 1898, 1903, 1907, 1910, 1913, and (7th ed.) 1915; also posthumous eds. by Messer; Eng. trans., 1897 and 1901.

1897. Zur Lehre der Aufmerksamkeit, *Zsch. f. Philos. u. philos. Kritik*, 110, 7-39.

1899. Die ästhetische Gerechtigkeit, *Preuss. Jahrb.*, 98, 264-293.

1899. Ueber den associativen Faktor des ästhetischen Eindrucks, *Vtljsch. f. wiss. Philos.*, 23, 145-183.

1900. *Welche Moral ist heutzutage die beste?*

1902. *Die Philosophie der Gegenwart in Deutschland;* successive editions in 1903, 1905, 1911, and (5th ed.) 1914, Eng. trans., 1913.

1902. Zur Frage nach der Beziehung der ebenmerklichen zu den übermerklichen Unterschieden, *Philos. Stud.*, 18, 328-346.

1902. Ueber die Objektivierung und Subjektivierung von Sinneseindrücken, *Philos. Stud.*, 19, 508-556.
1903. Ein Beitrag zur experimentellen Aesthetik, *Amer. J. Psychol.*, 14, 479-495 (Stanley Hall Commemorative Volume).
1904. Versuche über Abstraktion, *Ber. ü. d. I Kongr. f. exper. Psychol.*, 56-68.
1907. *Immanuel Kant, Darstellung und Würdigung;* 2d ed., 1908; 3d ed., 1912.
1908. Ein Beitrag zur Gefühlslehre, *III internat. Kongr. f. Philos.*, 1909, 546-555.
1909. Zur Psychologie der Gefühle, *VI Congr. internat. de psychol.*, 1910, 183-196.
1910. Pour la psychologie du sentiment, *J. de psychol. norm. et pathol.*, 7, 1-13.
1910. *Erkenntnistheorie und Naturwissenschaft.*
1912. *Die Realisierung: ein Beitrag zur Grundlegung der Realwissenschaften*, vol. I. Vols. II and III were published posthumously under Messer's editorship in 1920 and 1923, respectively.
1912. Contribution to the history of the concept of reality, *Philos. Rev.*, 21, 1-10.
1912. *Psychologie und Medizin.*
1912. Ueber die moderne Psychologie des Denkens, *Internat. Monatschr. f. Wiss., Kunst u. Technik*, 6, 1069-1110. Reprinted in 2d ed. of *Vorlesungen*, 1922, *v. infra.*
1915. Zur Kategorienlehre, *Sitzber. d. kgl. bayr. Akad. d. Wiss. zu München (philos.-philol. Kl.)*, 1915, no. 5.
1915. *Die Ethik und der Krieg.*
1920. *Vorlesungen über Psychologie*, posthumous, edited from ms. by K. Bühler. The 2d ed., 1922, pp. 297-331, has added the article Ueber die moderne Psychologie des Denkens (1912, *supra*) in lieu of the missing chapter on thought.

For Külpe's work in esthetics, see A. A. Bäumler and Th. Ziehen, *Zsch. f. Aesthetik*, 11, 1916, 193-197.

The reader can discover the effects of Külpe's experiments on abstraction (1904) by reference to C. Rahn, *Psychol. Monog.*, 16 (no. 67), 1913, esp. 76-85; and to E. B. Titchener, *Amer. J. Psychol.*, 26, 1915, 262-264.

Notes on the Würzburg school follow in a later section.

On the similarity of Külpe to Husserl, see H. Schräder, *Die Theorie des Denkens bei Külpe und bei Husserl*, 1924.

The author here makes acknowledgment to Professor R. M. Ogden of Cornell for the transcription of certain of his letters from Külpe about the psychology of thought.

Mach

On Mach in general, see H. Henning, *Ernst Mach, als Philosoph, Physiker und Psycholog*, 1915. The book gives a bibliography.

Mach's most important experimental contribution to psychology is the *Grundlinien der Lehre von den Bewegungsempfindungen*, 1875.

By far his most important book is *Die Analyse der Empfindungen und das Verhältnis des Psychischen zum Physischen*, 1886, 2d ed., 1900, 6th ed., 1911, Eng. trans. of first ed., 1897, of 5th ed., 1914. Less well known is *Erkenntnis und Irrtum, Skizzen zur Psychologie der Forschung*, 1905, 2d ed., 1906.

We ought also to mention here Mach's popular scientific lectures, which were popular in form and enjoyed popularity among the public: *Populär-wissenschaftliche Vorlesungen*, 1895, 5th ed., 1923, Eng. trans. of first ed., 1895, of 4th ed., 1910. These lectures contain, however, only a little psychological material.

Mach openly acknowledged the practical identity of his theory with Avenarius's by devoting chap. iii of the 2d ed. of the *Analyse* to the subject.

Limits of space have made it impracticable to illustrate in the text Mach's vivid manner of arguing for sensations as the only materials of science. The reader must go to the *Analyse* for himself; for instance, to the classic picture (chap. 1, fig. 1) of Mach's real world as he lies on his sofa: parts of the room, framed above by an eye-brow and below by a mustache.

On Mach and Pearson, on their notions of correlation in relation to Humian causality, see the notes on Hume in chap. 10. Mach recognized his debt to Hume in the dedication of *Erkenntnis und Irrtum.*

Avenarius

There is a brief, partly biographical account of Richard Avenarius in H. Höffding, *Moderne Philosophen,* 1905, 117-127, Eng. trans., 130-140.

As the text has already said, Avenarius's great book was the *Kritik der reinen Erfarhung,* I, 1888, II, 1890. Secondary sources upon this difficult work are: F. Carstanjen, *Mind,* N.S. 6, 1897, 449-475; H. Delacroix, *Rev. de métaphys. et de morale,* 5, 1897, 764-779; 6, 1898, 61-102; W. T. Bush, Avenarius and the standpoint of pure experience, *Columbia Univ. Contrib. to Philos. and Psychol.,* 10, 1905, no. 4 (also recorded as *Arch. Philos.,* no. 2).

For Wundt's polemic against Avenarius, see *Philos. Stud.,* 13, 1896, 1-105.

The Würzburg School

The publications cited in the text as constituting the printed record of Külpe's 'Würzburg school' of imageless thought are as follows:

A. Mayer and J. Orth, Zur qualitativen Untersuchung der Associationen, *Zsch. f. Psychol.,* 26, 1901, 1-13.

K. Marbe, *Experimentell-psychologische Untersuchungen über das Urteil, eine Einleitung in die Logik,* 1901.

J. Orth, *Gefühl und Bewusstseinslage,* 1903.

H. J. Watt, Experimentelle Beiträge zur einer Theorie des Denkens, *Arch. f. d. ges. Psychol.,* 4, 1905, 289-436.

N. Ach, *Ueber die Willenstätigkeit und das Denken,* 1905.

A. Messer, Experimentell-psychologische Untersuchungen über das Denken, *Arch. f. d. ges. Psychol.,* 8, 1906, 1-224.

K. Bühler, Tatsachen und Probleme zu einer Psychologie der Denkvorgänge: I. Ueber Gedanken, *Arch. f. d. ges. Psychol.,* 9, 1907, 297-305; II. Ueber Gedankenzusammenhänge, *ibid.,* 12, 1908, 1-23; III. Ueber Gedankenerrinerungen, *ibid.,* 24-92.

It was Bühler's papers that touched off the criticism. Wundt specifically objected to Bühler's method: *Arch. f. d. ges. Psychol.,* 11, 1908, 445-459. E. Dürr, one of Bühler's observers, was moved to differ from Bühler in his interpretations: *Zsch. f. Psychol.,* 49, 1908, 313-340. E. von Aster criticized the entire movement: *ibid.,* 56-107. To these papers Bühler replied: *ibid.,* 51, 1909, 108-118.

In America the effect of the work of the school as a whole was considerable. E. B. Titchener at Cornell summarized and criticized the work in his *Lectures on the Experimental Psychology of the Thought-Processes,* 1909, esp. lectures iii and iv, and he undertook constructive research by way of his students in 1909-1911. Subsequently J. W. Baird, who bore to Titchener somewhat the relation that Titchener bore to Wundt, made 'systematic experimental introspection' of the 'higher mental processes' the central topic of the Clark laboratory.

On the distinction between *Beschreibung* and *Kundgabe,* which is mentioned in the text, see Titchener, Description vs. statement of meaning, *Amer. J. Psychol.,* 23, 1912, 165-182.

Titchener

The most complete biographical sketch of Edward Bradford Titchener is the author's, *Amer. J. Psychol.*, 38, 1927, 489-506. (On the similarity of Titchener to Wundt, mentioned in the text, see pp. 442 f.) See also the sketches by H. C. Warren, *Science*, 66, 1927, 208 f., and C. S. Myers, *Brit. J. Psychol.*, 18, 1928, 460-463; and for a picture of Titchener at Leipzig, F. Angell, *J. Gen. Psychol.*, 1, 1928, 195-198.

For a complete bibliography of Titchener's publications and the publications from the Cornell laboratory, see W. S. Foster, *Studies in Psychology: Titchener Commemorative Volume*, 1917, 323-337 (which extends to 1917) and K. M. Dallenbach, *Amer. J. Psychol.*, 40, 1928, 120-125 (which brings the list up to the time of Titchener's death).

For a list of the students who took the doctorate with him, see K. M. Dallenbach, *Amer. J. Psychol.*, 38, 1927, 506.

There have been as yet no critical studies of Titchener's psychology as a whole.

Titchener's books are: *An Outline of Psychology*, 1896, 2d ed., 1899, Russian trans., 1898, Italian trans., 1902; *A Primer of Psychology*, 1898, 2d ed., 1899, Spanish trans., 1903, Japanese trans., 1904, 1907; *Experimental Psychology: A Manual of Laboratory Practice*, I, i (*Qualitative, Student's Manual*), 1901; ii (*Qualitative, Instructor's Manual*), 1901; II, i (*Quantitative, Student's Manual*), 1905; ii (*Quantitative, Instructor's Manual*), 1905; *Lectures on the Elementary Psychology of Feeling and Attention*, 1908; *Lectures on the Experimental Psychology of the Thought-Processes*, 1909; *A Text-Book of Psychology*, 1909-1910, German trans., 1910-1912, Russian trans., 1914; *A Beginner's Psychology*, 1915. For his diligent translation of the work of others in his younger days Titchener was rewarded by much translation of his own books when he came to write them.

Titchener's first psychological research is Zur Chronometrie des Erkennungsactes, *Philos. Stud.*, 8, 1892, 138-144; and his doctor's dissertation is Ueber binoculare Wirkungen monocularer Reize, *ibid.*, 231-310. There is a separate of the dissertation with a *Vita*.

For Titchener's controversy with Baldwin on the reaction times, see J. M. Baldwin, *Psychol. Rev.*, 2, 1895, 259-273; Titchener, *Mind*, N.S. 4, 1895, 74-81, 506-514; Baldwin, *ibid.*, N.S. 5, 1896, 81-89; Titchener, *ibid.*, 236-241. The author believes that this controversy was resolved in a wise and, after the fact, an obvious manner by J. R. Angell and A. W. Moore, *Psychol. Rev.*, 3, 1896, 245-258. On this belief of the author's and its implications, see *ibid.*, 36, 1929, 111 f.

For those sections of Titchener's unpublished and unfinished systematic psychology that were printed, see Brentano and Wundt: empirical and experimental psychology, *Amer. J. Psychol.*, 32, 1921, 108-120; Functional psychology and the psychology of act, *ibid.*, 32, 1921, 519-542; 33, 1922, 43-83.

The history of Titchener's fundamental dichotomy between psychology and not-psychology is long and has not been fully worked out. It begins with Mach and Avenarius whom Titchener read in Leipzig. The text has omitted mention of the next step, Titchener's opposition of "structural psychology" to the "functional psychology" which was formulated at Chicago. The *casus belli* was John Dewey's Reflex arc concept in psychology, *Psychol. Rev.*, 3, 1896, 357-370. Titchener replied with The postulates of a structural psychology, *Philos. Rev.*, 7, 1898, 449-465, and later, Structural and functional psychology, *ibid.*, 8, 1899, 290-299.

The next dichotomy was that implied in the use of the term *stimulus-error*. On it see Titchener, *Experimen-*

tal Psychology, II, i, pp. xxvi f.; *Text-Book of Psychology*, 202 f. The author has discussed the meaning of the stimulus-error in *Amer. J. Psychol.*, 32, 1921, 449-471, but the reader must be warned that Titchener expressly repudiated this interpretation of the stimulus-error, although not in print. Cf. also M. Bentley, *Field of Psychology*, 1924, 411 f. Titchener referred the notion of the stimulus-error to von Kries, but he was only modestly seeking the inevitable historical antecedent for his own idea. For other references to Titchener's use of the "stimulus-error" and for the reference to von Kries, see the author's article, *op. cit.*, 451.

On the dichotomy between mental process and meaning, and therefore for the context theory of meaning, see Titchener, *Thought-Processes*, 174-194, esp. 174-184; *Text-Book*, 364-373; also for a terse statement that had Titchener's approval, H. P. Weld, *Titchener Commemorative Volume* (*op. cit.*), 181 f. Jacobson's experiments left an opening for an attack from Würzburg, and Titchener sought to repair the gap in his clearest presentation of this dichotomy, Description vs. statement of meaning, *Amer. J. Psychol.*, 23, 1912, 165-182. This entire matter becomes clearer in connection with Titchener's contemporaneous articles on introspection: Prologomena to a study of introspection, *ibid.*, 427-448; The schema of introspection, *ibid.*, 485-508. It has been complained that Titchener and other psychologists have tended to use the word *meaning* ambiguously and too generally, as an escape from trouble; cf. M. W. Calkins, *ibid.*, 39, 1927, 7-22.

There is no printed record of what Titchener conceived phenomenology to be.

The student of Titchener's psychology will find many articles relevant to the subject-matter of the text which are not mentioned here. He should consult Titchener's bibliography, *locc. citt.* Here too he can find the references to the Cornell experimental studies cited, or in the index of the *Amer. J. Psychol.* (vols. 1-30, 1926).

The Psychological Physiologists

It would unduly encumber these notes to give here the references to the important psychological publications of all the men mentioned in the text as psychological physiologists. The text gives some titles and many dates. The student who wishes to go further can find most of the references for the nineteenth century by author and date in B. Rand's bibliography in J. M. Baldwin's *Dictionary of Philosophy and Psychology*, 1905, III, ii. For titles after 1894 he can consult the *Psychological Index*. For the period after 1890, the indices of the *Zsch. f. Psychol.* are a great help to the many articles published or reviewed there. These indices were published for successive groups of twenty-five volumes in 1902, 1909, 1918, and 1927. A surprising number of these references can also be found by way of the author indices in E. B. Titchener's *Experimental Psychology*, I, ii, 1901, and II, ii, 1905. On vision, cf. König's bibliography in H. v. Helmholtz, *Physiologische Optik*, 2d ed., 1896, 1017-1310.

The Periphery of the 'New' Psychology

In general, see the bibliographical sources cited in the preceding paragraph.

On Delbœuf, see Titchener, *Experimental Psychology*, II, ii, working by way of the index, but esp. pp. cxvii-cxxii, 211-218.

On Lipps, see notes to the subsequent chapter.

On Ziehen, see his own account of his life and thought, in R. Schmidt, *Philosophie der Gegenwart in Selbstdarstellungen*, IV, 1923, 219-236 (also separate). There is a bibliography of thirty-six titles.

On Münsterberg, see Margaret

Münsterberg (his daughter), *Hugo Münsterberg: His Life and Work*, 1922. The book gives the personal rather than the academic picture, but an appendix carefully adds an account of all his writings.

On Meumann, see G. Störring, *Arch. f. d. ges. Psychol.*, 34 (Heft 4), 1915, i-xiv, which gives a bibliography of fifty-one titles.

On Stern, see his own *Selbstdarstellung* in Schmidt, *op. cit.*, VI, 1927, 129-184 (also separate), and the brief discussion in the text and notes of chap. 22.

On Ribot, see *Amer. J. Psychol.*, 28, 1917, 312 f., where is a list of fifteen of his books. His well-known accounts of English psychology and of contemporary German psychology have been referred to repeatedly in preceding chapters. See also the appendix, *infra*.

On Binet, see E. Claparède, *Arch. de psychol.*, 11, 1911, 376-388; T. Simon, *L'année psychol.*, 18, 1912, 1-14. Neither of these fairly long sketches gives a bibliography. See also the appendix, *infra*.

Chapter 18

ACT PSYCHOLOGY IN RELATION TO EXPERIMENTAL PSYCHOLOGY

SYSTEMATICALLY the two foci of German psychology at the end of the last century were act and content as represented by Brentano and Wundt. The last chapter has shown how the psychology of content and the experimental method came to be associated. The historical fact is that content has lent itself persistently to experimentation, whereas act has not. It is tempting to imagine that this difference is fundamental to the nature of act and content, that experiment requires a more controllable subject-matter than the impalpable acts; nevertheless one must not conclude too readily that the accidents of historical association are necessary and causal. Perhaps contents gained some of their palpability by being experimented upon, and perhaps acts would have become less impalpable if Brentano had founded a laboratory for them. Moreover, act psychology has not been entirely unexperimental, and it is with the relation of the act school to experimental psychology that the present chapter is concerned.

Sometimes this school of act has been called the 'Austrian school.' Geographical distinctions of this sort are seldom precise, and yet there is no doubt that the proper home of the act, in the period of which we are speaking, was in Austria and adjacent southern Germany.

When this 'Austrian school' went beyond systematization and argument to fields that more properly belong within experimental psychology, it generally worked with the problems of space-perception and such related problems as esthetics. There is no mystery about this specialization. The problem of perception is the fundamental psychological problem that immediately lends itself to experimental and quasi-experimental demonstration. The school of content, by its union with sensory physiology, was led thence to the investigation of sensation, and then on to other

subject-matters by its endeavor to bring all problems under the experimental method. Act psychology, with no conscious desire to be always physiological or experimental and with no heritage of analysis from associationism, could afford to experiment with perception and then to leave off.

The most definite point of contact at the end of the last century between the Austrian psychology and experimental psychology lay in the doctrine of form-qualities (*Gestaltqualitäten*). This doctrine, which we shall consider presently, both met and criticized the view of perception as a composite of elementary sensations, the view that represented the current position of the school of content. Out of the school of form-qualities came systematically the modern *Gestaltpsychologie,* and also some of the experimental work on perception that is supposed to belong to this school. Lipps at Munich represented another connection, because of his broad interests and his influence in the 'new' psychology, and his work in space-perception and in esthetics. Much later we find at Graz the experimental work of Benussi on perception and the development of those bipartite psychologies of act and content that we mentioned in our account of Külpe in the last chapter.

One sees what is meant by the 'Austrian school' if one considers both men and universities. The important names are Brentano, Lipps, Meinong, Ehrenfels, Cornelius, Witasek, and Benussi. Stumpf comes into the list incidentally as Brentano's pupil, leaning systematically toward the act. Külpe comes in on the periphery, and with him Messer, because of their later systematic views. Mach had a little something to do with the form-qualities because he wrote of 'sensations of space' in his *Analyse.* The Austrian universities important for psychology were Vienna, Graz, and Prague. (The only others in Austria were Innsbruck and Cracow.) Munich in southern Bavaria in Germany must be added to this list. It is tempting to add Würzburg, also in Bavaria, because of the relation of Brentano, Stumpf, and Külpe to Würzburg, but one must beware of placing too much emphasis upon the relation of geography to philosophical point of view. Now let us sketch the picture.

Brentano was in Vienna from 1874 to 1894. Mach was at Prague, where he wrote his books that have influenced psychology the most, and he came to Vienna just after Brentano left. Meinong was a student of Brentano's at Vienna from the time

that Brentano went there; he was *Dozent* at Vienna; he went to Graz in 1882, established there the first Austrian psychological laboratory in 1894, and remained there until his death in 1920. Ehrenfels was a student of Brentano's at Vienna about the time Meinong went to Graz; then he was *Dozent* at Graz, then *Dozent* at Vienna, and finally professor at Prague. Witasek and Benussi were younger and studied with Meinong, not with Brentano. Witasek was at Graz from about 1900 until his death in 1915; Benussi was at Graz a little later, and went to Italy before his death in 1927. These men—Brentano, Meinong, Ehrenfels, Witasek, and Benussi at Vienna, Graz, and Prague—are the outstanding figures of the 'Austrian school.' Closely allied to them, however, are Lipps and Cornelius. Lipps succeeded Stumpf at Munich in 1894 and remained there until his death in 1914. Cornelius had been at Munich in Stumpf's time, and remained there with Lipps until 1910 when he went to Frankfurt.

As we have said, the first important thing that came out of this skein of relationships was the doctrine of form-qualities. We must see what this doctrine was.

Form-Qualities

The elementarism of the psychology of content led to an equivocal conception of perception. On the formal systematic side, perception was thought of as a composite of sensations. Perception was the compound and sensations were the elements. This view of perception is quite clear in the fusions and in the complications. A tone is a sensation, but, if two tones are given simultaneously, there is something new, the perception of a tonal fusion. Add to the sensations from the retina the sensations of accommodation and convergence of the eyes, and one gets the visual-kinesthetic perception of seen depth, a complication.

This chemical view of perception might have been satisfactory for a time had it not been for the fact that most of the problems of perception have actually been problems of space-perception, and that there were some problems of temporal perception that presented the same difficulties as did the spatial ones. Space and time have troubled the psychologist as well as the philosopher. Külpe made for them the separate perceptual class of colligations. Titchener derived them from sensory attributes coördinate

with quality and intensity. Other psychologists have given them special status.

The difficulty is at once apparent if one considers the case of spatial form in two dimensions—that is to say, independently of its complication by the factors that yield depth and distance. Sensations have ordinarily been distinguished by the elementarists in terms of quality. If one sees a red dot near a black dot, one would be said to have two sensations simultaneously. But suppose that one sees two black dots near each other. Does one have two sensations of the same quality? It seems incorrect to say that the two black dots are one sensation and the white background a second. Yet if one calls the two black dots different sensations, then it is plain that we are distinguishing between elements on the basis of spatial separation and not on the basis of quality. The difficulty is not solved if we agree to base elementarism on space as well as quality. Join the black dots by a black line, and what does one have? One sensation instead of two, or a row of sensations? If a row, how many sensations are there? What limits an element spatially?

This dilemma was never finally answered by elementarism, and in fact it was ordinarily not even clearly realized until modern *Gestaltpsychologie* pointed it out. Research in perception went on, and the results were in general cast as nearly in the terms of sensational compounds as proved possible.

Ernst Mach, as we have seen, wrote the *Analyse der Empfindungen* at Prague in 1886, In this influential book, Mach identified experience with sensations, making sensations into the observational data of both physics and psychology. Such a course is plainly an uncritical use of the term *sensation,* and Mach felt no hesitation in extending the concept to include differences in space and in time as well as in quality. He spoke of "sensations of space-form," like a circle, and "sensations of time-form," like the successive intervals in a melody. What he meant was, of course, that form is in itself an experience independent of quality. You can change the color or the size of a circle without changing its circularity, its space-form; you can change by transposition the actual notes of a melody without changing the melody, its time-form. Form is independently experienced; experience is sensation; therefore there are sensations of form.

It remained for *Christian von Ehrenfels* (1859-) to give

this naïve theory systematic formulation. Ehrenfels had been with Brentano at Vienna and with Meinong at Graz, and had returned again to Vienna as *Dozent* when in 1890 he published a paper that created the concept of form-quality (*Gestaltqualität*). His problem was to answer the question as to whether form in space and time is a new quality or a combination of other qualities, and he decided in favor of the former answer. A square can be formed of four lines. They are the sensations that underlie the perception of the square and can thus be called for this perception the *Fundamente;* or, all together, they may be said to constitute the *Grundlage.* However, 'squareness' inheres in no one of these elementary *Fundamente.* Only when they are brought together as a *Grundlage* does squareness appear, and, since the form is obviously immediately experiential, it must be a new element, a form-quality.

Ehrenfels elaborated the system much further. He distinguished two classes of form-qualities, the temporal and the non-temporal. The temporal form-qualities included musical melody, 'color-melody,' and any changing temporal course of sensation, like reddening or cooling. The non-temporal form-qualities were mostly spatial, but included also tonal fusions, clang tints, flavors, and the perception of movement. In all these cases the existence of form-qualities can be demonstrated by independent variability: if the qualities of the *Fundamente* can change without changing the form, then there must be an independent form-quality.

Ehrenfels also noted that the relationship of *Grundlage* to form-quality occurs at various levels. There are higher orders of form-qualities, which may have as their *Fundamente* the form-qualities of lower levels. One may get the higher orders by comparison, as of one melody with another, or by combination, as of melodies in a polyphonic composition. These complications of the system need not concern us further than to indicate the great scope of the new conception.

This theory of Ehrenfels is a logical analysis of certain perceptions, based upon argument with an empirical, but not an experimental, basis. Such a method is the usual method of act psychology, but there is no immediate necessity for a relationship between form-qualities and psychical acts. The form-qualities themselves were really new elementary contents and might have been created by the other school. As a matter of fact, however,

Ehrenfels, in the Austrian environment, related them to the acts. It is, he thought, the mental activity of comparing or of combining that elicits the form-quality from the *Grundlage*. One can see how real these acts can become to the psychologist, by mentally creating a square out of four dots and noting the experience of combining that is involved.

It is important to note at this point that Ehrenfels did not say that the form-quality arises out of the relations among the *Fundamente*. It is obvious that four lines do not yield a square unless they are given in a certain relation to one another, and it is tempting therefore to conclude that the form-quality is the relation and is given independently of the *Fundamente*. Ehrenfels, however, thought of the form-quality as distinctly secondary to the *Fundamente*, as variable independently of them but not independently given. He may have been wrong, but that is not the point.

Ehrenfels's system was next elaborated by *Alexius Meinong* (1853-1920), Brentano's pupil and the leader of the school at Graz. Meinong's view differed in its fundamentals but little from Ehrenfels's, but it used a new terminology. Meinong spoke of founding contents (*fundirende Inhalte*) and of founded contents (*fundirte Inhalte*). Ehrenfels's *Fundamente* were Meinong's founding contents, and his form-quality was Meinong's founded content. The relation of the two kinds of contents is relative and hierarchical, and the founding contents may be called *inferiora* and the founded content a *superius*.

In Meinong's view the founding and founded contents together may be said to form a complexion, and one finds both real complexions which are equivalent to perceptions, and ideal complexions which are equivalent to conceptions. Complexions are formed by acts of founding, but the real complexion (perception) is dependent primarily upon the relationships inherent in the perceived object, whereas the ideal complexion (conception) depends primarily on the act of founding. Here, then, we get the recognition of the importance of the relationships between the primary members of the perception, a recognition that was lacking in Ehrenfels. We get also, in the ideal complexions, a further emphasis upon the importance of the act.

Meinong recognized the relativity of this mental hierarchy. There may be, he thought, complexions of higher orders in which

the *superiora* of lower complexions become the *inferiora* of higher complexions in the founding of a higher *superius*.

Meinong, for all that he founded the first Austrian laboratory at Graz (1894), was a philosopher and not an experimental psychologist. He was a man of great ability and influence, and his espousal of the form-qualities did much to establish them in psychology.

On this showing, it begins to look as if the psychology of perception might have been getting out of the hands of the psychologists of content. It is true that the form-quality was only a new elementary content, but it seemed to require an act of founding to account for it. However, the argument was brought back nearer the traditional position of the experimentalists by *Hans Cornelius* (1863-) at Munich. Munich was just beyond the periphery of Austria, intellectually as well as geographically. Cornelius was at Munich when Stumpf was there and became *Dozent* when Lipps came there from Breslau. He is a philosopher and not a psychologist, but that fact was no bar to his participation in this argument.

In general Cornelius supported Meinong, but he suggested two significant changes in the system. In the first place, he held that the form-quality is not a founded content but a founded attribute. In the second place, he said that it is not so much that these attributes are founded by an act of founding as that they are disestablished by analytical attention. Experience is ordinarily given, he observed, in great unanalyzed wholes which possess characteristics that pertain to the wholes. A musical fifth is a unique experience. However, attention to the parts destroys the whole and alters the complexion by abolishing its founded attributes.

There may seem to be little more than a verbal difference between Meinong and Cornelius, but words can be very important affairs. The school of content claimed the ability to deal with attributes and with attention, whereas it denied the existence of new non-sensory elements and of acts, like the act of founding. Whether or not Cornelius changed the problem greatly does not matter. There was nothing incompatible with orthodox elementarism in saying that the compounds may have secondary attributes that belong only to the compounds or to admit attention into the system as a principle of explanation. Attention, of course, could

not by a Wundtian be thought of as an act; nevertheless it is true that it has always occupied an equivocal status between activity and content, a fact that modern *Gestaltpsychologie* has used as a criticism of orthodox elementarism.

Thus it was possible for Schumann, then Stumpf's assistant at Berlin, to seem effectually to dismiss the heterodox form-quality by his classical experimental studies of visual form. These papers study, and analyze into their conditions, a very great many visual forms and illusions without finding a necessity for appealing to the concept of form-quality. Schumann referred the phenomena in general to the objective conditions in the stimulus and to the effect of attention. His results were not sheerly speculative, but an attempt to interpret experimental observations. Thus he came to the conclusion that a visual perception of form is a grouping under the laws of attention, a grouping which is dependent in part upon the objective conditions and in part on the attentional attitude of the observer. It may involve certain subjective additions, like the tied images of perceptual completion, and a certain subjective selectiveness. Many perceptions depend upon the direction of attention to the 'total impression' and are altered by an analytical attention. Eye-movements may play a rôle, but in general Schumann thought them much less important than did Wundt. The perception, then, is a composite, but it has unity because attention tends to combine it into a group and also to abstract from those parts that are not essential to the perception.

It is true that Schumann called attention an act. It is also true there was nothing fundamentally incompatible in his findings with the Austrian position. Nevertheless, because he was experimental in his method, because he used familiar terms, because he avoided speculation and pure argument and did not seek to establish a system of act, his work, which is hardly more than the experimental test and extension of Cornelius, has been taken as a refutation of the doctrine of the form-quality.

The school of form-quality belongs to the '90's, when Ehrenfels, Meinong, and Cornelius wrote their papers. The point of view was kept alive in the next decade by Meinong's pupils, Stephan Witasek (1870-1915) and Vittorio Benussi (1878-1927). In general Witasek's contributions were systematic, both in his textbook of psychology and in his handbook of visual space-perception. His psychology of perception was built around the effect

of the psychical act of producing. The resulting complexions may be, he thought, simple (as in clangs or simple melodies), where the founding is practically automatic, or they may be complex (as in polyphonic composition). In the latter case the complexion is determined both by external factors in the stimulus-object and by the internal act of producing. Benussi was an able experimenter, in fact, the most productive and effective experimental psychologist that Austria has had. His researches were almost entirely upon problems of visual and somesthetic perception, and there is a long and important list of them. They contain a theory of perception, but for the most part they represent simply psychophysical work placed in the systematic setting of the 'Austrian school.'

If one takes the larger view of this movement, it becomes apparent that, although it was checked at the end of the century, its force persisted. The movement, within act psychology, attracted the attention of the elementarists because it claimed the discovery of a new element. It seemed for the time being that the movement had failed, because the alleged element did not gain acceptance, because alternative modes of explaining the fact presented themselves, and because the members of the 'Austrian school' itself shifted from an emphasis upon form-qualities to a discussion of complexions which, except for their dependence upon acts, were not necessarily unlike the composite perceptions of the school of content.

On the other hand, it is apparent now that the doctrine of form-quality was at bottom a criticism of elementarism, which failed in its original form because it added merely a new element instead of offering a new view of psychological analysis. The same criticism of elementarism with a new remedy was offered in 1912 by *Gestaltpsychologie*. Thus the movements of *Gestaltqualität* and *Gestaltpsychologie* have a common negative motivation in seeking to correct an untenable psychological chemistry, and a common positive aspect in choosing the field of perception as the ground of controversy. They are quite unlike in that the former sought to solve the dilemma by finding a new element, whereas the latter denies the validity of assuming the existence of real elements. The reader may take his choice as to whether he will consider the new school as the improved form of the old or as an entirely new movement. There can, however, be no question that the

Gestaltpsychologie is independent in personnel—as far as anything is novel in the development of thought. The modern *Gestaltpsychologie* did not grow up in the Austrian tradition.

Theodor Lipps

Theodor Lipps (1851-1914) stands alone amongst the act psychologists. He was primarily a systematist, and he was both an act psychologist and a self-psychologist. He held little in common with Brentano and Meinong, except belonging to their side of the opposition between act and content. He controverted Husserl. Moreover, as we have seen in the last chapter, he had contact with the school of content, although often by way of controversy. His psychological interests lay in theory, systematization, perception, and esthetics, and the two latter topics brought him into relation to experimental psychology.

Like Ebbinghaus, of whose generation he was, Lipps was a self-made psychologist without any particular master. He went as a student to Erlangen, Tübingen, and Utrecht, where he studied theology, mathematics, and natural science; then to Bonn, presumably about the time that his interests shifted to philosophy. He took his doctor's degree at Bonn and became *Dozent* there (1877-1889). He was *ausserordentlicher Professor* for a year at Bonn (1889-1890). His earlier theological and mathematical interests brought him at Bonn to the study of Hume and of Herbart, and thus to psychology. His dissertation was on Herbart. In 1883 he published *Die Grundtatsachen des Seelenlebens,* a large general treatise on psychology. Psychological texts were then new undertakings. Wundt had published only two editions of the *Physiologische Psychologie.* Wilhelm Volkmann's *Lehrbuch der Psychologie* (1875-1876), an Herbartian text, was also available. Lipps's book dealt largely with Lotze, Helmholtz, and Wundt, presenting their views, polemizing against them, and remodeling them into a new system. It is for this reason that Lipps seems to belong more to the school of content than other act psychologists, for he came into the school, as it were, as an invader, and he dealt in part with the same subject-matter. Lipps also published *Psychologische Studien* in 1885.

After Bonn Lipps went as professor to Breslau (1890-1894). Anschütz, his biographer, is of the opinion that this change coin-

cides with a second intellectual period of his life, a period of transition, when his psychological interests narrowed to space-perception and branched out thence to esthetics, and when he turned also to ethics and logic. Certainly some such change had become fixed after the end of a decade when Lipps had left Bonn and had been at Munich for several years. During this period his most important publication from our point of view was his *Raumaesthetik und geometrisch-optische Täuschungen* (1893-1897), an empirical, although not really an experimental, investigation of the optical illusions and the perceptual principles underlying them.

In 1894 Lipps went to Munich to replace Stumpf, who went to Berlin. Lipps was replaced at Breslau by Ebbinghaus, and stayed at Munich the rest of his life. Cornelius was at Munich and found Lipps an unsympathetic and difficult senior colleague, but then Lipps was not in the best of health. The third period of Lipps's life is said to be from about 1900 to 1908. These years saw the culmination of his work in systematization and in esthetics. His act psychology is most adequately set forth in his *Leitfaden der Psychologie,* which was published in 1903 and went into new editions in 1906 and 1909. The two big volumes of his *Aesthetik* came out in 1903 and 1906, and Lipps is perhaps best known for them. His esthetic theory of *Einfühlung* is famous, and is probably primarily responsible for the coining of the English equivalent, *empathy,* for that German word.

Lipps's health failed completely about 1908 and of the last six years of his life there is little to record.

His writings were numerous and lengthy. In the field of perception one finds papers on space-perception, geometrical illusions, eye-movement, form-quality, tonal fusion, and melody. The papers on esthetics and empathy are numerous, and there is one well-known paper on humor and the comic. In the general theoretical field he wrote on suggestion, the unconscious, consciousness and objectivity, elements and relations, psychic causality, and psychological quantity (thus touching the controversy in psychophysics). All these papers are, however, above the laboratory and at the theorist's level. Lipps was just on the edge of experimental psychology, not quite within it.

We have said that as a systematist Lipps was a self-psychologist as well as an act psychologist. Such a combination is not strange, for activity tends to be two-sided, to imply subject and

object. In Lipps's system, however, one does not find the subject, the act, and the object separated; it is rather that the activity is subjective and objective. Consciousness is activity only, but the self and the content are within it. This idea is similar to Brentano's intentional inexistence. Thus Lipps described this characteristic of mind by saying that it is of the essence of consciousness to "jump over its own shadow."

For the experimentalist, the main point of interest about a system of act psychology is to see what sort of a datum the act really is. In general, as we have said, it is a datum that does not lend itself to experimentation. What sort of a datum is it then? What is its experiential nature? In part we have answered this question in our discussion in the last chapter of the imageless thoughts. It will perhaps become clearer in the next section, where we shall consider Külpe and Messer. It is also quite plain in McDougall's psychology, which belongs in the next chapter. There is no necessity, therefore, for our attempting to enter here upon Lipps's very intricate and difficult psychological system.

Psychologies of Act and Content

Act and content have been in Europe the two horns of a dilemma. The empiricist, who has his attention always on the nature of his own consciousness, is driven to accept the act as the essence of mind. The experimentalist accepts content because he can work with it, and, having accepted it, introspection of his own consciousness fails to convince him of the validity of the act as mental stuff. The empiricist accuses the experimentalist of being prejudiced by his method. The experimentalist replies that casual empirical observation has always failed to yield the truth, and that science resorts to experiment for this reason. An *impasse* of this sort is always unsatisfactory; however, there have been movements toward the resolution of the dilemma.

In the first place, we must note that there has been some movement of act psychologists toward the experimental method; and there would, of course, be no dilemma, if the acts had been experimented upon more. Brentano was sympathetic with the new experimental psychology. Meinong founded a laboratory. Witasek's psychology, like Lipps's, tended to deal with the data of experimental psychology. Witasek was definitely of the school

of Brentano and Meinong, although he granted contents as well as acts in psychology. His experiments on space perception are especially interesting for the results were translated into terms of act. Benussi, as we have said, carried this tendency further. He also was of the school of Brentano and Meinong, but he was primarily an experimentalist and only indirectly a systematist—an unusual relationship for an act psychologist. In Witasek and Benussi it becomes clear that most of the data of perception can be expressed in terms of act. One needs only to consider the ordinary psychophysical experiment to see what happens. Such an experiment involves judgments as its data. The psychologist of content treats these judgments as observations of what is judged. The psychologist of act stresses the judging and not its content. Nevertheless, for all this work, integration of the two opposing views has not gone far in the hands of the Austrians.

A movement that has progressed further has been the forced union between act and content. Külpe led in this movement, although Messer has given the most complete example of it. The influence of Husserl was behind it, largely because Külpe and Messer took Husserl very seriously and sympathetically. This movement simply brings both act and content into psychology and leaves them there, to be dealt with, side by side. The result might be called a bipartite psychology, since there are now two kinds of very different materials, impalpable acts and palpable contents, to be wrought into a systematic whole. Thus one has, on the one hand, the pure act psychology of Brentano and Meinong, and on the other hand the compromise with content of such men as Husserl, Külpe, Witasek and Messer. We may begin with a rough sketch of the result in the hands of Messer, who wrote the only complete book from this dual point of view.

August Messer (1867-) is a philosopher with psychological interests, who as a student at Giessen came under the influence of the philosopher Schiller. After his student days, he occupied for a few years teaching posts in several *Gymnasien,* and then in 1899 became *Dozent* in philosophy at Giessen. He was made *ausserordentlicher Professor* there in 1904 and professor in 1910. The first *Kongress für experimentelle Psychologie* met at Giessen in 1904, and there Külpe read his paper on *Versuche über Abstraktion,* to which we have referred in the preceding chapter. Messer was much impressed by Külpe's outlook on philosophy

and psychology and decided to engage in work under his direction. Accordingly he spent the summer semester of 1905 with Külpe at Würzburg, and, as we now know, his *Experimentell-psychologische Untersuchungen über das Denken* (1906) was the result, one of the important studies of the Würzburg school. Messer's interests lay as much in epistemology as in psychology, and Külpe's interests were then becoming very much the same. Messer was only five years Külpe's junior and an intellectual friendship sprang up between the two men. In 1908 Messer published *Empfindung und Denken,* a book that grew out of his work at Würzburg. In it he attempted to formulate the alternative to sensationism by the discussion of such problems as perception, meaning, attention, abstraction, judgment, and thought, topics which can be made to fall within the borderland between psychology and epistemology. One gets the beginning of his dichotomy between content and act in his chapters on sensory elements and thought elements in perception. Thus Messer was greatly impressed by Külpe's *Die Realisierung,* of which Külpe published the first volume, the only volume which he himself completed, in 1912. In these days Külpe had come over to the dual point of view, as his posthumous *Vorlesungen* show. Messer made that approach to psychology explicit in his *Psychologie* of 1914 (second edition, 1920). Priority of explicit publication is therefore Messer's. Külpe originally was his inspiration. We do not know how much each got from the other in their friendly intercourse between Giessen and Würzburg, seventy-five miles apart, and since no one else has raised the question, we do not need to raise it ourselves.

The marriage of act and content, which Messer accomplished, was not a mere juxtaposition of incompatibles. Messer held that psychology deals only with intentional experiences, 'acts' taken in a broad sense. However, he argued, such experiences involve both an impalpable activity ('act' taken in a narrower sense) and the palpable content of the act. Psychology must therefore consider both, extending its scope from the contents to include the acts.

He distinguished three kinds of intentional experiences: knowing (consciousness of object), feeling (consciousness of state), and willing (consciousness of cause). For each of these classes he discussed both the elements of content and the elements of act that enter into them.

The contents of knowing were for him the sensations, the images, the temporal and spatial contents, and the impressions. Sensations and images are the obvious sensory material of which he was trying to take account. To spatial and temporal experience, always trouble-makers for the systematist, he gave palpable status, much as Titchener had done in making them sensory attributes. Messer's impressions were the palpable relations, experiences like *similar, different, greater, less,* and their kind. Even the experimental psychophysicist has been bothered by the exclusion of such data from the inventory of the palpable, for they seem introspectively to be more like immediate experience than mere judgments of contents. Messer, then, was very generous to the contents, but he could afford to be because he had so much left to say about the acts. The acts of knowing he gave an elaborate logical treatment. There are perception and its two opposites, memory and imagination, all of which are characterized by having a content; and there is also the coördinate system of thinking of present object, of past object, and of a construct, which have, however, no content. At the higher levels of complexity the acts become quite involved. Relating and comparing are simple acts of knowing; so are the paired opposites, affirmation and negation; so too would seem to be the series of acts from conviction to conjecture, a series which marks affirmation or negation with its degree. Judgment involves all of these acts, as does also its opposite, supposal.

Feeling has sensations as contents, and affective preference and feelings of value as acts. The simple feelings, which have made so much difficulty for the psychologists of content, are placed by Messer on the border-line, as being sometimes contents, but sometimes also, in so far as they are impalpable, acts.

Willing has sensations for contents. It has appetition, desire, and will for acts. Conation is equivocal, like the simple feelings; in part it is sensation, in part act.

Such is Messer's 'bipartite' psychology. The term is applicable, for act and content are for him not only as different as the impalpable and the palpable, but in particular cases they may be separable. If you wish to know what content is like without act, you have only, Messer thought, to consider the margin of consciousness where bare, meaningless content occurs. If you wish

to know act without content, you need only examine imageless thought.

The relevancy of such a system to experimental psychology lies not in any positive contribution, but in the removal of constraints. As long as mind was thought to have to be all act or all content, act psychology came to be, for the most part, opposed to experimental psychology. In a system like Messer's, content psychology, and thus the traditional experimental psychology, is given a clean bill of health and allowed to go about its business as long as it does not interfere with its partner in psychology.

It is plain from Külpe's posthumous and incomplete *Vorlesungen* (1920) that he was going the same way as Messer. One gets in this book very much more of the Wundtian tone, as one would expect from the author of the *Grundriss,* written in Leipzig in 1893. However, Külpe had come a long way in twenty years. The outcome of Würzburg had driven him toward Brentano and Husserl. To his earlier psychology Külpe added the acts (although, like Stumpf, he called them functions), just as Messer had added contents to acts.

To the present author, the most interesting thing in Külpe's book is his effort to give the criteria of difference between content and function. Here it seems almost as if Külpe, the experimentalist, had caught the will-o'-the-wisp of all experimentalists, the impalpable moment of mind.

In brief, Külpe's argument runs thus. Content and function are different facts of mental life. (1) They must be different because they are demonstrably separable in experience. Content occurs with but little function in dreams and in the bare givenness of an object; and function occurs with but little content in acts like bare noticing or bare expecting where there is no object. (2) Moreover the two are independently variable. Content changes without function when one perceives one sense-object and then another, keeping on perceiving all the time. Function changes without content when one successively perceives, recognizes, and judges the same sensory content. (This is not Brentano's doctrine, where the separation of act and content is much less sharp.) (3) Moreover, content and function are characteristically different. Contents are analyzable in consciousness, whereas functions are not, for analysis alters the function but not the content. Thus contents are observable by introspection,

but functions only by retrospection. Moreover, contents are relatively stable and functions are relatively instable. In these characteristics one comes to see just what has been meant by the impalpability (*Unanschaulichkeit*) of the functions or acts. (4) Contents and functions alike have both intensity and quality, but there is no relationship between the two classes. Qualitative differences among functions mean nothing for qualitative differences among contents, nor is an intense tone in any way comparable to an intense desire. Duration, however, they possess in common: the tone and the desire may be compared in their temporal persistence. (5) Finally, Külpe tells us that contents and functions can be distinguished because they obey different laws. The laws of content are association, fusion, contrast, relation to stimulus and sense-organ, and psychological correlation in general. The laws of function include the facts of the effect of a point of view or *Aufgabe,* and the laws of the determining tendency. Here Külpe had only the discoveries of the Würzburg school to draw upon for the laws of function, but he doubtless thought that these laws would multiply when more experiments had been undertaken upon the impalpable functions.

Külpe's early death is much to be regretted. One feels that, if he had lived to complete and work over this new psychology, he might have ended by making the acts seem much more reasonable and thus more useful to the experimental psychologist. As it is, one gets glimmerings of what might have happened, but no clear insight. Of course what did happen is another matter: experimental phenomenology and *Gestaltpsychologie* sprang up. These movements worked experimentally with acts and contents without so naming them because they admit all classes of experience into consideration. They are, as a matter of fact, almost too recent to be history at all; at any rate, they must be reserved for mention in a later chapter. Moreover, we must turn next to modern British psychology, where the Austrians have had great influence.

We may close the present chapter with a tabular key to the varying terminology of the act psychologists and to their epistemological views as to what constitutes the subject-matter of psychology. The different views are very confusing and a schema aids clarity even though it may imply a false rigidity. Lipps is

omitted from this table because he called the subject-matter of psychology content and described it as act, so that it is impossible to decide in which column to place him.

THE SUBJECT-MATTER OF PSYCHOLOGY IS PRINTED IN CAPITALS.

Thus the lines inclose the field of psychology.

The subject-matter of physics is printed in italics.

The subject-matter of phenomenology is printed in roman.

			Other Available Data	
	SUBJECT-MATTER OF PSYCHOLOGY	Intentional Data	CONTENT AS DATA	OTHER DATA NOT WITHIN PSYCHOLOGY
BRENTANO	ACT	= ACT	→ *Content*	
STUMPF	FUNCTION	= FUNCTION	↔ Phenomena	*Physical data*
WITASEK	MIND	= ACT	+ CONTENT	*Physical data*
HUSSERL	ACT	= ACT	→ *Content*	Phenomena
MESSER	ACT (broadly)	= ACT (narrowly)	+ CONTENT	*Physical data*
KÜLPE	CONSCIOUSNESS	= FUNCTION	+ CONTENT	*Physical data*

Notes

It is interesting to study Austro-German psychology with one's eye on the map. The tendency has been for experimental psychology to thrive better in the north and for act psychology to flourish in the south, although of course act and experiment are not necessarily incompatible. For instance, one can arbitrarily draw a line from Metz to Warsaw, leaving Vienna, Graz, Prague, Munich, and Würzburg to the south, and Leipzig, Göttingen, Berlin, Frankfurt, and Marburg to the north. If one tried to draw a picture of the psychology of each of these artificial groups, one would draw very different pictures, although one would by no means have created a clear dichotomy. Cf. the map of universities inside the covers of this book.

In Austria and southern Germany the influence of the Catholic Church is great. The psychology of Aristotle adapted to the modern world by Brentano is the appropriate psychology for this region. Act fits the Church better than content, for it tends less toward a mechanistic and therefore a deterministic psychology. Experimental psychology has to be deterministic; experimental science has no choice if it is to avoid suicide. It is impossible to know just how much religious faith and scientific faith are connected, but one should not expect complete dissociation in psychology. We have already seen how Brentano's psychological career was twice dramatically affected by the Church, and Stumpf also felt its influence.

Ehrenfels

Christian von Ehrenfels (1859-) was Brentano's student at Vienna (1882-1885), then associated with Meinong as *Dozent* at Graz (1885-1888), then *Dozent* at Vienna (1889-1896), then *ausserordentlicher Professor* at Prague (1896-1900) and professor (1900-). He is a man of broad cultural interests, and his important writings have to do with the drama, Wagner, sexual ethics, the theory of values, and cosmogony. He comes into the history of psychology

only incidentally as the originator of the doctrine of form-qualities. This paper of his is: Ueber Gestaltqualitäten, *Vtljsch. f. wiss. Philos.*, 14, 1890, 249-292.

Meinong

Alexius Meinong (1853-1920) was a student at Vienna (1870-1878), whom Brentano, when he came there in 1874, interested in philosophy. Meinong, after he had taken his degree in philosophy, remained at Vienna as *Dozent* (1878-1882), and then was *ausserordentlicher Professor* (1882-1889) and later professor (1889-1920) at Graz, where he founded the first Austrian laboratory of psychology (1894). For his own sketch of his life and evaluation of his work, see R. Schmidt, *Philosophie der Gegenwart in Selbstdarstellungen*, I, 1923, 101-160 (also separate). This gives a selected bibliography; for a complete bibliography, see his *Gesammelte Abhandlungen*, I, 1914, 631-634, or II, 1913, 551-554 (the two lists are the same). This first volume is *Abhandlungen zur Psychologie* and contains most of the psychological papers. See also A. Höfler's account of Meinong's psychology in *Zsch. f. Psychol.*, 86, 1921, 368-374.

Meinong's field was theoretical psychology and the theory of knowledge. A very great deal of his work was psychological, and he has had much influence wherever the 'Austrian school' has been influential (e.g., on Ward and Stout in England). He was one of the editors of the *Zeitschrift für Psychologie*. His name is associated with psychophysics because of his discussion of Weber's law and of mental measurement in general, for mental measurement raises an epistemological problem. His contributions to the doctrine of form-qualities are to be found in Zur Psychologie der Komplexionen und Relationen, *Zsch. f. Psychol.*, 2, 1891, 245-265; Ueber Gegenstände höherer Ordnung und deren Verhältnis zur inneren Wahrnehmung, *ibid.*, 11, 1899, 180-272.

Incidentally it may be remarked that *fundirende und fundirte Inhalte* should be translated as "founding and founded contents" and not as "funding and funded contents" (cf. Bentley, *op. cit. infra.*). It was tempting to the elementarists to think that Meinong meant by his verb "founding" (as in a foundry) = fusing, and hence a fusion = fund (*fundo, fundere, fusum*), whereas he really meant "found" in the sense of forming a fundamental basis or foundation (*fundo, fundare, fundatum*).

Cornelius

Hans Cornelius (1863-), after studying first mathematics and physics and then chemistry, became *Dozent* in philosophy at Munich (1894-1903) in the year that Lipps replaced Stumpf at Munich. Stumpf brought him to philosophy, but there was no sympathy between Lipps and Cornelius. The latter was later *ausserordentlicher Professor* of philosophy at Munich (1903-1910) and then professor of philosophy at Frankfurt (1910-). For his own account of his life and work, see Schmidt, *op. cit.*, II, 1923, 83-102 (also separate), which includes a short bibliography of his writings. His chief work has been epistemological and philosophical, but he has thrice contributed importantly to theoretical psychology. For his views referred to in the text, see especially Ueber Verschmelzung und Analyse, *Vtljsch. f. wiss. Philos.*, 16, 1892, 404-446; 17, 1893, 30-75; but for the more general systematic discussion of these matters, see his *Psychologie als Erfahrungswissenschaft* (a general textbook of psychology), 1897, 128-235, esp. 164-168; *Zsch. f. Psychol.*, 24, 1900, 117-141.

Schumann

We have already referred to Friedrich Schumann (1863-) in the two preceding chapters. He was at Göttingen (1881-1894), Berlin (1894-

1905), and Zürich (1905-1910), and is now at Frankfurt (1910-). His classical papers on visual space-perception are: Beiträge zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol.*, 23, 1900, 1-32; 24, 1900, 1-33; 30, 1902, 241-291, 321-339; 36, 1904, 161-185. For more recent research that carries on this point of view in the investigation of visual form, see M. J. Zigler, *Amer. J. Psychol.*, 31, 1920, 273-300.

Witasek

Stephan Witasek (1870-1915) spent his academic life at Graz. He was *Dozent* there (1900) and was later given the title of professor. The author knows of no account of his life or work. He was primarily a psychologist, with incidental interests in educational psychology, esthetics, and ethics. His most important writings are his textbook of psychology and his psychology of visual space-perception (both cited below). For his views on complexions and the act of producing, see his Beiträge zur Psychologie der Komplexionen, *Zsch. f. Psychol.*, 14, 1897, 401-435; *Grundlinien der Psychologie*, 1908, 222-246; *Psychologie der Raumwahrnehmung des Auges*, 1910, 291-338. It is the *Grundlinien*, of course, that most clearly represents his bipartite psychology of act and content, on which cf. E. B. Titchener, *Amer. J. Psychol.*, 33, 1922, 62-68.

Benussi

Vittorio Benussi (1878-1927) was at Graz from about 1902 until the World War, and he died at Padua. The author knows of no biographical record of him. His more productive period was before the war at Graz. He was unlike the other Austrians in that nearly all his papers were experimental and in that the list is long. He worked on the perceptions of time, of weight, of solidity, of optical illusions, of visual movement, and of tactual movement. There are fifteen such articles in the *Zsch. f. Psychol.* and the *Arch. f. d. ges. Psychol.* between 1902 and 1920.

Benussi's theory of perception is scattered through these experimental papers. K. Koffka has attempted to present it and examine it in *Zsch. f. Psychol.*, 73, 1915, 11-90, and lists there twelve important articles. There is much informative exposition of Benussi scattered through H. Helson's Psychology of Gestalt, *Amer. J. Psychol.*, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223 (also separate with an index). Helson gives a list of sixteen articles, pp. 217 f. In general Benussi followed Meinong, and the act of producing looms large, a fact to which Koffka, as exponent of modern *Gestaltpsychologie*, takes exception.

Form-Qualities in General

The doctrine of form-quality is excellently expounded and criticized by M. Bentley, *Amer. J. Psychol.*, 13, 1902, 269-293. G. F. Stout adopted the view and presented his version of it in his *Analytic Psychology*, 1896, I, 66-77. Seyfert, Schumann, and von Aster are all regarded as criticizing the doctrine of form-quality because they made experimental analyses of the perception of simple forms without appealing to this concept or to the concept of the act, but the reader will not find that their papers directly attack the Austrian view. See R. Seyfert, *Philos. Stud.*, 14, 1898, 5[illegible]-566; 18, 1902, 189-214; Schumann, *opp. citt.*; E. von Aster, *Zsch. f. Psychol.*, 43, 1906, 161-203.

Lipps

For a short account of Lipps's life and psychology, see G. Anschütz, *Arch. f. d. ges. Psychol.*, 34, 1915, 1-13. Anschütz gives the essential bibliography. The text has mentioned the more important psychological works of Lipps. For a discussion of the act and the self in Lipps's system, see Titche-

ner, *op. cit.*, 50-54, 69-75. Another shorter list of Lipps's writings occurs in *Amer. J. Psychol.*, 26, 1915, 160. On the systematic matters, see also O. Klemm, *History of Psychology* (cf. index).

Messer

For August Messer's (1867-) own account of his life and work, see Schmidt, *op. cit.*, III, 1922, 145-176 (also separate). Here a list of twenty-nine of his important publications is given. The text mentions explicitly those with which it is concerned. Cf. the discussion of Messer in the preceding chapter. On Messer's system, see Titchener, *op. cit.*, 58-68.

Külpe

See notes to the preceding chapter.

Act Psychology in General

See Titchener, *op. cit.*, 43-83. The reader may also here be referred again to Titchener's discussion of Brentano and Wundt, *ibid.*, 32, 1921, 108-120; 36, 1925, 313-323. Titchener has been the chief expounder of this school in English.

Chapter 19

BRITISH PSYCHOLOGY

FROM time to time throughout the preceding chapters we have gained insight into the manner in which science advances. At close view, the course of science seems discontinuous; all at once a 'genius' makes a discovery or formulates a theory, and productive research follows on immediately. At the greater range of historical perspective, the course of science seems to be continuous, and the 'genius' appears as an opportunist who takes advantage of the preparation of the times. Chance also plays a rôle, but the history of science selects for record those endeavors that have been fruitful and neglects the promising failures, thus obscuring the operation of chance. However, in addition to these primary factors in scientific progress, there is another force that has been especially evident during the last century, namely, the support of society. We have seen repeatedly how young men, faced with the problem of earning a living, have nevertheless in one way or another become psychologists or philosophers of note, but we have seen nothing of those who did not become eminent psychologists because of economic handicaps. In psychology the usual social support has come by way of university appointments. In Germany there have been no chairs of psychology, but men holding chairs of philosophy have been able to devote themselves to psychology and even to experimental psychology. Had there been no appointments for psychologists, psychology would not have prospered. As it is, this economic factor has led to a German psychology much less free from philosophy than has been the case in America, where the same relationship in professorial appointments has held much less rigidly. In America chairs of psychology were created early and the economic resources of the universities were placed directly behind the new science. In Great Britain there has never been the same support from universities. The result has been that, in general during the last forty years, Germany and America have

led the way in the new psychology, and Great Britain has followed.

In Germany and America the course of development has been from philosophical psychology to experimental psychology and on to applied psychology. Experimental psychology came to Germany before it came to America, but America was the first to make substantial progress in applied psychology. In Great Britain philosophical psychology in the hands of Ward and Stout remained in the saddle for a long time, and, quite recently, applied psychology has sprung up and flourished with the economic support of industry. Experimental psychology, which has been supposed in Germany and America to constitute a necessary intermediate stage, has not been lacking, but it has worked against difficulties. Oxford has resisted modern psychology, and no psychologist of note has held an important appointment there. In fact, it was only after the most bitter controversy in 1882 that this stronghold of the classics was brought to the establishment of a physiological laboratory. At Cambridge, Ward, as professor of moral philosophy since 1897, was as much the psychologist as the philosopher, but there was almost nothing of the experimentalist about him. Nevertheless, Cambridge had under Myers a psychological laboratory which has since led Britain in its experimental research. The director of this laboratory has not been, however, a professor. Spearman was professor of mind and logic at London; but, although trained in Germany and sympathetic to the new psychology, he was not exactly an experimentalist after the manner of Germans and Americans. Sully wrote about experiments but did not experiment. Stout, professor of logic and metaphysics at St. Andrews, was not an experimentalist. Galton in a sense initiated experimental psychology in England as he initiated so many other things, but he was independent of the universities. McDougall was an experimentalist and did much to establish experimental physiological psychology in Great Britain, but he never held an important post at a British university. Lloyd Morgan was professor of psychology and education at Bristol. Myers was once professor of psychology in King's College, London, but Rivers was never more than lecturer in physiological and experimental psychology, at Cambridge. From 1890 to 1910, when Germany and America were teeming with laboratories and professorial experimental psychologists,

Great Britain was advancing slowly in the new science only by way of the work of a few competent men, who must have had some independent means.

The result of this national difference has been that the new psychology became professionalized in Germany and America as it did not in Great Britain until the recent growth of industrial psychology. In Germany and America there was a living to be had for psychologists in universities. There were many laboratories. Young men studied for the doctor's degree in them, produced experimental research as doctoral dissertations, and then went out to join the staffs of other laboratories. Psychology was a profession. In Great Britain it was not.

The picture of modern British psychology is thus very different from the picture of modern German psychology, and in no way is the contrast so clearly seen as between Francis Galton and Wundt. Wundt we already know.

Galton was a genius. He was a genius in the technical sense that his youthful precocity was such that his intelligence quotient, had he been tested for intelligence, would probably have approached 200; that is to say, he belonged on this basis with the most intelligent persons who have been tested or whose biographies have been examined, with John Stuart Mill, Goethe, and Leibnitz. He was also a genius in the popular sense, for he was a brilliant, original, versatile, stimulating scholar, whose research opened large new fields of investigation. He had independent means and he never held an academic appointment. He was a free-lance and a gentleman scientist. In fact, the question as to whether he should continue creative work or settle down, like his brothers, as a country gentleman was once the serious dilemma of his life. His scientific contributions, beside his early ventures in exploration, include investigations in simple mechanics and the invention of apparatus, his persistent interest in meteorology, his continued study of inheritance and his foundation of the art of eugenics, his development of Quetelet's methods of statistics and their application to anthropological and psychological problems, his numerous researches in anthropometry, and his initiation of the experimental psychology of tests in England. He was forever seeing new relationships and working them out, either on paper or in practice. No field was beyond his possible interest, no

domain was marked off in advance as being out of his province. A restless interest and a vivid imagination had Francis Galton.

How different he was from Wundt! Was Wundt a genius? We hardly use this phrase of him in spite of his great erudition and his remarkable productivity. Wundt was erudite where Galton was original; Wundt overcame massive obstacles by the weight of his attack; Galton dispatched a difficulty by a thrust of insight. Wundt was forever armored by his system; Galton had no system. Wundt was methodical; Galton was versatile. Wundt's science was interpenetrated by his philosophy; Galton's science was discursive and unstructured. Wundt was interminably arguing; Galton was forever observing. Wundt had a school, a formal, self-conscious school; Galton had friends, influence, and effects only. Thus Wundt was personally intolerant and controversial, whereas Galton was tolerant and ready to be convicted of error. Wundt, as we have said in a previous chapter, was a professional psychologist, the first; Galton had no profession. Thus Wundt in his scientific affairs tended to be unsocial, but Galton, in close personal touch with his eminent contemporaries, was distinctly social. It is impossible to get the entire difference expressed in a phrase, but the important thing that Galton lacked was Wundt's professionalism. As a professional psychologist Wundt always bore upon himself the weight of his past, of the logic of his systematic commitments and of his philosophical predilections. He could work only within the shell of what he had made psychology to be. Galton was free. He had no major commitments. He was not a psychologist nor an anthropologist nor anything at any time except what his vivid interests made him. He had the advantage of competence without the limitation of being an expert.

This contrast between Galton and Wundt shows in the extremes the general difference between German and English science. Individuals, however, may vary from the national type. Helmholtz, as we noted in our discussion of him, was more like the British investigator. Brentano's influence was partly due to his personality. Nevertheless, the gross difference holds. German psychology was institutionalized, and for it we find schools and their leaders. In Great Britain, we have to deal with persons.

Systematic Psychology

We have already seen that it was British empiricism and associationism that furnished a full half of the preparation for experimental psychology. Experimental psychology resulted from the marriage of philosophical psychology and physiology. Although Herbart and Lotze were extremely important in making preparations for the new psychology, in general it is true that Wundt got the experimental method from physiology and the systematic structure of the new psychology from Mill and Bain and their British predecessors. The pattern of the 'new' psychology in Germany came from England, and even the act psychologists drew extensively from across the English Channel, where the most psychological philosophy was to be found. Since Great Britain was not backward in its pursuit of physiology, we might have expected this marriage to have taken place there; but the facts are otherwise. Experimental psychology began in Germany and came only by adoption to Great Britain.

After Bain the philosophical tradition in psychology was continued in Great Britain by Ward and Stout. *James Ward* (1843-1925), who acknowledged his psychological debt to Brentano, was, in a sense, an act psychologist. He built up an elaborate and brilliant system about the relation of the active subject to the object. He first presented his psychological views in the article on psychology in the ninth edition of the *Encyclopædia Britannica* (1886), and elaborated and perfected them for the eleventh edition (1911). Subsequently he rounded out the picture of his *Psychological Principles* (1918), a work which he characterized as having been in preparation for forty years. To the experimentalist this system seems almost an anachronism, so logically perfect is it, and so devoid of dependence on the accumulating mass of experimental fact. Ward was, however, primarily a philosopher, and his interest in psychology was philosophical after the English tradition. For all this, his influence was very great, and he was after Bain the senior psychologist in Great Britain, for his contemporary, Sully, was less influential. It must not be thought, however, that Ward was hostile to experimental psychology. He was trained at Cambridge, but he also studied abroad at Berlin and Göttingen (in Lotze's day; *ca.* 1874), where he learned to respect the new movement. When he returned to Cambridge (1875), he wanted

to start a psychological laboratory, but was prevented by an opposition that identified a laboratory for the study of mind with the support of materialism. However, Ward was not fitted by temperament for the technique of experimental research, any more than Brentano, who would have liked to found a psychological laboratory at Vienna.

Ward divided the subject-matter of psychology into cognition, feeling, and conation, and the schema whereby he distinguished these phases follows. Psychology deals with "a Subject, (1) non-voluntarily attending to changes in the sensory continuum [*Cognition*]; (2) being, in consequence, either pleased or pained [*Feeling*]; and (3) by voluntary attention or 'innervation' producing changes in the motor continuum [*Conation*]." This is the subjective schema which implies its objective complement. From the point of view of the object, we find that (1) cognition is the presentation of sensory objects, that (3) conation is the presentation of motor objects, and that (2) feeling is not a presentation, because it does not lie in experience but is the primordial consequent of sensory presentation and the condition of motor presentation, an explanatory middle term between the two kinds of presentation. The presentation, the experimental stuff of mind, is an *Erlebnis,* the modern equivalent, Ward thought, of Locke's idea; and, we must note, conation is just as much within experience as cognition. The presentation implies the subject-object relationship, the matter of psychology. We need not go into this system. It is sufficient to see the sort of interest that dominated Ward and how far he was from the psychology of the laboratory.

Ward was not a popular psychologist, for his ideas were too difficult of comprehension and for the most part found definite expression after psychology had already taken on an explicit form in England. The effective presentation of a similar point of view was left to *George Frederick Stout* (1860-), who studied at Cambridge (1881-1883) but not abroad. Stout, in the middle of his life, gained a great influence by writing systematic textbooks of psychology: his *Analytic Psychology* (1896); a *Manual of Psychology* (1899), with many revised editions; and a *Groundwork of Psychology* (1903). These books, although written from different points of view and using different terminology, present an act psychology resembling Ward's. Stout recognized primarily Ward's influence, and secondarily the influence of the

English school and of Herbart. It would also appear that the Austrians, especially Meinong, had something to do with his thinking. Activity comes into Stout's system in the famous doctrine of conation, the fact and the experience of striving. His *Manual* has been very widely used in Great Britain, and for want of a successful competitor is the best representative of the modern British systematic psychology. Stout's influence was also increased by the fact that he became Croom Robertson's successor as editor of *Mind* in 1892.

Stout holds that psychology deals with psychical processes that are in themselves subjective and that have mental objects like sensations. He resembles Ward in insisting that the subject-object relationship gives psychical processes their mental character. He is like Brentano in making sensations objects of the processes. He divided these processes into cognition and interest, and subdivided interest into conation and feeling-attitude. Conation he described as the equivalent of craving, desire, or will; it is characterized by its relation to its satisfaction or fulfilment, and disappears in its satisfaction. The object of conation is whatever appears as its end or as the means to the end. Here one must carefully distinguish between what appears as an end and the actual end. The object of conation that gives it its driving force is not necessarily the terminus to which the activity leads. One may seek salvation, but go to church. Thus Stout did not think that conation explains all activity. He had a great deal to say about unconscious psychical dispositions, which are known only by their effects, and which must therefore be localized in the brain. In general Stout's views, especially his doctrine of conation, interest us because they lead up to McDougall's systematic position.

William McDougall (1871-) is next in the systematic line to Stout, even though he developed his systematic views most explicitly after he left England for America in 1920. McDougall has been called a 'purposivist,' because he has organized his view of mind about the rôle that purposive striving, a child of conation, plays in mental activity. He is thus really an act psychologist like Ward and Stout, but his descent from Brentano is so remote as to leave little resemblance. Perhaps the difference is largely due to the fact that McDougall, unlike Ward and Stout, has been an experimentalist. Moreover, William James had a great

influence upon him, and systematically he took an inheritance more from the Scottish tradition of Dugald Stewart than did Bain, another Scot, who fell under the English influence of the Mills. McDougall had a medical training in Cambridge and London, as well as a period of study with G. E. Müller at Göttingen. He was a member of the Cambridge Anthropological Expedition to Torres Straits and undertook independent anthropological work in Borneo (1898-1899). On returning to England he became a reader in University College, where he commanded the very small psychological laboratory of which mention is made later. He was Wilde reader in mental philosophy at Oxford (1904-1920), where he had by informal personal arrangement a laboratory up to the time of the war (*vide infra*). Then he came to Harvard University in America, where psychologists were supposedly better treated than in England, and is now at Duke University in North Carolina.

McDougall has published numerous experimental researches, many of them on problems of vision, and a small *Physiological Psychology* (1905). His *Body and Mind* (1911), an examination of the theories of the mind-body relation, is a classic. During the World War he was engaged in psychomedical work in the British armies, and quite recently he has published an extensive *Outline of Abnormal Psychology* (1926). He has also been concerned with anthropology and social psychology, and his text on *Social Psychology* (1908, and later editions), has been extremely influential. He wrote a small general text, *Psychology, the Study of Behaviour* (1912), but the subsequent rise of behaviorism in America led him to abandon this term as no longer descriptive of his point of view toward mind. His systematic position is put forth in his *Outline of Psychology* (1923), a book that contrasts with American behaviorism. He has also published several books dealing with practical social problems. Thus McDougall typifies the broad discursive interests of the Englishman that differ so strikingly from German professionalism. He enters the picture of British psychology both because of his experimental contributions and because of his systematic position; nor does the geographical incident of his migration to America make him any less a part of the British tradition.

McDougall's purposive psychology resembles Stout's in its fundamental systematic assumption; that is to say, in respect

of the subject, the object, and activity. There is, however, less of the philosopher and more of the experimentalist about his emphasis. He has defined psychology as "the positive empirical science of the mind," and the mind of an individual organism as "that which expresses itself in his experience and in his behavior." As against introspectionism and behaviorism he represents a third corner of a triangle. His emphasis is always upon the purposive activity of the organism, and he is thus led to stress the behavior of the organism as resulting from the interaction of mind and body. Still he is not a behaviorist in the American sense, for he thinks of behavior as something different from mere movement and mechanical reflexes. In America he was forced to find objective criteria for the behavior that is peculiarly psychological and not merely physical; and his seven marks of behavior are: (1) "spontaneity of movement"; (2) "persistence of activity independently of the continuance of the impression which may have initiated it"; (3) "variation of direction of persistent movements"; (4) "coming to an end of the animal's movements as soon as they have brought about a particular kind of change in its situation"; (5) "preparation for the new situation toward the production of which the action contributes"; (6) "some degree of improvement in the effectiveness of behavior, when it is repeated by the animal under similar circumstances"; and (7) the totality of reaction of the organism. Action which meets these criteria is purposive; a reflex does not meet them.

It is plain that, in making purposive striving the central fact of mind, McDougall has appealed both to the observation of animal and human behavior and to human introspection. Every man finds that the obvious thing about his own mind is 'what he wants to do,' and when he comes to interpret the minds of others they appear to be similar. Effort and volition and freedom are everywhere apparent when mind is in question, and a careful examination of McDougall's criteria of purposive behavior reveals the fact that they involve some degree of indeterminateness, of freedom, that they are in part negative as against the observation of their necessary conditions or causes. Nor can there be any doubt that this element of freedom is exactly what McDougall wished to preserve as the distinguishing mark of mind. There is and must always be a certain indeterminateness about phenomena, and thus there is ample warrant for a view opposed to the deter-

ministic conception of science. When McDougall had developed this view in America, he met with scant sympathy, because of the dominance of mechanistic behaviorism, and also because freedom is out of fashion in science. However, the mechanistic experimental psychologist is never freed of his indeterminate probable error, and to the author it seems as if all the controversy that has arisen were nothing more than that, what the determinist calls a 'probable error,' McDougall calls 'freedom.'

At this point we must make mention of *James Sully* (1842-1923), a contemporary of Ward's, who played a prominent rôle in English psychology as a writer of textbooks. Sully was a man of modest means who thrice applied vainly for university chairs and was at last appointed Grote professor of mind and logic at the University of London, on Croom Robertson's death in 1892, and largely as a result of the publication of the two volumes of his *Human Mind*. He was a close younger friend of Bain, who took a paternal interest in him. He was personally acquainted with the men of science and letters of his day, and was one of the lesser members of the group of friends and correspondents that centered about Darwin and the great intellectual topic of the day, the theory of evolution. Sully's primary interests were psychology and esthetics. He studied abroad at Göttingen (1867-1868), where he came into only superficial contact with Lotze, and at Berlin (1871-1872), where he sought to learn psychology from Helmholtz and anatomy from Du Bois-Reymond. He became a writer, not a scientist. His first book of essays, called *Sensation and Intuition* (1874), received favorable comment from Darwin, and his second on *Illusions* (1881) from Wundt. After these publications he set himself definitely to supply the need for textbooks of psychology, for there had been nothing suitable since Bain's two volumes twenty-five years earlier. He published his *Outlines of Psychology* in 1884 and the book, very well written, was immediately successful. He followed it with a psychology for teachers, and then in 1892 his more ambitious *Human Mind*. After that he turned to writing on child psychology, following the example of Preyer in Germany, but drawing material in part from anthropological sources. Sully's texts fill in the gap between Bain and Stout. A writer of good textbooks is not without his place in the history of science, since he gives knowledge explicit form and diffuses it. On the other hand, Sully's name is probably

better known than his real importance to psychology would warrant, simply because he was the author of such widely used books.

Now we must turn from systematic questions to consider the influence of Charles Darwin and the doctrine of evolution upon British psychology.

Animal Psychology

We have already referred to the general theory of evolution in the first chapter of this book. There we saw how the theory had been anticipated by Goethe and by Erasmus Darwin, who was the grandfather of both Charles Darwin (1809-1882) and of Francis Galton; how Lamarck had put forth his view (1809) of the inheritance of characters acquired by adaptive effort and how Cuvier had opposed him; how Charles Darwin added in the *Origin of Species* (1859) chance variation and survival under natural selection to Lamarck's principle; and how Weismann challenged the doctrine of the inheritance of acquired characteristics (1883). The promulgation by Darwin of the theory of evolution was the great scientific event of the century in Great Britain and perhaps in the world. In England it permeated the thought of the entire group of intellectual men who, largely in friendly association, were the leaders of science. Violent opposition from theologically influenced conservatives only served to rivet attention upon the theory and thus to fix it more firmly in the opinions of its supporters. It was the intellectual ferment of the times, and biology and psychology were dominated by it.

In a limited sense, modern English psychology was created by it, for it raised the problems both of animal psychology and of mental inheritance. Out of Galton's interest in mental inheritance came whatever he contributed to experimental psychology, his tests of human faculty from which, in part, the modern mental tests are descended, his general work in biological inheritance and eugenics and thus the whole biometrical method and school, and thus also at another remove his correlational method of statistics, which constitutes the chief tool of individual psychology in Great Britain and America to-day.

The theory of evolution raised the problem of animal psychology because it demands continuity between different animal forms and between man and the animals. In a vague way the Cartesian notion still prevailed. Man possessed a soul and the animals were

believed to be soulless; and there was, moreover, little distinction then made between a soul and a mind. Opposition to the theory of evolution was based primarily upon its assumption of continuity between man and the brutes, and the obvious reply to criticism was to demonstrate the continuity. The exhibition of mind in animals and of the continuity between the animal and the human mind thus became crucial to the life of the new theory.

Darwin himself began the attack by the publication of his *Expression of Emotion in Man and Animals* (1872), where he drew upon his great wealth of observational knowledge to point out the dependence of emotional behavior in man upon the inheritance of behavior which was useful in animal life but no longer immediately useful to man. His explanation of the curling of the human lips in the sneer as a remnant of the useful habit of the carnivorous animal that bares its canine teeth in rage is only one of the very many examples with which this book is filled.

The next step in this scientific development was taken by *George John Romanes* (1848-1894) who published in 1882 his *Animal Intelligence.* Romanes was a friend of Darwin, a writer, and one of the group of English intellectuals who rallied to Darwin's standard. He wrote mostly on zoölogical topics and, in the interests of squaring religion with evolution, on theological topics.

Romanes's book on animal intelligence is the first comparative psychology that was ever written, and its author used this term believing that comparative psychology would come to rank alongside of comparative anatomy in importance. In this book Romanes did not touch the problem of mental continuity between animals and man, but was content to present a great mass of data on animal behavior, thus laying the groundwork for a subsequent argument for man. He got his material in general from an exhaustive combing of both the scientific and popular accounts of animal behavior, and his method has thus come to be called the 'anecdotal method.' In view of the fact that this procedure is regarded with disapproval to-day, it is only fair to Romanes to say that he himself was aware of the danger of "mongering anecdotes," and laid down certain rigorous principles of selection to which he strictly adhered. However, in observation the line between fact and interpretation is never clear, and the untrained observer too often reports his own interpretation

of the animal's mind instead of describing what he observed in the animal's behavior. In general he tends to 'anthropomorphize' the animal, and this tendency played into the hands of Romanes, who was seeking to find accounts of the highest levels of intelligence in animals in order to demonstrate the mental continuity from animals to man. For this reason the anecdotal method of Romanes has not only been discarded, but has become a term of opprobrium in animal psychology. However, in eschewing the anecdotal for the experimental method, psychologists should remember that Romanes got comparative psychology on its feet in that observational period which, in most scientific work, precedes the development of experimental technique.

Romanes did not regard this first book as his most important venture, successful and popular as it was. He meant in it simply to lay the factual groundwork upon which he could erect his defense of mental evolution. Nor did his second book, *Mental Evolution in Animals* (1883), give, as he had originally intended, the complete argument. It was devoted more to the demonstration of continuity among animal forms. The third book, *Mental Evolution in Man* (1887), completed the series and was the most important from the point of view of the argument about evolution. To us neither of these latter books seems as important as the first, because Romanes lacked a satisfactory classification of human faculties with which to work. He was thrown back upon Locke and the associationists for his terms. His general conclusion was that "simple ideas," like sensory impressions, perceptions, and the memories of perceptions, are common to all animals and man; that "complex ideas," the associative compounds, with which associationism dealt (and he might have added Wundt), belong to some animals and to man; and that "notional ideas," the concepts of abstract and general thinking, are the "unique prerogative of man." Again we should be lenient toward Romanes's analytical difficulties; modern psychology is still struggling by way of 'free images,' 'delayed reactions,' and the vague concept of 'insight' to find crucial differentiating facilities in comparative psychology.

The dangers of the anecdotal method were recognized by *C. Lloyd Morgan* (1852-), who undertook to offset the anthropomorphic tendency in the interpretation of the animal mind by an appeal to the 'law of parsimony.' This law as applied to

animal psychology is often known as 'Lloyd Morgan's canon,' and his formulation of it (1894) was as follows: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale." The justification of the canon lies in the need for offsetting one constant error by introducing another with the opposite effect. At the end of the last century, when the proof of the theory of evolution was uppermost in every one's mind, such a course was sound: if evolutionary continuity could be proved in spite of the adoption of the canon, then evolution must be true. If the proof had failed with the use of the canon, no harm would have been done; we should have had simply a failure of proof, not a falsification of fact. To-day, however, when our interests center primarily in the description of the animal mind, the canon is less of a safeguard; nature is notoriously prodigal; why should we interpret only parsimoniously?

Lloyd Morgan's reaction against Romanes was represented first by his *Animal Life and Intelligence* (1890-1891), which was later revised under the title *Animal Behaviour* (1900). His best-known book of this period is the *Introduction to Comparative Psychology* (1894), a quite general psychology that deals in detail with the relation of the animal mind to the human. It is this book that gives the methodological principles and the canon of interpretation. All these books contain many accounts of the author's own experiments upon animals, experiments which lie midway between the observation of the naturalist in the field and observation by way of artificial situations in the laboratory. They consist in the careful observation of animal behavior when the usual environment has been modified so as to create special situations. Thus to say that experimental animal psychology began with Thorndike's use of puzzle-boxes in 1898 is to limit the meaning of the word *experimental* to the formal laboratory with apparatus.

The conservative view of Lloyd Morgan received support at this time from Jacques Loeb (1859-1924), then in Germany, who put forth his theory of the tropism in 1890. In part Loeb's theory represented a return to the mechanistic view of Descartes that animals are automata, but Loeb was not so sweeping. He held that the possession of "associative memory" is the criterion of

consciousness and that only the lower animals are therefore unconscious. He was not reacting against Darwinism, but supporting his faith in the adequacy of physico-chemical methods for scientific study of physiology and behavior. As a matter of fact, it has proven impracticable to establish a criterion for mind. The behavior of even the lowest animals (and perhaps of some machines) is slightly modifiable through experience and might therefore depend on 'associative memory.' Nevertheless Loeb's theory and researches reacted against the anecdotal method, although more in America than in England.

In England there was other research of importance to support the work of Romanes and Lloyd Morgan. Sir John Lubbock (1834-1913) published *Ants, Wasps, and Bees* (1882) in the same year that Romanes's first book appeared. This book is full of information about the social insects, whose high order of civilization seems, except for the fact that it is not readily modified, to establish the existence of mind in animals. Leonard Trelawney Hobhouse (1864-1929) also belongs in the English picture because of his *Mind in Evolution* (1901), a book in which he argued out the whole matter and presented some of his own experiments. These experiments were of the same type as Lloyd Morgans's, and are not unlike Köhler's on apes (1917), although they lack the added significance of being related to a new system of psychology.

Outside of England there was also important work in progress, especially on the insects. J. Henri Fabre's (1823-1915) studies of entomological behavior ran in successive volumes from 1879 to 1904. The work of Auguste Forel (1848-), principally on ants, dates from 1874 to 1922, and his first important book on the sensations of insects came out while Romanes was publishing (1887). Albrecht Bethe (1872-1931) published his well-known research on ants and bees in 1898, a study which presented the mechanistic interpretation of the complex behavior of these social insects. Binet had published on the psychic life of micro-organisms in 1888; and this research was taken up very effectively in America by H. S. Jennings at the very end of the century and later. All this work had its roots in the new interest in the animal mind that Darwin's theory aroused.

At the beginning of the present century, the initiative in animal psychology passed to America, where Thorndike's subjection

of animals to formal laboratory technique began a very active period of research with mazes, puzzle-boxes, and physical apparatus for testing sensory discrimination, as well as the organization of special laboratories of comparative psychology.

Mental Inheritance

Another way in which Darwin stimulated the thought of his times was by arousing an interest in the problems of heredity and mental inheritance. Galton, his half-cousin, became the leader in the latter field. In 1869 he published *Hereditary Genius,* a careful biographical study of the tendency of genius to run in families. His argument that reputation is a reliable measure of the mental capacity that is genius can be questioned; nevertheless his work has preserved its importance for a half-century because many other facts have supported the belief in the inheritance of intelligence and because no one can question his primary findings that eminent men tend to have eminent offspring, however distinction is transmitted, socially or biologically. This book also represents the beginning of Galton's statistical work, not only in respect of the collection of data but also with regard to their method of treatment.

Galton got his ideas on statistical method from the Belgian statistician, Adolph Quetelet (1796-1874), who was the first to apply Laplace's and Gauss's normal law of error to the distribution of human data, biological and social. Quetelet found that certain anthropometric measurements, like the heights of French army conscripts and the girths of the chests of Scotch soldiers, were distributed in frequency in accordance with this normal law, the bell-shaped probability ('Gaussian') curve. The law had been developed originally in connection with the theory of probabilities in games of chance. It had also been applied to other cases of chance variation, and used by Gauss to express, among other things, the distribution of errors in observation. The coupling of this mathematical function with the notion of error seemed to imply that the law shows the variability that occurs when an ideal is aimed at and achieved only with varying degrees of success, like the distribution of shots on either side of a line at which aim is taken. Thus Quetelet assumed from the approximate applicability of the law to human variability that we might regard

such human variation as if it occurred when nature aimed at an ideal and missed by varying amounts. Since the curve is symmetrical, its average is medial and gives, from the distribution of errors alone, the position of the ideal that nature sought. In this manner we can understand Quetelet's doctrine of *l'homme moyen,* in which the average man appears as nature's ideal, and deviations toward the good as well as toward the bad (when human characters can be thus evaluated) appear as nature's mistakes of different degree. The average is the most frequent value and large errors are rare.

Galton believed that quantitative measurement is the mark of a full-grown science and he adopted Quetelet's use of the normal law in order to convert the frequency of occurrence of genius into measures of its degree; that is to say, he set up a scale of lettered grades of genius from "A," just above the average, up to "G" and to "X," which represented all grades above "G." For example, ability "F" was defined as that level attained by one man in 4,300, "G" as the attainment of one man in 79,000, and "X" as one in 1,000,000. Similarly there must be a descending scale from "a," below the average, to "idiots and imbeciles" at "f," "g," and "x." This procedure is a method of transforming observed statistical frequencies into another scale, in which adjustment has been made for the fact that cases pile up about the average and that two adjacent cases near the average are presumably separated by a smaller interval than two cases near the extreme, and it has become one of the fundamental processes of modern statistics and mental measurement, in spite of criticism that has been directed against its rigorous mathematical application.

Galton followed *Hereditary Genius* with his study of *English Men of Science* (1874) and then *Natural Inheritance* (1889). These were his books, packed full of biographical research and ingenious treatment and interpretation. Beside them he published between thirty and forty papers on problems of inheritance, of which the most important is a study of the dependence of the resemblances of twins upon "nature and nurture" (1876). In *Hereditary Genius* Galton had concluded that Athenian civilization was, in mental ability, about as far above present-day British civilization as the British are to-day above the Negro, and his mind was constantly occupied with the problem of improving

the race. To the project and science of substituting for natural selection an intelligent selection in the interests of racial improvement Galton gave the name *eugenics* in 1883, and the matter was much mooted at that time and later. Finally in 1904 he endowed at the University of London a research fellowship in eugenics, and Karl Pearson became the incumbent. The resulting Francis Galton Laboratory for National Eugenics was in University College and was conducted in connection with Pearson's older Biometric Laboratory. In 1911 the two were combined in a department of applied statistics under Pearson as Galton professor. Pearson's work is another story. It is well known how his development of the statistical methods has for more than two decades dominated the work in individual psychology in England and America.

Statistical Method

In general, experimental psychology and individual psychology have developed independently of each other. The results of laboratory investigations have had little effect upon research by the method of the mental test. There have been repeated attempts to bring the two psychologies together, but their union still lies largely in the future. Nowhere is this artificial dichotomy shown more clearly than in the fact that the histories of the psychophysical methods and of the statistical methods are largely independent of each other, although it is true that their relationship has been evident to William Brown and G. H. Thomson in England, and quite recently to L. L. Thurstone in America. However, in this book we can touch upon individual psychology and the mental tests only incidentally, and principally where the tests become linked with the work of the laboratories. Mental tests are, of course, fundamentally experimental; it is simply an historical artifact that the word *experimental* has come to possess a more circumscribed meaning.

The statistical methods, the chief tool of individual psychology, may be said to have their historical beginning in the work of Quetelet, of whom we have just spoken. We have also seen how Galton took from him the use of the normal law of error and applied it to the measurement of mental ability. It is impossible for us to review here the many varied, if trivial, uses to which the ingenious Galton put this law. An example must suffice. Gal-

ton used the law to determine the proper proportion between the amounts of the first and second prizes in a competition. As we have said, this law requires that individuals near the average of a group should differ from each other less than individuals at the extremes. Moreover, the differences at the extremes become relatively greater in larger groups. Galton's analysis indicates that ability would be fairly rewarded if (in groups of from ten to 100 persons) the first prize were about three times as large as the second prize. This method, because it applies the normal law to discrete cases, has wide implications for statistics. Its mention is appropriate here simply as illustrating the fertility of Galton's mind, the readiness with which he applied facts and principles to novel situations.

It was Galton who first worked out the method of statistical *correlation*. The idea was forming in his mind as early as 1877. He came at the concept by way of his principle of "reversion" or "regression toward mediocrity" that appears in the phenomena of inheritance. In studying, for example, the dependence of the stature of sons upon the stature of their fathers, one can think of a son's stature as partly derived from his father's and partly the result of other causes which are indeterminate from the available data. The sons' statures ought thus to be found more closely grouped about the mean than were the fathers', for the concurrence of the extremes in these two components would be much less frequent than the occurrence of an extreme in one alone. Galton's work in inheritance made him familiar with scatter diagrams of frequencies showing the relationship between paired measures, and he finally came, with some mathematical aid from J. D. H. Dickson, to a knowledge of lines of regression and of the nature of the frequency surface in such a diagram with its elliptical contour-lines, and to the expression of the relationship by a simple coefficient. He dealt with the law of regression toward mediocrity in his presidential address before Section H of the British Association in 1885, reprinting the paper in part the next year. Here he illustrated regression with a mechanical model. He also described an actual experiment with seeds, where the first generation showed the regression toward mediocrity that was required. It was in another paper in 1886 that, with Dickson's slight aid, he developed the "index of co-relation," which came presently to be called "Galton's function," until F. Y. Edgeworth christened it in

1892 the "coefficient of correlation." This coefficient has ever since been conventionally represented by the symbol r (regression).

It was *Karl Pearson* (1857-), however, who gave the theory of correlation its present mathematical foundation. It is true that the fundamental theorems had been worked out much earlier (1846) by the French mathematician, A. Bravais, but it was Pearson who used them in 1896 to develop the solution of Galton's problem. Pearson's investigations of the normality of biological distributions began two years earlier. He possessed more capacity for a technical and elaborate mathematical treatment of statistics than did Galton, and the promise of his later brilliance was already apparent at the first. In 1901 Galton, Pearson, and W. F. R. Weldon founded the journal *Biometrika* for mathematical researches in biology and psychology. The Biometric Laboratory under Pearson was also begun at the University of London in the same year.

Pearson's subsequent contributions to biometrical method are too extensive to find mention in this book. He and Galton established statistical investigation of psychological problems as one of the fundamental methods and other British investigators have employed them. G. Udny Yule (1871-), because of his text on statistical method, is well known, and represents in general a more cautious view about the potency of statistics than do Pearson and his immediate followers. It seems at times as if Pearson believed that inaccurate data could be made to yield accurate conclusions by statistical treatment, a view which the experimentalist seldom shares and which has been criticized by Yule. Godfrey H. Thomson and J. C. Maxwell Garnett are other modern English students of statistical method, and there are many, like Cyril Burt, who are applying the procedures to psychological and educational problems.

Charles Spearman requires here especial mention because of his very important application of the theory of correlation to the problem of intelligence. Just as Galton explained regression by reference to two components, a determinate and an indeterminate element, so Spearman undertook (1904) to interpret correlation between two variables as signifying the existence of a common factor and, in each variable, a specific factor. Measures of apparently different mental abilities are habitually, at first to the surprise and dismay of the psychologist, found to show correlations;

and Spearman, by a method of hierarchy into which we cannot enter, used this fact to conclude that there is a factor common to a wide range of mental ability—that is to say, a factor which may be called general intelligence. Thomson criticized Spearman's statistical method; Garnett came to Spearman's assistance; and the result is the very recent book by Spearman, *The Abilities of Man* (1927).

Galton as a Psychologist

There can be no doubt that Sir Francis Galton (1822-1911) was the pioneer of a 'new' psychology in Great Britain, that is to say, of an experimental psychology that was primarily, though not entirely, concerned with the problem of human individual differences. Karl Pearson would make of Galton a British Wundt, conceiving Galton as working independently of Wundt and concurrently, and as falling short of being the 'father' of British psychology only because the younger generation of psychologists unfortunately chose German parentage. Into this claim we need not enter. We have already compared Galton with Wundt at the beginning of this chapter. We have seen something of Galton's versatility and that psychology was but one of his many interests. There is no question as to his originality and the fact that his inspiration did not come from the Continent, although he was sufficiently conversant with the German work to make use of it and paid considerable attention to the implications of the Weber-Fechner law. As to Pearson's lament that modern experimental psychology has neglected most of Galton's work except the theory of correlation, it is quite true that many of Galton's ingenious ideas might have given rise to important methods and results had they been fostered by an enthusiastic band of followers, although one can only guess as to whether fertile germs lie hidden away in the many written words which this restless, inventive mind scattered so widely that no complete bibliography is likely ever to be made out. On the other hand, it is the author's opinion that the important psychological research of Galton, both as regards apparatus and as regards fact, has been fully assimilated by modern psychology, and that Galton's influence has not been greater for the simple reason that, with attention dispersed in so many other directions, his psychological productivity was not greater. After all, Galton was but half a psychologist and that

for only fifteen years. Wundt was nothing but a psychologist for sixty years.

Galton's psychological researches were entirely ancillary to his deep concern with the problems of human evolution, and thus he furnishes an excellent example of the far-reaching stimulation of Darwin and his theory. *Hereditary Genius* (1869), although published when Galton was forty-seven years old, stands almost at the beginning of its author's period of great productivity, and it appears that it was mental inheritance that concerned Galton from the beginning. During the next fourteen years his interest in the measurement of human faculties grew, and finally culminated in the *Inquiries into Human Faculty and its Development,* published in 1883. This famous book has sometimes been regarded as the beginning of scientific individual psychology and of the mental tests. Galton's own intention regarding the book was, however, different. The conflict between evolutionary doctrine and theological dogma was in these days acute, and the scientific men of England who upheld Darwin's view were generally regarded as religious agnostics. Galton, with that degree of scientific objectivity that characterized all his thought, weighed the question quite without emotion and concluded that he could find no evidence that the intensity of a belief measures its validity. A case in point is his discussion in the *Inquiries* of the objective efficacy of prayer and his conclusion that there is no evidence that physicians can take prayer into account as a therapeutic agent, or that meteorologists should consider prayer in predicting the weather, or that clergymen prosper more than others in business affairs. Galton convinced himself that there is very little difference between the lives of Roman Catholics, Protestants, Jews, and agnostics, either in their relations to humanity or in their own mental calm, and he was seeking in the *Inquiries* for a new scientific Creed to give the world. He would have had the world substitute for current religious dogma a belief in evolutionary progress as the end toward which men should strive, and he held up as the goal of human effort, not heaven, but the superman.

Thus the *Inquiries* became an attempt to measure man as he is to-day with the emphasis, not upon his attainment as the lord of creation, but upon his limitations as the defective ancestor of better generations. One finds Galton assuming an almost religious attitude and substituting human defect for sin. However, Galton's

visions of the future did not take him from the laborious and careful examination of the present. In part, the *Inquiries,* which includes the first formulation of the program of eugenics, presents this vision; but mostly it is a description of man with primary emphasis upon his mental faculties. Sometimes Galton wrote general psychology, as if to show the limitations of humanity taken altogether; but usually his emphasis was upon individual differences, because these show the variation that already is in existence and so provide the immediate possibility of the intelligent selection of the more fit.

However, intelligent selection requires in the first place a survey of available human assets. Here Galton's statistics and psychology went hand in hand. To measure the capacities of a large number of persons, and thus to sample the population, requires as a practical matter the development of apparatus and methods by which the measurements of a single individual can be easily and quickly made; such errors as are introduced by the casual procedure are expected to cancel out in the mass results. For this purpose Galton invented the 'test,' and in particular the 'mental test,' an experimental method of measurement which is characterized by its brevity and which contrasts with the elaborate psychophysical procedure of German psychology. Since the mental test is the tool of individual psychology and not of general psychology, it aims to exhibit human differences and not to gain an exhaustive analysis of some mental phenomenon with a few subjects considered as typical of all persons. This fact also leads to the result that the test deals ultimately with performance and not with the detailed physiological or conscious conditions that lie back of the performance. In America behaviorism was able readily to assimilate the mental test because both are primarily concerned with performance without regard to its conscious causes. Galton anticipated this view; he wrote: "We do not want to analyze how much of our power of discriminating between two objects is due to this, that or the other of the many elementary perceptions called into action. It is the total result that chiefly interests us."

In spite of this very practical trend in Galton's psychology, he was also really an introspectionist. He argued, against the philosophers, that the report of a man as to what goes on in his own mind is as valid as the report of a geographer about a new country. He was himself an excellent observer of conscious as

well as of objective events. By observing his own mind as he walked along the streets of London, he came to his first conclusions about the variety of its associative processes, and also as to the great extent of unconscious processes that occur in "the antechamber of consciousness." On the basis of such careful introspection, he also formed his own conclusion against the freedom of the will, noting how, in choice, ideas fluctuate until one dominates without any conscious act of will. This conclusion he reached independently of the introspective work of the German laboratories, but of course not without the influence of his predisposition toward deterministic science and away from current theology. He attacked the problem of the religious consciousness introspectively, for he put up a comic picture of Punch and made believe in its possession of divine attributes, addressing it "with much quasi-reverence as possessing a mighty power to reward or punish the behaviour of men toward it"; and he was finally rewarded by the acquirement of a superstitious feeling toward the picture and the possession in "a large share of the feelings that a barbarian entertains towards his idol." This result must have been a great triumph for a nature so little subject to superstition. Galton also tried a personal excursion into insanity. He undertook to invest everything he met, "whether human, animal, or inanimate, with the imaginary attributes of a spy"; and he succeeded in establishing in himself a paranoid state "in which every horse seemed to be watching him, either with pricked ears or disguising its espionage."

Galton's greatest contribution to introspective psychology was, however, his study of imagery, and of individual differences in imagery. With Fechner in Germany and Charcot in France he is one of the three originators of the conception of ideational types. His questionary for determining types and for measuring the vividness of the imagery for the different senses is known to every psychologist. He was astonished at the differences that he found among individuals. He discovered synesthesia, and his examples of 'color associations' are well known. He discovered the existence of number-forms and collected a great mass of representative data about them.

Galton's other contributions to psychology consist for the most part in the invention of apparatus for mental tests. He constructed a whistle for determining the highest audible pitch and tested, not only people, but also animals. He had one whistle set at the

end of a hollow walking-stick with a rubber bulb for its operation in the handle at the other end, so that he could experiment with animals at the Zoölogical Gardens and on the street. For very high tones—and some animals, he thought, have a limit of hearing above the human limen—he used coal-gas or hydrogen with the whistle. The 'Galton whistle,' in much improved form and calibrated by modern technique, is now a standard piece in all psychological laboratories.

He invented a bar with a variable distance upon it for testing the ability of persons to estimate visual extension, and also a disk to test capacity for visual judgment of the perpendicular. The 'Galton bar,' although generally used with the elaborate psychophysical procedure which Galton avoided, is now standard, and some laboratories use the angle disk also.

For the muscular sense he arranged sets of three weights each, which the subject was, in every case, to arrange in order of heaviness. Originally he made the weights of cartridge cases, but later they were put on the market, beautifully finished, in brass. This particular test is no longer used, but it has its descendant in the discrimination test for nine-year-old intelligence in the Binet scale. Its inclusion in the intelligence test would seem to be one of the few instances of a practical acceptance of Galton's contention that tests of sensory discrimination are indicative of judgment and intelligence.

Galton's other apparatus for tests has had less subsequent history. He had a pendulum device of his own design for measuring reaction times. He devised an ingenious apparatus for measuring the speed of a blow struck with the arm. He made an instrument for measuring discrimination of difference in the depth of color, cards for the determination of visual acuity, and a set of wools for the discrimination of colors. He planned an apparatus for testing color-blindness. He was also intimately concerned with the problem of establishing a standard scale of colors, and at one time planned to appeal to the Vatican for samples of its 25,000 differently colored pieces used for mosaics. He employed sets of bottles containing different substances for olfactory discrimination, an obvious procedure that is still the usual technique; and he adopted the esthesiometer for testing tactual spatial discrimination, i.e., the compass test originally used by E. H. Weber. After the publication of the *Inquiries,* we find him taking up Jacobs' work on

memory span (published in *Mind* in 1887) as a measure of "prehension," and working out ways of measuring fatigue in schoolchildren.

Outside of psychology proper lies all of Galton's work in composite portraiture. He developed photographic methods for superposing a large number of portraits in a single picture of the 'type,' using many checks to show that every element had equal value. Thus he secured many pictures of 'generalized' criminals, families, races, thoroughbred horses, and so forth. The technique was excellent, but differences of physiognomy do not clearly appear and there has been nothing to do with the results. He also at one time concerned himself intensely with the identification of criminals and other human beings, and especially with the problems of fingerprints. This matter is, however, primarily anthropometric and not psychometric.

One sees most clearly what Galton intended in his psychological work in an account of the demonstrational Anthropometric Laboratory, which he opened in 1884 at the International Health Exhibition, and later transferred to the South Kensington Museum in London, where it was maintained for six years. Instruments were provided for making a number of anthropometric and psychometric measurements. They were arranged on a long table at one side of a narrow room. Persons were admitted for threepence at one end and passed along the table with the superintendent, who filled up a schedule card as the measurements were successively made. An account of the laboratory lists the data as "height, weight, span, breathing power, strength of pull and squeeze, quickness of blow, hearing, seeing, color sense, and other personal data." Data for 9,337 persons were recorded during the life of the laboratory. The relation of this work to Galton's eugenic program is obvious. Galton wished to get the statistics of the range of human capacity in a large number of attributes and faculties. No important generalizations as regards human individual differences appeared, however, unless we should note Galton's erroneous conclusion that women tend in all their capacities to be inferior to men. The Anthropometric Laboratory was, nevertheless, a grand and dramatic experiment. It represents the ideal of the psychologist in which he is forever defeated by the variability of individual human beings and by the impossibility of establishing constant conditions with such material.

We must conclude here our account of Galton's psychological work. When Pearson complains that Galton, the pioneer in experimental psychology in England, has been disregarded in later times in favor of the German tradition, the author cannot agree with him. There is no doubt that Galton was relatively a great man, that his ingeniousness and versatility were of the order of genius. Some of his apparatus and some of his methods are still with us. His work on imagery is an extremely important contribution to positive knowledge. He founded individual psychology and thus the mental test. However, a great deal of his work came to nothing. What it is that makes the intuitions of one scientist right and therefore productive, and of another wrong, we cannot say. It may be chance, for ingenious ideas all look alike until one has been found to lead somewhere and another nowhere. Perhaps it was Galton's very versatility that prevented him from becoming the father of an English school of experimental psychology; perhaps, if he had dissipated his efforts less, the facts might have led him within a narrower field to some more positive and permanent results. In all he was a stimulating originator where he touched psychology, and it is an open question as to how much he influenced Cattell and the American tradition of individual psychology and the mental tests. Here, as in so many things both fertile and futile, he was first.

Experimental Psychology

Experimental psychology, never at any time encouraged by the British universities, was late in making its beginning in England. Galton's work led to no school of the psychological laboratory. The last fifteen years of the nineteenth century gave rise to little human psychology in Great Britain other than the work of Ward, Sully, and Stout—the empirical but unexperimental systematizing of psychology, the interpretation of the German movement, and the writing of texts. In 1899 Cambridge University sent an anthropological expedition to the Torres Straits, and McDougall, *W. H. R. Rivers* (1864-1922), and *C. S. Myers* (1873-) were included among the investigators, in order that they might make a psychological study of a primitive people, largely by way of mental tests. Here, it is true, the faith of Galton was coming alive again, the faith that anthropology must con-

sider mental as well as physical characteristics and that mental characteristics can be measured quickly by skilful tests. The numerous results that were obtained were probably less important than the fact that they were obtained, for the expedition revived the belief that psychometry is a part of anthropometry and helped to establish, although somewhat insecurely, experimental psychology in England.

Rivers, the oldest of these three men, had published before the expedition several papers on visual perception and on mental fatigue. The publication of the other two men begins practically with the results of the expedition. Rivers and Myers continued psychological-anthropological work for almost a decade, and most of their writings in this period deal with the primitive mind or primitive culture. McDougall, medically trained, returned to physiological psychology.

It is also true that the way for the introduction of experimental psychology into England was made easier by the psychological significance of the researches of the physiologist, *Charles S. Sherrington* (1857-). Sherrington's work of the '90's on color vision, and in particular on flicker, is still cited. He contributed the chapters on tactual and muscular sensibility to Schaefer's *Text-Book of Physiology* (1900). The publication of his *Integrative Action of the Nervous System* in 1906 not only placed these researches in perspective, but had considerable effect in directing psychology in both Great Britain and America toward physiology. Sherrington came in 1905 to Oxford, where McDougall was Wilde reader in mental philosophy.

One sign of the introduction of the 'new' psychology in Great Britain was the organization in 1902 of the British Psychological Society. Another was the founding two years later of the *British Journal of Psychology*. Of course, as compared with Germany and America, the British psychology was progressing very slowly, and the new *British Journal* was able to issue only a small volume every two years for the first eight years of its existence. Ward and Rivers edited the first three volumes (1904-1910), then Rivers and Myers were editors (1911-1913), then Myers alone (1913-1924). There were nineteen papers published in the first volume, and seven of them may be described as 'experimental.' Of these seven, five were written by McDougall. It is true that experimental psychology was appearing in *Mind* and the physiological journals;

nevertheless it is quite obvious that the new science had not yet passed into the hands of the many.

The record of the development of psychological laboratories in Great Britain is not very clear, largely because there are no printed accounts. Of course Galton's Anthropometric Laboratory was the very first. Among the universities Cambridge and University College, London, were the pioneers.

Ward worked in Ludwig's laboratory at Leipzig, and, when he returned to Cambridge, he and J. Venn joined in urging that Cambridge inaugurate a laboratory (1877). The proposal was dropped because of the opposition of a mathematician who scented materialism in the project, but Ward worked in Michael Foster's physiological laboratory. In 1891 Ward obtained a small grant for apparatus to use in the investigation of the special senses. In 1897 Michael Foster got established a lectureship on the physiology of the special senses, and a room in his own laboratory was set apart for research on this topic. Rivers, at Cambridge since 1893, held this post, but, after the expedition to Torres Straits, he was diverted to ethnology and Myers succeeded him. In 1901 Cambridge University sanctioned the payment of a small sum of money for the accommodation and teaching of experimental psychology, thus recognizing the new 'science,' and housing it in a dilapidated cottage. Two years later the laboratory was moved to another cottage, and an annual grant for apparatus was begun. Myers now found the governing board for moral science well disposed toward the project of a laboratory, and collected enough money to establish a larger and better laboratory, which the university undertook to support. This laboratory was formally opened in 1913, with Myers as director, a post in which he was succeeded by F. C. Bartlett, when Myers turned his attention in 1922 to the development of the National Institute of Industrial Psychology.

The laboratory at University College, London, may be said to have been 'founded' in the same year as the Cambridge laboratory, 1897. This was the year when Münsterberg finally left Freiburg to come permanently to Harvard, and friends of University College purchased a great deal of his Freiburg apparatus for the college. Galton was one of this group of purchasers, but the project was Sully's. Rivers (1897-1898) and E. T. Dixon (1898-1899) were first in charge of this new laboratory. When McDougall went to University College as reader in 1900 he found

the apparatus in a room in a top story, a small unsuitable place, which he nevertheless continued to use as a laboratory (supplemented by a private laboratory at his home) until Spearman inherited it in 1907.

Meanwhile McDougall had in 1904 become Wilde reader in mental philosophy at Oxford, a meager appointment that excluded in its foundation experimental psychology and psychic research. However, it was not necessary for McDougall to be Wilde reader all the time, and in 1907 he secured the use of three very good rooms in the physiological laboratory, space which he held until the World War. Here William Brown, Cyril Burt, J. C. Flügel, May Smith, and others worked under McDougall, and a number of publications resulted, although McDougall himself was much preoccupied with ethnological writing.

There may have been other early laboratories. There is no printed record. Bedford College, London, is said to have had a laboratory quite early. In general, some sort of provision for experimental work tended to go with a formal appointment in 'psychology.' Lloyd Morgan was appointed professor of psychology and education at University College, Bristol, in 1901. This appointment is probably the first use of the word *psychology* in a professorial appointment in Great Britain. Myers became professor of psychology at King's College, London, in 1905. Spearman was Grote professor of mind and logic at University College, London, and this title meant *psychology* in the modern sense when Spearman held it. In 1928, however, the chair returned to philosophical psychology, and Spearman was made professor of psychology. About five years earlier, T. H. Pear was made professor of psychology at Manchester.

In general it has been only in this gradual way that experimental psychology has gained a foothold in Great Britain, a somewhat precarious foothold.

During the first decade of the century, McDougall published a score of experimental papers. Nearly half of them dealt with problems of visual sensation, a field in which he is extensively cited by British authors. Some were on physiological topics, and in 1905 he printed his little *Physiological Psychology*. His well-known theory of inhibition by drainage was published in 1903. His work on retinal rivalry was of 1903 and his spot-pattern test of apprehension of 1905. He published a series of papers on at-

tention, and touched the problem of the localization of sound. In 1908 his *Social Psychology* appeared, an incomplete prophecy of the turn which his interests were to take.

Meanwhile Myers was publishing papers on the psychology of primitive peoples and preparing to do what he could to establish experimental psychology in England, much as Titchener had undertaken to do in America a decade earlier. We have just mentioned the Cambridge laboratory. In 1908, with H. A. Wilson, Myers published two important experimental papers on the localization of sound, papers that support the view that localization is dependent upon difference of phase of a tone at the two ears and not upon difference of intensity. This work is fundamental to one side of a long controversial series of researches that have only just recently led to an intelligible synthesis. In 1909 Myers published an *Experimental Psychology* and in 1911 a second edition, a book that includes a laboratory manual and that thus helped to classify psychology with the laboratory sciences. The World War, however, took him into military psychology, and afterward he became engrossed in industrial psychology. He was instrumental in founding in 1921 the National Institute for Industrial Psychology "for the application of psychology and physiology to industry and commerce," and became its director. This development, finding support in industrial fields as experimental psychology did not in academic circles, has far outstripped its academic parent.

Rivers, diverted from experimental psychology to anthropology by the Cambridge expedition, nevertheless in this first decade of the century participated in one very important experimental research. The phenomena of the changes in tactual sensation that occur when a cutaneous nerve is regenerating after injury are very puzzling. Henry Head, the neurologist, undertook to study this situation by severing in his own arm two cutaneous nerves, studying returning sensibility with Rivers as the experimental psychologist, and with J. Sherren comparing the results with clinical cases. The outcome was the theory of two systems of cutaneous sensation, the protopathic and the epicritic, and the important basis of this theory is the paper by Head and Rivers. The theory still continues to be accepted or rejected with equal positiveness, but the experiment has stimulated much other research, and the new terms have been employed in a wider

capacity. Rivers was lecturer on physiological and experimental psychology in St. John's College, Cambridge, and died, when he was only fifty-eight, in 1922.

There is also *Charles E. Spearman* (1863-). He worked in Germany at Leipzig, Würzburg, and Göttingen, took his doctor's degree at Leipzig, and published half a dozen papers in Germany. He represents much more closely the German psychological tradition in England than any of these other three men, being *au courant* with experimental psychology. He came to University College, London, in 1907, to the 'Freiburg laboratory' that McDougall had had, and became later Grote professor of mind and logic at University College. He had published in Germany experimental papers on space-perception. Nevertheless, he is not quite an experimental psychologist in the narrower sense of the term. Since his first paper in 1904, his main interest has lain in intelligence and subsequently in the cognitive processes in general. His theory of the common factor of intelligence combined with specific factors is of fundamental importance, and has rescued for scientific analysis the problem of intelligence from the merely practical mental tests. He has worked always in the careful spirit of the laboratory, and his attempt at analysis represents the spirit of science as opposed to the spirit of technology. Nevertheless his data in general have not been of the laboratory, but rather of the school-room, and, in the author's opinion, the results bear definitely upon the question as to whether scientific precision can be gained by the statistical treatment of results that were obtained under less precise conditions than the laboratory can provide. The reader must choose for himself as to whether this careful consistent research of twenty years on a single problem is or is not experimental psychology, and the notes will give him the important references.

Among the somewhat younger men, who were beginning to emerge in the period of which we are speaking, especial mention should be made of Henry J. Watt (1879-1925). He was trained in Germany and we have met him already as the contributor in 1905 of one of the important studies of imageless thought in Külpe's school at Würzburg. Like Spearman, he showed always the effect of the German background. He published variously during this decade on experimental studies of association, and wrote in 1909 an important little book, *The Economy and Training of*

Memory. Later he turned to problems of hearing, and, although interned in Germany during the World War as a British subject, published *The Psychology of Sound* in 1917 and *The Foundations of Music* in 1919. He was lecturer on psychology at the University of Glasgow; and he died when he was only forty-six.

In recent years the development of British psychology has been rapid. The British Psychological Society now numbers over 700 members. It has separate sections for medical psychology, educational psychology, industrial psychology, and esthetics. The *British Journal of Psychology* not only completes a volume every year, but has a child, the *British Journal of Medical Psychology*. The growth has been greatest in these practical offshoots from the parent stem, in educational, medical, and industrial psychology. The National Institute of Industrial Psychology is a symbol of the degree to which this field has prospered. At the present time industrial psychology has been more successful in Great Britain than in 'practical America.'

While experimental and general psychology may have lacked their full share in the new prosperity, they have grown. New workers have appeared. The work of the Cambridge laboratory has become important. Other laboratories have been established, like the one at Edinburgh under James Drever. T. H. Pear has been given the title of professor of psychology at Manchester and has a laboratory there. Spearman, Grote professor of mind and logic at London, has had his title changed to professor of psychology. All this change is still too recent to be counted as history; the perspective of time is needed to smooth its unevenness. However, it would have been unfair to British psychology simply to have left it, struggling for air in 1910, without indicating the fact of its later well-being.

Notes

Ward

James Ward (1843-1925) received his philosophical education (1872-1875) primarily at Cambridge, but also at Berlin before Ebbinghaus went there, and at Göttingen in Lotze's later days, just after Stumpf and Müller had left and before Müller returned as *Dozent*. His contact with the 'new' psychology was thus no greater than what Stumpf and Müller also had with Lotze; and the study in Germany was not entirely without effect on Ward, as his article on Fechner's law in the first volume of *Mind* (1876) shows. In England he was appointed successively at Trinity College, Cambridge, fellow (1875), lecturer (1881), and professor of moral

philosophy (1897). There is a short and inadequate account of his life by W. R. Sorley, *Mind*, N.S. 34, 1925, 273-279.

There is a bibliography of Ward's writings by E. B. Titchener and W. S. Foster, *Amer. J. Psychol.*, 23, 1912, 457-460, which has been reprinted with corrections and extension to the date of Ward's death in *Monist*, 36, 1926, 170-176. One sees from these lists that psychology dominated Ward's thought up to about 1880 and became increasingly less important to him thereafter. On the other hand, he kept the subject with him throughout his life. He wrote the article "Psychology" for the ninth edition of the *Encyclopædia Britannica* (1886) and then rewrote it, a masterpiece of difficult systematic work, in the eleventh edition (1911). His one book on the subject is *Psychological Principles*, 1918, for which he says he had laid down the plan forty years before (1878). For an excellent account of Ward as a psychologist by the man best fitted to write it, see G. F. Stout, *Monist*, 36, 1926, 20-55. See also J. Laird on Ward and the Ego, *ibid.*, 90-110.

On Ward's philosophy, see the six other articles in *ibid.*, 1-169; also G. Dawes Hicks, *Mind*, N.S. 34, 1925, 280-299.

Stout

George Frederick Stout (1860-) studied in philosophy and psychology at Cambridge (1881-1883), primarily with Ward. He was appointed fellow in St. John's College, Cambridge (1884) and later lecturer in moral science (1894-1896). Then he went for two years to Aberdeen as Anderson lecturer in comparative psychology. He was Wilde reader in mental philosophy at Oxford (1898) and subsequently examiner for the University of London. These were the years when he was able to employ his leisure for the production of psychological textbooks (1896-1903). Since 1903 Stout has been professor of logic and metaphysics at St. Andrews in Scotland. The author knows of no biographical account or bibliographical list for Stout.

As the text has made clear, Stout's importance to psychology comes by way of his textbooks: *Analytic Psychology*, 2 vols., 1896; *Manual of Psychology*, 1899, 3d ed., 1913, 10th impression, 1924; *Groundwork of Psychology*, 1903. On Stout as an act psychologist, see E. B. Titchener, *Amer. J. Psychol.* 33, 1922, 69 f., 73. Stout was editor of *Mind* from 1892 to 1920.

McDougall

William McDougall (1871-) studied at Manchester (1887-1890) and Cambridge (1890-1894), at St. Thomas's Hospital in London (1894-1898), and also in Göttingen (1900). He has a medical degree from Cambridge. He worked with Müller at Göttingen. He was appointed fellow in St. John's College, Cambridge (1898-1904), reader in University College, London (1900-*ca.*1907), Wilde reader in mental philosophy at Oxford (1904-1920), and extraordinary fellow in Corpus Christi College, Oxford (1912). During the World War he was engaged in psycho-medical work. In 1920 he accepted a call to Harvard University in America as professor of psychology, the chair made vacant by Münsterberg's death in 1916. In 1927 he went to the newly endowed Duke University in North Carolina. No biographical sketch or collected bibliography is at present available.

His important books are: *Physiological Psychology*, 1905; *Introduction to Social Psychology*, 1908, 20th ed., 1926; *Body and Mind*, 1912, 6th ed., 1923; *Psychology, the Study of Behaviour*, 1912; *The Group Mind, a Sketch of the Principles of Collective Psychology with Some Attempt to Apply Them to the Interpretation of National Life and Character*, 1920, 2d ed., 1928; *Outline of Psychology*, 1923; *Outline of Abnormal Psychology*, 1926. Recently

he has published more than half a dozen books of a psychological-social nature.

For McDougall's systematic position, see the *Outline, op. cit.;* also *Psychol. Rev.*, 30, 1923, 273-288; *Psyche*, 5, 1924, 13-32; *Sci. Mo.*, 19, 1924, 305-312; *Psychologies of 1925*, 1926, 273-305.

Perhaps the text should have mentioned McDougall's name in connection with its discussion of Darwin and mental inheritance. At Harvard McDougall undertook the experimental study of the inheritance of acquired mental capacities, using white rats as material. See *Brit. J. Psychol.*, 17, 1927, 267-304. McDougall believed that his experiment told seriously against Weismannism and for the Lamarckian hypothesis. However, science at large has accepted not only Darwin but also Weismann, has made this 'neo-Darwinism' over into conservatism, and is, in this frame of mind, unlikely to be moved by any single experimenter until his results have been confirmed in other laboratories.

McDougall's experimental work is discussed briefly in the last section of the text. His theory of nervous inhibition by drainage appeared originally in *Brain*, 26, 1903, 153-191. For his work on retinal rivalry, see *Mind*, N.S. 12, 1903, 473-488. He developed a method for the measurement of attention with G. E. Müller at Göttingen; see *Brit. J. Psychol.*, 1, 1905, 435-445. For his 'spot-pattern' test, see *Physiological Psychology*, 129-234.

Sully

James Sully (1842-1923) studied principally at London. The text mentions his visit to Göttingen and to Berlin. In 1892 he succeeded Croom Robertson as Grote professor of mind and logic at University College, where he was later in turn succeeded by Spearman. For many years this chair was the nearest appointment to a professorship of psychology that Great Britain had. For biography, see Sully, *My Life and Friends*, 1918, which is unfortunately more a book of reminiscences than an account of his intellectual development with side-lights on British psychology.

Sully's important psychological texts are: *Sensation and Intuition*, 1874, 2d ed., 1880; *Illusions*, 1881; *Outlines of Psychology*, 1884, 3d ed., 1896; *Teacher's Handbook of Psychology*, 1886, 5th ed., 1910; *Human Mind*, 1892; *Studies of Childhood*, 1896, 2d ed., 1903; *Children's Ways*, 1897; *Essay on Laughter*, 1902.

Darwin

On Charles Darwin (1809-1882) see the end of chap. 1. On his life as a whole, see Francis Darwin, *Life and Letters of Charles Darwin*, 1887. The dates of his three most important books are: *Origin of Species*, 1859; *Descent of Man*, 1871; *Expression of the Emotions in Man and Animals*, 1872. An "Essay on Instinct" was published posthumously in Romanes's *Mental Evolution in Animals* (*infra*).

Romanes

For Romanes (1848-1894), see *Life and Letters of George John Romanes*, 1896, by his wife. His three books on comparative psycholoy are: *Animal Intelligence*, 1882; *Mental Evolution in Animals*, 1883; *Mental Evolution in Man*, 1888.

Romanes fills a chronological gap between Darwin and Lloyd Morgan. His first book, *Animal Intelligence*, appeared in the year of Darwin's death. Lloyd Morgan's second book, the *Comparative Psychology*, appeared in the year of Romanes's death.

Lloyd Morgan

Conwy Lloyd Morgan (1852-), after holding a lectureship in South Africa (1878-1883), became first professor of zoölogy and geology at University College, Bristol (1884), and

then principal of the college (1887-1909). His important contributions to comparative psychology, in the period we have discussed, are *Animal Life and Intelligence*, 1890-1891; *Introduction to Comparative Psychology*, 1894; *Habit and Instinct*, 1896; *Animal Behaviour*, 1900, 2d ed., 1908. Of his later books *Emergent Evolution*, 1923, is best known.

For Lloyd Morgan's canon, see chap. 3 of his *Comparative Psychology*. The law of parsimony has sometimes been referred to as William of Occam's 'razor': "Entia non sunt multiplicanda, praeter necessitatem." Cf. Karl Pearson, *Grammar of Science*, appendix, note iii. On the question of the exact origin of the phrase, cf. the discussion in *Mind*, N.S. 24, 1915, 287 f., 592. Sir William Hamilton called it the "law of parsimony"; cf. his *Discussions on Philosophy*, 2d ed., 1853, 628-631. For all its antiquity the author does not believe that the 'razor' is a useful tool of science except for its uncertain use in offsetting a bias of interpretation. Thus Lloyd Morgan was justified in employing it against the tendency to anthropomorphize animals reinforced by the desire to prove the Darwinian theory; but conditions have changed to-day in comparative psychology. Cf. D. K. Adams's criticism of the use of the law, *Psychol. Rev.*, 35, 1928, 235-252.

Animal Psychology in General

Jacques Loeb's (1859-1924) theory of the tropism was promulgated in *Der Heliotropismus der Thiere und seine Ueberstimmung mit dem Heliotropismus der Pflanzen*, 1890. Loeb published later *Einleitung in die vergleichende Gehirnphysiologie und vergleichende Psychologie*, 1899, Eng. trans., 1900.

As bearing on the question of historical priority, it is important to mention that Spalding performed experiments on chicks even before Romanes began writing and before he himself had read Darwin's *Expression of the Emotions*. The reference is D. A. Spalding, Instinct: with original observations on young animals, *Macmillan's Mag.*, 27, 1873, 282-293; reprinted in *Pop. Sci. Mo.*, 61, 1902, 126-142.

Leonard Trelawney Hobhouse (1864-1929) published *Mind in Evolution* in 1901, 2d ed., 1915. Most of his work has lain in metaphysics, epistemology, the philosophy of the state, and sociology.

The classical papers on the social insects are: J. Henri Fabre, *Souvenirs entomologiques*, 1879-1904; Sir John Lubbock, *Ants, Bees, and Wasps*, 1882; Auguste Forel, *Expériences et remarques critiques sur les sensations des insects*, 1887, Eng. trans., 1908; Albrecht Bethe, Dürfen wir den Ameisen und Bienen psychische Qualitäten zuschreiben? [*Pflüger's*] *Arch. f. d. ges. Physiol.*, 70, 1898, 15-100; 79, 1900, 39-52.

On the psychic life of protozoa, see A. Binet, *Étude de psychologie expérimentale: la vie psychique des micro-organismes*, 1888, 2d ed., 1891, Eng. trans., 1889. H. S. Jennings took up this point of view in America in 1897 *et seq.*: see *J. Physiol.*, 21, 1897, 258-322; *Amer. J. Psychol.*, 10, 1899, 503-515; *Amer. Natural.*, 23, 1899, 373-390; *Amer. J. Physiol.*, 2, 1899, 311-341, 355-393; 3, 1899, 229-260; *etc.* See also his monograph, *Contributions to the Study of the Behavior of the Lower Organisms*, 1904 and 1906. On Jennings, cf. also chap. 21.

On Thorndike's use of formal laboratory technique with animals, see E. L. Thorndike, *Animal Intelligence, Psychol. Monog.*, 2, 1898, no. 4; reprinted outside the monograph series, 1911.

For a bibliography of comparative psychology, see M. F. Washburn, *Animal Mind*, 1908, 3d ed., 1926.

On the history of comparative psychology, see C. J. Warden, *Psychol. Rev.*, 34, 1927, 57-85, 135-168; esp. 145-164, which deals with this period.

Mental Inheritance

See notes on Galton, *infra.*

Statistical Method

The theory of probabilities underlies all statistical method, and on the history of this theory see I. Todhunter, *History of the Mathematical Theory of Probability*, 1865, the standard historical work beginning with Pascal and Fermat (1654) and extending to Laplace (1812). The normal law of error, which gives the bell-shaped curve of distribution, is usually called the 'Gaussian law,' is certainly more properly accredited to Laplace (1786), and appears now, in the light of a recent discovery, to have been known to de Moivre in 1733: cf. K. Pearson, *Biometrika*, 16, 1924, 402-404. Gauss (1809) dealt with the applications of the law, as well as deriving it from a different principle, and his name became attached to it in the same way that 'promoters' are often confused with discoverers in the history of science. For the recent history of this law as applied to mental measurement, as well as for a criticism of some of its applications, see E. G. Boring, *Amer. J. Psychol.*, 31, 1920, 1-33.

On the application of the law to anthropometric and social data by Adolphe Quetelet (1796-1874), see E. Mailly, *Essai sur la vie et les travaux de L. A. J. Quetelet*, 1875; F. H. Hankins, *Adolphe Quetelet as Statistician*, 1908; C. Lurguin, *Science*, 60, 1924, 351 f.; Boring, *op. cit.*, 10-14. Quetelet's statistical researches, which began about 1825, culminated in his *Essai de physique sociale*, 1835, which contains the doctrine of *l'homme moyen*. Almost equally important for our interest is his *Lettres sur la théorie des probabilités*, 1846, Eng. trans., 1849.

Quetelet was a mathematician and astronomer of Brussels. He was the founder of modern statistics, and in his hands the word broadened its meaning from denoting data concerning the state to its present significance. He has also been looked upon as the founder of modern sociology.

Not only did Galton in *Hereditary Genius* adopt Quetelet's use of the normal law and turn it into a more effective instrument of measurement, but he became in a sense Quetelet's successor, building up in England a gospel of statistical science, which culminated in part in his advocacy of eugenics and his foundation of the Research Laboratory for National Eugenics. For Galton statistics and anthropometry were inseparable, and his anthropometry included such a large measure of the study of mental faculties that his individual psychology and statistics become inseparable also. (*Vide* Galton, *infra.*)

For Galton on regression, see first his paper, Regression towards mediocrity in hereditary stature, *J. Anthropol. Inst.*, 15, 1886, 246-263 (reprint of part of his presidential address of 1885). The conception of correlation becomes clearer in Family likeness in stature, *Proc. Roy. Soc.*, 40, 1886, 42-73 (a paper which includes the mathematical appendix by J. D. H. Dickson, 63-66), and in Family likeness in eye-colour, *ibid.*, 402-406. Finally the shift of emphasis from regression to correlation is complete in Co-relations and their measurement, *Proc. Roy. Soc.*, 45, 1888, 135-145. Here Galton called r the "index of co-relation." For a general discussion of regression, see Galton, *Natural Inheritance*, 1889, 95-110.

F. Y. Edgeworth established the term, *coefficient of correlation*, for r in a mathematical paper in *Philos. Mag.*, 5 ser., 34, 1892, 190-204.

For Karl Pearson's development of the method of correlation and its establishment in the form of 'product-moments,' see his Mathematical contributions to the theory of evolution: regression, heredity, and panmixia, *Philos. Trans.*, 187A, 1896, 253-318.

What we have here is the application to observed data of the old principles of the problem of "the laws of error of a point in space." Bravais's theorems, which Pearson applied, are to be found in A. Bravais, *Mém. de l'Acad. roy. des sci. de l'inst. de France, sci. math. et phys.*, 9, 1846, 255-332.

On the history of the theory of correlation in general, see Pearson, *op. cit.*, 261; T. L. Kelley, *Statistical Method*, 1923, 152. The author knows of no thoroughgoing account. Pearson's third volume of the *Life of Galton*, which should contain the discussion of correlation, is not yet published.

It is quite impossible to give here an account of the development of biometrical statistics under Pearson's leadership; the reader can consult the files of *Biometrika*. The publication of Pearson's *Tables for Statisticians and Biometricians*, 1914, 2d ed., 1924, shows how far this tool has been developed on the mathematical side, as do also the various books on statistical method, like Kelley, *op. cit.*, a book in the Pearsonian tradition.

G. Udney Yule's well-known text is *Introduction to the Theory of Statistics*, 1911, 6th ed., 1922. His remarks about the limitations of statistics were directed specifically to Brown and Thomson, *Brit. J. Psychol.*, 12, 1921, 105-107.

William Brown published the *Essentials of Mental Measurement*, 1911, and the book, revised by Godfrey H. Thomson, appeared under their joint authorship in 1921. The volume represents the attempt to combine psychophysical and psychological statistical methods under a single treatment. Many of Thomson's additions deal, however, with controversial matters.

The development of hierarchies of intercorrelations between measures of mental abilities is the method to which Spearman appeals in support of a common factor of general intelligence. It is possibly one of the most important developments in statistical method since the early work of Galton and Pearson, but, having only just passed the initial controversial stage, it cannot yet be evaluated in terms of wide use. Charles Spearman first developed this notion in *Amer. J. Psychol.*, 15, 1904, 72-101, 200-292. The mathematical treatment for determining hierarchy is worked out by B. Hart and Spearman, *Brit. J. Psychol.*, 5, 1912, 51-84. Godfrey Thomson then undertook to show that, while the existence of a common factor would yield a hierarchy, nevertheless the argument is not reversible, since hierarchy may also be given by the occurrence of many group factors, each common to only a limited number of measures. See Thomson, *Brit. J. Psychol.*, 8, 1916, 271-281; and cf. his summary, *Psychol. Rev.*, 27, 1920, 173-190. Then J. C. Maxwell Garnett became interested in the problem, and in the ten years following Thomson's criticism Spearman, Garnett, and others have published extensively in an effort to clarify the apparent controversy and to extend the method. The outcome has been Spearman's *Abilities of Man: Their Nature and Measurement*, 1927, and the reader can find the references to Garnett's and Spearman's later papers in the mathematical appendix, pp. i-xxiii, of this book. In the later development of this theory the notion of hierarchy has been replaced by the "tetrad difference," which is a precise mathematical expression for the relationship that was loosely expressed by *hierarchy*, and which must equal zero for perfect hierarchy, as originally defined by Spearman, to exist. As to the point of difference between Spearman and Thomson, it seems that both are right: hierarchy may be given by a common factor or by overlapping group factors, and the decision between the two possible analyses depends in part upon how the tetrad differences turn out and in part upon the degree of psychological reality that can be given to the resultant factors.

For a summary of the contributions of Spearman and Thomson to the theory of two factors and the mathematical resolution of this controversy, see S. C. Dodd, *Psychol. Rev.*, 35, 1928, 211-234, 261-279.

For bibliographies of papers on statistical method, see Yule, Brown and Thomson, and Kelley, *opp. citt.*; C. B. Davenport, *Statistical Methods*, 1899, 3d ed., 1914.

Galton

On the life and work of Francis Galton (1822-1911), there is first his *Memories of My Life*, 1908, which contains an incomplete and often inaccurate bibliography of 183 titles. (This book is referred to hereinafter as *M.*) Then there is Karl Pearson's account of his hero, *Life, Letters and Labours of Francis Galton*, I, 1914, II, 1924. The first volume includes the story of Galton's early life and the account of his ancestry. The second volume (referred to hereinafter as *P*) includes the account of his geographical, his anthropological, his psychological and, in part, his statistical work, and is important for us, especially the chapter on "Psychological Investigations" (chap. 11, 211-282). The third volume, which is intended to include chapters on correlation, the statistical theory of heredity, personal identification, and eugenics, is not yet published; and presumably for this reason an index, sorely needed for a man whose versatility makes strict classification of material under significant headings impossible, is also yet to come.

On Galton's remarkable intelligence, as estimated posthumously by those experienced in testing intelligence, see L. M. Terman, *Amer. J. Psychol.*, 28, 1917, 209-215; and cf. C. M. Cox, *Early Mental Traits of Three Hundred Geniuses* (*Genetic Studies of Genius*, II), 1926, for Galton's parity with the most intelligent men of this list: Leibnitz, Goethe, and J. S. Mill.

Galton's important books were all on the subject of inheritance, for the *Inquiries* should be included in the list. They are: *Hereditary Genius*, 1869, 2d ed., 1892; *English Men of Science, Their Nature and Nurture*, 1874; *Inquiries into Human Faculty and Its Development*, 1883 (referred to hereinafter as *I*), reprinted with the omission of two chapters, 1907; *Natural Inheritance*, 1889. Galton's important paper on the history of twins is in *J. Anthropol. Inst.*, 5, 1876, 324-329. Cf. *I*, 216-243.

For Galton's attitude on religion, see the memorandum which Pearson publishes for the first time, *P*, 425. For Galton on the "objective efficacy of prayer," see *I*, 277-294; *P*, 249 f. For Galton on the doctrine of evolution as (practically) a religious creed, see esp. *I*, 331-337.

Pearson's volumes furnish a great mass of material about Galton upon which it has been impracticable for the text to touch. It is also not practicable to burden these notes with the original references to all of Galton's scattered contributions to psychology: the reader can find most of them in Pearson. The following list of references to three secondary sources may, however, be of aid to the person who seeks immediate information about some one of the many items mentioned but briefly in the text.

Mental tests for performance, without analysis of conscious conditions: *P*, 373.

Introspection as observation: *P*, 243.

The antechamber of consciousness: *I*, 203-207; *P*, 256.

Free will: *P*, 245-247.

Introspective experiments on [paranoid] insanity and the religious attitude: *M*, 246 f.; *P*, 247 f.

Imagery, the questionary, synesthesia, and number-forms: *I*, 83-203, 378-380; *P*, 236-240, 252-256.

The Galton whistle: *I*, 38-40, 375-378; *M*, 247 f.; *P*, 215-217, 221 f.

Discrimination of visual distance (Galton bar) and of the perpendicular: *P*, 222 f.
Discrimination of lifted weights: *I*, 34-38, 370-375; *P*, 217 f.
Reaction time and chronoscope: *P*, 219 f., 226.
Speed of a blow: *P*, 220 f.
Visual acuity and color discrimination: *P*, 222 f.
Color-blindness: *P*, 227.
Color scale and standards: *P*, 223-226.
Discrimination of smells: *P*, 223.
Tactual space-discrimination (compass test): *P*, 223.
Memory span (prehension): *P*, 272.
Mental fatigue: *P*, 276-278.
Composite portraiture: *I*, 8-19, 340-363; *M*, 259-265; *P*, 283-333.
Fingerprints and personal identification: *M*, 252-258.
The Anthropometric Laboratory: *M*, 244-251; *P*, 357-362, 370.
Women inferior to men: *I*, 29 f. (but cf. 99); *P*, 221 f.
In not all cases do these sources furnish the original reference.

Experimental Psychology

For the account of the psychological examination of the primitive people at Torres Straits in 1899 by Rivers, McDougall, and Myers, see *Reports of the Cambridge Anthropological Expedition to Torres Straits; II. Physiology and Psychology*, pt. i, 1901, pt. ii, 1903. There is no reference in this volume to Galton as a pioneer, except that Rivers remarked that the examinations were more thorough than any that had been performed on other large groups, either primitive or civilized, since not more than a half-hour apiece had been devoted to the persons who passed through Galton's Anthropological Laboratory.

On the history of the Psychological Laboratory at University College, London, see G. Dawes Hicks, *J. Philos. Studies*, 3, 1928, 478.

On McDougall, *vide supra*.

Charles Samuel Myers (1873-) was a student of natural science at Cambridge University (1893-1896). He was later professor of psychology at King's College, London, and reader in experimental psychology and director of the Psychological Laboratory at Cambridge. For his work with Wilson on binaural localization of tones, see H. A. Wilson and Myers, *Proc. Roy. Soc.*, 80A, 1908, 260-266; *Brit. J. Psychol.*, 2, 1908, 363-385. His important book from our point of view is the *Text-Book of Experimental Psychology*, 1909, 2d ed., 1911, 3d ed., 1925. There is also a little *Introduction to Experimental Psychology*, 1911, 3d ed., 1914. His more important recent books are: *Present-Day Applications of Psychology with Special Reference to Industry*, 1919; *Mind and Work*, 1921: *Industrial Psychology in Great Britain*, 1926.

William Halse R. Rivers (1864-1922) received a medical training as a student, and was at the time of his death lecturer in physiological and experimental psychology at St. John's College, Cambridge. His work with Henry Head that led to the theory of protopathic and epicritic sensation is in *Brain*, 31, 1908, 323-450. The preliminary paper announcing the discovery is in *ibid.*, 28, 1905, 99-116. Cf. the reprints of these papers and others related to them in Head, *Studies in Neurology*, 1920. This work led to criticism and further research by W. Trotter and H. M. Davies, M. von Frey, and the present author; the references to these papers are given with Head's reply, *ibid.*, II, 811-824. Rivers wrote many books in the last years of his life, and some were published posthumously. We may mention *Dreams and Primitive Culture*, 1917-1918; *Instinct and the Unconscious*, 1920, which extends the use of the terms *protopathic* and *epicritic* and so robs them of their earlier exact anatomical meaning; *Conflict and Dream*, 1923.

Charles E. Spearman (1863-) has been referred to above in the notes on Statistical Method. The ref-

erences given there outline the course of his primary contribution to psychology. Everything is brought together in his two recent books: *The Nature of 'Intelligence' and the Principles of Cognition*, 1923; *The Abilities of Man: Their Nature and Measurement*, 1927. A third volume in this series has been promised.

Psychopathology, Psychic Research, and Philosophical Psychology

There were in Great Britain some important influences that lay just at the periphery of experimental psychology. The text has shown how the experimental psychologists tended, when they did not go over into anthropology, to work in medical psychology or physiological psychology. The relation of Sherrington to psychology is an example of the influence of physiologists. There is, however, also a definite *psychopathological* tradition. John Hughlings-Jackson (1835-1911) affected psychology by way of his doctrine of the evolutionary strata of the brain and the mind. Henry Maudsley (1835-1918) quite early published a very influential book, *The Physiology and Pathology of Mind*, 1867. In the third edition it was entirely rewritten and divided into two books, *The Physiology of Mind*, 1876, and *The Pathology of Mind*, 1879. Maudsley also wrote on other psychological topics; cf. his *Body and Mind*, 1870. Recently Henry Head (1861-) has had great influence, not only in his theory of cutaneous sensibility, but in his work on aphasia and on the functions of the thalamus in feeling and emotion. See his *Studies in Neurology*, 1920; *Aphasia and Kindred Disorders of Speech*, 1926.

Psychic research has also lain at the periphery of psychology in England. F. W. H. Myers (1843-1901) was very influential. Cf. his *Science and Future Life*, 1893; *Human Personality and Its Survival of Bodily Death*, 1903. Edmund Gurney (1847-1888) was another leading spirit. Cf. E. Gurney, F. W. H. Myers, and F. Podmore, *Phantasms of the Living*, 1886. Gurney also contributed to the experimental research on hypnosis. Henry Sidgwick (1838-1900), also involved in the investigation of 'supernormal' phenomena, was the president of the first International Congress of Psychology which was held in London in 1892. In fact the emphasis placed upon psychic research at this congress led to a definite reaction away from the topic in the succeeding congresses.

Finally there were always on the periphery of psychology the *psychological philosophers*. Men like Ward straddled the two fields. Some others of the philosophers, however, were definitely concerned with writing psychology, as the foundation of *Mind*, and subsequently its contents, attest. In addition to the names that receive mention in the text, we should call attention to the writings of F. H. Bradley (1846-1924) and Bernard Bosanquet (1848-1923), both of whom sought to contribute to the new psychology and were influential in determining its thought.

Chapter 20

AMERICAN PSYCHOLOGY: ITS PIONEERS

IT is well for us to trace the outlines of American psychology before attempting to fill in the detail. James began psychology in America with his recognition of the significance of the new experimental physiological psychology of Germany. He was not an experimentalist himself, but he introduced experimentalism to America, and he put upon the new psychology the seal of America by emphasizing the functional meaning of mind. Stanley Hall was technically James's pupil, although they breathed different atmospheres, and he was the pioneer of the psychological laboratory, of educational psychology, and in fact of everything new. Like the pioneer, he moved with the frontier and left others to settle in. Ladd was the Sully of America, and gave it its early texts, even before James put out his famous *Principles*. He too was a functional psychologist before there was any such school. From about 1888 to 1895 a wave of laboratory-founding swept over America, lagging only a little in phase behind a similar wave in Germany. Americans were going to Germany, mostly to Wundt at Leipzig, and coming back filled with enthusiasm for making American psychology secure in experimentalism. Titchener and Münsterberg were imported in 1892. On the face of things, America was attempting to duplicate Germany; but under the surface, quite unrecognized at first, a psychology that resembled Galton's as much as Wundt's was being formed. Cattell, the senior after James, Ladd, and Hall, had returned from Leipzig but little impressed with the importance of the generalized normal human adult mind and bent upon the investigation of individual differences in human nature. Baldwin supported him. Finally, under the influence of John Dewey and pragmatism, the systematic structure of American psychology began to show above the surface at Chicago, where philosophers and psychologists were working together. American functionalism came into being, in bold relief against the Wundtian background which Titchener was there

to provide. Cattell himself never had a system, but the influence of Columbia was with functionalism.

By 1900 the characteristics of American psychology were well defined. It inherited its physical body from German experimentalism, but it got its mind from Darwin. American psychology was to deal with mind in use. Cattell himself never had an explicit system, but his faith filled out part of the picture. Thorndike brought the animals into the formal laboratory, and an animal psychology of the laboratory began forthwith. Thorndike then went over to the study of school-children, and the mental tests increased. Hall helped here too with his pioneering in educational psychology. In 1910 American psychology embraced experimental human psychology, animal psychology, and mental tests, with Freud beginning to be discovered. Some conservatives were Wundtians, some radicals were functionalists, more psychologists were agnostics. Then Watson touched a match to the mass, there was an explosion, and behaviorism was left. Watson founded behaviorism because everything was all ready for the founding. Otherwise he could not have done it. He was philosophically inept, and behaviorism came into existence without a constitution. Ever since, the behaviorists have been trying to formulate a satisfactory epistemological constitution and thus to explain themselves. These are the outlines of the picture. Now let us turn to the details.

William James

William James (1842-1910) is an important figure in the history of experimental psychology, although he was not by temperament nor in fact an experimentalist. He was, however, the pioneer of the 'new' psychology in America, and he was its senior psychologist. Because James in America was interpreting and criticizing the new psychology of Germany, we are apt to forget how early he seized upon the new movement. In age he was only ten years Wundt's junior, and he was almost as much again older than Stumpf and G. E. Müller. With an appointment in physiology at Harvard, he was offering instruction in physiological psychology in the year that Wundt went from Zürich to Leipzig (1875) and at about this time he had space set apart for the experimental work of students, actually several years before Wundt founded the Leipzig institute (1879), which is said to be the first psychol-

ogical laboratory in the world. Nevertheless, the laboratory was not more than a personal conviction with James; it never became a personal habit. He thoroughly appreciated its significance and often made light of its actual working. This paradox becomes quite clear in James's *Principles of Psychology,* published after twelve years of laborious work interrupted by much ill health, in 1890, after Ladd had already published a textbook of physiological psychology. The *Principles* both supported and condemned the new German movement. It supported it by presenting with painstaking care a great many of its experimental findings to American readers and by interpreting them in the light of James's systematic views. It condemned it often in the interpretation of these results and in the fact that it was a presentation of a different approach to psychology. In this point of view, James was consistent with the dominating spirit of American psychology, and it is impossible to say how far he determined this spirit and how far he only reflected it. The key to his influence lies, however, in his personality, his clarity of vision, and his remarkable felicity in literary style. His books, in fact, contain his personality, and there is no occasion to attempt to distinguish between the man and his writings. James was both positive and tolerant. He was clear and gifted in happy expression; thus he was persuasive. The operation of a brilliant mind through assurance and good humor upon a great fund of positive knowledge was bound to be effective through the medium of personal contact and of publication. There can be no doubt that James is America's foremost psychologist, in spite of the fact that he was but a half-hearted experimentalist influencing a predominantly experimental trend.

William James, after some schooling abroad and a year in studying art in America, entered the Lawrence Scientific School of Harvard University when he was nineteen. Here he studied chemistry under Charles W. Eliot (later the president of Harvard) and comparative anatomy. Two years later he entered the Harvard Medical School. When he was twenty-three, he accepted the unusual opportunity to go with Louis Agassiz on a naturalist's expedition to the Amazon. James contributed little to the work of the expedition, but he made in Brazil one important discovery: he found that he was a philosopher. He always admired Agassiz, but he could not bring himself to any degree of enthusiasm for mere observational fact divorced from speculation as to causes and

meanings. After the expedition, James returned to Cambridge for a year's continuation of his medical studies, and then went for the same purpose to Germany for a year and a half. His health was poor and failed him almost completely while abroad, so that he did not accomplish his purpose in medical study except for a limited private reading. He was becoming more and more the philosopher, but planning, if possible, to teach physiology. After his return to America, he obtained his medical degree from Harvard in 1869, but he was condemned for three years to an invalidism, which he improved by extensive systematic reading. His always restless, creative mind was restricted by his health, but in no way changed in quality.

In 1872 James was appointed an instructor in physiology in Harvard College. The appointment led him presently to decide between philosophy and physiology as his life-work, or at least so it seemed to him. The life of the philosopher seemed to James the larger and also the more difficult undertaking, and he determined to content himself with physiology, intending that, within physiology, physiological psychology should be his field and that in this way he could be indirectly a philosopher; for James, like Stumpf, saw no valid demarcation between philosophy and physiological psychology. James's instruction at Harvard was very successful, and it was in connection with his course that he offered laboratory work in that early, informal, and unchristened psychological laboratory that antedated even Wundt's. His health had improved greatly under the stimulus of definite teaching and his success. In 1876 he was made assistant professor of physiology. In 1878 he actually contracted with the publishers for what was to become his *Principles of Psychology,* hoping to finish in two years the book that finally took him twelve. By 1880 James, the philosopher, was recognized as such: he was made assistant professor of philosophy; and in 1885 he was appointed professor. All through this decade he was working steadily on the *Principles,* reading philosophy, psychology, and physiology, and becoming enthusiastically interested in other affairs like psychic research, exhibiting an irrepressible vitality irregularly limited by an uncertain health. In 1889 his title was changed to professor of psychology, and in the following year the *Principles* appeared, to score an immediate triumph. In the forty years that have

elapsed since its publication, its vigor and freshness have remained undimmed, and its insight has refused to become anachronistic.

The changing of James's title from professor of philosophy to professor of psychology was out of phase with his mental development. The completion of the *Principles* marked for him the close of the domination of his philosophical life by psychology. He had begun the *Principles* as a manual of the new scientific psychology; when he had finished he wrote in letters that it proved nothing but "that there is no such thing as a science of psychology" and that psychology is still in "an ante-scientific condition." He believed in the laboratory, but he did not like it. He wrote to Münsterberg in 1890: "I naturally hate experimental work, but all my circumstances conspired (during the important years of my life) to prevent me from getting into a routine of it, so that now it is always the duty that gets postponed."

James had been attracted to the first parts of Münsterberg's *Beiträge*. He felt that there was a freshness and originality about Münsterberg's work. He disliked as strongly as his kindly tolerance allowed the pedantry of Wundt and Müller; and Müller's "peculiarly hideous" attack upon Münsterberg's researches drew him even closer to the young psychologist at Freiburg. He schemed to get Münsterberg to Harvard and finally succeeded. Münsterberg came for three years (1892-1895) on trial, as it were, and then, after two years to think over a change of country, came to stay in 1897. In this year James's title was changed back to professor of philosophy, and Münsterberg was left in command of the laboratory.

The last twenty years of James's life saw his development as a philosopher against the odds of a precarious health. He never ceased to be a psychologist, but he grew away from psychology. The *Principles* were rewritten briefly for textbook purposes—the "Briefer Course"—in 1892. The *Talks to Teachers* came out in 1899 and the *Varieties of Religious Experience* in 1901-1902. In 1907 he resigned his active duties at Harvard and at that time his more important philosophical books began to appear: *Pragmatism* (1907); *A Pluralistic Universe* (1909); *The Meaning of Truth* (1909). The conflict of the mind with the body was terminated by his death in 1910 at the age of sixty-eight, but he left behind him a tradition, an influence, and an intellectual ideal of

personality, of literary style, and of thought, much more effective than a formal school could have been.

If James was not an experimentalist, why then did he exercise such an influence upon psychology in an age when psychology was dominated by experimentalism? Why is he so often regarded as the greatest of American psychologists? There are three reasons. The first is personal: a crabbed personality, a pedantic, obscure writer, might have said the same things with but little effect. Such a man could have been ruled out of consideration as an anachronism in the experimental movement. The second reason is negative in the way that all movements and schools are negative: James opposed the conventional elementarism of the current German psychology, offering an alternative picture of the mind. The third reason is positive: this alternative picture contained implicitly the possibilities of the new American psychology which has since come into being, that is to say, functional psychology, with its cousin, the mental tests, and its child, behaviorism.

Of James's personality we need say no more. The reader who has never read James will discover it for himself when he does.

James's opposition to the elementarism of Wundt and all the others appears most clearly in his discussion of the 'stream of thought.' There was no question in James's mind but that analysis is the necessary scientific method; however, he believed that the analytic description of mind should not be taken to mean that the real mind is a mere congeries of elements. Psychology had, he thought, lost the real whole in seeing only the elementary artifacts of its method. The main thing about consciousness is that it "goes on"; it is a stream. For this same reason Wundt had argued that mind is process, but we have seen that the elementarists did not always remember this principle, and that processes in their hands had a way of getting fixed. James had valid grounds for objection.

Beyond this primary datum we can find, James said, other essential characteristics of consciousness. (1) In the first place, it is obvious that consciousness is *personal*, that every thought "is owned" by some one, and that we have here an essential fact about consciousness. Thus James was agreeing with subject-object psychologists like Ward and Stout and was laying a foundation for self-psychology; on the other hand, he was doing little

more than dealing in a new way with Avenarius's principle that some experience is dependent upon the individual.

(2) Next James pointed out that consciousness is forever *changing*. He took the extreme view. "No state once gone can recur and be identical with what was before"; nor did he mean here simply the logical point that a recurrence is not an identity because time is different. He meant that every conscious state is a function of the entire psychophysical totality and that mind is cumulative and not recurrent. Objects can recur, but not sensations or thoughts. Here James anticipated *Gestaltpsychologie*, and especially its objection to the 'constancy hypothesis': when the stimulus-object comes again it finds a different mind, and together the old object and the new mind yield a brand-new conscious state. James's objection to elementarism on this score is so clear and complete that it has been said that he began *Gestaltpsychologie* a quarter of a century before its birth. For instance, he wrote: "A permanently existing 'Idea' which makes its appearance before the foot-lights of consciousness at periodical intervals is as mythological an entity as the Jack of Spades."

(3) Moreover, James continued the argument, consciousness is sensibly *continuous*. There may be time-gaps, as in sleep; nevertheless, when Peter and Paul awake after sleep, Peter is still Peter, and Paul still Paul; they never get mixed. In waking life, the changes in consciousness are never abrupt. It is true that there are relative differences: there are relatively stable substantive states of consciousness, where the stream of thought is quiet or caught in an eddy; and there are fleeting, instable, transitive states where the flow of the stream is rapid and defies description. These states are, of course, comparable to the palpable contents and the impalpable acts of which we have had so much to say. We must note further that the complication of the conscious stream is, in James's view, bidimensional; change occurs, not only in time, but also in cross-section. The states of consciousness have overlapping "fringes," "halos of relations," "psychic overtones." Usually this extra dimension of consciousness (which is related to time as space is related to time in the physical world) is thought of as an attentional dimension, but James is here talking more about the range and degrees of awareness than about attention.

(4) Finally James laid down as an essential characteristic of

consciousness the fact that it is selective, that it "chooses." Here he was thinking less of freedom, which has to do with the grounds of choosing, than with the nature of selection and thus with attention. It is plain that only a small part of the potentially effective world of stimulus comes to consciousness, and the principle of selection is, James thought, "relevance." Hence consciousness selects so that it tends to run in logical grooves and trains of thought arrive at rational ends. Wundt would have abhorred this view of mind, but we shall see in a moment that James was considering the mind as containing 'knowledges' (or 'meanings,' as we sometimes say), and he had therefore to take account of its logical nature. Philosopher-psychologists seldom separate the laws of thought from the laws of logic.

Such was James's picture of mind. In itself it was positive. In its effect upon experimental psychology it was negative, for it was an argument against the accepted view and yet gave no suggestion as to how experimental research should be altered to meet the objection. What is one to do scientifically with this stream of mind except to analyze it, except to fix it photographically in its various states?

This question was not asked of James, and he did not have to answer it; however, there is a more positive aspect to James's psychology which has turned out to constitute an answer. James's point of view has never been given a class name. Titchener once called his psychology a "theory of knowledge"; he would have done better, in view of James's reliance upon the empirical method, to have said a "science of knowledge," except that Titchener would have found the terms of this phrase contradictory. It is quite true, however, that James in his psychology was facing the problem of conscious knowledge or awareness, and this fact puts him partly in the Austrian tradition along with the modern British systematists like Ward and Stout. Consider, for example, James's formula for the "irreducible data of psychology." They are (1) the psychologist, the subject who knows, who makes the consciousness personal; (2) the thought studied, the material of psychology, but only, as it turns out, its part-object; (3) the thought's object, which is implicit in the thought, just as Brentano believed that a content is intended by an idea; and finally (4) the psychologist's reality, which is the psychological fact, the generalized relation between and among thoughts and their objects, the essential, if arti-

ficial, scientific construct. This is a schema of knowledge and not of content, as that term has come to be used. It gives the data *of* consciousness, but not the data *in* consciousness. It provides furthermore, in the psychologist's reality, for the troublesome artifacts of method that make mind look more like a mosaic of elements than it really is. In itself, however, the schema seems to the present author no more useful than the schemata of other act psychologists.

Unlike the act psychologists, however, James also had the notion of function in his psychology—not the *Funktion* that is like the *Akt,* but the biological function that derives from Darwin and not from Brentano. Mind has a use and it can be observed in use. Thus we find in James what came later to be the central tenet of American functionalism. James thought of consciousness as if it were an organ with a function in the psychophysical economy. "The distribution of consciousness," he wrote, "shows it to be exactly such as we might expect in an organ added for the sake of steering a nervous system grown too complex to regulate itself.' "Consciousness . . . has in all probability been evolved, like all other functions, for a use—it is to the highest degree improbable *a priori* that it should have no use."

We have spoken successively of these cognitive and functional aspects of mind in James's psychology, but they really belong together. Cognition is a primary function of mind. Even sensations, James believed, are cognitive and have their objects, the sensible qualities; the function of sensation is that "of mere *acquaintance*" with its homogeneous object, the quality; and "perception's function is that of knowing something *about* the fact." It is plain that the principal use of mind to the organism is knowledge, and that knowledge about the external world (perception) is one very important kind of knowledge. James was dealing with consciousness; he was writing, if you like, introspective psychology. But he was not ignoring the nervous system, nor the organism, nor the world in which the organism lives. For this reason, in spite of his preoccupation with consciousness, we see in him an ancestor of behaviorism, which, in the present author's view, is the only scientific psychology of cognition and of meaning. In observing the relation between stimulus and response, one observes essentially a cognition, although it was some time before behaviorists recognized this fact.

It is not possible to press many finer points of definition against James. Unlike Wundt, he had the courage to be incomplete. The justification for this course he made later when he espoused pragmatism. In his psychology he was a pragmatist. He threw himself *in medias res* and went ahead, trusting to results for his final sanction. He was thus freeing himself from formal, traditional constraints, accepting the obvious about mind, and attempting to show the use to which he could put the new freedom. It is no wonder then that he anticipated in some ways *Gestaltpsychologie* and in others behaviorism, both of which are revolts against the same traditional dogmas. More immediately James prepared the way for American functionalism, although he is not conventionally classified as in that school. His fellow pragmatist, John Dewey, was so classified, and had the most to do with the birth of the school.

James, let us say again, was not an experimentalist. It is too bad, but no one has ever yet succeeded in being both a good philosopher and a good experimentalist. James first glimpsed that truth with Agassiz on the Amazon, and he acted upon it when he got Münsterberg to come over to America to command his laboratory. But James, nevertheless, formulated a psychology which could become a frame for experimental work, as the act psychologies of Austria, Germany, and England could not. In so doing, James set a stage for American psychology. The players might have acted in the same way without the setting; no one knows. Yet the setting was there and the picture complete.

There was only one specific psychological theory of James's that ever became famous and led to extended discussion and research, and that was his theory of emotion. James first formulated the theory in 1884. He argued then that there are certain innate or reflex adjustments of the nervous system to emotional stimuli, adjustments which lead automatically to bodily changes, mostly in the viscera and the skeletal muscles, that some of these changes can be felt, and that the perception of them *is* the emotion. His schema is that an object in relation to a sense-organ gives rise to the apperception of the object by the appropriate cortical center, and that this apperception is the "idea of object-simply-apprehended"; that this idea gives rise to reflex currents which pass through preordained channels and alter the condition of muscle, skin, and viscera, and that there is an apperception of

these changes which is the "idea of object-emotionally-felt." In 1885 C. Lange of Copenhagen published a theory that was quite similar, except that it did not go so far, and that it stressed the vasomotor changes. James republished his theory, taking account of Lange and making some changes and amplifications, in the *Principles* in 1890. There was much criticism. Some of it was based upon the oversimplification that resulted from James's picturesque style. James made the emotion the result, and not the cause, of the bodily changes, and it was easy to summarize his view in such phrases as "we feel sorry because we cry, afraid because we tremble"—forceful catch-phrases in view of the well-established belief in the opposite causal order. James took account of various objections in a second paper in 1894, a paper in which he modified the simple theory in the direction of its necessary complications. He argued then, in the first place, that the stimulus to the bodily changes is not a simple object but a "total situation": we may run from a bear, but we do not run if the bear is chained or if we are hunting him with a rifle and are skilled in shooting. In the second place, James distinguished emotion definitely from feeling-tone; in fact he practically defined emotion as the kind of seizure that includes these bodily changes. These modifications limit the theory, but doubtless bring it nearer the truth.

In recent years W. B. Cannon has attacked the problem experimentally in the physiological laboratory, and has described some of these bodily changes by showing that the sympathetic nervous system is stimulated in emotion and gives rise to many bodily changes that fit the organism for violent action like fighting or fleeing. He has also demonstrated that the stimulation of the sympathetic system liberates adrenin in the blood stream and that free adrenin itself tends to produce these same changes. Very recently Cannon has also espoused the view that feeling-tone, resulting from excitation of the thalamus, is a principal factor in emotion. In general, Cannon's view has been that his results show all emotions to be alike in their bodily changes and that the James-Lange theory is therefore incorrect. Some psychologists, however, have interpreted Cannon's results in the opposite way as indicating the general nature of emotion as such, although failing to provide differentiæ between emotions.

James's theory of emotion is important for its own sake because

of its novelty and its general acceptance. It is also the best example of the way in which a theory, given within the frame of his psychology, can lead to experimental research of a 'functional' kind.

G. Stanley Hall

It would be hard to find two psychologists more different in personality and in the character of their work than G. Stanley Hall and William James, and yet there is one point of resemblance. Both were pioneers in the new psychology, and both through their personalities exerted a very great influence upon American psychology in its formative period. Neither had a school, but each had a large following. There are in America today many psychologists whose chief intellectual debt is to James and many others who derive their origin from Stanley Hall. There are probably none who acclaim both as great: the disciple of Hall could hardly appreciate the narrower scope of James nor his philosophical bent; the disciple of James would find Hall superficial and discursive. Yet both men were very important.

Hall, always in excellent health, led a life of action. He was, of course, for thirty years a university president, but he was not led away from psychology to administration or he would have no place in this history. He was an 'inside' president and a teaching president with little time for raising money or for creating academic administrative structures. He was a 'founder,' if there ever was one in psychology. His life was punctuated with the foundation of laboratories, journals, and institutes. More important is the fact that he was, in a sense, forever 'founding' ideas, that is to say, he would, under the influence of a conviction, bring together certain new ideas, that were not original with himself, add to them a supporting mass of other ideas drawn from his omnivorous reading, and then drive the resultant mass home in a book, on the lecture-platform, in his seminary, and on every other occasion that presented itself. Hall himself admitted that his intellectual life might be viewed as a series of "crazes"; nevertheless, his biographers have managed to find more continuity in the discursive chain than the casual student would expect. James philosophized, but Hall speculated—for there is a difference. Both were enthusiasts, but, where James's penetrating, sympathetic whimsi-

calities won him a quiet supporter, Hall's torrential, fervid vividness won him an ardent disciple.

Granville Stanley Hall (1844-1924) was born on a farm at Ashfield, Massachusetts, and passed his youth there, attending the country school. He had an isolated boyhood, for he was already beginning to develop intense interests, one after the other, and for them he found no sympathy among his natural companions. Quite early he revolted against becoming a farmer, and finally, with his mother's support and his father's opposition, it was decided that he should go to college and prepare for the ministry, which in Ashfield appeared to be the most obvious of the learned callings. He went away to a seminary for further study, taught school for a term in Ashfield, and then entered Williams College (1863-1867). At first he shone neither scholastically nor socially, a country boy, the only one of his community to go to college; but by the time of his graduation he had won a number of scholastic honors. He also achieved an enthusiasm for philosophy, especially for John Stuart Mill, and an admiration for the theory of evolution, an attitude that is one of the unifying threads of his varied intellectual life. He took his philosophy with him to the Union Theological Seminary in New York City, and so much the philosopher and so little the theologian was he that, after his trial sermon, the member of the faculty whose custom it was to criticize, despairing of mere criticism, knelt and prayed for his soul. Henry Ward Beecher was more sympathetic; he advised Hall to go to Germany to study philosophy.

Hall went, on borrowed money (1868-1871). He studied first at Bonn and then at Berlin, where he was particularly impressed by the great Trendelenburg (the man who made an Aristotelian of Brentano) and where he took Du Bois-Reymond's course in physiology. Presently he returned to New York, took his degree in divinity, was for ten weeks a preacher in a little country church, and after that was a tutor in a private family. The future course of his life was anything but clear. However, he shortly secured a professorship at Antioch College in Ohio (1872-1876), where he taught at different times English, modern languages, and philosophy. Philosophy was still his *métier,* but the appearance in 1874 of Wundt's *Physiologische Psychologie* brought his interest about to the new psychology. Like so many others, in those days at least, Hall felt that philosophy was impractical; yet he lacked the

technique for science. To such men the new psychology opened a *via media.*

Hall wanted to go to Germany at once to study with Wundt. He was persuaded to stay a year at Antioch. Then, on his way to Germany, President Eliot side-tracked him at Harvard with the offer of a tutorship in English, which the impecunious Hall could not afford to refuse. So he stayed at Harvard for two years, tutoring in English, studying philosophy and, with William James, psychology. In 1878 he took his doctorate with a dissertation on the muscular perception of space. The experimental work was performed in the laboratory of the physiologist, H. P. Bowditch, and Hall received at the hands of James what is presumably the first doctorate of philosophy in the new psychology to be granted in America. There was almost no difference in age between these two men, but great difference in temperament. Each appreciated the other's qualities, but they were already on different tracks and drew far apart in later years. Hall was a comet, caught for the moment by James's influence, but presently shooting off into space never to return.

After Harvard, Hall went to Germany as he had originally planned (1878-1880). First he went to Berlin, where he worked with von Kries and Kronecker. Then at last he came to Leipzig, where he lived next door to Fechner, studied physiology in Ludwig's laboratory, and became Wundt's first American student in the year of the founding of the Leipzig laboratory. He was sampling everything, not so superficially but that he published jointly with von Kries and Kronecker.

After his return to America, Hall was still without a position. He settled near Cambridge and was asked by President Eliot of Harvard to give a course of Saturday morning lectures on educational problems. The lectures were a great success, and Hall came into public notice.

In 1881 he was invited to lecture at the new graduate university, Johns Hopkins (founded 1876). Again he was successful, and he was given a lectureship in psychology in 1882 and a professorship in 1884. At Hopkins he found a group of younger men who were later to become famous in psychology: John Dewey, J. McK. Cattell, H. H. Donaldson, E. C. Sanford, W. H. Burnham, Joseph Jastrow, and others. He 'founded' in 1883 what is called "the first psychological laboratory in America." Of course, as we have seen,

experimental work in psychology brings laboratories into existence wherever it occurs, and James had for several years had space for experimental instruction and thus a laboratory at Harvard. However, James's laboratory came into being, whereas Hall founded his. The difference between *having* and *founding* is a difference between the temperaments of the two men. In 1887 Hall began—'founded'—the *American Journal of Psychology,* the first psychological journal in America, and the first in English except for *Mind,* to which the few American experimental psychologists had been contributing their papers.

In 1888 Hall accepted the unexpected call to become the first president of the new Clark University at Worcester, Massachusetts. Hall more or less determined the nature of the initial undertaking at Clark, and the university was organized along the lines of Johns Hopkins and the Continental universities, a graduate institution with its primary stress upon investigation and not upon instruction. Hall spent a year abroad, studying universities and visiting almost every country in Europe. The story of his great disappointment after his return does not belong in this history. He had expected large funds from the founder, who, instead of giving more and more, gave less and less, leaving at death in 1900 an endowment which was largely diverted to the founding of an undergraduate college. The university was constituted of five scientific departments, and Hall remained professor of psychology, in fact as well as name, while he was president. The laboratory at Clark he gave over, however, to E. C. Sanford, who came to Clark from Hopkins. Quite early a department of pedagogy, which in the Clark atmosphere was almost synonymous with educational psychology, was established under W. H. Burnham, another man who had been at Hopkins with Hall. Thus psychology prospered at Clark.

The *American Journal of Psychology* came, of course, to Clark with Hall. It was his personal property. In 1891 Hall founded the *Pedagogical Seminary,* the second psychological journal in America, for it really was as much psychological as Hall was a psychologist. The American Psychological Association was planned in 1892 in a conference in Hall's study, and began its career later in the year with Hall as its first president. In 1904 he founded the *Journal of Religious Psychology,* which however lapsed after a decade of publication. He attempted to organize an institute of

child study about 1909, but sufficient funds were not available and the institute, much to Hall's sorrow, never came into independent being, except as a museum of education. Then in 1915 he founded the *Journal of Applied Psychology,* but psychological journals were no longer new; there were already fifteen others in existence in America.

Hall was elected for the second time president of the American Psychological Association in 1924. Only to James had the honor of reëlection previously been given. However, Hall died before the year was out, at the age of eighty.

For all his 'foundings,' Hall was not primarily an administrator. The journals and other organizations were but deposits of his restless mind. It seemed rather that he developed a new interest, carried it through the pioneer stage, and then, already caught by the next topic, tried to perpetuate the old by creating for it a new professorship, a journal, or an institution. Let us turn to this torrent of interests.

Hall's earliest serious intellectual concern was, as we have seen, philosophy. Within it he came upon psychology, and ultimately turned the tables by saying that psychology furnished the true approach to philosophy, that is to say, the psychoanalysis of men is the key to the significance of their opinions. Within philosophy he also assimilated the doctrine of evolution, and thus his psychology was always an evolutionary psychology or, as he called it, genetic psychology. When Hall first came into direct contact with the new psychology, it was the problem of movement that fired his imagination. His thesis, showing the influence of the associationists and of Wundt, held that sensations of movement are the basis of all space-perception. The muscle sense, he argued, is the organ of will. The reaction experiments seemed to him, as they did to most other psychologists at that time, to be a remarkable tool for the measurement of the mind. Hall took this view with him to Hopkins and its new laboratory, and there we find him working on motor sensations, bilateral asymmetry, rhythm, and dermal sensitivity, topics dull enough in themselves, but furnishing in Hall's hands material for vivid pictures of the human mind as the agent of civilization. Nevertheless Hall felt that the laboratory work of the new psychology was proving too narrow. Certainly it lacked the breadth and the broad significance that he demanded, and that he forced upon it when he dealt with it. He was also im-

patient of the psychology of consciousness that was characteristic of the new psychology, and he had quite early, long before he became interested in Freud, come to the conclusion that introspection is inadequate to account for many psychological phenomena. So he continued to draw freely upon a very wide field for his psychological data, and came to uphold what he called "synthetic psychology." This phrase was simply Hall's name for the eclectic view with the broadest possible scope. It is a negative characterization. Positively Hall was a genetic psychologist, that is to say, a psychological evolutionist who was concerned with animal and human development and all the secondary problems of adaptation and development.

At Clark, with the laboratory safely in Sanford's hands, Hall's geneticism brought him to child psychology, to pedagogy, and presently to the special study of adolescence. His most important work is the two huge volumes called *Adolescence: Its Psychology, and Its Relations to Physiology, Anthropology, Sociology, Sex, Crime, Religion, and Education* (1904). This work, coming at the time when psychology was supposed to be about to unlock the door to scientific education, had a tremendous vogue. After its publication, his interest in child study increased still further. Growth, imagination, and play were some of the topics that especially engaged his attention. He made extensive use of the questionary for collecting statistical data, so that this dubious psychological method has come to be particularly associated in America with Hall's name, although Galton invented it. All this work should have culminated appropriately in a Child Institute, but, as we have seen, the project failed for lack of funds.

From boyhood Hall had an intense interest in animals, and along with the child study went his lectures on animal life, habits, and instincts. When psychoanalysis and the work of Freud and Jung came to be known in America, Hall took up with the new doctrines. They fitted in with his belief in the inadequacy of introspection as the sole psychological method and with many of his dicta that had a behavioristic sound long before behaviorism was ever heard of. Psychoanalysis led Hall still further into the psychology of sex than had his concern with adolescence, and he often suffered from the *odium sexicum* that was thus attached to his name. However, he was dauntless when driven by one of these intellectual curiosities of his, and he was not deterred from filling

out a new portion of the synthetic psychology. Another incidental interest of these years was his concern with the psychology of alimentation and presently with the work of Pavlov. For a time this topic assumed tremendous importance as the others had done: temporarily the mind migrated to the stomach.

One of Hall's later fields of concentration was the psychology of religion, a revival of an old interest which culminated in his publication of *Jesus, the Christ, in the Light of Psychology* (1917). And this book brought to him the *odium theologicum*. In old age, still feeling young, Hall met the problem of his retirement from the presidency of Clark as he had met all other problems: by trying to understand old age psychologically. He wrote his *Senescence* (1922).

Looking back on this crowding host of enthusiasms, we can see that Hall contributed mostly to educational psychology in America, and much less to experimental psychology, which represented but an early phase of his productive life. The laboratory could never have become his professional locus; it was too far removed from the living problems of human nature that fascinated him. On the other hand, experimental psychology was never removed from his synthetic view, and, while as professor of psychology at Clark he might be impatient of it, as president he supported it and the work of Sanford and of J. W. Baird, who successively directed the laboratory. His work in founding the early laboratory at Hopkins and its subsequent work were very important in the pioneer days of psychology. His leadership in the founding of the American Psychological Association "for the advancement of psychology as a science" was an event in the formal history of psychology. So was the founding of the *American Journal of Psychology*. At one time it seemed as if the majority of American psychologists had been associated with Hall either at Hopkins or at Clark, although they did not all derive their primary inspiration from him. In 1890, just before the wave of laboratory-founding had reached its height, there were probably not more than ten psychological laboratories in America, and at least four of these, beside Hopkins itself, had begun life under the direction of a pupil or associate of Hall's at Hopkins. Hall's personal influence lies therefore mainly outside of experimental psychology; nevertheless the laboratory in America is indebted to him as a promoter and a founder.

Ladd and Scripture

We have called Ladd the Sully of America because it is his textbooks that are his contribution to psychology. His importance is a function of the time at which he wrote. In the '80's James, Hall, and Ladd were the only psychologists in America who had as yet 'arrived.' Persons like Baldwin, Cattell, Jastrow, and Sanford belong just a bit later. Hall was the first president of the American Psychological Association, Ladd the second, and James the third. Before 1890 there were few general textbooks of the new psychology to read. In German, beside Volkmann, there were three editions of Wundt, and there was Brentano. In English, Bain and Spencer were out of date, and there was Sully's *Outlines* but not yet his *Human Mind*. In America there was nothing, except Dewey, until Ladd began.

George Trumbull Ladd (1842-1921) was just as old as James and two years older than Hall. He graduated from Andover Theological Seminary (1869) and spent ten years in the ministry in the Middle West. Then he was appointed professor of mental and moral philosophy at Bowdoin College (1879-1881), and while there he began the study of "the relationships between the nervous system and mental phenomena," just as an up-to-date professor of mental philosophy ought in those days to have done. From Bowdoin Ladd went to Yale (1881-1905) without change of title. He said that at Yale he continued to study physiological psychology in the laboratory, but it is doubtful if he had any laboratory of importance at Bowdoin. At Yale, however, he did have an informal laboratory where he worked with the assistance of J. K. Thatcher, the physiologist. This statement means essentially that Yale was started in practical experimental psychology at the time that Hopkins was getting under way, but not that Ladd anticipated Hall in 'founding' a psychological laboratory. In general these dates are unimportant: the laboratories came into being because they had to come, not because Hall, shortly after Wundt, had set the custom. Ladd's principal interest lay, however, in working up his lectures on physiological psychology, and it was from them that his first psychological book issued.

This book was the *Elements of Physiological Psychology* (1887). It met with a warm welcome. Ladd said that he had had only Wundt to point the way as he wrote, and Wundt, beside

being in German, was difficult for most readers. The literature of the new psychology was becoming large. Ladd went through it carefully and conscientiously, and presented the first English handbook for the scattered subject-matter. In America and England the volume made a great impression. In fact it long remained almost alone among psychological compendia in the stress which it laid upon the physiology of the nervous system; and for this reason it was revised in 1911 by R. S. Woodworth and became again a standard text.

Ladd published an abbreviated edition of this book in 1891. In 1894 he published a little *Primer of Psychology* and a big *Psychology, Descriptive and Explanatory*. The latter reappeared in abbreviated form four years later. The large text of 1894 met with a less cordial reception than its predecessor of 1887. For one thing there were more books now, James and Baldwin in English and Külpe and Ziehen in German, beside others less important. James found Ladd's second compendium "dreary." Hall denied that it was dreary, but he found no stimulus in it, and, while admiring its accurate thoroughness, deplored, all unconscious of what the future was to bring forth in America, the multiplication of textbooks. However, viewed historically, this book has the advantage of being a systematic text that is really a functional psychology in the American sense. It is one of several events that show that functional psychology was of the American atmosphere and not simply a unique discovery at Chicago. Here was Ladd by himself at Yale 'getting up' the new psychology for lectures and books, making an excellent job of it, and writing, not as Wundt wrote, but of mind as a useful organ. We shall return to this point in a moment.

During Ladd's first decade at Yale the work in the psychological laboratory was increasing and getting out of hand. Accordingly in 1892 Scripture was appointed instructor and the laboratory given into his charge, that is to say the Yale laboratory was, after an informal existence, at last ready to be 'founded.' Ladd kept on with his books for a bit, but, with his theological background, he was never an ardent promoter of the new and mechanistic (materialistic) psychology. He reverted to philosophy. On three occasions he went to the Orient to lecture. In 1905 he retired and became *professor emeritus*. As a psychologist his influence really belongs only to the eight years during which his

psychological books were appearing, or at most to the twenty years from the time when he first was 'getting into' physiological psychology at Bowdoin to the end of the century, when he faded out of the active picture.

In calling Ladd a functional psychologist, it is necessary for us to see just what is meant by the term. (1) In the first place, it appears that Ladd, like Ward and Stout and James, accepted the necessity for a self within psychology, a subject which is active. In this way consciousness is seen to be a matter of activity, the activity of a self. Ladd, however, was trying to mediate between his convictions about the soul and the contents of Wundt, so that we find him describing and explaining consciousness in two ways, as active function and as passive content. This dual psychology is not like the bipartite psychology of Messer or Külpe, where acts lie on the one hand, and contents on the other; for Ladd every conscious fact, even a sensation, was completely described and explained only by both accounts.

(2) The notion of the active self—the fact, as James said, that consciousness is personal—led Ladd into the biological notion that the function of consciousness is to solve problems. Mind is to be explained in terms of its use. This was physiological psychology, and it is perhaps not strange that the biological point of view should come out of the physiological. Psychologically, consciousness is the activity of a self. Physiologically, there are the nervous system and the organism to represent the self. If consciousness is thus tied up to a person, it would be the most natural thing in the world—after Darwin—to explain it on evolutionary grounds, that is to say, in terms of its use to that person. The function of mind is adaptation.

(3) However, if mind has adaptive value for the organism, then it has a purpose; and psychology becomes teleological. Teleology is the third mark of functional psychology, and was a principle that the theologically minded Ladd was glad to admit.

(4) Finally, we find that the result of the biological point of view is to make psychology practical. You have a person (the self, the organism) with a mind (content) acting (function) to adapt him to his environment (biology) in the ways for which his mind is fitted (teleology). For something to be practical is for it to have a use in the business of life, and the science of living is the one great applied science. In general, functional

psychology in America has thus led with the utmost facility directly into applied psychologies. Ladd, however, was content to let psychology be propædeutic to philosophy, which demanded then (as well as now) a psychology of human nature.

These four points are really not independent. Given the evolutionary atmosphere, one follows from the other. They are all in Ladd at the level of theoretical systematization. The history of American psychology is little more than their working-out in reality.

We may now return to Scripture, who took from Ladd's shoulders the burden of the Yale Laboratory.

Edward Wheeler Scripture (1864-) we have mentioned in an earlier chapter as one of Wundt's students. He spent three semesters with Wundt at Leipzig, a semester at Berlin where he heard Ebbinghaus, Zeller, and Paulsen, and a semester at Zürich, where Avenarius taught him pedagogy. He took his degree with Wundt in 1891 with a thesis on the association of ideas. After that he was a fellow at Clark for about a year (1891-1892). Then Ladd brought him to Yale as instructor in experimental psychology (1892-1901). He was in charge of the laboratory from the start, and its official director later (1898-1903). He was assistant professor during these last two years (1901-1903).

Scripture spent an energetic and effective decade at Yale. He was a great contrast to the theological and philosophical Ladd, coming into the laboratory with a strong conviction as to the scientific nature of psychology and its mission to work quantitatively upon the mind, ever approaching more and more nearly to the precision of measurement that obtains in physics. Most of the younger men of this period were devotees of the laboratory, but in none does the spirit of the times show more clearly than in Scripture's writings. He wrote in this period two popular books, *Thinking, Feeling, Doing* (1895) and *The New Psychology* (1897). They are both packed full of pictures of apparatus, graphs, and other apt illustrations. The style is terse; the content is factual. Figures are given wherever possible. There is no argument, no theory, no involved discussion. The books still carry the fervor of the '90's: a *new* psychology, soon to be as accurate as physics!

Under Scripture's leadership the Yale laboratory grew, at least in apparatus and technique. Scripture began at once the *Studies*

from the Yale Psychological Laboratory, of which ten annual volumes were issued during his decade there. Their emphasis was at first upon reaction times and the sensory problems of tone. An examination of the studies shows, however, that Scripture was himself the chief contributor and that the Yale laboratory drew no large group of psychologists who were later to become distinguished, as Hall did in the corresponding period. Of the forty-five studies published during the ten years, eighteen were by Scripture alone, five were contributed by him with some other person, and the other half were by investigators in the laboratory. C. E. Seashore, who has since devoted his life to the psychology of tone, is easily the most distinguished of the group. He took his degree at Yale in 1895. The only other names that are generally known now are J. E. W. Wallin and M. Matsumoto.

After this period Scripture dropped out of American psychology. His interest in acoustics had already been passing to phonetics, and during the last quarter-century he has devoted himself primarily to the problems of speech and its defects. He took his medical degree at Munich in 1906 and is now professor of experimental phonetics at Vienna. C. H. Judd followed him at Yale.

James Mark Baldwin

Generations among psychologists are measured by decades. James, Hall, and Ladd were in the '80's the first generation of the 'new' psychologists in America. In the '90's other leaders appeared: Sanford, Cattell, Baldwin, Jastrow, Münsterberg, Scripture, and Titchener—to name them in order of age. Titchener was eight years younger than Sanford; and in 1892, when they had all in a sense 'arrived,' Sanford was thirty-three and Titchener twenty-five. Beside these men there were, of course, others, who were then and later philosophers, or who were drawn off from psychology to administration or to other activities. The seven men in the list all left their impress upon psychology, and we see that the new science was at first in the hands of very young leaders. Perhaps this is the reason why every one of them tried to lead in a different direction. Youth is dynamic, ambitious, egotistical, and even quarrelsome. Sanford, the oldest, was the only pacifist in this group and left perhaps the faintest

impression. Most of the others mellowed with time in greater or lesser degree, but the '90's were a furious decade in American psychology. Perhaps youthful ambition was enhanced by the polemical spirit of Germany, from which America obtained, with its doctorates, its psychological tradition.

Of the psychologists now left to America, Cattell is the acknowledged senior, but we may turn first to Baldwin as making the readiest transition from Ladd.

James Mark Baldwin (1861-) was primarily a psychological theorist and writer, who, like Hall and almost all of the native-born American psychologists, was impressed by the bearing of the theory of evolution upon psychology. He was also half a philosopher, or, to be more exact, somewhat of a philosopher-psychologist in the '90's, and mostly a philosopher in his later professional life. He was an experimentalist, quite as much as the philosopher-turned-psychologist is ever an experimentalist, and he founded the Toronto laboratory, and the Princeton laboratory, and then reëstablished Hall's lapsed laboratory at Hopkins. He was a man of the world, and he had both the ability and the will to meet and mingle with important persons—and finally to write his reminiscences of them. However, while he was one of the 'new' psychologists, his skill was the philosopher's ability in speculative theorizing. In 1895 he wrote of "that most vicious and Philistine attempt, in some quarters, to put psychology in the strait-jacket of barren observation, to draw the life-blood of all science—speculative advance into the secrets of things,—this ultra-positivistic cry has come here as everywhere else and put a ban upon theory. On the contrary, give us theories, theories, always theories! Let every man who has a theory pronounce his theory!" This cry was not the slogan of the laboratory men, and one can guess how Scripture and Titchener, for example, received it. Moreover, Baldwin was a writer as well as a theorist. It is not merely that his theories found their way into print; it is rather that he wrote them out of himself. His reader is present at the scientific act of creation, and thus theory and its verbal expression were closely associated in Baldwin's mind.

Baldwin was born in South Carolina, at the outset of the Civil War, of a prominent family of Northern descent and Northern sympathies. After an undergraduate course at Princeton, he spent a year abroad at Berlin and Leipzig (1884-1885) studying

philosophy, but gaining from Wundt what he particularly wanted, an introduction to the 'new' psychology. After two years at Princeton as instructor in modern languages and as a student in the theological seminary, he was appointed professor of philosophy at Lake Forest University in Illinois. His doctorate was in philosophy; in his thesis, at the insistent direction of President James McCosh (1811-1895) of Princeton (one of the last of the old school of philosopher-psychologists who therefore finds no other mention in this book), Baldwin refuted materialism, although his inclinations took him to no such topic. In 1889 he went to Toronto to the chair of metaphysics and logic, but with a growing interest in psychology which led him to inaugurate a small laboratory there. In 1893 he was called back to his *alma mater* as professor of psychology, and he spent at Princeton the ten most effective years of his life, organizing again a new American laboratory, writing books, and making several visits to Europe. In 1903 he went to Hopkins to revive the laboratory that had lapsed when Hall left for Clark; nevertheless he was in these days becoming more of the philosopher than the psychologist. He spent the winter of 1906 at Oxford, where he was one of the two first to receive the new honorary degree of doctor of science. Twice he visited Mexico to advise on the organization of the National University. Then in 1908, after five years at Hopkins, he resigned to go to Mexico for more active work in this advisory capacity.

In the following years Baldwin seemed first to have divided his time between Mexico and Paris, and then to have settled down abroad, mostly in France. There was in this period only one excursion into psychology, a very clear and readable little *History of Psychology*, 1913. His more philosophical writings began to give place to the consideration of the problem of the relation of France to America, and these interests were suddenly intensified by the World War and, at first, by American neutrality. He supported the cause of the Allies vigorously and won recognition from the French government.

His first work was his *Handbook of Psychology*, divided into two volumes, *Senses and Intellect* (1889) and *Feeling and Will* (1891). The former appeared just as he was going to Toronto, and there was a second edition of it in the next year. After the custom of Ladd and James, he also got up a briefer course,

which came out as *Elements of Psychology* just as he was about to leave Toronto. It was these books that first made Baldwin's reputation. They were meant to be just what they claimed, handbooks; but they contained too much theory and too little experimental fact to make a lasting impression in the days of the 'new' psychology. Baldwin's felicitous literary style, surpassed only by James, gave a transient vitality to his ideas; but his effect was not permanent.

At Princeton he published his two books on mental development: *Mental Development in the Child and the Race* (1895) and *Social and Ethical Interpretations in Mental Development* (1897). These books present clearly and forcibly the evolutionary principle in psychology, and even seek to modify Darwin's theory by the conception that Baldwin called "organic selection." The evolutionary view permeated American psychology and dominated Hall. Hall, however, had not given it clear or elegant expression; his *Adolescence* was still more than half a dozen years away. James adopted the point of view only in so far as he dealt with the organism in relation to its environment. Baldwin made it his theme. The group at Clark looked askance at these books because they contained so much personal speculation and so little observational fact, but then of course these were the days when most psychologists looked askance at most of the others.

In 1898 Baldwin published a little book that went into many editions: *The Story of the Mind*—"my only novel," he called it later.

He was also engaged in two very important coöperative undertakings. The first was the founding of the *Psychological Review* in 1894, with its supporting members, the *Psychological Index* and the *Psychological Monographs*. The *Psychological Bulletin*, a shoot from the same stem, came a decade later. Cattell and Baldwin began this venture together, two very positive personalities, both unaccustomed to brook interference. They got along for a time by each assuming primary responsibility in alternate years, but in 1903 they agreed to part company, and Baldwin bought the journals. Later, when he went to Mexico, they became the property of H. C. Warren, and they have recently been purchased by the American Psychological Association.

The other coöperative venture was Baldwin's *Dictionary of Philosophy and Psychology*. It was a huge undertaking, involv-

ing the work of more than sixty philosophers and psychologists in Europe and America. Two large volumes, comprising more than 1,500 pages, were issued in 1901-1902, and Benjamin Rand's bibliography of almost 1,200 pages followed in 1905.

There remains only the question of Baldwin's actual experimental work. There was little of importance. Of course there were experiments which he and his assistants conducted and published, but Baldwin's genius did not lie in experimentation. He was a philosopher at heart, a theorist. In discussing Titchener in an earlier chapter, we have already examined his controversy with Baldwin about the reaction times. Some of the data on Baldwin's side of this argument were of his own acquisition, but they were meager. In this argument a principle, not a fact, was primarily at stake: Should psychology study human nature and therefore individual differences (Baldwin) or should it study the generalized mind (Titchener)? The influence of Darwin upon psychology has always been to favor the study of individual differences, as we have seen with Galton, and as was equally true with Hall and Baldwin.

James McKeen Cattell

In some ways Cattell is a contradiction: his influence upon American psychology has been so much greater than his individual scientific output. It follows that his influence has been exerted in personal ways. He was for twenty-six years in charge of psychology at Columbia, America's largest university, where undoubtedly more students of psychology have come into contact with him than has been the case with any other American psychologist. Moreover, he had the executive temperament, a man of many affairs in the psychological and scientific world, so that a great number of organized projects within American psychology have felt his touch. He has been editor of at least six important journals in psychology or in general science. It would be easy to say that this man of pronounced ideas and fearless aggression did more than any one other to make American psychology what it is; however, Cattell might not have seemed so effective had he been a voice in the wilderness, like Titchener, instead of an able representative of the American trend. Leaders may make the times, but the times also make

the leaders. Let us see how Cattell was the man for his generation in America.

Of course psychology was new. It needed young, aggressive leaders, and Cattell was just such a man. So too were Hall, Baldwin, Münsterberg, Scripture, and Titchener.

Evolution had taken hold of American thought, and evolution, as we have seen, meant in psychology an emphasis upon individual differences. Cattell was convinced of the importance of the psychology of individual differences before he went to Wundt, and he stuck to his belief. He did not get this idea from Galton, but presumably got it out of the same atmosphere that gave it to Galton.

We have seen that the psychology of individual differences, because it deals with the particular and not with the general, tends to be practical. The mental test has become its method. Certainly Cattell has always been intensely practical without any philosophical system in psychology to keep him in the narrow path of 'pure' science. It is conventional to call America a practical nation or even a 'dollar-grubbing' nation. The author does not think, however, that it was the immediate concern with practice or with wealth that led to the interest in individual differences and the mental tests, at least not at the crude, direct level which this charge implies. American science tended to be naïve and unphilosophical in the way that youth exhibits these attributes. In the land of the free, one was not compelled to work out the systematic presuppositions of a point of view and to bind himself by them. There was a freedom from the constraint of tradition, much greater in the American universities than in the British. Within this freedom, it was the idea of evolution that set the course for psychology. Evolution affected James, and was the dominating influence in the thought of Hall and Baldwin.

One phase of this American atmosphere was its carelessness of philosophy. In this respect American psychology has always been two-faced. Its psychology was imported from Germany as part of philosophy. James, Ladd, and Baldwin were essentially philosophers. The German and British psychologists were all philosophers. There were therefore the philosophical psychologists in America as elsewhere, and they still exist. Nevertheless there is something incompatible between experimentation and philosophizing; they belong together in principle, but do not blend in

the personality. America, with few traditions, was able to establish professorships of psychology where Germany and England, bound by the past, had only chairs of philosophy. Certainly Cattell was not a philosopher, and thus he was prepared to assume leadership in the direction in which movement was freest.

Another way of expressing this same thought is to say that psychology, when it finds itself free of conscious philosophizing, becomes catholic in its concerns. In this sense Cattell was catholic, and he avoided, not only philosophy, but also psychological systematization, which is apt to be nothing else than an inexpert philosophizing—the philosophical wolf dressed in the skin of the experimental sheep. Not being a systematist, Cattell did not repudiate introspection formally, but he dismissed most of the careful distinctions to which it led, and did his share in paving the way for the latest American revolt, behaviorism.

The thesis is, then, that Cattell represents the natural tendency of American psychology when all the variations about the mean are removed, and that he was thus prepared to lead. His vigorous personality and his contact with many persons and many affairs gave him his opportunity. The Columbia laboratory was the most important single factor in distributing his effect. He never wrote a psychological text, and his own research is small as compared with his influence. It is only in this way that he can be explained.

James McKeen Cattell (1860-), after an undergraduate course at Lafayette College, went abroad for two years to study (1880-1882). His interest in human capacities was fixed at this time by Lotze at Göttingen (the year before Lotze's death) and by Wundt at Leipzig. Cattell returned to America for a year's study at Hopkins, and while he was there Stanley Hall arrived to take command of psychology. Thus Cattell was a student of Hall's by accident for a semester. In 1883 he returned to Leipzig and, as we have already seen, informed Wundt in *ganz amerikanisch* fashion that he needed an assistant and that he, Cattell, would be it. His three years at Leipzig were very productive. He published over half a dozen articles in the *Philosophische Studien,* in *Mind,* and in *Brain,* all of them about reaction times or about individual differences. Some of these papers are now classic. The excitement about the reaction experiment as a tool for mental measurement was then at its height, and Cattell combined his

conventional interest in reaction times with his unconventional concern about the individual, so that some of the papers were contributions to both topics. He took his doctorate with Wundt in 1886.

In 1887 he was lecturer in psychology at the University of Pennsylvania and at Bryn Mawr College. In 1888 he was lecturer at Cambridge University, and at that time he came first into contact with Galton. The similarity of their views drew them together, although they had come independently at the problem of individual differences. Then Cattell became professor of psychology at Pennsylvania for three years (1888-1891) and began the psychological laboratory there. The most important outcome of this period was his monograph *On the Perception of Small Differences* (1892), which he published with G. S. Fullerton, a philosopher temporarily bewitched by experimental psychology. This paper, supported by another on errors of observation in the following year, shows Cattell already bringing statistical method to bear upon the conventional procedures in psychophysics, his revolt against the fine distinctions of introspective psychology, and his respect for the probable error. The difference between individual psychology and general psychology can be regarded as a matter of statistical emphasis; the former centers attention upon deviations from the mean, the latter upon the mean. General psychology was so well established that the probable error needed a champion, and it had one in Cattell.

From Pennsylvania Cattell went to Columbia, leaving Witmer in control of the new laboratory. He founded another laboratory at Columbia and remained in charge of it for twenty-six years (1891-1917). His first interests continued, although they now found less expression in personal publication. Up to 1906 there are perhaps six important papers of Cattell's on reaction times, individual differences, and tests. In 1896 he published with L. Farrand the classical study of physical and mental measurements of the Columbia students. We have already seen that he began with Baldwin the *Psychological Review,* and he remained an editor until Baldwin bought him out (1894-1904). Quite early in the new century he became especially concerned with the problem of eminence in men of science, and he has since published many statistical studies on the measurement and the conditions of eminence. His wide association with scientific organizations and

journals, and his editorship of *American Men of Science* have contributed to this work.

In 1917, at the time of America's entry into the World War, Cattell was dismissed by Columbia University because of his pacifistic stand. He had always been fearless in his frank expressions of opinion, though often in the minority, and there was an emotional background to this occurrence which was brought to a focus by the new emotions induced by the war.

In recent years Cattell has been active in his editorships, in the Psychological Corporation, an organization that he promoted for the sale of expert psychological services to industry and to the public, and in many organized projects where his advice was sought. He has become, by the death of others or their deflection from psychology, America's senior psychologist, and was elected president of the Ninth International Congress of Psychology, the first of these congresses to meet in America (1929).

Cattell's research is scattered, but six of his students undertook in 1914 to bring it together under six topics, and to point to its significance. We may mention these topics briefly.

With respect to research on *reaction times,* Henmon classes Cattell with Wundt. Cattell began his investigations at Hopkins and took them with him to Leipzig. By 1902 he had touched almost the entire field. On the side of apparatus he made various improvements in the chronoscope and in its control apparatus, and he invented a lip-key and a voice-key for vocal reactions. In the interests of objectivity of method he established the practice of disregarding extreme deviates in times in accordance with a statistical rule, instead of intuitively on the assumption that all seemingly wide deviates must have been due to lapse of attention. His investigation of times of reaction as dependent upon sense-organ and the peripheral location of tactual stimuli was begun at Leipzig and finished in a thorough study with C. S. Dolley at Columbia. This was Helmholtz's old experiment of the rate of conduction of sensory and motor impulses. Cattell and Dolley did not, however, arrive at a simple conclusion because so many other factors affected their results, as one can readily understand from the results of Cattell's other investigations. Cattell worked further with the times for discrimination and cognition (between which he did not, like the good Wundtians, distinguish) and with the times for choice ("will"); and his resultant table for "percep-

tion-times" and "will-times" for lights, colors, letters, pictures, and words has often been cited. Nevertheless he was himself doubtful of the validity of the 'subtractive procedure' by which these times are got, a procedure which went out of fashion even in Leipzig with Külpe's criticism of it. Cattell was also skeptical of Lange's classical distinction between the sensorial and muscular reactions and of the standard difference between them of one tenth of a second. He characterized the muscular time as a subcortical reflex, and later, as we have seen, he contributed results which lent aid to Baldwin in his controversy with Titchener. The sensory type of observer gives the sensorial reaction more quickly than the muscular, Baldwin's argument ran; and Cattell of course was glad to find an individual difference here. Cattell also investigated association times, first constrained associations and then free associations. These results are also classical in the literature of this experiment. He studied the relation of reaction time to intensity of stimulus, to attention, and to discrimination, and his results indicate a possible measure, by way of reaction time, of each of these factors. The last two studies have led to researches at the hands of others in which degree of attention or the amount of differences between sensations is measured by the reaction experiment. Obviously the latter measurement is a new psychophysical method.

Cattell's work on *association* has just been mentioned. The paper on controlled association is of 1887 and the paper on free association, of 1889. These papers, however, share with only a few others the distinction of being the foundation for the whole association method as it is used to-day. Cattell vaguely realized their significance when he remarked that the association reactions "lay bare the mental life in a way that is startling and not always gratifying." He also anticipated the work that has had its culmination in the lists of Kent and Rosanoff of normal associations, for he prepared such lists for a small group. The investigation of the association method has been carried much further by his students at Columbia.

In the matter of the times of perception and of reading, Cattell was also a pioneer. He investigated the retinal time of visual perception. Then he studied "the time it takes to see and name objects," forms, colors, letters, sentences. He used the tachistoscopic procedure and found that the time for each item was de-

creased when the number of simultaneously presented objects was increased up to a small number, say five. These data have been the classical data for the *range of attention,* although Cattell's interest was not in this systematic point but in the relation of the times to familiarity. More letters can be perceived in a short time when they form a word, or more words when they form a sentence, because the combination is familiar. Similarly Cattell showed that the reading time for different languages varies with the familiarity of the language, without the knowledge of the reader that the times are different. Cattell also investigated by this method the legibility of different letters of the alphabet and of different types, a study that has been carried on by others subsequently with some important practical results.

In *psychophysics* Cattell, in Galtonian fashion, brought the law of error to bear. He was quite out of sympathy with the classical German work of Fechner, Müller, and Wundt because of the stress that it laid upon introspection. He did not believe that reliable judgments of a sense-distance, with introspection supplying the only criterion, could be made, and he would have taken his stand against the distinction that is implied by Titchener in the phrase *the stimulus-error,* had that term then been coined. One judges stimuli, he thought—not sensations, which are something different; and the problem is one of the accuracy of the judgments. To such a problem the law of error can be applied because it is—or has been supposed to be ever since Gauss—a law of errors of observation. "It seems to me," he said in 1904, "that most of the research work that has been done by me or in my laboratory is nearly as independent of introspection as work in physics or zoölogy." Cattell's classic monograph is the one with Fullerton in 1892. In it they criticized the method of average error on the ground that the time of the adjustment of the equating stimulus by the subject is not controlled and many variable elements are thus introduced. They criticized the method of just noticeable differences on the ground that the category *equal,* applying to a range of stimuli, is dependent solely upon introspection and in practice yields great variability. They were left with the third Fechnerian method, the method of constant stimuli. This is the method that makes use of the normal law of error, and Cattell may have been favorably disposed toward it on this account. It also uses the category *equal;* but Cattell and Fullerton

got rid of this source of error by dividing the 'equal' judgments equally between the other two categories. The result was, not the limen which measures the range of the region of equality, but probable errors of judgments, a result which harmonizes with Galton's work much more than with Fechner's. Baldwin had remarks to make about Cattell's reverence for the probable error; certainly he raised it to a high office when he substituted it for the reigning limen.

Beside the construction of psychophysical apparatus, Cattell made two other important contributions to this field. He and Fullerton proposed, as a substitute for Weber's law, a law of the square root—for $S = k \log R$, $S = k \sqrt{R}$—although it was not sensation, S, but the error of observation that they were describing. The substitution of this formula for the other seemed to be indicated, partly on theoretical grounds connected with the law of error and partly by their empirical results. Here, too, we see that no tradition was too sacred for Cattell's iconoclastic hand. The other contribution we have mentioned; it is Cattell's new psychophysical method whereby the amount of sensory difference is measured by reaction time.

Just outside of psychophysics lies Cattell's invention of the *method of order of merit* or of relative position. This method he first developed in 1902 in connection with the placing of grays in order, a happy case for a first use, since the true order could be determined photometrically as a check. The subsequent history of the method in Cattell's hands has lain, however, with his studies of eminence, especially in the case of American men of science. On the basis of many rankings by different judges, he determined the central tendency of position for each individual and its probable error. The central tendency is taken, of course, as the true position. The method has had wide use, especially in the hands of Cattell's students, and he himself has carried his studies of eminence far along.

All these studies, it will be seen, have for their central theme the problem of *individual differences*. Cattell published in 1890 a paper on mental tests and measurements, and another in 1893 on tests of the senses and faculties. In general, however, his own research on the subject has been the work cited and his study of *Homo scientificus Americanus*. "It is surely time," he wrote, "for scientific men to apply scientific methods to determine the cir-

cumstances that promote or hinder advancement of science." This was his motive. His influence, however, has been very much wider in support of the mental tests. E. L. Thorndike was his student, and, after devising mental tests for animals in the form of the mazes and puzzle-boxes of his doctoral dissertation, was advised by Cattell to undertake the same sort of thing with children at the new Teachers College founded at Columbia. Thorndike has been the leader in the psychology of mental tests in America. Columbia has been the leading university in this movement. Between them Cattell and Thorndike created at Columbia an irresistible atmosphere, and Cattell, who refused to follow Wundt, can now see his own influence all over America spread by Columbia students—his children, his grandchildren by way of Thorndike, and even his great-grandchildren. Of course America as an atmosphere helped. As we have said, it was ready for just this kind of psychology, and it responded. Not all the leaders in this field to-day are Columbia men. Terman, fostered by Stanley Hall, is one notable exception.

However, Cattell's psychology and thus the psychology of America's largest university is something more than mental tests and reaction times and statistical method and the resultant objective judgments that are not introspections. It is a psychology of human *capacity*. It is motivated by the desire to determine how well men can do in this or that situation. It is concerned little with an analysis of capacities into conscious causes, and only a little more with the physiological causes. It seeks a description of human nature in respect of its range and variability, just as Galton sought the same end. This psychology of capacity is, of course, functional psychology, but it is perhaps not wise to give so unphilosophic a movement this formal systematic name. Nevertheless, it is important to realize the significance of the movement, because it, more than any other 'school,' has been typical of the American trend. In the author's view, behaviorism, instead of being a brand-new movement, is simply a formulation of the American faith, which came about in a way that made it look new, but assimilated everything to itself because it was not new. We shall return to behaviorism in a later chapter.

It is difficult to give a picture of Cattell's effect upon American psychology by way of his students. Columbia University has given many more doctor's degrees in psychology than any other

university, and this statement is true for any period of several years that may be selected. In the latter part of Cattell's incumbency, however, many of these doctorates represent Thorndike's and Woodworth's influence. Out of a list about four times as large and one that contains many other well-known names, we may mention those psychologists who received their doctorates in psychology at Columbia while Cattell was in command and who have been at some time placed by Cattell in the list of the fifty leading American psychologists, lists determined at intervals by Cattell's statistical treatment of judgments of order of merit. (The date of the doctorate is given with each name.) Here is the list: E. L. Thorndike (1898), who began the formal laboratory work upon animals, who has been America's most distinguished leader in the field of the mental tests, and who is now at Teachers College at Columbia; R. S. Woodworth (1899), who was long Cattell's support at Columbia and who succeeded him in his position there and to some extent in his relation to American psychological thought; S. I. Franz (1899), famous for his work on the localization of cerebral functions; Clark Wissler (1901), anthropologist and psychologist of the psychological institute at Yale; W. F. Dearborn (1905), Harvard's educational psychologist; F. L. Wells (1906), the champion of psychometrics at the Boston Psychopathic Hospital; Warner Brown (1908), the senior experimentalist at the University of California; H. L. Hollingworth (1909), of Barnard College and therefore still of the Columbia group, and the author of many books on a wide range of psychological topics; E. K. Strong (1911), known for his work in industrial psychology; A. T. Poffenberger (1912), now Woodworth's support at Columbia as Woodworth used to be Cattell's; T. L. Kelley (1914), Thorndike's pupil and Stanford University's copy of Karl Pearson, perhaps now America's leading psychologist-statistician; A. I. Gates (1917), professor of psychology with Thorndike in Teachers College and the youngest of this distinguished list.

Other Pioneers

We have considered the more influential of the American 'pioneers' in experimental psychology. Now let us mention the other men who loomed large in the 1880's and 1890's.

Joseph Jastrow (1863-), born in Warsaw, Poland, was a

student at Hopkins when Hall went there, and received his doctorate in 1886 for work done with Hall. He then went in 1888 to Wisconsin as professor of psychology, began a laboratory there, and remained in this position until his retirement in 1927.

Jastrow's original work was in psychophysics and he got his first interest in psychology from C. S. Peirce and not from Hall. While he was at Hopkins, he presented to the National Academy of Sciences in collaboration with Pierce an important paper on the method of determining the differential limen, and a little later he published a general critique of the psychophysical methods. In this work he anticipated Fullerton and Cattell in recommending the substitution of the probable error for the conventional limen, and thus the notion that the critical frequency in differential sensitivity is 75 per cent and not 50 per cent. In the method of constant stimuli, the limen had been defined as the point where the judgment *greater* (or *less*) is given as often as it is not, i.e., at 50 per cent. However, this criterion implies the acceptance of the category *equal,* for, if there be only two categories, *greater* and *less,* then, when *greater* is given 50 per cent of the time, *less* must be given the other 50 per cent, the two liminal points coincide, and there is not the interval between them that the concept of the limen requires. Jastrow, like Cattell, mistrusted the subjective criterion for the equality judgments, and he had to choose the probable error or the judgmental frequency of 75 per cent in order to preserve the fundamental psychophysical notion. Within the accepted categories Jastrow attempted to establish different degrees of confidence, as did Fullerton and Cattell later, and F. M. Urban still later. Degrees of confidence, however, introduce again the subjective variability that Jastrow and Cattell did not want, and their estimation has never become the established practice. On the other hand, the use of 75 per cent as a critical frequency has found supporters in every decade.

The '90's were the heyday of the 'minor studies.' Experimental psychology was very new. Almost any one with patience and a little knowledge or advice might, according to the faith of the times, contribute important experimental data. Little formal training was required because there was little to be had. In some measure this faith was justified. Many important studies have appeared thus labeled "minor." It was Jastrow who started this custom with twenty-five minor studies published from Wisconsin

in three years (1890-1892). Clark and Cornell soon followed suit; later Michigan had such a series for a time; still later Vassar under M. F. Washburn began a long series which still continues. Jastrow did not continue with what he started, but the early series contained many short psychophysical studies which had to be taken into account at the time.

Jastrow is also known for his popularization of scientific psychology, which he accomplished in a much more dignified manner than did Scripture. Many of his essays and lectures were finally brought together in his *Fact and Fable in Psychology* (1900), a book that considers the psychology of the occult, psychic research, mental telepathy, deception, spiritualism, hypnotism, the dreams of the blind, and similar topics.

Edmund Clark Sanford (1859-1924) went to Hopkins to study psychology with Hall and received his degree there in 1888. Then he migrated with Hall to Clark, where, as we have seen, Hall turned over to him the new laboratory. His psychological life was cut short by his appointment in 1909 to the presidency of Clark College, the undergraduate addition to the graduate Clark University.

He began, and maintained at an excellent standard, the minor studies from Clark. He was something of a technician, and constructed with his own hands many original pieces of psychological apparatus. His vernier pendulum chronoscope has become the standard instrument for elementary work with reaction times. His publications were often of a literary nature with a strain of whimsicality in them. His health was never good, and that fact probably accounts for his lack of aggressiveness, both in productivity and in the professional quarrels that were characteristic of his generation.

However, Sanford accomplished one very important service for psychology. He wrote the first laboratory manual for the new science, antedating even Titchener. This *Course in Experimental Psychology* began to appear in the *American Journal of Psychology* in 1891, and the book, altered from these advance issues, appeared in 1898, three years before Titchener's first manual. Sanford's book covered only the topics of sensation and perception; it was supposed to be the first volume of a complete work, but he never wrote Part II. However, within the most highly developed field of the new psychology, this handbook was pioneer

work of exceptional merit. In spite of the other manuals that have followed upon it, the psychologist still turns to Sanford's *Course* for references, for experiments, and for apt lecture demonstrations.

Titchener was another pioneer in America, but not in American psychology as we have used the phrase in this chapter. Titchener, though not a German, worked always in the German tradition and stood apart from the American trend. For this reason we have already discussed his contribution to psychology in chapter 17. He had the closest association with Sanford, who was one of the few prominent American psychologists, all older than Titchener, who welcomed the young Englishman to America in 1892.

Münsterberg was the other foreign importation into America in 1892. He was a German, but did not follow the German tradition which he had represented at Freiburg. Instead he turned to promoting, in the broadest sense, applied psychology in America. His should be one of the names to associate especially with the mental tests, but on the whole his scientific influence was much less than Cattell's. His philosophy was explicit and it was not American functionalism. He never belonged to America. His popularization of applied psychology only brought him into disrepute with his colleagues. Here lies presumably the reason why Münsterberg's influence died with him, as Cattell's, already passed beyond his own control, cannot. We have mentioned Münsterberg in chapter 17 and must be content to let our acquaintance with a forceful, far-seeing enthusiast rest thus.

There were still other men of the early days in America. There was E. B. Delabarre (1863-), an American student of Münsterberg's at Freiburg, who has been at Brown University since 1891. He contributed early experimental work. There was W. L. Bryan from Clark, one-time president of the American Psychological Association; but very early in his career he lapsed into the presidency of Indiana University. There was Livingston Farrand, the anthropologist, who was associated with Cattell in testing the students at Columbia, and who has recently become the president of Cornell. There were the philosophers in the days when psychology, even experimental psychology, was still part of philosophy: J. G. Schurmann and J. E. Creighton of Cornell; Fullerton, who experimented with Cattell at Pennsylvania; Josiah Royce of Harvard, who wrote a psychology; and John Dewey,

who also wrote a psychology and who played an important rôle in the movement of functional psychology, which we shall consider in the next chapter. All these men were actively concerned with the new psychology in its early days; all were members of the American Psychological Association, which was founded in 1892, and played active parts in its affairs. However, except for Dewey, the perspective of the present does not show that they influenced greatly the course of psychology in America.

Notes

James

On the life of William James (1842-1910) see Henry James, *The Letters of William James*, 1920. James could hardly put his pen to paper without leaving some record of his vivid personality, and the *Letters* well repay reading by whoever would catch the flavor of the man. The volumes omit technical letters, but the psychologist may care especially to refer to the six letters (out of more than 300) to Stumpf, Münsterberg, and Sully: I, 247-249, 262-267, 312 f.; II, 119 f., 140-142, 320 f.

James's writings were widely scattered, and often original writing is almost lost in book reviews. This difficulty is relieved by the annotated bibliography of some 300-odd excursions into print, by R. B. Perry, *The Writings of William James*, 1920. The *Letters* gives a list of books, II, 357-361. Perry's list is based originally upon the less complete, unannotated, but more available list in *Psychol. Rev.*, 18, 1911, 157-165. Of course a very great deal has been written about James; but especial mention needs, perhaps, to be made only of E. Boutroux, *William James*, 1911, Eng. trans., 1912.

James's important psychological books are: *Principles of Psychology*, 1890; *Text-Book of Psychology: Briefer Course*, 1892; *Talks to Teachers on Psychology*, 1899; *Varieties of Religious Experience*, 1901-1902. The philosophical books are: *Pragmatism*, 1907; *A Pluralistic Universe*, 1909; *The Meaning of Truth*, 1909; and four posthumously printed books, three of which are collections of essays, largely reprinted.

On the opinions of American psychologists on the importance of James in the history of psychology, see M. A. Tinker, *Amer. J. Psychol.*, 38, 1927, 454.

There has been a controversy as to who 'founded' the first psychological laboratory in America. James was giving experimental instruction in rooms especially assigned to this purpose in 1874-1876 (approx.); Stanley Hall established a laboratory at Hopkins about 1883. The Harvard laboratory was formally established in 1892. See C. A. Ruckmick, *Amer. J. Psychol.*, 23, 1912, 520; James's *Letters*, I, 179 (note), 301; James, *Science*, N.S. 2, 1895, 626 (cf. also 734 f.).

On the stream of thought, see *Principles*, I, 224-290. On consciousness as cognitive and functional, and for the quotations of the text, see *Principles*, I, 144; *Text-Book*, 103 and 13 f. On James's use of the word *function*, see Ruckmick, *Amer. J. Psychol.*, 24, 1911, 111.

On James's theory of emotion, see his writings: *Mind*, 9, 1884, 188-205; *Principles*, 1890, II, 442-485; *Psychol. Rev.*, 1, 1894, 516-529. C. Lange's paper is *Om Sindsbevoegelser*, 1885; *Ueber Gemütsbewegungen* (German trans.), 1887. James's paper of 1884 is reprinted in his *Collected Essays and*

Reviews, 1920. It and the chapter from the *Principles* (but not the important paper of 1894) have been reprinted with the first English trans. of Lange's monograph, Lange and James, *The Emotions* (Psychology Classics, I), 1922.

E. B. Titchener has argued that the James-Lange theory was not new, but clearly had its origin in the writings, for the most part, of the earlier French philosopher-physiologists: see *Amer. J. Psychol.*, 25, 1914, 427-447. Titchener is right: it is the paradox of thought that new ideas are always old, that a novelty is established only by summation.

On the immediate critical discussion which James's theory of emotion produced, see E. Gurney, *Mind*, 9, 1884, 421-426; W. L. Worcester, *Monist*, 3, 1893, 285-298; D. Irons, *Mind*, N.S. 3, 1894, 77-97.

W. B. Cannon's work was originally brought together in his *Bodily Changes in Pain, Hunger, Fear and Rage*, 1915. For his recent views, see *op. cit.*, 2d ed., 1929; *Amer. J. Psychol.*, 39, 1927, 106-124; *Feelings and Emotions: Wittenberg Symposium*, 1928, 257-268. For a criticism of Cannon, see J. R. Angell, *Psychol. Rev.*, 23, 1916, 251-261.

Hall

There is a great deal of information available about the life and work of G. Stanley Hall, but there is no very full direct biographical account. The most direct is L. N. Wilson, *G. Stanley Hall: a Sketch*, 1914, but there are many omissions in it. It may be supplemented by *Publications of Clark Univ. Library*, 7, 1925, no. 6, which contains a further sketch by Wilson, sketches by E. C. Sanford (reprinted from *Amer. J. Psychol.*, 35, 1924, 313-321) and W. H. Burnham (reprinted from *Psychol. Rev.*, 32, 1925, 89-102), extracts from the letters of numerous psychologists about Hall, a list of 81 persons who took their doctor's degrees with Hall at Clark, and a bibliography. Very informative is Hall's own *Life and Confessions of a Psychologist*, 1923, which exhibits Hall's characteristic inaccuracy as to detail, but gives the inner picture of the man, and also reveals for the first time the inside history of the difficulties at Clark in 1890-1900 when the founder, the president, and the faculty were all at odds. Then there is L. Pruette, *A Biography of a Mind*, 1926, which is a collection of sketches without an index, and is interesting reading but inconvenient for reference. S. C. Fisher, The psychological and educational work of Granville Stanley Hall, *Amer. J. Psychol.*, 36, 1925, 1-52, really deals with Hall's psychology and only incidentally with his biography. Under her hand Hall begins to show some signs of systematic stability beneath the turbulent surface of his stormy interests. On the relation of Hall to Bowditch, cf. W. R. and C. C. Miles, *Amer. J. Psychol.*, 41, 1929, 326-336.

For a bibliography to 1914 (339 titles), see Wilson, *op. cit.*, 119-144; to 1922, *Life and Confessions*, 597-616; complete (439 titles), *Publications of Clark Univ. Library*, *op. cit.*, 109-135. Hall was prolific.

For an excellent picture of Hall as a man, see Burnham, *op. cit.* For an estimate of him as a psychologist, based on his own method of the questionary and thus summarizing the opinions of American psychologists, see E. D. Starbuck, *Psychol. Rev.*, 32, 1925, 103-120.

If the reader questions the propriety of including an extended account of Hall in a history of experimental psychology, he should refer to the editorial in *Amer. J. Psychol.*, 7, 1895, 1-8, esp. 1 f. Here he will discover that the enthusiastic group about Hall at Hopkins and Clark thought of him as the prime mover in the new psychology in America, and thus in American experimental psychology. Of course their enthusiasm ran away with them, as *Science*, N.S. 2, 1895, 626-628 shows (though cf. here *ibid.*, 734 f.).

For Hall as the propagandist of the new psychology, see *Andover Rev.*, 3, 1885, 120-135, 239-248; *Mind*, 10, 1885, 245-249. It must be remembered that Clark University was really only a scientific institute, and that psychology was the dominant department in it. This unique reversal of the usual academic situation made psychologists think of Clark as a psychological university, and Clark and Hall are inseparable in thought. Moreover, Titchener, relatively isolated in American psychology, threw in his lot, *faute de mieux*, with Hall in the *American Journal of Psychology*, and Clark and Cornell standing together formed a strong party.

Hall's influence is further shown by the long list of his pupils and their record of productivity in after years. Of the eighty-one doctorates that he gave at Clark, the author ventures here to list twenty-five names which seem to him best known to psychologists. The list is chronological and naturally includes mostly the older persons. All but one of them received their degrees before 1912, and two thirds of them before 1900. The list: H. Nichols, W. L. Bryan, A. H. Daniels, J. A. Bergström, F. B. Dresslar, T. L. Bolton, J. H. Leuba, C. A. Scott, E. H. Lindley, E. D. Starbuck, L. W. Kline, F. E. Bolton, H. H. Goddard, E. B. Huey, H. D. Sheldon, W. S. Small, N. Triplett, A. W. Trettien, J. Morse, L. M. Terman, D. S. Hill, G. Ordahl, H. W. Chase, E. S. Conklin, F. Mateer. Hall's executive personality made him a practical psychologist, and thus an educational psychologist, and thus a president. Do pupils take after their 'master'? Well, the foregoing list contains four college presidents, four college deans, and one superintendent of schools—a third altogether.

Ladd

No list of Ladd's very many books on philosophy and topics connected with his interest in the Orient is necessary here. He was a persistent writer, and Stanley Hall complained in 1894 that his new book was "the eighth large volume put forth by the author within the last few years, to own all of which now costs the devoted reader twenty-five dollars and fifty cents": *Amer. J. Psychol.*, 6, 1894, 477 f. Hall had been much more cordial to the first book: *ibid.*, 1, 1887, 159-164. Two of these eight books are unimportant outlines of Yale courses; one cannot be readily identified; the others and two that came out later are: *Elements of Physiological Psychology*, 1887, revised by R. S. Woodworth, 1911; *Introduction to Philosophy*, 1890; *Outlines of Physiological Psychology* (the abridged *Elements*), 1891, 8th ed., 1908; *Primer of Psychology*, 1894; *Psychology, Descriptive and Explanatory*, 1894; *Philosophy of Mind*, 1895; *Outlines of Descriptive Psychology* (abridged from the book of 1894), 1898.

George Trumbull Ladd was a distinguished person, but there seems to be no adequate account of his life and work. For short necrological sketches, see A. C. Armstrong, *Philos. Rev.*, 30, 1921, 639 f.; and esp. E. B. Titchener, *Amer. J. Psychol.*, 32, 1921, 600 f.

For an analysis and criticism of Ladd's system, see Titchener, *ibid.*, 519-542, esp. 521-531. This article deals with functional psychology in general, and the four characteristics of a functional psychology, which the text gives, are derived from Titchener; cf. esp. p. 542.

Scripture

For Edward Wheeler Scripture, the text cites the two important books and the *Studies from the Yale Psychological Laboratory* which make up the more important part of his psychological work from 1892-1902. After 1902 the trend of his work in phonetics is shown by *Elements in Experimental Phonetics*, 1902; *Researches in Experimental Phonetics: the Study of Speech Curves*, 1906; and *Stuttering and Lisping*, 1912, 2d ed., 1923. There were

many other publications within the same field.

It is perhaps well to add to the comment of the text that Scripture's enthusiasm for the 'new' psychology contained an element of exaggeration and another of egotism (cf. the account of John Elliotson in chap. 7). It thus made him few friends and perhaps explains why the productivity of the Yale laboratory was limited so nearly to his own. Both his early books revealed a faith in the Yale laboratory similar to that of the modern 'booster' in an American town. The conservatives—and even Hall is a conservative in this context—disliked such popular appeals as a picture of all the European monarchs holding hands in a chain reaction. It was Scripture who invented the term *arm-chair psychology*. His first book was "written expressly for the people" with the hope that it could "be taken as evidence of the attitude of the science in its desire to serve humanity," a statement that is not wholly without diagnostic value for the American trend. Scripture's confidence in himself is shown by his statement in the preface of this book that Wundt is the greatest of psychologists but intelligible only to the expert, and that "no one else has produced a book explaining the methods and results of the new psychology." He added: "This is my reason for writing one"—in 1895!

Baldwin

James Mark Baldwin's more psychological books were: *Handbook of Psychology: Senses and Intellect*, 1889, 2d ed., 1890; *Handbook of Psychology: Feeling and Will*, 1891; *Elements of Psychology* (the abbreviated *Handbook*), 1893; *Mental Development in the Child and the Race*, 1895, 3d ed., 1907; *Social and Ethical Interpretations in Mental Development*, 1897, 4th ed., 1907 (the word *Ethical* was added to the title after the ms. was completed, enabling the author to win the decennial gold medal of the Royal Academy at Copenhagen which prescribed the social foundations of ethics as the field for competition that year); *The Story of the Mind*, 1898; *Fragments of Philosophy and Psychology* (collected essays), 1902; *History of Psychology*, 1913. Baldwin's most important undertaking in the Hopkins days was the writing of *Thought and Things or Genetic Logic*, 3 vols., 1906-1911. There are four other books dealing with the philosophy of evolution and four about America, France, the Allies, and the World War.

For Baldwin's life and much personal information about the psychology of 1890-1905, see his *Between Two Wars: 1861-1921*, 1926. The first volume is *Memories*, and is the more important in this connection. The second is *Opinions and Letters*. The *Memories*, although somewhat discursive, make capital reading, for Baldwin writes in a detached, half-humorous way about himself and others—many others. He probably holds among psychologists the record for close approach to persons of royal blood.

In this book Baldwin chuckles over the incident of having to pay duty on Bain's gift of the two large volumes of his *Psychology*. Baldwin protested the duty on the ground that scientific books were then duty-free, and Washington replied: "Our experts report that these books are in no sense scientific." One wonders at once whether Baldwin's books might not have evoked the same judgment.

The text asserts that Baldwin was essentially a writer in the sense that his theories were closely associated with their verbal expression. The grounds for this statement lie in Baldwin's tendency to quote verbatim his own statements and *bons mots*. Cf. Baldwin's own discussion of this point in the preface of the *Story of the Mind*.

The flavor of the reaction of the more experimentally or empirically minded group to Baldwin's theorizing

is given in T. L. Bolton's review of Baldwin's *Mental Development in the Child and the Race;* see *Amer. J. Psychol.*, 7, 1895, 142-145. See also the controversy with Titchener, cited in chap. 17.

On Baldwin's laboratory at Toronto, see *ibid.*, 3, 1890, 285 f.

Cattell

The only discussion of James McKeen Cattell's psychological work, scattered in many short articles in many journals, is the commemorative volume issued by six of his students, The psychological researches of James McKeen Cattell, *Arch. Psychol.*, no. 30, 1914. Here V. A. C. Henmon treats of reaction time, W. F. Dearborn of reading and perception, F. L. Wells of association, R. S. Woodworth of psychophysics, H. L. Hollingworth of the method of order of merit, and E. L. Thorndike of individual differences. Henmon's chapter is an excellent account of the entire history of the reaction experiment.

Beside other publications, Cattell has written about sixty articles of a psychological nature. Many of them are short, and some of these are English accounts of research published more fully in German. The papers to which the text refers directly or indirectly are as follows:

Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben, *Philos. Stud.*, 2, 1885, 635-650, the first paper on times for cognition.

Ueber die Trägheit der Netzhaut und des Sehcentrums, *ibid.*, 3, 1885, 94-127, a paper on times of perception tachistoscopically determined, which thus becomes data for what others have called the range of attention. Brief English account of the same in *Brain*, 8, 1885, 295-312.

Psychometrische Untersuchungen, *Philos. Stud.*, 3, 1886, 305-335, 452-492; 4, 1887, 241-250, the classical series of papers on reaction times, apparatus for the chronoscope, times of discrimination, choice, and association, and the effect of attention and practice on the times. Brief English account in *Mind*, 11, 1886, 220-242, 377-392, 524-538; 12, 1887, 68-74.

Influence of the intensity of the stimulus on the length of the reaction time, *Brain*, 8, 1886, 512-515.

Mental tests and measurements, *Mind*, 15, 1890, 373-380, Cattell's first advocacy of mental tests and the first use of the phrase.

With G. S. Fullerton: *On the Perception of Small Differences*, 1892, the classical psychophysical monograph.

On errors of observation, *Amer. J. Psychol.*, 5, 1893, 285-293, a paper which follows up the preceding item.

Tests of the senses and faculties, *Educ. Rev.*, 5, 1893, 257-265, a further argument for mental tests in education.

With C. S. Dolley: On reaction-times and the velocity of the nervous impulse, *Mem. Nat. Acad. Sci.*, 7, 1895, 391-415; more briefly, *Psychol. Rev.*, 1, 1894, 159-168.

With L. Farrand: Physical and mental measurements of the students of Columbia University, *Psychol. Rev.*, 3, 1896, 618-648, the practical trial that corresponds to Galton's establishment of his Anthropometric Laboratory.

On relations of time and space in vision, *Psychol. Rev.*, 7, 1900, 325-343.

The time of perception as a measure of differences in intensity, *Philos. Stud.*, 19, 1902, 63-68, the method of reaction time for psychophysical discrimination.

A statistical study of eminent men, *Pop. Sci. Mo.*, 62, 1903, 359-377.

Statistics of American psychologists, *Amer. J. Psychol.*, 14, 1903, 574-592.

Conceptions and methods of psychology, *Pop. Sci. Mo.*, 66, 1904, 176-186.

Statistical study of American men of science, *Science*, N. S. 24, 1906, 658-665, 699-707, 732-742.

Further statistical study of American men of science, *Science*, N. S. 32, 1910, 633-648, 672-688.

Families of American men of science, *Pop. Sci. Mo.*, 86, 1915, 504-515; *Scient. Mo.*, 4, 1917, 248-262; 5, 1917, 368-377.

Cattell's editorships are: *Psychological Review*, with Baldwin (1894-1903); *Popular Science Monthly* (1900-1915), continued as *Scientific Monthly* (1915-); *Science* (1904-) *American Naturalist* (1907-); *School and Society* (1915-); *American Men of Science*, 1906, 4th ed., 1927.

The articles on the ranking of men of science are variously reprinted in the editions of *American Men of Science*. See the 4th ed., 1111-1117, for the last selection of fifty psychologists, of which account is taken in the text in naming Cattell's more eminent students.

On Cattell's laboratory at the University of Pennsylvania, see *Amer. J. Psychol.*, 3, 1890, 281-283. See also his reminiscences of early psychological laboratories, *Science*, N.S. 67, 1928, 543-548, or *Feelings and Emotions* (*Wittenberg Symposium*), 1928, 427-433 (the two articles are identical).

Jastrow

Joseph Jastrow's important psychophysical contributions are: C. S. Peirce and Jastrow, On small differences of sensation, *Mem. Nat. Acad. Sci.*, 3, 1884; Jastrow, Critique of psychophysic methods, *Amer. J. Psychol.*, 1, 1888, 271-309. For a recent argument for making the critical frequency 75 per cent in determining the differential limen, see G. H. Thomson, *Psychol. Rev.*, 27, 1920, 300-307.

The minor studies from the Wisconsin laboratory are scattered through the *Amer. J. Psychol.*, vols. 3-5.

For an account of the early Wisconsin laboratory, see *ibid.*, 3, 1890, 275 f.

Sanford

For accounts of Edmund Clark Sanford's life and work, see W. H. Burnham, *Ped. Sem.*, 32, 1925, 2-7; E. B. Titchener, *Amer. J. Psychol.*, 36, 1925, 157-170; also *Publications of Clark Univ. Library*, 8, 1925, no. 1, which reprints the first two articles and adds others. The Clark pamphlet gives a bibliography, including a list of the minor studies. Titchener gives the same, and also a list of articles that resulted from Sanford's work with students. The minor studies occur in *Amer. J. Psychol.*, vols. 5-19 (and one in vol. 35 after Sanford had retired from the presidency of Clark College and returned to psychology for his few remaining years).

The *Course in Experimental Psychology, Part I: Sensation and Perception*, 1898, had been appearing in the *Amer. J. Psychol.*, vols. 4-7, 1891-1896. These dates are important: Sanford was much more of a pioneer in the matter of a laboratory course in experimental psychology in 1891 than he was in 1898. Psychology moved rapidly in that decade.

On Sanford's vernier chronoscope, see *Amer. J. Psychol.*, 3, 1890, 174-181; 9, 1898, 191-197; 12, 1901, 590-594.

Chapter 21

AMERICAN PSYCHOLOGY: MOVEMENTS

IN the last chapter we have discussed the establishment of American psychology, a process that was pretty well accomplished by the end of the century. The character of American psychology was by that time well formed, but it is doubtful if any one then understood its nature. It seemed that America had adopted the new psychology from Germany and that its deviations from the type were due rather to the idiosyncrasies of individuals than to any national trend. However, it was soon to become apparent what had been in the making. There were during the first fifteen years of the twentieth century four movements that, at least in the author's opinion, were the natural outcome of what had gone before. (1) First there was put forward at Chicago a systematic view that became the platform of the school of functional psychology. Although this school was limited at first geographically and later, as its members migrated to other places, in its personnel, it was nevertheless in a broad way an expression—the only formal communal expression—of the epistemological attitude in American psychology in general. (2) Second, there were the mental tests, an entirely unsystematic movement toward practical psychology, but a movement that came, as the last chapter has tried to make clear, directly out of the new American faith. (3) Animal psychology also got rapidly under way with its own laboratories, its own body of research, and ultimately its own journal. It too belongs to a psychology leavened by the theory of evolution rather than the theory of introspection. (4) Finally, if we are to make the picture complete, we must not forget that general experimental psychology of the human mind, the daughter who took more after her German than her English parent, showed a continued development along lines that paralleled the German research. The American experimental movement was, however, not quite the same as the German pattern. Except for Titchener and those

few of his students who remained true to his convictions, this fourth movement was tempered in various degrees by contact with the first three.

In the present chapter we must examine the first three of these four movements, leaving the last to wait until a later chapter, where we shall undertake from another perspective to view the entire course of experimental work.

Functional Psychology

It was the philosopher, *John Dewey* (1859-), who was the organizing principle behind the Chicago school of functional psychology. Dewey was one of the men whom Hall found at Hopkins when he went there in 1882. Dewey took his degree at Hopkins two years later and went to Michigan to a series of appointments in philosophy. He was there for ten years (1884-1894), except for an intervening year at Minnesota. In 1886 he published a *Psychology*. This book was the first venture by an American to write a text for the 'new' psychology, and the first attempt in English except for Sully's *Outlines* of 1884. The text was popular and reached a third edition within five years. It was, however, overshadowed by the other texts that appeared shortly, Ladd, Baldwin, James, and the rest, largely because it represented the philosopher's approach to a science that had rebelled against philosophy. Dewey said then, as philosophers say now, that an exposition of psychology depends upon the philosophical assumptions implicit in it, and that it is better to have these assumptions explicit and out in the open than to use them, pretending that they are not there. This argument, however, has never made headway with the psychologists, who are inclined to point to the unphilosophical nature of the other sciences in explanation of their own philosophical naïveté. Nevertheless, Dewey's book was important at the time because of its priority, and it is important now because he foresaw the point of view that became explicit later.

In 1894 Dewey went to the University of Chicago for ten years as professor of philosophy. G. H. Mead came with him from Michigan as assistant professor of philosophy. A. W. Moore was assistant in philosophy the next year. Both Mead and Moore are still at Chicago. In 1894 James R. Angell also came to Chicago

as assistant professor of psychology and director of the psychological laboratory which had been started the year before. In this year, Dewey, the oldest, was thirty-five; Angell, the youngest, was but twenty-five. Dewey was half a psychologist. Mead and Moore were interested in psychology. Dewey, the senior, a brilliant man, was able to exert considerable influence upon the systematic tenets of the others. They were all young. It was just the situation that might lead to a school founded upon a systematic point of view.

The first important paper in the establishment of this functional psychology was Dewey's criticism of "the reflex arc concept in psychology," an article that appeared in 1896. Let us see what he had to say.

In the first place, he had a great deal to say against elementarism, either phenomenal or physiological. There was nothing new in this position. We went all over that ground with James in the last chapter. Objections to elementarism had been raised ever since James Mill reduced the principle to an absurdity by carrying it to the ultimate extreme. Even Wundt was not the obvious elementarist that his critics take him for, as his insistence on the use of the word *process* and his theory of actuality show. However, Dewey went pretty far: he objected to the analysis of the reflex arc into stimulus and response, to the analysis of total acts into reflex arcs, and even at times to the isolation of total coördinations from their past and future. It is the totalities with which we must deal in psychology, he argued. Once again then in this book we have to point to an anticipation of the modern theory of *Gestalt;* like James, Dewey was a *Gestalt-psychologe* twenty years too soon. Dewey also objected to the analytical dichotomy of mind and body, an outworn metaphysical dualism derived from Plato. This objection, of course, assimilates him at once to the entire American movement from James to Cattell, and is the reason why he could be writing about the whole of psychology in discussing reflex arcs.

What Dewey offered as the materials of psychology were coördinations. Sensation and response were too artificial as elements for him to allow them, but he was ready to accept the reflex arc, provided it was regarded only as a first developmental stage of larger coördinations. Here his point was that the entire arc is the minimal unit. It is not sensation followed by idea fol-

lowed by movement, but a single integral act. The response is to the sensation, the sensation is for the response; neither has meaning alone. So too the arc is really too elemental when represented as having a definite beginning and end, for what really happens is circular, and the end of an arc is also the beginning of another arc, as in walking, when we have a total act which is only artificially broken up into a succession of arcs.

All this may be at first a little confusing, for it would seem that there are successive physiological phases within the total event which might well be distinguished, if for no other reason, because they are displaced from one another in time. Dewey admitted such an account to be "a rough practical way of representing the process." It was, however, for him a misrepresentation of the truth, because he felt that the key to unity lay in the *function* of an act. A coördination is "an organization of means with reference to a comprehensive end." "The stimulus is that phase of the forming coördination which represents the conditions which have to be met in bringing it to a successful issue; the response is that phase of one and the same forming coördination which gives the key to meeting these conditions, which serves as instrument in effecting the successful coördination. They are therefore strictly correlative and contemporaneous. The stimulus is something to be discovered . . . It is the motor response which assists in discovering and constituting the stimulus." Thus the stimulus, and the sensation too, have no real separate existence, but may be factored out of the whole by reference to their function, and their function is the conditioning phase of the mental act. The stimulus, or the sensation, exists only logically, and exists thus *for* the act. For this reason Titchener could say that the datum of functional psychology is not an "*Is*," but an "*Is-for*."

It is quite plain that we have here a recognition of biological function. The key to the act is the "successful issue," the end. The coördination is an adaptation. Unity is given teleologically. Functional psychology is the study of the psychophysical organism in use.

It is not necessary for us to show again how such a view leads at once to a practical psychology. Darwin's theory of survival and of adaptation is the greatest practical theory of living that has ever been promulgated. A psychology that is biological in the Darwinian sense is inevitably practical. Dewey's later life illus-

trates this point. In 1900 he made his presidential address before the American Psychological Association on "Psychology and Social Practice," a plea and a program for educational psychology. Two years later he was made director of the School of Education at the University of Chicago. In 1904 he was called to Teachers College, Columbia, as professor of philosophy, undoubtedly the most suitable eminent psychological philosopher in America for contributing to the practical problems of education. While he has had in the last twenty-five years little effect upon psychology proper, he has led a very influential life in its effect upon intellectual America, expounding repeatedly the problems of human nature.

James R. Angell (1869-) must have fallen in with Dewey's ways of thinking almost at once. In the same year that Dewey discussed the concept of the reflex arc, Angell with Moore, the young philosopher, published an experimental study in reaction times. This investigation was the one that resolved the controversy between Baldwin and Titchener—a very important paper, therefore. It did not require Dewey's psychological approach to show that there is nothing incompatible between the existence of individual differences in naïve subjects and a general principle of response in highly practised subjects, but the paper has the additional interest of using Dewey's phraseology for the description of an actual experimental situation and result. It was published two months before Dewey's article, and Dewey referred to it as a concrete illustration of his views.

Two years later (1898) functional psychology was given further definition by meeting with explicit opposition. Titchener adopted from James the phrase *structural psychology* as opposed to *functional psychology*. Actually his paper was a reply to a criticism by some one else, but it is plain that he was also replying to Dewey indirectly. Titchener listed the biological sciences, distinguished between morphology as structural and physiology as functional, and drew the parallel for psychology. It was in this article that he distinguished between an "*Is*" and an "*Is-for.*"

With the turn of the century, the school of functional psychology became more clearly outlined. Educational psychology was becoming important. The movement of the mental tests was getting under way, although Binet was only just ready to publish his classical study of intelligence. Experimental animal psychology

was developing. The practical and biological aspects of psychology were becoming obvious, and were thus at hand to support the functional view. In 1903 Irving King published a *Psychology of Child Development* which definitely reflected Dewey's teaching and helped establish the new way of looking at psychology.

It was in the same year that Angell and Mead published papers that bore on the situation. Mead's discussion of the definition of the psychical had only an indirect effect, but Angell took the bull by the horns. He called his article "The Relations of Structural and Functional Psychology to Philosophy." It is an excellent paper, illuminating many of the points which we have brought out.

In 1904 Angell's *Psychology* came out. It is a book that illustrates, but does not explicate, the functional point of view. It was, as a general psychology, a tremendous success in the colleges and especially in the normal schools. It went at once into a second edition, and into a fourth edition in 1908. It is never possible to say how much such a book influences thought and how much it merely reflects thought. There is a circular relationship. Certainly it is partly true that Angell's book owed its success to being what American students of psychology wanted.

In 1906 Angell made his presidential address to the American Psychological Association on "The Province of Functional Psychology." This paper is the best and clearest of his expositions. In it he pointed to three conceptions of functional psychology.

1. Functional psychology may be regarded as the "psychology of mental operations in contrast to the psychology of mental elements." This is the view which sets functionalism off as the antithesis to structuralism. In dealing with complete coördinations, the answer to the question "What?" includes the answer to the questions "How?" and "Why?" Structural psychology rigorously distinguished between these questions, but the more thoroughgoing description of functionalism embraces all. The reader (recalling Hume perhaps) will readily see that a complete description of a series of events, when inductively generalized, may be equivalent to a statement of effective causes and appropriate ends.

2. Or functional psychology may be thought of as the "psychology of the fundamental utilities of consciousness," in which mind is "primarily engaged in mediating between the environment and the needs of the organism." The function of the psycho-

logical act is "accommodatory service"; the function of consciousness is "accommodation to the novel," since consciousness wanes in the face of an habitual situation. This view is a narrow view because it limits functional psychology to the consideration of conscious phenomena.

3. However, there is a broader view of functional psychology as psychophysics, that is to say, the psychology of the total mind-body organism. Such a view leaves psychology room for the consideration of well-habituated acts, where consciousness has almost or entirely lapsed.

Angell chose no one of these views, but argued for the interdependence of the three. In a sense his paper may be taken as a credo of functional psychology, and it is a fact that he founded for a time a school. On the other hand, we must note that the existence of a school was not of Angell's overt doing. He held that he was interpreting psychology in general, that it was the structuralists (and here he meant Titchener's paper) who first separated themselves off from the body psychological, that animal psychology and psychopathology already showed the spirit of the functional view, and that any rigid creed for the new science would be suicidal. The previous chapter, as far as American psychology is concerned, developed this thesis; and more recent events have supported it. When behaviorism came along, with assimilative powers equal to those of functionalism, functional psychology gradually faded out of the picture. Angell himself, true to his original pronouncement, made no reference to functional psychology as a particular kind of psychology when he wrote his *Chapters from Modern Psychology* in 1912.

It should not be necessary to point out, in closing this account of functional psychology, that the systematic view had a very definite relation to experimental research. It justified animal psychology, psychopathology, educational psychology, the mental tests, and applied psychology. All these movements would have developed without there being a systematic theory of them; nevertheless the existence of the theory helped to remove constraints from their development and to give them a right of way. New movements are always in part protests; protests are merely negative in form, but they may also possess the positive value of dispensing freedom.

Mental Tests

The history of mental tests can conveniently be excluded from the history of experimental psychology. It is, of course, conventional to speak of experimental psychology as if it were a department quite separate from the mental testing. Unfortunately, there is often drawn from this convention the false conclusion that the mental tests are not experimental. On the contrary, the tests exist and develop only by a reliance on the experimental method. It is, however, of the essence of a test to measure easily and simply, even though there be a lack of precision, each individual that belongs to the large group that may be under investigation in individual psychology, the psychology of which the tests are the primary tool. There is, therefore, a real cleft between mental testing and the experimental psychology of the laboratory; the latter requires initial precision of method, the former does without and makes up for the lack in the statistical *a posteriori* precision of large numbers of cases. Another thing that separates the two psychologies is the fact that the laboratory has advanced farthest in the study of sensory and perceptual mental processes, whereas individual differences exist at the more complex levels where laboratory technique is less adequate. There is, therefore, a distinction both in precision of method and in complexity of subject-matter. The result has been that the two fields have had more or less independent developments; neither has markedly contributed to the other, except at the very beginning when this distinction had not become clear. There is ample ground, therefore, for drawing the conventional line in this book.

On the other hand, the tests were at the beginning interpenetrated with the psychology of the laboratory. They have never been wholly ousted, and there are signs to-day that the estrangement between these two psychologies is diminishing. We must, therefore, say something about the tests for the sake of making complete the account of experimental psychology, even though we fail to deal with the tests for their own sake.

As a matter of fact, we have already said a great deal about the tests. We have seen what Galton did in England and how he established temporarily his Anthropometric Laboratory. We have seen the kind of psychological soil that was prepared for the tests in America, especially by Stanley Hall and Cattell. We have

just seen how this spirit of America was crystallized by Dewey and Angell at Chicago.

The test movement takes its origin from Galton and Binet, unless we are to speak in more general terms and go back to Darwin for the motivating force behind the movement. Galton has the priority over all others and a certain direct effect upon the early phases of the movement. Binet, however, had more to do with the development of the tests than any one other individual. His effect was due partly to the fact that he kept at the topic of the experimental measurement of individual differences from 1886, when he was only twenty-nine years old, until his death in 1911, twenty-five years later. He published in this period more than fifty articles that bear on the general subject. Moreover, the influence of Binet was enhanced by the course of discovery. Binet was seeking to measure the intellectual faculties, the complex mental processes, whereas Galton measured only simple capacities, hoping vainly that they might have some significance for the 'intellect.' Binet was right, Galton was wrong; presently the attention of the mental testers came to be centered almost entirely upon intelligence; and Binet is now one of the best known of psychologists.

The movement in psychological testing is essentially American. This is not to say that Europe has not made important contributions to the development of testing, but merely that the mental test belongs most appropriately to the total picture of American psychology. Germany was dominated by the problem of the analysis of consciousness by way of the experimental method. England had predominantly a philosophical psychology somewhat in the tradition of Brentano, and Galton was an exception. France, in spite of Binet, kept primarily to abnormal psychology. America, however, although in the early days it was consciously patterning itself upon Germany, was actually coming to be guided by its concern with human capacities, that is to say, in the psychological problems of the relation of the individual person to his environment. American psychology can be understood only by referring it to Darwin as well as to Wundt. The mental test is the tool of the psychology of capacity: without regard to the dualism of consciousness and behavior, the test determines the way in which the total psychophysical individual is related to life.

On the other hand, the student of the history of the tests will

find that priority in many new developments or discoveries often belongs to a European. Galton in England and Binet in France are the two pioneers, and later Stern in Germany exerted a very considerable influence. However, where America did not originate, it nevertheless adopted, innovations. Perhaps it is nearer the truth to say that the movement in tests is essentially American because America has always consistently promoted the tests and has carried out the greatest mass of detailed investigation.

Let us look at the development of testing in its beginnings at the end of the nineteenth century. Galton, under the Darwinian influence, had originated them in England, but he had no immediate successors. Kraepelin, from Wundt's laboratory, and his student Oehrn developed tests in connection with psychopathological research in the '90's. Ebbinghaus in 1897 met a request to aid the school authorities at Breslau by the invention of tests, of which the completion test has survived; and Ebbinghaus's original work on the measurement of memory was really more a method of testing memory than it was an analysis of the memorial consciousness. However, these events in Germany were incidental; Stern did not come to the development of his psychology of individual differences until the present century. As history evaluates the growth of the mental tests, the most important work of the '90's was the research in France of Binet and Henri, who were then laying the foundation for tests of intelligence.

In America there was in the '90's nothing comparable in ultimate importance to the work of Binet and Henri, but there was a more general interest in mental tests than abroad. Cattell, who had made his research with Wundt into a study of individual differences, first used the term *mental tests* in 1890. Jastrow published a set of tests in 1890. Boas devised tests, which T. L. Bolton described in 1891. Münsterberg exhibited tests at the World's Fair in 1893, and Gilbert published results of testing school-children in the same year. Various committees on tests were formed but accomplished little. Cattell and Farrand put out their results upon Columbia freshmen in 1896. Even Titchener, by way of the publication of his student, Stella E. Sharp, was drawn in 1899 into contact with the movement. At the turn of the century, Thorndike and Woodworth and the anthropologist, Wissler, entered the field at Columbia, and Helen B. Thompson (H. T. Woolley) worked with tests at Chicago. This list of the

more important studies gives an indication of American activity in mental testing during the closing decade of the last century. Undoubtedly Cattell, the most important factor in the promotion of the tests in America, got his ideas less from Galton than from the American atmosphere, and all this interest would not have occurred had it not belonged to the time and place.

There are two important things to be said about this early American movement. The first is that it failed to have the same effect as the work of Binet and Henri because it was based upon an error of fact. America in the '90's worked principally upon the assumption of Galton that simple tests of sensori-motor character would reveal the most important facts bearing on the adjustment of persons to life, and particularly to life as it occurs in getting an education. Binet and Henri were working toward the same end by way of tests of complex intellectual functions, and, as we have said, they turned out to be right and Galton proved to be wrong. America had to discard the one tradition for the other.

The other important fact about the American testing of the '90's is that it was a product of the newly created laboratories. America thought that it was working in the Wundtian tradition. This fact explains why it turned to the simple tests of sensory and motor capacities, for these functions were measurable by the laboratory technique of the new physiological psychology. There was at that time none of the clear-cut distinction between mental tests and experimental psychology that has since grown up. To this extent, then, the tests are an outgrowth of the psychology of the laboratory.

We cannot, however, in a history of experimental psychology, attempt to follow the development of the mental tests beyond this point of bifurcation, although they have been one of the most important movements in American psychology, were related to functional psychology, and led into behaviorism. In the first decade of the century, the unity of the movement was signalized by G. M. Whipple's publication of his *Manual of Mental and Physical Tests,* a task that he began in 1906 and completed in 1910. Stern in Germany first attacked the problem of individual differences in 1900, and later clarified the concept of intelligence and invented the "mental quotient," a measurement that L. M. Terman later renamed the "intelligence quotient." Binet with

Simon created a scale for testing intelligence in 1905. They revised it in 1908; Goddard also revised it in 1911; and Terman created the "Stanford revision" in 1916. Spearman began his analysis of intellectual activities in 1904 and has continued it ever since. In the second decade, the performance tests and the group tests both came into use, and the extensive employment of intelligence tests in the United States Army during the World War did much to stimulate mental testing and to advertise psychology at large. Then too there have been the questions of the inheritance of intelligence and of feeble-mindedness, originally raised by Galton in 1869, but occupying a great deal of attention in the twentieth century. However, all this is a history in itself of which the present author cannot undertake to treat. In America the mental tests began in the psychological laboratory and then wandered away from it. Presently they may come back.

Animal Psychology

As a progressive field of scientific research, animal psychology, or comparative psychology, as it has been more often called since Romanes coined the phrase, began in England. This phase of its development we have already examined in the chapter on British psychology. Darwin was the Messiah, Romanes was his apostle, and Lloyd Morgan founded the scientific school. The self-conscious movement, however, belongs to America. It was there that animal psychology came to be looked upon as a formal division of psychology, that special laboratories were begun, and that a journal was ultimately founded. This American movement is, therefore, very important in the history of American psychology, even though a great part of the research that supported it was accomplished in Europe. It was this self-conscious child of the American psychology of function and capacity that presently as behaviorism tried to eat up its parent, with what success we shall see in the next chapter.

Our survey of British psychology showed us that Darwin's theory of evolution required the establishment of continuity between man and the animals, and that it indicated as a special necessity the establishment of mind, and, in as far as possible, a quasi-human mind, in animals, for the greatest discontinuity would appear if man had mind and soul while animals lacked both.

Romanes, in seeking to establish evolutionary continuity, fell into the anthropomorphic error of interpreting as much mind in animals as observation of their behavior would permit, and he committed the further mistake of admitting as evidence the casual observation of persons untrained in scientific work and often prejudiced in favor of the capacities of their pets. Romanes accepted anecdotes as scientific material only under certain rigorous rules, but the anecdotal method nevertheless came into disrepute. Lloyd Morgan led the reaction in England against Romanes's method, and laid down his famous canon of parsimony for the interpretation of mind on the basis of behavior. Loeb, then in Germany, proposed a mechanistic interpretation as applicable in many cases, especially to the lower organisms. The tendency is now to look upon Morgan and Loeb as the founders of animal psychology, and Darwin as the necessary forerunner who prepared the way.

The problems of casual anthropomorphism and of the anecdotal method never loomed large in America. They appeared only as errors to be avoided. On the other hand, the problem of the animal mind or consciousness became acute and remained for a long time the outstanding issue in this field. The issue here was really the issue between biology and psychology. An animal's discriminative behavior is open to experimental observation. How does he react to this situation or to that? This question many biologists and other persons, who because of the nature of their research would be classed as biologists, set about answering. When the textbooks of animal psychology came to be written, fully half of the factual material about different animal forms was drawn from such biological research. However, such study of discriminative behavior, while it yielded a consistent body of scientific fact, left open the question as to whether there is such a thing at all as animal psychology. To determine the response of an animal to a given stimulus or situation, or to find his capacity for response under varying conditions, is to leave unanswered every question as to his mind, as mind was conceived at the end of the last century and the beginning of the present.

The new human psychology was essentially a psychology of consciousness. Introspection, in one form or another, was its primary method. Can animals introspect? If they cannot, perhaps there can be no animal psychology.

The answer to this question was generally met in the way that human psychology avoids solipsism, that is to say, by the argument from analogy. The theory of introspection assumes that every man is his own observer, and that the experimenter collects observations of many such observers and brings them together for his generalizations. Ordinarily no one raises the question of how the observer communicates with the experimenter, for language is held to be adequate. If, however, the problem of communication is forced into consideration, then it can only be said that words derive their community of meaning by analogy. If in a relatively simple situation, as when faced by a color, for example, a subject uses the same form of words that the experimenter would use, the experimenter concludes that the subject's experience is the same as his own. From such a beginning, community of meaning can be established, so that presently a subject can describe an experience that the experimenter does not or cannot share. So it is with behavior that is simpler than the verbal response. The experimenter penetrates the subject's mind by way of analogy with his own. If one dislikes, as Titchener did, this emphasis on behavior in discussing introspection, one can substitute the conception of 'empathy' for analogy. The experimenter feels himself empathically into the subject's mind while observing the subject's behavior, gross or verbal. The two views come to the same thing in the end, and they make possible a kind of animal introspection.

One can understand an animal's consciousness if, in a similar situation, one notes his own consciousness. In brief, if you want to know what the animal experiences, place yourself under the same conditions, and, if you behave the same way, you may know. This formula is a bald statement of the underlying belief of the times, but it is scarcely too strong. We do not need to criticize it, for history itself has shown the extreme view to be unworkable, largely because, with gross differences in structure, human and animal behavior differ so greatly that it is not possible to penetrate the animal's consciousness by way of analogy.

Reduced to its simplest terms, the question of the animal's consciousness becomes the question as to whether or not it has any consciousness at all. It is possible to deny mind to all animals or to ascribe it to all, but the extreme views were difficult to hold because the belief in continuity in the animal scale had been

established. Not many persons at the time of which we are speaking wished to deny consciousness to man. On the other hand, it seemed gratuitous to many to ascribe mind to protozoa. The tendency was, therefore, to attempt to lay down criteria for mind, thus defining its appearance in the ascending scale of organic complexity. There is no doubt that the argument from analogy worked in the determination of these criteria. An animal must be said to have mind when it exhibits the kind of behavior that is characteristically conscious in human beings.

There grew up in this way two schools, the mechanistic school and the psychological school, with Loeb and Lloyd Morgan as their founders. The mechanistic school worked mostly with the simpler organisms, where mechanical principles like the tropism could be regarded as more or less adequate. Since the denial of consciousnesss to man is absurd, most of the mechanistic biologists, if they faced the problem at all, were forced to say where mind first appears, and Loeb, as we have said in another chapter, fixed upon associative memory as the criterion. Thus the mechanists worked from below up. The psychologists worked from above down. In the higher vertebrates, the argument from analogy, a very rigorous and limited kind of anthropomorphism, seemed to yield some results, but became less and less satisfactory as the subject of research descended in the animal series. Jennings forced mind all the way down to the protozoa. The critical point in the series could never be fixed, because, of course, the obvious thing about the series is its continuity. Had the series been discrete, it might have been that the approach from above down would not have overlapped the approach from below up, that a limit for mind would have been fixed upon, and that there would have been no conflict.

The mechanistic case was naturally upheld by many, though not all, of the biologists, and it was strengthened by the great mass of valuable research that was done along these lines. *Jacques Loeb* (1859-1924) is regarded as the leader of this movement. He was a German zoölogist and general physiologist of exceptional brilliance, who spent most of his productive life in America, and thus brought his influence geographically close to the American comparative psychologists. From 1891 until his death he was successively at Bryn Mawr College, the University of Chicago, the University of California, and the Rockefeller Insti-

tute in New York. His application of the tropistic theory to animals was first put forth in a paper in 1890. The conception of the tropism as applied to the behavior of plants goes back as far as 1835 (de Candolle). Max Verworn is said to have anticipated Loeb's extension of the mechanical mode of explanation to animal behavior. Loeb, however, is as much the founder of the movement as Wundt was the founder of physiological psychology. His research and publications strengthened the view. His *Comparative Psychology and Physiology of the Brain* (1899) not only reinforced the argument, but served to keep the issue alive as psychological. Later books, like *The Mechanistic Conception of Life* (1912) and *Forced Movements, Tropisms, and Animal Conduct* (1918) represent the cumulative effect of the school. Quite early other Germans came to his support, among whom the most notable are Th. Beer, A. Bethe, and J. von Uexküll. These three men in 1899 published a joint paper in which they proposed to discard all psychological terms like sensation, memory, and learning, and to substitute such 'objective' terms as *reception* for *sensation, reflex* for *fixed movement, 'antiklise'* for *modifiable movement,* and *resonance* for *memory* or any dependence of behavior upon past stimulation. In America the tropisms receive a great deal of attention at the hands of biologists. S. J. Holmes and S. O. Mast have contributed perhaps most notably to this literature, especially to the subject of the response of organisms to light.

It is plain that the motive back of the mechanistic school is the scientific faith in determinism. To the physiologist and zoölogist, mind seems to include a certain degree of indeterminateness, and of course they are right if they mean that for the mind, control is less precise, variability is greater, and the working out of causal relationships is less sure. Reliance on the older physical sciences gives a sense of security. The culmination of this ideal is found to-day in the subject-matter that is called the physiology of conduct.

On the other hand, not all the biologists took the mechanistic view. *H. S. Jennings* (1868-), at Hopkins since 1906, quite early (1899) undertook to study the behavior of protozoa more psychologically. The thesis to which Jennings has held for years is that the behavior of the simplest organisms is not to be explained by simple physico-chemical reactions of the sort that

were generally held to underlie a tropism. The reactions of these animals are too variable, too readily modified. Now it is just this modifiability that creates the suspicion of mind, not because it is indeterminate, but because it makes possible a wide variety of adaptive responses of the sort that are associated with consciousness in man. In appreciating the significance of this argument, the reader needs to recall the position of the functional psychologists who thought of mind as an organ of adaptation. Jennings's observations of protozoa, therefore, gave the psychologists an opportunity to argue that mind extends to the bottom of the animal scale.

At this point the psychological argument was really a little stronger than the mechanistic one. The mechanists could not deny consciousness to man, and had to fix an arbitrary point in the evolutionary scale for the appearance of mind, a view that was inconsistent with the evident continuity of the scale. The psychologists, on the other hand, came by way of such observations as Jennings's to the view that mind and animal life were coextensive, and so reached a conclusion consistent with evolutionary continuity. They also received some support from studies of the 'social' instincts by biologists. For instance, the work of W. M. Wheeler on ants has, in spite of the fixity of the behavior of these animals, given a picture of their life which is quite favorable to description in the terms of human analogy.

With the biologists divided on the question of attributing consciousness to all animals, and with almost no biologists denying consciousness to the higher evolutionary forms, it is no wonder that animal psychologists came into being and flourished. In America, while deriving much of its support from biology, the movement was distinctly one within psychology. The records seem to show that research laboratories for comparative psychology were established at Clark, Harvard, and Chicago in 1899-1903, and that courses in comparative psychology were offered at these institutions at the same time. By 1910 there were at least eight laboratories of comparative psychology established in the United States, and at least twice as many universities offering courses in comparative psychology. In 1911 the *Journal of Animal Behavior* was begun with an editorial board of psychologists and biologists. Shortly after, however, the coming of behaviorism changed the complexion of American psychology and obliterated

the line between research upon animals and research upon man. *Comparative psychology*, which in thirty years of use had become a synonym for animal psychology, now took on the new meaning of general physiological psychology, animal or human. The *Journal of Animal Behavior* was merged with *Psychobiology* in 1921 to form the *Journal of Comparative Psychology*, in which almost any sort of physiological psychology found place. There are now three times as many research laboratories of comparative psychology in America as there were in 1910, and more than twice as many formal courses. Our present concern is, however, mostly with the animal psychology of the days before behaviorism.

The experimental movement in comparative psychology began with the classical experiments of *E. L. Thorndike* (1874-) in 1898. It was Thorndike who first brought certain higher animal forms into the laboratory and subjected them to experimentation with special apparatus. His study was given the title: *Animal Intelligence, An Experimental Study of Associative Processes in Animals*. He invented the puzzle-box, a box from which a confined animal can escape by operating in a particular way certain catches or other devices connected to the fastenings. He used these puzzle-boxes with cats and dogs. He also worked with chicks, studying their learning in getting out of simple pens, constructed of books standing on end and in part resembling very elementary mazes. The work was begun at Harvard, but the greater part of it was conducted at Columbia in Cattell's laboratory.

Thorndike's conclusions were that these animals show in their learning no evidence of inferential reasoning or what we should nowadays call 'insight,' but learn simply by the chance formations of associations in their random experience. Two years later this type of learning was called by Lloyd Morgan learning by "trial and error." Thorndike also found no evidence of learning by imitation; one animal could not profit by watching another who had learned the trick. He concluded, therefore, that these animals learn by the simple formation of associations, but that they have little or no command of 'free images' with which the rational process in man goes on. Thus, too, they lack memory in the sense of conscious recall in imagery, although of course the associations when formed are relatively permanent, and animals have memory in this sense. What is true of dogs, cats, and chicks, Thorndike

argued, ought to be true of all animals below the primates until the contrary is shown, and he thus drew a broad inference from his results about the limitation of the animal mind. Having denied imaginal representation to animals, he was left with a puzzle as to how it came about that the correct chance associations were eventually retained and the incorrect ones lost. To explain this fact he invented his well-known theory of the retroactive effect of pleasure upon the formation of associations, a principle which he later called the "Law of Effect." The pleasure of success comes of course after the correct associations have occurred and when, on Thorndike's view, the acts leading up to success can no longer be conscious, since the animal lacks the necessary imagery. He supposed, however, that the pleasure acts retroactively to stamp in the immediately preceding associations, and thus formulated his law of learning.

A few years later Thorndike published a monograph on the mental life of monkeys, in which he investigated primarily the problem of imitation, and concluded that in general monkeys show little more capacity in this regard than do dogs and cats.

Thorndike's importance at the beginning of the new movement resulted not only from his initiation of the laboratory experiment upon mammals, but also from the challenging nature of his negative conclusions. The anthropomorphism of Romanes had been checked by Morgan and Loeb. Morgan, however, had only counseled parsimony without prescribing a positive method. Loeb had not disproved consciousness, but had proposed to get along without it wherever possible. He did not touch the problem of the higher vertebrates. Common sense is seldom consistent. It could agree with Descartes that animals have no souls and yet ascribe human intelligence to a pet dog. Thus Thorndike's conclusions were shocking. They also brought out the criticism that the situations of the laboratory are artificial and not therefore tests of normal intelligence—a criticism that we can afford to ignore. However, they stimulated a great deal of research. In 1902 A. J. Kinnaman found some evidence for imitation in monkeys, and in 1907 L. W. Cole appeared to demonstrate imitation in raccoons. Nevertheless, the question as to whether the mental life of animals is a matter of mere mechanical, imageless association was mooted for two decades, with opinion inclined to favor the belief in a mental hiatus between man and all other animal forms. Only

recently has research on the anthropoid apes by Köhler and by Yerkes tended to swing the pendulum in the other direction, to indicate that the apes at least may learn suddenly by 'insight,' and to eliminate the missing link in the mental series.

Before 1898 there had been hardly any American papers published in comparative psychology. Immediately afterward there began a steady flow. One of the more important of these was W. S. Small's study of the white rat at Clark. In this investigation Small hit upon the labyrinth or maze method, a procedure suggested by the natural habits of the rat. He reproduced the Hampton Court maze and studied the learning of rats in it. This is really the beginning (1900) of the method of the maze that has ever since had such extensive use in comparative psychology, for Thorndike's maze-like pens for chicks are hardly to be considered as mazes.

R. M. Yerkes (1876-) began his research in animal psychology about 1900. He may be regarded as the leader of the American movement in animal psychology because of the volume of his work, his persistence in this field, and the way in which he threw his influence toward an organized movement. It might almost be said of his research that he has climbed steadily up the evolutionary scale, for from 1900 until the present time he has worked successively on various lower animal forms, then on the crab, the turtle, the frog, the dancing mouse, the rat, the worm, the crow, the dove, the pig, the monkey, and finally the anthropoid apes. Yerkes has also made notable contributions to method. The most important are his study with Watson of methods for investigating vision in animals (1911) and his development of the method of multiple choice. For visual work with animals Yerkes and Watson devised an elaborate apparatus for the use of monochromatic light. The colors of such objects as colored papers depend upon the properties of the stimulated retina, and to use such objects in studying animal vision is likely to result in a kind of anthropomorphism. One works more surely by determining the discriminative responses of animals to different wave-lengths of light without regard to the question of how these appear to the human eye. The method of multiple choice was first used by G. V. Hamilton while he was at Harvard in contact with Yerkes, but Yerkes has since been its chief sponsor. It is a test of 'intelligence' or generalizing ability, for it aims to discover how compli-

cated a rule of selection can be learned by an animal or a human subject in a discriminative response. The subject may be required always to choose the right-hand stimulus irrespective of the number of stimuli presented, or the middle stimulus, or for human subjects a very complicated 'key' to the situation may be used.

Yerkes's work on the apes is too recent for evaluation, but it seems to the present author that it is more significant than anything else that he has done. The reaction against Romanes, reinforced by experiments like Thorndike's, tended to establish a belief in a mental missing link between the animals and man. The effect of the work on the apes has been to establish overlapping and hence continuity between the apes and man, and to banish the missing link. It is true that Köhler somewhat stole Yerkes's thunder by his dramatic experiments on chimpanzees at Teneriffe during the World War, but it was Yerkes who anticipated this result in the light of Cole's work on the raccoon (1907), it was Yerkes who had long planned an elaborate project for research on the primates, and it is Yerkes who, since Köhler, has produced the cumulative and detailed evidence that tends to establish the general view.

We have remarked that comparative psychology had come to be nothing other than animal psychology. In 1913 Yerkes made a plea that the term be used in the truly comparative sense for the comparative study of differences among animals (including man) and plants, peoples and groups, children and adults, normal and abnormal persons. This paper reflects his broadening interests at that time. He extended his own research to include abnormal psychology, and the Point Scale for measuring intelligence was one of the results. Thus at the time of the World War he was in a position to assume leadership in the project of selecting recruits for the United States Army by way of the intelligence tests. Afterward he supported various projects that had to do with racial psychology. In general, however, Yerkes did not change the accepted meaning of the phrase *comparative psychology*.

John B. Watson (1878-) entered the movement for animal psychology at Chicago about 1903, when the Chicago laboratory for comparative psychology is said to have been begun. His early classic paper (1907) is a study of the white rat in the maze. By eliminating the functioning of one sense-department after another, Watson concluded that the rat's accurate running of a learned

maze is dependent almost entirely upon kinesthetic 'memory,' since kinesthesis was the residual sense that he could not eliminate. The primary effect of this study was to show, still more than Thorndike had done, the danger of inferring the nature of animal mind from the human analogy. We can also see in it the anticipation of Watson's stand for behaviorism. If the rat can get along in adjustment to a complicated situation with kinesthetic processes alone, then the special senses assume less importance, and the muscular mechanism with its afferent cues and its efferent control becomes very important. If behaviorism, as some have said, is a psychology of 'muscle twitches,' it may have been born in this rodent maze. We can also see how later Watson could be brought to make all imagery muscular, and, therefore, thought laryngeal and muscular. The fact of imagery was one of the main reliances of the introspectionists, but kinesthetic imagery can seldom be positively distinguished from sensation.

Watson's later contributions to animal psychology have been important, but his most significant act was, of course, his extension of the 'objective' methods of animal psychology to man and his establishment of behaviorism. To this topic we shall come in the next chapter.

One very important development in animal psychology was the bringing together of the method of investigating discrimination and learning in animals and the method of extirpation in the study of the functions of different areas of the cerebrum. To *Shepherd Ivory Franz* (1874-) belongs the credit for this new and fruitful type of study. The reader will recall that Flourens (*ca.* 1824) was the pioneer in this work, and that the work of Fritsch and Hitzig (1870) began a great quantity of research on the cerebral localization of motor functions. The development of a technique for testing sensory discrimination now made possible the study of sensory centers in the brain, and the learning methods made it possible to examine the brain for association areas. Franz began this work in 1902, and during the next fifteen years published, beside minor articles, six important papers. On the positive side he was able to establish experimentally some relations between the frontal lobes and the acquisition and retention of habits, and between the occipital lobes and visual functions. These relationships had, of course, been

already accepted on the basis of clinical evidence. The *visual areas* and the *association areas* were usual terms for these cerebral regions. However, Franz's work also had an important negative bearing. It became clear that the cerebral 'areas' or 'centers' were by no means sharply defined, that even their approximate locations were subject to variability, and that destruction of an area, while it might abolish some past acquisition, nevertheless did not in general interfere with the function. Destruction of part of the frontal lobes might lead to the loss of some habits, especially those more recently acquired, but did not prevent their reacquisition. All this was very startling to the psychophysiologists who had, since Fritsch and Hitzig, accepted the "new phrenology," as Franz now called the theory of exact localization. In fact, Franz's work represents the beginning of a swing of the pendulum back to Flourens, who refused to accept exact localization of function within any one of the main parts of the brain, and who held that each part, besides having an indefinitely localized *action propre,* also contributed to the *action commune* of the brain as a whole.

Franz's work along these lines has since been taken up by *K. S. Lashley* (1890-), who, while a student at Hopkins, worked with Franz in Washington. Lashley, in equally able work, has found that many different regions of the brain are 'equipotential' for certain functions that were originally assigned to a limited region and that no small part of the brain is essential for any single function. To some extent intelligence would seem to be decreased in proportion to the total amount of cerebral destruction. All this work of Franz's and Lashley's is extremely important, and perhaps stands out more than anything else in animal psychology, because, although performed on monkeys and rats as subjects, its significance is not comparative but general. It seems to represent both a fundamental advance in the psychophysiology of the brain and a return to the position of Flourens a century before. The next step is quite unpredictable.

Beside Thorndike, Yerkes, Watson, and Franz, the history of animal psychology in America must mention *Margaret F. Washburn* (1871-). Not only has she contributed research to the movement, but she has also been its secretary and encyclopedist. In 1908 she published *The Animal Mind,* a very thorough handbook of all the research in the field, and this book was revised

in 1917 and again in 1926. A movement in a field of research can hardly be said to have passed adolescence until there is a compendium that represents it, and Washburn's book constitutes just such a symbol of self-conscious unity and also a history of the movement.

Another important book was C. S. Sherrington's *Integrative Action of the Nervous System,* first published in 1906. Sherrington, the distinguished British physiologist, gave in this book an account of the mechanisms of nervous action in relation to the organism taken as a whole. Such a perspective is essentially that of physiological psychology, and the line between physiological psychology and animal psychology was never distinct, as Franz's work indicates. Nowadays such physiologists as C. J. Herrick and C. M. Child, both of Chicago, deal similarly with the neurology and physiology of behavior.

The great body of animal psychology consists of a large mass of facts about different animal forms, ordered to some extent in accordance with the evolutionary scheme. Since the available number of forms is legion, there would seem to be no end to this process of amassing facts. However, the evolutionary setting has tended to make investigators select cases from representative parts of the scale and to study them in respect of their maximal capacities in answer to the question: How much mind can be found at this point?

Almost all of this work falls under the headings of sensory discrimination and of modification by experience. Discrimination is divided as to sense-department or quality or in relation to spatial discrimination. The immediate data are discriminatory responses to controlled situations, but the more psychological investigators, even after the rise of behaviorism, have to a considerable extent been influenced by the problems of perception as they have arisen in human introspective psychology. The other dominant problem, that of modifiability of response by experience, was at first dictated by the effort to find criteria of mind; however, with the general admission that mind must be regarded as existing throughout the animal kingdom if present in it at any point, this question of modification has persisted because the amount of modifiability seems to be in a sense a measure of the degree of mind.

For this reason, one finds running through the history of ani-

mal psychology a single consistent interest which gives unity to the entire subject-matter. The original question that Darwinism raised is still the principal question: How much mind here, and there? Romanes answered the question in favor of the animals. Lloyd Morgan was more conservative. Loeb was dubious and actually negative. He influenced animal psychology greatly, but not enough to cause its suicide. Thorndike's results were more pessimistic than Morgan's conclusions, but there were optimists who still believed that the animals might be brighter than Thorndike made them out to be. During the first decade of the century there was, among the psychological investigators, much discussion about free images. Thorndike denied free images in the associative processes of his animals. If the free images go, reasoning, inference, and inferential imitation all disappear also. Hence there was a great deal of work on learning, in order to determine the nature of association, and on imitation, in order to test the possibility of functional images. There arose the possibility that all animals but man might lack free images; that the mental scale is thus discontinuous; and that there is for this reason actually a mental missing link, for the presence or absence of images is extremely important in determining the make-up of the mental life. On the other hand, there remained considerable doubt about monkeys and raccoons. If raccoons turned out to have imagery, it would of course seem probable that other mammals ought to be as well endowed, in spite of negative results on dogs and cats. The anthropoid apes still represent an uncompleted chapter in comparative psychology.

In the second decade of the century the rise of behaviorism tended to make the overt discussion of images unpopular, but the problem was not altered with the terminology. The method of multiple choice was an attempt to find a means for determining the capacity of animals in such mental operations as could be most readily performed imaginally and inferentially by human beings. W. S. Hunter about the same time (1912) employed the method of the delayed reaction for the same reasons. The apes came under investigation by Köhler and proved to be almost human. They were the missing link. In general the picture of animal intelligence was improving. The pig claimed a remote community with man on the grounds of his ability to solve more than the very simplest problems in multiple choice.

The last decade has seen more research upon the primates, and there are signs that the original problem of animal psychology is approaching its general answer, and that the attention of investigators is turning to the analysis of particular kinds of animal behavior, to individual differences, and to the problems of general psychology with some animal simply playing the rôle of the representative of mind.

Experimental Psychology

It is in accordance with the spirit of American psychology, as it came to a systematic focus in functional psychology, that mental tests, animal psychology and physiological psychology should all develop and thrive. The first two are characteristic American movements. On the other hand, it must not be supposed that experimental human psychology of the normal mind had no place in America outside of Titchener's laboratory and its satellites. Work of this sort went on in almost all the laboratories continuously, affected on the one hand by the American interest in function and practice, and on the other by the research in Germany. However, it does not seem to the author that American experimental psychology constitutes a separate American movement in the same degree as do the mental tests and animal psychology. For this reason we shall have to content ourselves with brief comment upon it in a later chapter, which surveys the researches of the different decades without regard to geographical distinctions.

Notes

Functional Psychology

For John Dewey's early views, see his *Psychology*, 1886, 3d ed., 1891. At this time he wrote also The psychological standpoint, *Mind*, 11, 1886, 1-19; and Psychology as philosophic method, *ibid.*, 153-173.

Dewey's classic paper is The reflex arc concept in psychology, *Psychol. Rev.*, 3, 1896, 357-370. His paper on educational psychology is Psychology and social practice, *ibid.*, 7, 1900, 105-124. For his modern views, see his *Human Nature and Conduct*, 1922.

James Rowland Angell (1869-) worked at Harvard in James's day (1892), studied abroad without staying long enough in any one place to receive the German impress, and then, after a year at Minnesota (1893), came to Chicago (1894-1920). The coming into being of a Chicago school, when Angell, the antithesis of Wundt, sought no school, is partly the result of Angell's magnetic personality, which inspired his students and gave them a group consciousness. It may have been this same gift that drew Angell off into administrative

work at Chicago, and then made him, first the president of the Carnegie Corporation, and then president of Yale University (1921-).

Angell's paper on reaction times with A. W. Moore must have been almost his first published experimental research: Angell and Moore, *Psychol. Rev.*, 3, 1896, 245-258. His earlier formal discussion of functional psychology is The relations of structural and functional psychology to philosophy, *Univ. Chicago Decennial Publ.*, Ser. I, vol. 3, pt. 2, 1903, 55-73; also separate; also reprinted in *Philos. Rev.*, 12, 1903, 243-271. G. H. Mead's contribution follows Angell's in the *Chicago Decennial Publications*, The definition of the psychical, *op. cit.*, 77-112; also separate. Angell's presidential address is The province of functional psychology, *Psychol. Rev.*, 14, 1907, 61-91. It is to this clear and lucid exposition of the matter that the reader should be primarily referred.

Angell's *Psychology*, 1904, 4th ed., 1908, is an illustration of the way the functional point of view works in the writing of a text. His *Introduction to Psychology*, 1918, replaced it later. The *Chapters from Modern Psychology*, 1912, while colored by their author's outlook, show more than anything else the catholicity of his mind.

Under Angell the University of Chicago became one of the most important schools for American psychologists. In mere quantity of psychologists trained it ranks second only to Columbia, and well in advance of Cornell and Harvard. The relation of psychology to education was naturally close, especially at first before either division had grown too strong for an alliance. Dewey was the director of the School of Education (1902-1904); then after an interval, C. H. Judd, the psychologist who succeeded Scripture at Yale, was director (1909-). Animal psychology was recognized early in the Chicago laboratory. Mead gave a course in it in 1899, the year that Clark first offered a course under Sanford. The animal research was organized under J. B. Watson in 1903, a laboratory venture anticipated presumably only by Clark and Harvard. Almost half a hundred psychologists took their doctorates at Chicago while Angell was head of the department. Of these the most distinguished (those starred in *American Men of Science*, 1927) are, in the order in which they took their degrees: Helen T. Woolley, known for her work in child psychology and now at Teachers College, Columbia; J. B. Watson, the founder of behaviorism; H. A. Carr, the comparative psychologist at Chicago; June E. Downey, the author of the tests of personality; Joseph Peterson, best known for his work in acoustics and in individual psychology; W. V. Bingham, the industrial psychologist; W. S. Hunter, G. Stanley Hall professor of genetic (animal) psychology at Clark; and L. L. Thurstone, at Chicago working in the theory of statistical psychology. The tendency of the Chicago laboratory is plain in its graduates. They deviate toward educational or some other sort of applied psychology, or else toward animal psychology. For all of Angell's catholicity, functional psychology seems nevertheless to have had a bias.

The impetus toward making a self-conscious school of functional psychology was really given from without by Titchener's criticism of it. See E. B. Titchener, Postulates of a structural psychology, *Philos. Rev.*, 7, 1898, 449-465; Structural and functional psychology, *ibid.*, 8, 1899, 290-299.

If the contrary fact were not so emphatically established, it would be interesting to wonder why Titchener was not found in the functional camp. He grew up in England at the time when the theory of evolution reigned supreme in almost all advanced thought. He was saturated with English philosophy and English science. All his life he was accustomed to distinguish between biology, the teleological science of life in the Darwinian sense, and

physiology, the experimental study of bodily processes. His early publications were in this sense biological. In his lectures he was fond of this sort of biological illustration and speculation, especially in his asides and digressions. Yet in psychology he remained the morphologist for all his appreciation of Darwin. He ought by early training to have been the leader of functional psychology, whereas his only contribution to it was to emphasize it by opposing it. Perhaps if Oxford had received him back from Wundt, instead of forcing him into an isolation in the "colonies," he would have gone the way of British and American psychology. His loyalty to Wundt would seem to have had in it an element of resentment against England, all the stronger because of his stanch loyalty to Britain.

It is pertinent to remark here that the word *function* in the sense of 'mental function' has a long history and was derived by psychologists from phrenology (cf. chap. 3). See K. M. Dallenbach, *Amer. J. Psychol.*, 26, 1915, 473-484. Even the psychologists who opposed phrenology could gain from it the notion of an analysis of the total personality into functions. Moreover, phrenology is an ancestor of the mental tests, which at first sought measures of the different mental functions. The relationship of functional psychology and the tests in the twentieth century is quite as real as this common ancestry suggests.

Mental Tests

There is available a very excellent history of the movement in mental tests, one which stresses the intelligence tests and carries the account from ancient times to about 1910. It is: Joseph Peterson, *Early Conceptions and Tests of Intelligence*, 1925. It contains a list of twenty-one standard books on tests and also a bibliography of 232 titles. No other source is nearly so complete or thorough. An excellent account of modern methods for testing intelligence, with some indication of the historical background is to be found in W. Stern and O. Wiegmann, *Methodsammlung zur Intelligenzprüfung, Beihefte z. Zsch. f. angew. Psychol.*, no. 20, 1920, 3d enlarged ed., 1926. There is an excellent discussion of the history of the measurement of intelligence in G. Murphy, *Historical Introduction to Modern Psychology*, 1929, 347-372.

We cannot here attempt a bibliography. Peterson's list gives most of the books that contain brief historical sections. The author must content himself with listing, without titles, the references to the articles cited in the text:

A. Oehrn, *Experimentelle Studien zur Individualpsychologie*, 1889; reprinted in *Psychol. Arbeiten*, 1, 1895, 92-152.

H. Münsterberg, *Centbl. f. Nervenhk. u. Psychiat.*, 14, 1891, 196-198.

J .Jastrow, *Amer. J. Psychol.*, 4, 1892, 420-427.

T. L. Bolton, *ibid.*, 4, 1892, 362-380.

J. A. Gilbert, *Studies from Yale Psychol. Lab.*, 2, 1894, 40-100.

E. Kraepelin, *Psychol. Arbeiten*, 1, 1895, 1-91.

J. McK. Cattell and L. Farrand, *Psychol. Rev.*, 3, 1896, 618-648.

A. Binet and V. Henri, *L'année psychol.*, 2, 1896, 411-465. This is the crucial article. There are seven articles by Binet and Henri at about this period, and almost half a hundred more by Binet from 1886 to 1911. See the bibliography in Peterson.

H. Ebbinghaus, *Zsch. f. Psychol.*, 13, 1897, 401-459.

S. E. Sharp, *Amer. J. Psychol.*, 10, 1899, 329-391.

W. Stern, *Ueber Psychologie der individuellen Differenzen*, 1900.

E. L. Thorndike, *Psychol. Rev.*, 7, 1900, 466-489, 547-579.

Thorndike and R. S. Woodworth, *ibid.*, 8, 1901, 247-261, 384-395, 553-564.

C. Wissler, *Psychol. Monog.*, 3, 1901, no. 6.
H. B. Thompson, *The Mental Traits of Sex*, 1903.
C. Spearman, *Amer. J. Psychol.*, 15, 1904, 72-101, 201-293.
Binet and Th. Simon, *L'année psychol.*, 11, 1905, 191-366.
Binet and Simon, *L'année psychol.*, 14, 1908, 1-94.
H. H. Goddard, *Training School*, 6, 1910, 146-155.
G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 2d ed., 1914-1915.
W. Stern, *Die differentielle Psychologie*, 1911.
W. Stern, *Die psychologischen Methoden der Intelligenzprüfung*, 1912, Eng. trans., 1914.
L. M. Terman, *The Measurement of Intelligence*, 1916.
Psychological Examining in the United States Army, *Mem. Nat. Acad. Sci.*, 15, 1921.

Animal Psychology

See in general the discussion of animal psychology in England, chap. 19, text and notes.

The reader is fortunate in having for reference M. F. Washburn's *Animal Mind*, 1908, which is available in the third edition of 1926. This edition contains a bibliography of 1,135 titles which must serve in place of more complete notes at this place. The book is both a handbook of results of research and a systematic orientation of the dominating problems of animal psychology. Without any such intention, it is virtually a history of the movement.

Another handbook of animal psychology is J. B. Watson's first book in support of the behavioristic movement: *Behavior, an Introduction to Comparative Psychology*, 1914. A typical handbook in the thoroughgoing German style is G. Kafka, *Einführung in die Tierpsychologie*, I, 1914, or *Handbuch der vergleichenden Psychologie*, I, 1922, 11-44, both of which have large bibliographies.

Beside Washburn on the history of animal psychology, see C. J. Warden, *Psychol. Rev.*, 34, 1927, 56-85, 135-168, reprinted separately as *A Short Outline of Comparative Psychology*, 1927. This covers the entire period from the ancients to the present, and pp. 150-164 of *Psychol. Rev.* is especially relevant to this present chapter. There is also another account by Warden which stresses the recent period more, *Quart. Rev. Biol.* 3, 1928, 486-522. Both the first and last references have bibliographies; the book-reprint has not. On the formal development of laboratories and the introduction of university courses, see Warden and L. H. Warner, *Psychol. Rev.*, 34, 1927, 190-205. Kafka is not satisfactory for history. On the history of tropisms, see Mast, *op. cit. infra*, 1-58.

On many fundamental matters of method, like the difficulties of the anecdotal method, see Washburn, *op. cit.*, chap. 1. On the criteria of mind (and the impossibility of establishing any), see Washburn, chap. 2; cf. H. A. Carr, *Psychol. Rev.*, 34, 1927, 87-106.

Jacques Loeb's original application of the tropistic theory to animals is *Der Heliotropismus der Tiere und seine Uebereinstimmung mit dem Heliotropismus der Pflanzen*, 1890. His greatest influence upon comparative psychology has been exerted through his *Einleitung in die vergleichende Gehirnphysiologie und vergleichende Psychologie*, 1899, Eng. trans., 1900. His more recent important books are *The Mechanistic Conception of Life*, 1912; and *Forced Movements, Tropisms and Animal Conduct*, 1918. See further the twenty references in Washburn, 1926.

For the extreme mechanistic view to which the text refers, see Th. Beer, A. Bethe, and J. von Uexküll, *Biol. Centbl.*, 19, 1899, 517-521, or *Centbl. f. Physiol.*, 13, 1899, 137-141 (the two articles are identical). There is one

other paper, long before behaviorism, that is often cited as upholding the view that an appeal to consciousness in these psychological problems should be avoided, even in the case of man. The paper is J. P. Nuel, *Arch. de psychol.*, 5, 1906, 326-343.

The best approach to H. S. Jennings's more psychological account of the behavior of the lower animal forms is his monograph, *Contributions to the Study of the Behavior of the Lower Organisms*, 1904 and 1906. For some of the references to his early work, see the notes of chap. 19. For a list of twenty-one of his publications that are significant for psychology, see Washburn's bibliography.

Important American contributors to animal psychology, who are not mentioned in the text, are: G. W. and E. G. Peckham (1887-1905) on spiders and wasps, A. M. Field (1901-1905) on ants, E. P. Lyon (1898-1918) on various tropisms, and G. H. Parker (1896-) on many topics, but especially on fishes. See Washburn, 1926, for all these references.

Mast, Holmes, and Wheeler, whom the text mentions, can be represented here only by the citation of a single book for each: S. O. Mast, *Light and the Behavior of Organisms*, 1911; S. J. Holmes, *The Evolution of Animal Intelligence*, 1911; W. M. Wheeler, *Ants, Their Structure, Development and Behavior*, 1910. For many more references to each of these investigators, especially the first two, again see Washburn, 1926.

Edward Lee Thorndike's work in animal psychology occupies only the period of his graduate study at Harvard and with Cattell at Columbia, and the first year or so after he had joined the staff at Teachers College, Columbia, where he still is. It was, however, a natural transition from the study of learning and intelligence in animals to a study of the same functions in children and other subjects of the educational process. His classical study discussed in the text is Animal intelligence, *Psychol. Monog.*, 2, 1898, no. 4. With it should be considered The mental life of monkeys, *ibid.*, 3, 1901, no. 4. Thorndike published three other studies of animal behavior in 1899 (see Washburn). These studies have been reprinted as a book under the title *Animal Intelligence*, 1911. His theory of learning persists as the Law of Effect; cf. his *Educational Psychology*, 1913, I, 172 f., II, 6-16. This theory invites constant criticism because the retroactive effect of pleasure, in stamping in associations that are already past, offends the person who thinks in terms of cause and effect with the conviction that a cause must precede the effect. Of course Thorndike holds this same view of cause: it is only the persisting effects of the past associations that pleasure stamps in. In view of the fact that these associative effects are not open to direct observation except by way of recurrent imagery, and that imagery cannot be examined in animals and is difficult of precise introspective examination in man, the theory remains without disproof.

The learning that Thorndike described in 1898 is learning by 'trial and error,' but this much-used phrase originated with Lloyd Morgan, *Animal Behaviour*, 1900, 139-141. However, Morgan was adapting E. H. Lindley's earlier phrase for the same situation: "sense-trial and error," *Amer. J. Psychol.*, 8, 1897, 478.

Robert M. Yerkes held various positions at Harvard from assistant to assistant professor (1899-1917); then he was engaged in organizing and directing the intelligence testing in the United States Army for the period of the World War (1917-1919); after this he continued his administrative activities along similar lines in the newly organized National Research Council at Washington (1919-1924); and since that time he has been professor of psychology at Yale. He has been the comparative psychologist throughout in the broad sense of this term. In the

first decade at Harvard he was concerned with little more than the animal mind. The period at Yale is the period of his anthropoid research.

See Washburn, 1926, for a list of thirty-four of Yerkes's publications on animal psychology. The paper on methods of testing vision in animals is Yerkes and J. B. Watson, *Behav. Monog.*, 1, 1911, no. 2. The multiple choice method originated with Hamilton, who had worked at Harvard with Yerkes: G. V. Hamilton, *J. Animal Behav.*, 1, 1911, 33-66; but Yerkes and his associates continued the method. Cf. C. A. Coburn and Yerkes, *ibid.*, 5, 1915, 75-114. For some years before the World War, Yerkes was planning a project for a station for research on anthropoid apes, a station which was not to be realized for many years. For his research on the apes, see his *Mental Life of Monkeys and Apes, Behav. Monog.*, 1916, no. 12, and his monographs on the gorilla, *Genetic Psychol. Monog.*, 2, 1927, nos. 1, 2, 6. Interest in anthropoid research was stimulated by Köhler's simultaneous work at the ape station in Teneriffe in 1914 just before the war. W. Köhler first published in 1915, but see his *Intelligenzprüfung an Menschenaffen*, 1917, 2d ed., 1921, Eng. trans., 1925.

For Yerkes's broad definition of comparative psychology, see *J. Philos.*, 10, 1913, 580-582. His excursion into human comparative psychology led to the making of the Point Scale; see, e.g., Yerkes, J. W. Bridges, and R. S. Hardwick, *A Point Scale for Measuring Ability*, 1915. Yerkes was editor-in-chief of the collective effort that resulted in the mammoth report on the army intelligence tests, *Mem. Nat. Acad. Sci.*, 15, 1921.

John B. Watson began his professional career in comparative psychology. His first publication was Animal education, *Univ. Chicago Contrib. to Philos.*, 4, 1903, no. 2. His classical monograph on the white rat is Kinæsthetic and organic sensations: their rôle in the reactions of the white rat to the maze, *Psychol. Monog.*, 8, 1907, no. 2. Both the title and the text of this monograph perform lip-service to the convention of the times that psychology deals with consciousness and that animal behavior is studied for the light that it will throw upon consciousness (cf. pp. 3, 93-96); however, one can see that Watson was already vitally concerned with the purely behavioral approach to his problem. Watson worked on the behavior of monkeys and of terns before he launched the behavioristic movement; see Washburn, 1926, for the references. We return to Watson and behaviorism in the next chapter.

Margaret F. Washburn has been in charge of psychology at Vassar College (1903-) almost since she left Cornell, where she was Titchener's first doctoral student. Her work has by no means been limited to animal psychology, although the great success of *The Animal Mind* has emphasized her attainment in that field.

Physiological Psychology

A full half of Shepherd Ivory Franz's research has lain in psychopathology, but his work on cerebral localization by the experimental method is his most significant contribution. He was Cattell's student at Columbia, but his doctor's dissertation on after-images was so unsatisfactory to him that he determined to keep to the physiological approach and to avoid even the slight introspective coloring that this Columbia study had. After a few years at Dartmouth (1901-1904) and at the McLean Hospital for the Insane (1904-1907), he became psychologist at the Government Hospital for the Insane at Washington (1907-1924). He is now at the University of California at Los Angeles.

The list of his more important papers on cerebral localization is as follows: *Amer. J. Physiol.*, 8, 1902, 1-22 (learning and retention as dependent upon the frontal lobes); *Arch. Psychol.*,

no. 2, 1907 (the same topic); *Amer. J. Physiol.*, 28, 1911, 308-317 (dependence of the visual-motor functions on the lateral portions of the occipital lobes); *Psychol. Monog.*, 13, 1911, no. 4 (dependence of both visual-sensory and visual-motor functions on the occipital lobes); *J. Comp. Neurol.*, 21, 1911, 115-127 (somesthetic sensory functions of the post-central area); *Psychol. Monog.*, 19, 1915, no. 1, 80-161 (variability of the motor centers); with K. S. Lashley, *Psychobiol.*, 1, 1917, 3-18, 71-140 (learning and retention as dependent upon the frontal lobes of the rat). For more general accounts of this work see Franz, *Science*, N.S. 35, 1912, 321-328; *Psychol. Rev.*, 28, 1921, 81-95.

Franz's last monograph was a joint study with K. S. Lashley, who has since continued this type of research with notable success. We may repeat here the references to Lashley's work already given in chap. 4: *Psychobiol.*, 2, 1920, 55-128; *J. Comp. Psychol.*, 1, 1921, 453-468; *Amer. J. Physiol.*, 59, 1922, 44-67; *Brain*, 44, 1921, 255-285; *Arch. Neurol. and Psychiat.*, 12, 1924, 249-276. This is not a complete list, but Lashley's work is too recent for detailed discussion or evaluation. See C. J. Herrick, *Brains of Rats and Men*, 1926, for a book that takes account of much of Lashley's work.

In general the paragraphs on Franz and Lashley should be read primarily in the light of chap. 4, where the early work on localization of function in the brain is discussed.

The remark in the text that physiologists are nowadays concerning themselves with the physiology of conduct should be supported by citation of the recent books of Herrick and Child: C. J. Herrick, *Neurological Foundations of Animal Behavior*, 1924; C. M. Child, *Physiological Foundations of Behavior*, 1924. The numerous publications of the physiologist W. J. Crozier in the field of tropisms also belong to this point of view.

Chapter 22

GESTALT PSYCHOLOGY AND BEHAVIORISM

DURING the last twenty years the 'new' psychology of the end of the nineteenth century has been giving ground to still newer psychologies, of which *Gestalt* psychology in Germany and behaviorism in America are the prominent examples. New movements, as we have repeatedly observed, always begin as protests, and it is this negative aspect of a new movement that is for some time its most obvious characteristic. As protests, *Gestalt* psychology and behaviorism stand upon common ground: they are both reactions away from the old 'new' psychology. On the positive side they are almost antipodal. *Gestalt* psychology cultivates phenomenological description of conscious experience, 'introspection' in the broad sense; behaviorism ignores consciousness. *Gestalt* psychology is friendly to the philosophical approach; behaviorism in general eschews philosophy. Behaviorism focuses attention upon animal psychology; *Gestalt* psychology, while it may include animal investigation, deals primarily with human material, and has been most effective in attacking the problems of perception. Both *Gestalt* psychology and behaviorism have this one positive attribute in common: they have incited valuable and effective experimental research. While perhaps the authors of most experimental investigations would not to-day desire to claim affiliation with either of these schools, still it may be said that there is now very little experimental work of broad scope or great significance that has not in some way been affected by one or the other of these new movements.

When we say that the 'new' psychology of thirty years ago is now the 'old' psychology, the question arises as to what we mean by this old 'new' psychology. In a way the old 'new' psychology is the subject-matter of the present book. It has been primarily experimental psychology. It was originally human

experimental psychology. It equated mind to consciousness, and, when it dealt with behavior, it intended an inference about consciousness. It was always interpenetrated with systematic psychology because of the German tradition that links philosophy with psychology. When it denied philosophy, it did so on philosophical grounds. Internally not only was it dominated by the emphasis upon introspection, but it took its character largely from the analytical theory of introspection: it was elementaristic and associationistic, and sought to account for mind by the combination of sensations, images, feelings, and occasionally other pretenders to elementary status. It held also to the dualism of mind and body, and, since mind seemed to be a sum of elements, it tended to create an elementaristic body to parallel the mental mosaic. This view of mind was the starting point of both *Gestalt* psychology and behaviorism, the psychology against which they reacted, the cause of the repulsion that brought them into being. Just how different components of this repelling force projected the two new psychologies in different directions, we shall see in the succeeding sections.

Gestalt psychology was born in 1912; behaviorism, in 1913. They are almost exactly contemporaneous.

Gestalt Psychology

The 'founder' of *Gestalt* psychology was *Max Wertheimer* (1880-1943), although Köhler and Koffka have been its equally able and more vocal exponents. Wertheimer began his psychological career at Prague, his native city, with Martius. He went thence first to Stumpf at Berlin and then to Külpe at Würzburg, where he took his doctor's degree, *summa cum laude,* in 1904. Afterward he returned to Berlin, and in the summer of 1910 went to Frankfurt, where Schumann, successively Müller's and Stumpf's assistant, had just come. Köhler and Koffka, Stumpf's pupils, were also at Frankfurt at this time and served as the principal observers in Wertheimer's investigation of seen movement, the crucial study that began the entire movement of *Gestalt* psychology. After this Wertheimer returned to Berlin (1922-1929) and then was appointed to Schumann's chair at Frankfurt, when Schumann retired. In general the new movement centered at Berlin, and indeed it was largely an affair of Stumpf's

pupils, although Stumpf himself is not to be included within the 'school.'

Now how did the experimental study of seen movement ever come to inaugurate the school of *Gestalt* psychology? We see in this research of Wertheimer's the way in which a protest, seemingly purely negative in character, can nevertheless acquire a positive value in freeing investigation from limiting constraints that would prevent its realization. Psychology had hitherto not been successful in dealing with the problem of visually perceived movement. It could translate the physical definition of movement into mental terms and say that seen movement is a change of space in time. It could go further and appeal vaguely to fusion or to attention to explain the unitary character of movement; it could call movement a form-quality; or it could reject phenomenal visual movement and attempt to refer the experience to movement of the eyes. Primarily the difficulty lay in the fact that psychologists thought of perceived movement as involving a series of sensations, a view that arises out of sensationistic elementarism. It is true that, in definite systematic discussion, sensations were generally differentiated in respect of quality and not of space; nevertheless the conventional habits of thought included space as a differentia, so that in the formation of a square from four lines, as handled by the older school of *Gestaltqualität*, the *Grundlage* was thought of as four sensations corresponding to the four lines.

Wertheimer's brilliant thought consisted merely in denying the validity of this sensory analysis. Movement can be perceived as such. It is a recognizable phenomenon. It is not perceived as a congeries of sensations. It is in itself an experience. Then why not deal with it as such? In this attack upon the problem, Wertheimer was helped by the facts of stroboscopic movement, now so familiar to all of us in the moving pictures. Movement is perceived when a simple object, like a line, is discretely displaced without actual movement. In this situation, which formed the primary basis for his experiments, there is no moving stimulus to the perception of movement, and the temptation to think of movement as a spatial series of sensations becomes less, since there is no corresponding physical series.

What, then, is movement? It is a phenomenon. Is it a sensation? Wertheimer realized that the concept of sensation as an atomic unit had led to the entire difficulty in meeting the problem.

He knew that if he called movement a sensation, he would be asked to define its quality and intensity and perhaps other attributes of it, in accordance with orthodox practice. Plainly movement is a phenomenon and it is not necessarily this kind of sensation. Hence he gave the experience a name of its own; he called it the *phi*-phenomenon, and thus avoided all the traditional systematic constraints that were attached to the concept of sensation. In so doing he was advocating an experimental *phenomenology*, which underlies the work of all *Gestalt* psychology.

Throughout Wertheimer's paper, which was published in 1912, there are many indications of what *Gestalt* psychology was to be. Movement furnished the text for the entire exegesis. There is no one-to-one relation between stimulus and phenomenon; the "constancy hypothesis" does not hold. Conventional analysis is ineffective: movement is to be regarded as a whole, a *Gestalt*, and not as a "bundle" of sensations. For the complete understanding of perceived movement, we have to consider the phenomenon and the stimulus as a total system: the larger *Gestalt* is psychophysical. Vague concepts like attention are useless in the explanation of such a phenomenon; they form only cloaks for ignorance. Wertheimer's sanction for breaking the traditional bonds of systematic orthodoxy lay, of course, in the positive nature of his results. He contributed the first of what is now a large body of new facts about perceived movement: psychology could get ahead when unencumbered by sensationistic atomism.

When Wertheimer had finished his experimentation at Frankfurt, he discussed the meaning of his method and results with Köhler and Koffka, and the three of them saw then (1911) that they had in hand a new approach to the problems of experimental psychology. We can outline briefly the subsequent history of this movement before we attempt to characterize it.

Kurt Koffka (1886-) went from Frankfurt to Giessen (1912-1927), where he was soon engaged in the experimental work that resulted in his *Beiträge zur Psychologie der Gestalt* (1913-1919), papers which contribute to the early phases of this movement. *Wolfgang Köhler* (1887-), as we have seen in the last chapter, went to Teneriffe (1913-1917) to work on the psychology of apes. His resultant book illustrates many of the principles of *Gestalt*. For instance, Köhler, freed from conventions, was able to introduce the new term *insight* in the explana-

tion of the intelligent behavior of the apes, a term that has since been adopted by many psychologists both within and without the school of *Gestalt*.

After the World War, Köhler published *Die physischen Gestalten in Ruhe und im stationären Zustand* (1920). This book represents the maturation of *Gestalt* psychology as a definite movement, and is an attempt to demonstrate the appropriateness of the *Gestalttheorie* to the problems of biology and physics. *Gestaltpsychologie* had broadened out into *Gestalttheorie* by this time: it had become a more general principle within science at large. It is doubtful if physics, for all that it is sometimes said to deal with atoms and electrons, needed the liberating help of the *Gestalt* theory. It was progressing very nicely with all sorts of other analytical units than atoms and electrons: forces, stresses, masses, centers of gravity, specific heats, and what not; and it dealt readily with total systems whenever it found the need, as Köhler's examples, from which he points a moral to psychology, readily show. Biology perhaps needed to have the importance of the total organism emphasized. On the whole, Köhler's book was of most importance to psychology, which always goes to the older sciences for its sanctions, in showing that 'mental chemistry' had outdone even the physical chemistry in analysis, and that associationistic sensationism was more elementaristic than the physiology which it had been supposed to match. The book was a brilliant exposition of these points of view, and undoubtedly helped to get Köhler his chair at Berlin in 1921.

In 1921 Koffka published *Die Grundlagen der psychischen Entwicklung*, a study of the growth of the mind from the point of view of *Gestalt* psychology, and a book which further presents many of the general principles of *Gestalt*. In the same year E. Rubin's work on visual perception, the study that establishes the concepts of figure, ground, and contour as phenomenological terms for description of these perceptions, was translated from the Danish into German, and lent support to *Gestalt* psychology, although it was a product of G. E. Müller's laboratory and Rubin had no such polemical intention.

It was also in 1921 that the *Psychologische Forschung* was begun as an organ of the new school. Koffka, Köhler, and Wertheimer were, of course, editors, and K. Goldstein and H. Gruhle were added to the group. The file of this journal now constitutes

the principle objective justification of the new movement. Whatever the systematic beliefs of psychologists may be, there are few who do not find in the *Psychologische Forschung* interesting and important experimental research, the larger portion of which has this movement for its background and stimulus.

In the first volume of the *Forschung,* Wertheimer published the declaration of independence of *Gestalt* psychology, a categorical formulation of what it was not. There is no doubt that Wertheimer has personally done a great deal to guide the movement which he originated. However, he has crossed the threshold of the printed page less readily than have Köhler and Koffka, who have been the spokesmen and missionaries. In fact, beginning in 1924, both Koffka and Köhler visited America as its invited guests and did much to spread the new gospel abroad. Koffka has even now become a resident member of the American psychological community (at Smith College; 1927-).

With *Gestalt* psychology coming directly out of the air in this fashion, it is obvious that it was relatively indifferent to its historical antecedents. Two of these antecedents are, however, too important to ignore. They are the psychology of *Gestaltqualität* (form-quality) and phenomenology.

The doctrine of *Gestaltqualität,* as we have seen, represented a similar protest against the "bundle hypothesis" for the explanation of *Gestalten.* This school, however, made the mistake of considering the form-quality as another dependent element of a higher order; it failed to free itself of the elementarism which had created the difficulty that it was trying to overcome. Thus in one way the two schools of *Gestalt* are antipodal, whereas in another the earlier is really the ancestor of the later. It is true that in general the *Gestalt* psychologists do not recognize this ancestry, and it would be mere speculation to wonder whether Wertheimer at Prague absorbed enough of the Austrian psychology to give it a new form at Frankfurt.

Phenomenology, as we have also seen, came into vogue, as a word, by way of Husserl. Stumpf got it from Husserl and made something else of it; he practically equated it to what at the time made up the larger part of psychology. While Külpe never espoused a psychological phenomenology, he was motivated by the same desire for freedom from the conventional constraints, a freedom that experimental phenomenology gives. He was still

under the influence of elementarism when he created the imageless thought as a new element; he had freed himself from elementarism when, like Stumpf, he adopted the psychical functions. After Prague Wertheimer worked with both Stumpf and Külpe. *Gestalt* psychology is mostly the product of Stumpf's pupils. It requires very little imagination to see how the phenomenological trend aided in the creation of *Gestalt* psychology.

Thus far the voice of *Gestalt* psychology has been most explicit in protest, and we shall, therefore, find it appropriate first to examine this negative aspect of the new movement. There are at least four major complaints that it makes. They were all expressed by Wertheimer in 1921 and repeated by Koffka in 1922.

1. *Gestalt* psychology, in the first place, protests against analysis into elements or, as it has aptly anathematized the older theory, the "bundle hypothesis." This objection is, of course, the essence of *Gestalt* theory. Sensations, images, and feelings are but artificial elements of experience, and to put emphasis invariably upon this kind of analysis is to create an equally artificial psychology of compounds, mosaics, or "bundles." Moreover, such bundles, as Wertheimer said, lack any intelligible principles of connection: they are mere *Und-Verbindungen* ("and-connections").

There can be no doubt that, in the early days of *Gestalt* psychology, this protest against analysis was exaggerated. The new school gave the impression that it would abolish all analysis in psychology, and the objection was at once raised that scientific method is essentially analytic, that any description must deal with elements and their relations. Köhler (1926) was finally forced to meet this objection by defining the type of analysis that *Gestalt* psychology admits as "differential analysis"—that is to say, the sort of unreal but useful analysis that is used in differential calculus, where convenient elements are assumed for the purposes of work and disappear again in the final result. R. M. Ogden subsequently made it quite clear that the protest of *Gestalt* psychology is not directed against analysis as such, but against analysis into real elements.

It is hard to evaluate this protest. It was, of course, anything but new, although the plaintiffs made little effort to show the antiquity of the objection. We have studied the same complaint in James (1890) and in Dewey (1896). We have seen how Wundt was conscious of the difficulty and tried to meet it, although

there can be no doubt that Wundt was the chief culprit against whom this cry was raised. Still earlier, John Stuart Mill for the same reasons had substituted his mental chemistry for James Mill's mental mechanics—the *Und-Verbindungen* of James Mill. Historical sophistication is not, however, the starting point of a new movement; it is probable that too sophisticated a movement would never move.

Nor must it be thought that orthodox psychology had ever taken its own credo about elements any too seriously. It was accustomed to do homage to elements and their attributes, as it were, on Sunday, and then to play with what were really *Gestalten* all through the week. The strength of *Gestalt* psychology in this regard was that it asked every one to do what he had for the most part been doing, and that it wished, therefore, to affirm the psychology of actual research rather than to remake it. There is hardly any research that has been accomplished under the name of *Gestalt* psychology that might not have been done under the older auspices. On the other hand, the reader must not make too much of this fact, for it is doubtful if a great deal of the work would have been done except for the new movement. Many good ideas doubtless never ventured into the laboratory in the old days because they were indecorous. *Gestalt* psychology legitimized common practice and removed a sense of guilt from many investigations.

2. The second protest of *Gestalt* psychology was against the "constancy hypothesis"—that is to say, the belief in a one-to-one fixed relation between the stimulus and the phenomenon. Here the situation resembles the preceding case of analysis. The constancy hypothesis lay at the back of the minds of psychologists, and affected research in subtle ways. If a square stimulus has four sides, does not its perception consist of four sensations? Must there not be a nerve fiber for every tone, or, if we go back far enough in history, a brain cell for every idea?

However, it is not true that the constancy hypothesis was ordinarily effective. The strength of this rejoinder to the protest lies in the very sort of psychology that was supposed to employ the hypothesis, for it is this kind of psychology that is always seeking to state the relationships between stimulus and sensation. If there were constancy of relationships, there would be but little research needed to formulate them, but psychology has prospered largely

because the fact of variability gave it problems to solve. In so far as red light yields the sensation red, there is little to be said; but the laws of color mixture, adaptation, after-image, contrast, and twilight vision all proclaim the fact that red light does not always yield the sensation red, and that the sensation red is not always the result of red light. Similarly all psychophysics has come about because the constancy hypothesis is invalid.

3. A third protest of *Gestalt* psychology was against associationism. In part this protest was identical with the objection to real elements. The unreality of analysis into elements was obscured by their associative re-synthesis. In part the protest was an attempt to direct attention to other structural principles for forming wholes beside mere experienced temporal contiguity. In so far as *Gestalt* psychology has brought forward additional principles (and it has not been barren in this respect), there can be no doubt that its protest was well directed. In so far as it means to deny completely the interpretation of the results of most of the experimental work on memory, its case is, in the opinions of most psychologists, still unproven.

4. Then *Gestalt* psychology raised the hue and cry against the concept of attention. It called it vague and meaningless. It regarded *attention* as a term of explanation that did not actually explain, as a delusory means of escape from ignorance. It raised the same charge against the concept of attitude. The validity of this complaint is almost inescapable. The critics of *Gestalt* psychology have in general been content to cry *Tu quoque,* and to point, for example, to the similar concept of levels, of which *Gestalt* psychology has made use.

If this negative aspect were all that there is to *Gestalt* psychology, it would never have become an important movement. There is, however, a positive side.

In the first place, *Gestalt* psychology has formulated a new set of psychological concepts and laws, which apply most directly to the psychology of perception. Foremost among these is the concept of wholeness, the cardinal principle of *Gestalt,* that the whole is not a mere summation of parts, but a unitary structure, in which the change of any part changes the whole, and conversely. On the other hand, even this principle may be limited by law, as it is by the law of transposability of *Gestalten* without other changes, e.g., when an animal, responding to the darker

of two grays of a pair, still responds to the darker of a new pair, even though his response in the second case may be toward the gray that it was away from in the first case.

There are also fundamental laws of the structure of *Gestalten.* There is the law of pregnancy, that *Gestalten* tend to complete or to emphasize their natural form. The law of closure, that incomplete normal figures tend to complete themselves, is a special case of the law of pregnancy. So is the law of figure and ground, whereby visual perceptions, at least, tend to divide into a figure with contour and a ground that is less objective in its character and is generally seen behind the figure.

All these laws derive from the experimental phenomenology that Wertheimer began. *Gestalt* psychology did not, like its contemporary, behaviorism, reject the description of consciousness, but accepted it, when freed of atomism, as fundamental to psychology. Köhler and others have, however, gone farther and attempted to deal with psychophysiological *Gestalten.* Köhler's conception of the brain, as a colloidal body of imperfectly insulated tracts where the *Gestalten* are electrostatic fields, is the principal instance, although there is also Wertheimer's suggestion that the *phi*-phenomenon indicates a cortical short-circuit between two stimulated regions. While much of this physiological work comes under the opprobrium of speculation, nevertheless it is consonant with Lashley's careful research, which indicates that projection of the periphery upon the cortex by means of definite tracts is not fixed, and that perhaps no single small region is essential to any single mental function.

In actual experimental research *Gestalt* psychology has contributed mostly to the problems of perception, and here predominantly to visual perception. The work on seen movement was first extended by A. Korte, who deduced a number of laws for it. The literature is now large. The problem of movement was also transferred to the skin, principally at first by V. Benussi, and similar results were found. There has been a great deal of work on visual space-perception and on color. Most of this research is recent and cannot be set forth here in the detail that would be necessary. It is available to the reader both at first and at second hand.

Although the experimental work of *Gestalt* psychology in perception is the most important consideration for the purpose of this book, we must note the fact that the new movement has spread

far afield. It has included animal psychology and the problem of intelligence from the start. Thus it has passed readily into educational psychology and child psychology. In a very different form, it is the basis of the 'developmental psychology' of Felix Krueger, Wundt's successor at Leipzig. It is to be found in abnormal psychology, and some of the work in abnormal psychology has contributed directly to the work in perception. Köhler, as we have seen, has argued for the extension of the theory to biology and physics. In experimental psychology there is almost no problem that cannot be undertaken under the auspices of the *Gestalt* school. In fact, it is the universality of the *Gestalt* principle that is both the strength and the weakness of the movement. One may applaud the new movement because of its universal applicability, or one may discount it as simply psychology itself. It does, however, definitely exclude the conception of sensations as real elements, and in this matter it represents a genuine change from the older orthodoxy. It is in this essential point that it differs, for example, from its most immediate ancestor, the school of *Gestaltqualität*.

Behaviorism

Both behaviorism and *Gestalt* psychology were reactions away from what had become the orthodox psychology in the beginning of the present century. The 'new' psychology that had already become old was, as we have said above, (1) experimental, (2) introspective, (3) systematic, (4) elementaristic, and (5) associationistic. Both *Gestalt* psychology and behaviorism accept experimentalism, but in every other way they differ in their reactions. *Gestalt* psychology accepted introspectionism in the modified form of phenomenologism; behaviorism rejected introspection completely as a method, and this rejection is its outstanding characteristic. *Gestalt* psychology retained its interest in system and thus a certain amount of the relation to philosophy; behaviorism in general has been unsystematic, although some systematic questions have been forced upon it. *Gestalt* psychology rejected elementarism completely both in phenomena and in bodily processes, and this rejection is its primary characteristic; whereas behaviorism rejected the elementarism of conscious elements like sensations, images, and feelings along with its rejection of consciousness, but did not wholly avoid nervous elements like the

reflex arc. Finally *Gestalt* psychology flatly rejected associationism, whereas behaviorism practically accepted it for the nervous system under the title of the conditioned reflex.

Thus behaviorism came into being as what is called an 'objective' psychology—that is to say, a psychology that ignores consciousness. There was nothing new about objective psychology. We have already seen the effect of Loeb's theory of the tropism, and how Beer, Bethe, and von Uexküll suggested a completely objective psychology. Diserens has recently traced the history of objective psychology from the Greeks to the present day, showing how it was asserted in Descartes's dualism and his thesis that animals were automata, how materialism and positivism in France and Germany have always tended toward the depreciation of consciousness, and how the modern critiques of introspection and the establishment of the existence of the subconscious by psychopathology have reinforced this tendency. Like *Gestalt* psychology, behaviorism did not, however, come into existence as the result of historical pressure, but naïvely, without historical sophistication, to meet an immediate practical scientific need.

One especially important influence that lies behind the foundation of behaviorism is the work of the Russian physiologist, *Ivan Petrovitch Pavlov* (1849-[illegible]), which began about 1903 and has continued up to the present time. It was in 1903 that Pavlov first reported the conditioning of the salivary reflex, and at this time he spoke of this new method for investigating intelligence and other mental phenomena in animals as "psychic," and the salivary flow under conditioning as "psychic secretions." It is plain, however, that these phrases were even at the first hardly more than a physiologist's pleasantry, the remark of a physiologist who believes that psychology, in so far as it is scientific, is nothing other than physiology. In 1904 he was saying: "The physiology of the highest parts of the central nervous system of higher animals can not be successfully studied, unless one utterly renounces the indefinite presentations of psychology and stands upon a purely objective ground." He has reiterated this view with greater and greater emphasis ever since—at least six times before 1913.

Now Pavlov was not simply expressing his faith in the physiological method and his distrust of the psychological method. He was definitely presenting his new method of the conditioned reflex as a means of solving problems that had hitherto been thought

to be psychological, and that had not been brought to a successful solution, upon animals at least, by the psychological methods. For instance, he was able (1912) to determine the differential threshold for tone in the dog by means of the salivary conditioned reflex. The dogs were given food along with a tone of 1,000 vibrations until the reflex was conditioned—that is to say, until the saliva would flow with the presentation of this tone without food. Then he was able to show that a tone of 1,012 vibrations would not produce the response, thus exhibiting an absolute discriminatory ability of about one tenth of a musical whole tone. (This is a degree of absolute pitch approximately twice as fine as has been found in the average person.) The tones, however, are only an example. The method is a way of showing to what sensory stimuli an animal can respond discriminatively. It can be used to discover what the animal 'perceives' after destruction of different parts of the brain as well as to test his normal equipment. It opens up, as Pavlov was constantly pointing out, an entire realm of physiology that deals with problems that were thought formerly to be psychological.

There was also in Russia the work of Vladimir M. Bekhterev, along lines similar to Pavlov's. It is these two men jointly who gave physiology and psychology the conditioned reflex. Americans were slow in learning the full significance of the new method, although attention was called to it in English as early as 1909. There is no doubt that both these Russians did much to make behaviorism appear plausible when it appeared.

John B. Watson (1878-) 'founded' behaviorism in 1913. He crystallized the growing demand for an objective psychology, and presently furnished the name for it. He was just as conscious of starting a definite movement as Wundt had been. If he made extravagant claims for the school, still it must be said that there would probably have been no school had there not been in some one a positive faith strong enough to warrant exaggeration.

Watson, as we have seen, was working in experimental animal psychology. He had little patience for philosophical niceties and a great love of exact experimentation. The large probable error of introspective work was distasteful to him and the more 'objective' technique for the animals appealed to him. He felt that, in the quantitative experimental observation of animal behavior, he was achieving valuable scientific results, and that there was little value

or certainty in the conventional attempts to infer the nature of an animal's consciousness from the observations of its behavior—a convention that was usually met at the end of a report on research by an unnecessary addendum and often accompanied by the phrase, 'if the animal has consciousness at all.' It was really behavior—selective, discriminatory behavior—in which he and most of the investigators of animal psychology were interested. Why befog the problems by an unsafe anthropomorphic inference? And, since the objective work on animals was so much more precise than the introspective work on persons, as he thought, why not extend the method for the animals to human beings?

So Watson sought to drive consciousness and introspection out of psychology, both human and animal. The new movement dates from his first polemic, *Psychology as the Behaviorist Views It*, in 1913. Two other papers followed shortly, and in 1914 he published *Behavior, an Introduction to Comparative Psychology*, making his case for the animals by way of a complete exposition of animal psychology from the behavioral, or what he called the "behavioristic," point of view. To complete the case in the concrete it was necessary for him to exhibit the application of the method to human beings, and Watson undertook at once to accomplish this extension of the method by his study of the behavior of infants. In 1919 his *Psychology* appeared, a complete textbook of psychology and a document showing explicitly the scope of behaviorism.

It was not difficult for Watson to avoid reference to attention and will, the vague conventional topics of psychology that have never given satisfaction and that *Gestalt* psychology has also abandoned. He could avoid using the words *sensation* and *perception*, because he substituted as the scientific datum the objective evidence of sensation and perception, the discriminatory response. In the same way he could avoid reference to *association*, because he had the equivalent in conditioned reflex. All these matters can be only terminological, and Watson's text of 1919 shows that the older psychology can be rewritten without new research simply by a translation of the terms.

However, Watson was in difficulty with the problems of imagery and of feeling. Both image and affection seem to be introspective data and not open to objective observation. What he did in 1913 was to deny both.

Against the image it is possible to make a stronger case than the introspectionist would at first expect. It is practically impossible to demonstrate tactual and other somesthetic images conclusively. There is always peripheral excitation and it is never certain when one has an image or when one is simply attending to sensation already potentially present. Olfactory and gustatory images are notoriously rare and are suspect when reported. Auditory imagery is rare except when accompanied by kinesthetic voci-motor processes, and it is possible to argue that the voci-motor processes are the whole of the 'image.' The case is difficult only against visual imagery, and is aided even here by the confusion of the purely visual processes with processes representing eye-movements. In the case of visual imagery, the introspectionists thought that Watson was denying the obvious; on the other hand, he did not lack good company.

With the images gone, thought seemed also about to disappear. However, Watson saved thought as a psychological category by appealing to peripheral voci-motor processes. Much thought is verbal, and language and thought have usually been held to have developed together in the race. Even the introspectionist knows that in a great deal of thinking there are actually innervations of the vocal apparatus, the larynx and the tongue. Watson met the problem of thought by supposing that in thinking, and in a great deal of imagining too, the events are verbal and are carried on by "implicit language responses"—that is to say, by voci-motor movements too small to be recorded by any apparatus then available.

Feelings—pleasantness and unpleasantness—were the other introspective categories that gave behaviorism difficulty. They were much better established as categories of introspective data than they are now, and Watson could not ignore them. He referred them, therefore, to tumescence and detumescence of the genital organs and the whole bodily reverberation accompanying these processes. These responses had to be thought of as implicit, but of course Watson hoped that they might become explicit by the invention of some apparatus or method for detecting them.

In this way behaviorism came to be a psychology of *stimulus* and *response,* the two terms that are objectively observable at the periphery. Their relationship is realistically envisaged in terms of the nervous system, and thus largely by reference to reflex

arcs, constellations of reflex arcs, and, in the more complicated relationships, conditioned reflexes. Such a psychology passes readily over into physiology, and Pavlov held that it is physiology. Watson wanted the close relationship of psychology to physiology, but, nevertheless, he was not ready to give up psychology. Instead he sought to differentiate the two by defining psychology as the study of the total individual, the highest level of his organization, and physiology as the study of organs or systems, a mid-level of organization, with the lowest level reserved for the physics and chemistry of the cell. In a way Watson may seem to have been opposing elementarism in psychology, while wishing it upon physiology and biophysics; but it is not true that this issue was important in behaviorism, which remained in spite of it quite elementaristic, with the reflex as the element. Behaviorism for this reason is very closely allied to physiology. Probably the only way it is to be distinguished from physiology is by its problems, which came, not from within itself, but from the conventional psychology. To this point we shall return in a moment. It is enough here to note that work like Lashley's on the brain fitted behaviorism nicely because it was physiological in method.

Watson distinguished between *explicit* and *implicit* responses. The explicit responses are the external, the observable, the overt ones, and of necessity make up the observational data of research on the side of response. The implicit responses would better have been called covert, because they are internal and not readily observable. It is probable that an implicit response that becomes overt enough for observation becomes by that fact explicit. The word *implicit* is for this reason, nevertheless, technically correct, for they are implied by the necessities of a behavioristic psychology and not to be observed.

In his *Psychology* of 1919, Watson included *verbal report* among the explicit responses. In this manner he was able to assimilate all the results of introspection to behaviorism, for introspection is itself behavior. Of course he did not attempt to take over those introspective results on the higher mental processes which led originally to his revolt, but he was able to include the results of work on sensation and perception by psychophysical methods and other simple research based on verbal responses of this sort where the precision is relatively high. It is an interesting question to ask just what happens when the datum is shifted

from sensation to the verbal report of sensation. The conventional theory placed sensation as a middle term. The series is illustrated by: red light—stimulation—red sensation—verbal report of red. The older view held that the red sensation is observed and can be then reported in the way that all observations in any science are reportable. Watson shifted the focus of observation to the report, which he held to be observed by the experimenter. In theory there is no existential difference in the chain of events in the two cases; there is simply a shifting of emphasis as to what is called the datum. It is for this reason that it is possible to change an introspective observation *ex post facto* into a behavioristic one at any time after its completion.

As behaviorism began to absorb most of the content of the older psychology and to expand to include social psychology, the word *stimulus* began to lose much of its precision. It came often to be used for any initial term in the relation that ended in response. Thus a stimulus, instead of having an exact physiological meaning, was often a situation or an involved object with meaning encrusted upon it. A chair, a dinner-table, a symphony, a loved person, a piece of conversation might be called a 'stimulus.'

So it came about that the very adaptability of behaviorism tended to defeat the original purpose of Watson in 'founding' it. With hypothesized implicit responses admitted, with any kind of situation or mass-object playing the rôle of stimulus, with all uttered words counting as responses, much of the original precision of physiological method was lost. Not all of these loose extensions of behaviorism can be originally charged to Watson, but it is true nevertheless that he committed the error that opened the door to them.

What Watson did, as Wundt and almost every systematist and textbook writer had done before him, was to take the scope of psychology as externally defined. The invention of behaviorism was sound enough. The question as to whether it is to be called psychology is a matter of personal preference. Watson, however, instead of letting behaviorism be whatever the behavioristic methods gave, proceeded to force it into the conventional mold already formed for psychology, and most of the behaviorists after him have followed his example. He could have ignored feeling and the image as phenomena, which, if they exist at all, are at least not behaviorally demonstrated and, therefore, of no in-

terest to behaviorism. He could have avoided hypothesizing implicit responses and have waited until they might come into observation. Instead he bowed the knee to most of the categories of the older psychology, which he could have ignored, and tried to fit behaviorism to outworn clothes instead of leaving it only its own skin. In so doing, Watson was simply doing what every one else had done. Systematic texts had always, for instance, included chapters on emotion and thought, and for a time a chapter on will, for no better reason than that these categories were accepted as belonging to psychology, and any claimant to the position of *a* psychology must write about them, guessing where there were no facts.

The result of all this subservience to convention has been that behaviorism as a systematic body of knowledge is not so very different from the body of any other psychology. It meets the same problems and states many of the same facts. It is a curious situation that behaviorism has faced: its success in extending its scope has tended to destroy its individuality. As a method of research, however, it has been very effective and undoubtedly it will leave a mark on history.

We cannot do more than sketch briefly the rise of behaviorism after it passed beyond Watson's hands. E. B. Holt, as philosophically sophisticated as Watson was naïve, was one of its first supporters. He saw in the relation of stimulus to response the essential elements of the cognitive relation, and he was led in 1915 to proclaim behaviorism as "the one great luminary of the psychological sky." To the matter of the relation of behaviorism to the psychology of meaning we shall return in a moment. At the same time Holt enthroned the Freudian wish within dynamic psychology, and this act of his had much to do with the way in which behaviorism has absorbed the psychology of psychoanalysis.

Quite early, too, A. P. Weiss espoused the behavioristic cause, and issued polemics against functional and structural psychology. W. S. Hunter, another behaviorist, has been one of the psychologists who have taken the categories of behaviorism from outside and have sought to find the behavioristic accounts of consciousness, of thought, and of cognition. E. C. Tolman has advanced far in this same quest, and has further formulated a new synthetic position in which he combines behaviorism, *Gestalttheorie,* and purposive psychology. The synthesis comes about easily in his

hands, for he regards the total organization of behavior as a *Gestalt* and shows that the final term is just as important to the total structure as the initial one. Lashley also is a behaviorist, and has once deserted his experimental research for a polemic in its favor.

Of these men, Hunter, Tolman, and Lashley are primarily engaged in animal research, and behavioral terms are their most natural medium. Their work, however, has taken on a new significance of broader scope than animal psychology ever had, because it is now becoming apparent that animal psychology is a sort of general psychology (if we use that phrase as analogous to general physiology), and that the behaviorists when they work upon animals are not merely studying the animal mind, but are studying mind through the medium of animal material.

In the opinion of the author, one very important contribution of behaviorism to psychology has been the creation of a satisfactory psychology of meaning. Holt, Hunter, and Tolman have all contributed to this end. Introspective psychology never fully met the problem of meaning. The best-known example of its effort, Titchener's context theory of meaning, depends for its generality on the existence of unconscious meanings, of meanings carried by the nervous system only. The discriminatory response has in it, however, the germ of cognition. To know or to perceive is to respond selectively, and it would seem that the problem of meaning, to which we have repeatedly referred from John Locke to Titchener, is in the way of being met by behaviorism. If some psychologist would only now say that the accrual of a conscious context to a conscious core is but a specific case of a response to a situation, then Titchener's theory might be included in a super-behavioristic theory of meaning.

It should be plain to the reader of the last two chapters why behaviorism has thrived in America. It is essentially a psychology of adaptation and capacity. Its remote ancestor is Darwinism. It has replaced functional psychology as an active school, for it adopts functional psychology's chief tenet of showing what mind is used for. It is true that behaviorism has disclaimed the parentage of functionalism, but this repudiation has arisen only because functional psychology held to consciousness as the datum, though it sought to show what consciousness was for. The mental tests have readily been assimilated to behaviorism, because in general

it is simpler in them to consider the response itself as the datum and to ignore its implications for consciousness. They have a social value, and in the organization of society it does not matter what people think as long as they act in accordance with social requirements. For similar reasons psychopathology, in spite of all the introspective implications of psychoanalysis, comes readily under the protection of behaviorism. Behaviorism, as Watson originally intended it to be, is an essentially practical psychology when sufficiently extended in scope. Hence it is certainly not too much to say that behaviorism in the present day is the typical American psychology, in spite of the refusal of perhaps the majority of American psychologists to call themselves behaviorists.

Other Contemporary Psychologies

Except for American behaviorism, psychology in America and Great Britain seems, for the time being at least, content to settle down to work without the formation of new schools. Such a state of affairs would represent the acquisition of scientific maturity, where experimental research is what matters, where only factual results are the subject of controversy, and where the solution of controversy lies in more experimentation. The schools have in general been built about convictions that were given *a priori,* and are a symptom of a science not yet able to be careless of philosophy. Whether the present lull in American and British systematic psychology is such a symptom, or whether it will prove to be of long duration, it is too early to state.

In Germany, however, there are many new views and new schools. In all of them the general principle of the *Gestalt* seems to have been accepted, and in all of them the trend is consciously and vigorously away from elementarism and mental chemistry. We have dealt at length with *Gestalt* psychology proper, because it shows the focus of modern psychological thought in Germany, and because it brings together many of the loose threads which we have carried all the way through the present text. It has also been a predominantly experimental movement. However, we need briefly to indicate the scope of modern German psychology, even though we but name these other movements.

In the first place there is the psychology that centers about *Felix Krueger* (1874-), Wundt's successor at Leipzig. It may

be called *developmental psychology* (*Entwicklungspsychologie*). Krueger accepts the principle of *Gestalt* as fundamental in psychology, but he goes further in holding that psychological facts can be understood only in terms of their developmental history. Under this stimulus, the Leipzig laboratory, like the Berlin laboratory, has turned to research upon the problems of perception. However, Krueger's vision is not limited to the laboratory, and he sees in his psychology the possibility of coming at an understanding of cultural phenomena and also the opportunity for practical applications of psychology to social problems. Within such a broad universe there have issued from Leipzig numerous publications under the authorship of Krueger and others.

Marburg is the seat of the school of *Erich R. Jaensch* (1883-), where the knowledge of eidetic imagery and of eidetic types has originated. The eidetic image is a percept-like image, which resembles an hallucination in almost every way except that it is known by its possessor to be an image. These images are quite common in childhood. The frequency of their occurrence in children cannot be stated numerically, because there are many degrees of vividness and faithfulness, and the statistics depend on the criterion chosen. The images may persist into adult life, although they normally disappear. There are two types of eidetic images, but we cannot enter into the details of all these findings here. The striking thing about Jaensch's work is, however, that, beginning with these facts about the images, he has gone further, using differences in imagery as presumptive of difference in individual types. This work has led over into psychopathology, and has resulted in the establishment of two fundamental physiological types of persons, the B-type and the T-type, with indications that there are more types to come. The daring of Jaensch's pronouncements, and the enthusiasm of himself and his colleagues, have made a considerable impression on current psychology, and the future will show how permanently fruitful this approach to the general problem of individual differences can be.

William Stern (1871-) at Hamburg stands nowadays for a *personalistic psychology*. His concern with the problems of educational psychology early led him to be one of the pioneers in differential psychology, and this "critical personalism" is the culmination of many years of study. His system accepts the individual as the primary *Gestalt*, and is systematically sympathetic with the

most rigorous experimental psychology. On the other hand, it seems to have led to less actual experimentation than have the views of Krueger and of Jaensch.

All these three psychologies, taken together with *Gestalt* psychology proper, belong in the group of German psychologies that purport to be natural science. If interpenetration with physiology can be taken as an indication that psychology is a natural science, then Jaensch and Köhler are the nearer to this ideal, whereas Stern and Krueger are somewhat more remote. We find in Krueger a clear recognition of the limitation of the method of the laboratory and thus a *rapprochement* to *Geisteswissenschaft*, which is the alternative to natural science.

The *geisteswissenschaftliche* ('cultural') psychologies have also shown great vitality in Germany in recent years. They are more nearly related to act psychology than to any of the other older psychologies which we have discussed. Husserl especially has influenced them. The name that is best known in this field is *Eduard Spranger* (1882-) of Berlin, who has developed a striking theory of personal types which has found many enthusiastic adherents. We may say, for want of a better term, that he has worked by an 'intuitive method'; certainly the experimental devices of the laboratory or of the mental tests are about as remote from his procedure as they well could be.

Notes

Gestalt Psychology

Various English equivalents have been suggested and used for the German word, *Gestalt*, in this context. In America Titchener's suggestion of "*configuration*" (*configurationism*) has been most commonly employed; cf. Helson, *op. cit. infra.* In Great Britain the word *shape* has been used; cf. C. Spearman, *Brit. J. Psychol.*, 15, 1925, 211-225. *Shape* is peculiarly objectionable as indicating mere outline and lacking the more general meaning of *form*, except in such colloquial phrases as "in good shape," "shipshape." *Configuration* is open to the same objections in milder degree. Both tend to suggest vision, which is unfortunate, since so much of the *Gestalt* work has been visual and the general theory should not be limited to the medium through which it has happened most frequently to be expressed. When Koffka first published in English (1922; *op. cit. infra.*), he was persuaded to use the term *structural psychology*. This phrase is excellent, for *Strukturpsychologie* is a German equivalent for *Gestaltpsychologie*. Unfortunately, however, *structural psychology* has long had the meaning of that which Titchener opposed to functional psychology in America, and it is thus the very thing against which *Gestalt* psychology is protesting. Quite recently there was a suggestion that the word *organic* might be employed:

Hsiao, *op. cit infra.* Köhler, however, has consistently used the German word both as noun and adjective in English text, and this practice now seems likely to prevail.

Before we list the secondary sources on *Gestalt* psychology, we may cite the more general publications of the *Gestalt* psychologists themselves.

MAX WERTHEIMER:

Experimentelle Studien über das Sehen von Bewegungen, *Zsch. f. Psychol.*, 61, 1912, 161-265: the original paper on seen movement. Reprinted in *Drei Abhandlungen, op. cit. infra.*

Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1, 1921, 47-58: a general discussion of the principles of the new movement, and a paper that lies back of Koffka's similar discussion the next year.

Ueber Gestalttheorie, 1925: a printed lecture of 24 pp. which the present author has not seen.

Drei Abhandlungen zur Gestalttheorie, 1925: a reprint, repaged, of the initial article on apparent movement (*op. cit.*) and of two other papers on thought, one from *Zsch. f. Psychol.*, 60, 1911, 321-378, and the other a separate, *Ueber Schlussprozesse im produktiven Denken,* 1920.

WOLFGANG KÖHLER:

Intelligenzprüfung der Menschenaffen, 1917, 2d ed., 1921, Eng. trans., 1925: the work on the apes, which illustrates some of the principles of *Gestalt* psychology and establishes the concept of insight.

Die physischen Gestalten in Ruhe und im stationären Zustand, 1920: the book that applies the new theory to physics and biology, supplies the physical analogies for psychology, and presents some of its author's psychophysiological views.

Gestaltprobleme und Anfänge einer Gestalttheorie, *Jarhesber. ü. d. ges. Physiol.*, 1922, 512-539: a rather inaccessible but very clear presentation of some of the principles of *Gestalt* by way of examples.

The problem of form in perception, *Brit. J. Psychol.*, 14, 1924, 262-268: a short paper considering especially the problem of relations and transpositions.

Komplextheorie und Gestalttheorie, *Psychol. Forsch.*, 6, 1925, 358-416: a reply to G. E. Müller's criticism of *Gestalttheorie,* a book with the same title, 1923.

Intelligence of apes, and An aspect of Gestalt psychology, two articles, in *Ped. Sem.*, 32, 1925, 674-723; both reprinted in *Psychologies of 1925,* 1926, 145-195: two printed lectures resulting from Köhler's first mission to America. The latter contains the discussion of "differential analysis."

Gestalt Psychology, 1929, a general exposition in English which has appeared since the text of this chapter was written. It becomes at once the best single means of insight into the new point of view. It is also modest and conservative, and thus avoids some of the criticisms that have been directed against early presentations, especially those that were primarily negative in tone.

KURT KOFFKA:

Beiträge zur Psychologie der Gestalt, 1919: a collection of papers which consists of the original printing of F. Kenkel's research on seen movement, and reprints of A. Korte's experimental establishment of the laws of seen movement, of Koffka's critique of Benussi, and of Koffka's theory of seen movement. The reprints are respectively from *Zsch. f. Psychol.*, 72, 1915, 193-

296; 73, 1915, 11-90; 82, 1919, 257-318. This is the work of Koffka and his students while the new movement of *Gestalt* psychology was in the process of formation.

Die Grundlagen der psychischen Entwicklung, 1921, Eng. trans., 1925: child development from the point of view of *Gestalt*.

Perception: an introduction to the *Gestalt-theorie*, *Psychol. Bull.*, 19, 1922, 531-585: a general discussion that represents the introduction of the new movement to American psychologists. It is clearly responsible for the first general interest in America in *Gestalt* psychology. It contains a bibliography of sixty-six papers.

Introspection and the method of psychology, *Brit. J. Psychol.*, 15, 1924, 149-161: a defense of that kind of introspection which is phenomenological and not analytical.

Mental development, *Ped. Sem.*, 32, 1925, 659-673; reprinted in *Psychologies of 1925*, 1926, 129-143: a lecture during Koffka's second visit to America.

In spite of this accumulating literature, there was considerable difficulty, in America and Great Britain at least, in understanding the new movement. The difficulty arose because many of the publications that were held to exhibit the movement dealt with problems of research which were only particular illustrations of the *Gestalt* point of view, and because the theoretical papers were so largely devoted either to illustrations or to protestant negative statements as to what *Gestalt* psychology is not. In the future much of this difficulty may, however, be removed by the appearance of Köhler's more positive and cautious exposition, *Gestalt Psychology*, 1929.

The greatest single service of exposition and interpretation of the *Gestalt* movement from outside the school is the series of four articles by H. Helson, *Amer. J. Psychol.*, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223. These have been reprinted separately with an index and without repaging, and with a bibliography of 236 titles.

Even Helson's organization of a large and varied literature results in an elaborate exposition, and recently H. H. Hsiao has sought to present a brief compendium of laws and principles of *Gestalt* psychology, *Psychol. Rev.*, 35, 1928, 280-297. His list of laws has been given in the present text.

Other accounts that are primarily expositions of *Gestalt* psychology are: G. W. Allport, *Psyche*, 4, 1924, 354-361; G. Humphrey, *Amer. J. Psychol.*, 35, 1924, 353-359 (a comparison of the theories of relativity and of *Gestalt*); M. W. Calkins, *Psychol. Rev.*, 33, 1926, 135-147 (the rest of the article is critical); R. M. Ogden, *ibid.*, 35, 1928, 136-141; H. Klüver, in G. Murphy, *Historical Introduction to Modern Psychology*, 1929, 426-434. We should perhaps add Ogden's early paper arguing against the reality of sensations, *Amer. J. Psychol.*, 33, 1922, 247-254.

There have also been numerous papers critical of *Gestalt* psychology. We have cited G. E. Müller's critique in connection with Köhler's reply, *supra*. Other criticisms, inspired mostly by the invasion of America by Koffka and Köhler, are: J. R. Kantor, *J. Philos.*, 22, 1925, 234-240 (criticizing various points); C. Spearman, *Brit. J. Psychol.*, 15, 1925, 211-225 (partly expository, but primarily critical); M. W. Calkins, *op. cit.*, 147-158 (showing among other things the place of James as an anticipator of *Gestalt* psychology); M. F. Washburn, *Amer. J. Psychol.*, 37, 1926, 516-520 (arguing for the greater adequacy of a motor theory); W. B. Pillsbury, *J. Abn. and Soc. Psychol.*, 21, 1926, 14-18 (criticizing the nature of analysis in *Gestalt* psychology); H. Helson, *op. cit.*, 189-216 (summarizing critically his survey of the field).

Behaviorism

On the history of 'objective' psychology and thus on the antecedents to behaviorism, see C. M. Diserens, *Psychol. Rev.*, 32, 1925, 121-152.

Pavlov's work, mostly in Russian with scattered articles in French and German, has now become available in English translation: I. P. Pavlov, *Conditioned Reflexes: an Investigation of the Physiological Activity of the Cerebral Cortex*, 1927; *Lectures on Conditioned Reflexes*, 1928. The latter book contains forty-one lectures, arranged chronologically from 1903 to 1928, and a biographical sketch of 21 pp. For his criticisms of the psychological method as compared with the physiological, see pp. 75, 113, 121, 169, 192, 219, and 329 f. It ought to be said that some of his results are so remarkable as to have met with incredulity. An early English account of Pavlov's work is: R. M. Yerkes and S. Morgulis, *Psychol. Bull.*, 6, 1909, 257-273.

There is no similar easy road to a knowledge of V. M. Bekhterev's researches. There are many scattered articles in French and German beside those in Russian.

On John B. Watson's early work, see chap. 21. Watson went from Chicago to Hopkins in 1904 and stayed in Baltimore until 1920, when he went into business in advertising in New York. His original polemics in support of behaviorism are Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177, and Image and affection in behavior, *J. Philos.*, 10, 1913, 421-428. The first book in the new movement is *Behavior, an Introduction to Comparative Psychology*, 1914. The first chapter reprints with minor changes, slight omissions, and considerable additions the two articles just mentioned. The more general text is *Psychology from the Standpoint of a Behaviorist*, 1919, 2d ed., 1924. When he went into business he did not cease to lecture on behaviorism or to polemize in its behalf; see *Behaviorism*, 1924; and the three lectures in *Psychologies of 1925*, 1926, 1-81.

Edwin B. Holt's article on behaviorism and the problem of cognition is Response and cognition, *J. Philos.*, 12, 1915, 365-373, 393-409. The book which the text mentions is *The Freudian Wish and Its Place in Ethics*, 1915, which reprints also the article just mentioned.

The references to the other articles supporting behaviorism and mentioned in the text are as follows:

A. P. Weiss, *Psychol. Rev.*, 24, 1917, 301-317, 353-368; *J. Philos.*, 26, 1919, 626-634; cf. *A Theoretical Basis of Human Behavior*, 1925.

E. C. Tolman, *Psychol. Rev.*, 25, 1918, 423-442; 27, 1920, 217-233; 29, 1922, 44-53, 140-145; 30, 1923, 217-227; 33, 1926, 352-369; 34, 1927, 433-439.

K. S. Lashley, *Psychol. Rev.*, 30, 1923, 237-272, 329-353.

W. S. Hunter, *Psychol. Rev.*, 31, 1924, 1-31, 478-497; 32, 1925, 153-170; *Amer. J. Psychol.*, 36, 1925, 286-302; *Psychologies of 1925*, 1926, 83-107.

Watson's protest against orthodox psychology was of course met with a counterblast of protest. Titchener appropriately was the first to reply, admitting the validity of Watson's point of view, but ruling it out of psychology: E. B. Titchener, *Proc. Amer. Philos. Soc.*, 53, 1914, 1-17. A mild refusal to go to Watson's extremes in excommunicating consciousness is to be found in a quarter where it might not have been expected: E. L. Thorndike and C. J. Herrick, *J. Animal Behav.*, 5, 1915, 462-470. Then there is the critical exposition of J. S. Moore, *The Foundations of Psychology*, 1921, 31-65. M. F. Washburn directed her presidential address before the American Psychological Association against behaviorism: *Psychol. Rev.*, 29, 1922, 89-112. A. A. Roback has written an entire book unfavorable to behaviorism:

Behaviorism and Psychology, 1923. Diserens's account of the historical antecedents of behaviorism, *op. cit.*, is a criticism of behaviorism's claim to originality.

These notes give only the more constructive and destructive work directly upon the methodological problem of behaviorism. A very great deal has been written in America about behaviorism, and the word is not only constantly on the tongue of every American psychologist but is becoming familiar to the lay public. Certainly Watson founded a movement, no matter how little of its distinctive thought he originated.

Other Contemporary Psychologies

The reader must seek other sources than this book for a knowledge of the contemporary movements in Germany. An excellent short summary of the movements named in the text is the one by H. Klüver in G. Murphy, *Historical Introduction to Modern Psychology*, 1929, 417-455. See also H. Henning, *Psychologie der Gegenwart*, 1925.

On F. Krueger's developmental psychology, see his *Ueber Entwicklungspsychologie, ihre sachliche und geschichtliche Notwendigkeit*, 1915. This publication begins Krueger's *Arbeiten zur Entwicklungspsychologie*, of which the successive volumes by various authors serve as concrete illustrations of this point of view. For the more experimental work and the theory related to it, see *Neue Psychologische Studien* of the Leipzig laboratory, and especially vol. 1, 1926, Komplexqualitäten, Gestalten und Gefühle, which includes Krueger's introduction, Ueber psychische Ganzheit, pp. 5-121.

For the Marburg school, see E. R. Jaensch *et al.*, *Ueber den Aufbau der Wahrnehmungswelt*, 1923, 2d ed., 1927. A short book is Jaensch, *Die Eidetik und die typologische Forschungsmethode*, 1925, 2d ed., 1927. There is already a large literature, which has been very well summarized by Klüver, *Psychol. Bull.*, 25, 1928, 69-104, an article that gives a bibliography of 196 titles. A sixth of these articles are by Jaensch himself, and the reader can find many of Jaensch's studies in the *Zsch. f. Psychol.*, from vol. 85, 1920, on. For a more general review of the modern German work on individual types (the work of Jung, Rorschach, Klages, Jaensch, Kretschmer, Ewald, Kronfeld, Birnbaum), see Klüver, *J. Nerv. and Ment. Diseases*, 62, 1925, 561-596.

On William Stern, see his *Person und Sache*, I, 1906, 2d ed., 1923; II, 1918, 3d ed., 1923; III, 1924. See also his own account of himself and his philosophy in R. Schmidt, *Philosophie der Gegenwart in Selbstdarstellungen*, 6, 1927, 161-182.

Eduard Spranger's most important book is *Lebensformen: geisteswissenschaftliche Psychologie und Ethik der Persönlichkeit*, 1914, 2d ed., 1921; Eng. trans. as *Types of Men*, 1928.

SURVEY OF EXPERIMENTAL PSYCHOLOGY

Chapter 23

GENERAL SURVEY OF EXPERIMENTAL PSYCHOLOGY: FECHNER TO THE PRESENT

IN the foregoing chapters we have been considering the history of modern experimental psychology in terms of longitudinal strands that represent the work and influence of some man or some school. The emphasis has been personal largely because in psychology, so young a science, personalities have mattered very greatly. By such an expository method we have been able in part to treat the history of psychology psychologically, to see some of the dynamic factors that have been at work in determining thought and research, and to examine the trends of nations, the effects of schools, and the influences of certain important persons. We have, however, gained this insight into the dynamic forces that have directed the development of psychology at the expense of historical scope for successive periods of time. By taking longi-sections, we have missed total cross-sections. We have not drawn the picture of psychology as a whole in the decade after Fechner's *Elemente,* nor in any of the succeeding decades or similar periods. It behooves us, therefore, to make good this deficiency at the present time, and to review the entire period, ignoring personalities and schools and national interests, and placing our emphasis rather upon the books and research that concerned psychologists in Europe and America, as psychology grew and prospered after its foundation as an independent science. Thus we begin about 1860.

The Setting for the *Elemente* (1860)

G. Th. Fechner's *Elemente der Psychophysik* appeared in 1860. It is looked upon as the starting point of experimental psychology, because it provided the experimental methods for the measurement of mind that are still fundamental in psychological research. Moreover, it helped to fix for a time the problems of psychophysics and

of Weber's law, and indirectly the problems of sensation, as psychology's primary concern. However, the *Elemente* was not the sole cause of the 'new' psychology. There was, while Fechner was at work, other research in progress that might have had eventually a similar effect had Fechner never turned psychologist.

The first section of Wundt's *Beiträge zur Theorie der Sinneswahrnehmung* was published in 1858, and the book came to completion in 1862. Wundt in these researches brought the experimental method to bear upon the problems of the visual perception of space and contributed the classic analysis of the perception of the third dimension. The work might be described as sense-physiology were it not for the fact that Wundt gave it a psychological cast and distinguished perception from sensation by the doctrine of unconscious inference. In the preface of 1862 he may be thought of as founding experimental psychology, and also as laying down a complete program for it, a program that he more or less succeeded in carrying out during the remaining sixty years of his life. Wundt worked independently of Fechner, although he accepted the *Elemente* on its publication as having fundamental significance for experimental psychology. He worked independently of Helmholtz, although they were formally associated at Heidelberg.

In this same period Helmholtz had turned to the problems of the psychophysiology of vision. The first section of his *Handbuch der physiologischen Optik* appeared in 1856, although the work, interrupted by his researches on tone, was not completed until 1866. It is true that the first portion of the *Optik* was anatomical and physiological. Helmholtz became more psychological only as he progressed. His theory of vision is given in the second volume (1860), although it had been formulated several years before (1852). It is the third volume (1866) that contains his discussion of perception in general, of empiricism in perception, and of unconscious inference.

There were many other isolated researches in experimental psychology in the decade before 1860. J. Plateau (1853) and J. Clerk Maxwell (1857) had studied color mixture. Plateau's law is fundamental, and the rotating paper disks that are used in color mixing were formerly called Maxwell's disks. A. W. Volkmann helped Fechner develop the method of average error and also published (1858) experiments on the cutaneous two-point thresh-

old, a problem that E. H. Weber had originated. J. N. Czermak had published work on the two-point threshold (1855) and H. Aubert and A. Kammler on cutaneous localization (1858). Perhaps also, in view of the importance which the question assumed later, we should mention Sir William Hamilton's discussion of the range of attention (1859).

Meanwhile the astronomers were carrying forward their study of the personal equation. The invention of the chronoscope (1842) and of the chronograph (*ca.* 1850) somewhat altered the problem and directed it toward what later became the reaction experiment. O. M. Mitchel (1858) reported experiments in which he measured reaction times for auditory and tactual stimuli as well as visual, and J. Hartmann (1858) suggested a psychological analysis of the process that in some ways anticipates Ludwig Lange's discovery of the effect of 'attention.'

Also outside of psychology, but destined eventually to have upon it a profound effect, there occurred what was perhaps the greatest scientific event of the scientific century, the publication in 1859 of *The Origin of Species* by Charles Darwin. This book, appearing the year before Fechner's *Elemente,* focused the active attention of the world of thought upon biology and stimulated biological thought and research as no other event has done. Psychology presently came to share in this revitalization of biology.

The First Decade of Experimental Psychology: 1860-1870

The '60's began, as we have just seen, with Fechner's *Elemente,* a book which, beside both founding and developing psychophysics, was fundamentally psychological in tone, as, for example, in its discussion of imagery and of the memory after-image. Fechner really discovered the imaginal types of thought before Charcot and Galton. Wundt's *Beiträge* was completed in 1862, and in the following year he brought together his Heidelberg lectures on psychology in the *Vorlesungen über die Menschen- und Tierseele* (1863). Immediately it became apparent that the new psychology had scope, that it could include both the animal and the human mind and could embrace such topics as sensation, perception, attention, action, and the will. The *Vorlesungen* was Wundt's first text, but in the history of psychology it represents a transition. It

was more experimental than Lotze's *Medicinische Psychologie* (1852) or Bain's two books (1855, 1859), but it was not the complete systematic experimental text that the *Physiologische Psychologie,* a decade later, was to be.

The greatest activity of this decade occurred in connection with research in the problems of *visual sensation and perception,* the field of sense-physiology that was already most highly developed. Thus we see that the proportioning of psychology of to-day is little more than a matter of its history: sensation is the field that leads all others, and within the topic of sensation there is most to be said about vision. Helmholtz's *Optik* was coming out (1860, 1866), and the completed three volumes were available in 1867. The importance of this work can hardly be overestimated. This first edition, as more 'correct' than the second, was translated into English over fifty years later, not as an historical document, but because it could still be regarded as the fundamental handbook for physiological optics.

However, Helmholtz was not to remain alone in this field. Ewald Hering, the physiologist, who had come under Fechner's influence at Leipzig (*ca.* 1860), published, while still at Leipzig as *Dozent,* his *Beiträge zur Physiologie.* It appeared in five parts (1861-1864), and they all dealt with the physiology of vision. The last part contains his first statement of his nativistic theory of visual space-perception, for which he is famous. In 1868 Hering published *Die Lehre vom binocularen Sehen,* a book which extends the study of binocular perception from the point where Wundt and Helmholtz had left it. Hering was destined to become an important influence in the new psychology, but his most significant work was not until later.

A. W. Volkmann, Fechner's friend and brother-in-law, who had first published important work on visual sensation in 1836, now returned to this field. His *Physiologische Untersuchungen im Gebiete der Optik* (1864) is of little importance nowadays, but at the time it served to emphasize the dominance of this subject-matter. The first of Hermann Aubert's two important books on vision came out the year after Volkmann's; it was called *Physiologie der Netzhaut* (1865), and was much broader in scope. It discussed light and color sensation, monocular and binocular perception including stereoscopy, and the phenomena of contrast and after-image. Reference to it is still frequently to be found in cur-

rent literature. In this decade E. Brücke was continuing his research on the problems of vision, and contributed most notably (1864) to the knowledge of the phenomena of flicker. S. Exner began (1868) his studies of vision which continued for thirty years. M. Schultze discovered (1866) the different functions of the retinal rods and cones, a finding that paved the way for von Kries's duplicity theory of vision much later.

Had it not been for Helmholtz, we could say that no other sense-department beside vision received any serious attention in the period which we are discussing. However, the indefatigable Helmholtz interrupted his research in optics to investigate *physiological acoustics. Die Lehre von den Tonempfindungen* appeared in 1863 between the second and third volumes of the *Optik*. This book is as much the classic in the psychology of hearing as is the *Optik* in the psychology of vision. It contains Helmholtz's observations on beats and combination tones, his doctrine of consonance, his physiological analysis of music, and, perhaps what is most important of all, his resonance theory of hearing, which has been all but overthrown several times in the last sixty-five years and yet still resists destruction.

In the psychophysiology of sensation, Helmholtz's influence was determinative, but Fechner's *psychophysics* also had an immediate effect. Volkmann included psychophysical papers in the work which we have just mentioned (1864), and Aubert challenged Weber's law in his book (1865). Mach published a small and rather unimportant volume on psychophysics in 1863. J. R. L. Delboeuf made his experiments on brightness in 1865-1866 after discovering Fechner the year before. J. Bernstein's theory of projection of the sense-organ upon the brain and of irradiation in the brain (1868), a theory that has had considerable influence in psychophysiology, is traceable to Fechner's influence.

The work on the *time-sense* began in the '60's with Mach and Vierordt. E. Mach's experiments (1865) were directly inspired by Fechner, for he sought to test the applicability of Weber's law to the perception of time. K. Vierordt's *Der Zeitsinn* (1868) stands more by itself, and is for this period the important monograph on the time-sense. Both studies show the new experimental psychology reaching out to embrace the dimension of time in addition to space, intensity, and quality.

Meanwhile investigation of the *personal equation* was con-

tinuing in astronomy, and the problem was beginning to pass over into psychology. Both Helmholtz and Wundt had touched the problem. Helmholtz had used the reaction method in the attempt to determine the rate of the nervous impulse in sensory nerves (1850), and Wundt had come quite early to realize the significance of the personal equation for psychology (*ca.* 1861). There were several important instances where astronomers determined personal equations for the correction of their results in the determination of longitude: A. Hirsch (1863), Hirsch and E. Plantamour (1864), and R. Wolf (1869). There were also at this time at least six different determinations of the rate of the sensory impulse (1861-1868). Priority in the attempt to analyze the reaction times into psychophysiological elements seems to belong to J. J. de Jaager (1865), a Dutchman, but the fundamental study is that of another Dutchman, F. C. Donders, a few years later (1868). Donders really invented the compound reaction and the subtractive procedure. He employed two stimuli in the reaction experiment. In one method, where the subject responded to one stimulus and refrained from reacting to the other, Donders attributed the increased time of reaction, over the simple reaction time, to the time required for discrimination. The still further increase in time, when the subject is required to respond with one hand for one stimulus and with the other hand for the other stimulus, he considered as caused by the addition of choice to the discrimination.

Except for the birth of the compound reaction in Holland, the new psychology in the '60's was still almost entirely a German affair. It was not yet to be found in France, England, or America. In France, P. Broca, by his dramatic discovery of the speech center in 1861, had overthrown Flourens's theory that the mental functions have no specific localization in the brain. Charcot's work began at about this period, for it was in 1862 that he first formed the connection with the Salpêtrière that lasted throughout his life. However, his important contributions to abnormal psychology came later with his adoption of the method of hypnosis. All the modern work in abnormal psychology owes a certain debt to the German philosopher, Eduard von Hartmann, who published his *Philosophie des Unbewussten* in 1869, thus laying a foundation for the doctrine of the subconscious mind that was to emerge much later.

In England there were three very important books published in this decade. The first was John Stuart Mill's *Examination of Sir William Hamilton's Philosophy,* 1865. Bain was perhaps the foremost British psychologist, but Mill, a much greater man, had a correspondingly greater influence, not only at home but also abroad. Then there was Henry Maudsley's *Physiology and Pathology of Mind,* 1867, an influential book which forms the background for the tradition for medical psychology in Great Britain. Finally, still much more important for psychology, there came in 1869 Francis Galton's *Hereditary Genius,* the book that marked the first impact of Darwinism upon psychology, and that foreshadowed Galton's contributions to mental inheritance, eugenics, mental tests, and statistical measurement. These events were, however, isolated. English psychology was still associationism.

The Second Decade of Experimental Psychology: 1870-1880

In the '70's there appeared more certain signs that the new psychology was indeed to be a new subject-matter, and its character became largely determined by the further development of the tendencies of the '60's.

The principal activity was, of course, in Germany, and the most important single event of the decade was the publication by Wundt of the first edition of his *Physiologische Psychologie* in 1873-1874, just as he was accepting the chair at Leipzig. This book, with its systematic completeness and its exhibition of the applicability of the experimental method to a wide range of problems, created, or, perhaps we should say, defined, psychology as something that is neither philosophy nor physiology. Almost simultaneously Brentano published the *Psychologie vom empirischen Standpunkte,* 1874. It was a fortunate coincidence. The new psychology needed the guidance of competent opposition. These two books, Wundt's and Brentano's, while both supporting the empirical approach to psychology, represented opposing views of psychology. Each became, therefore, a starting point for a tendency in psychology, and the two tendencies are still with us to-day. Wundt represents the empiricism of the laboratory, Brentano the empiricism of philosophy. There has been a constant correction

of one view by the other, and the issue between them has not yet been settled, certainly not in Germany.

At the very end of this decade, in 1879, Wundt founded the psychological laboratory at Leipzig. The date is less significant than 1874 because books appear suddenly, no matter how long they have been in the writing, whereas laboratories come gradually into being. However, this laboratory was the first formal institutionalized laboratory of psychology in the world, and it became almost immediately the primary source of experimental research in the new psychology. For many years there was no place in the world from which so much experimental psychology issued as from Wundt's laboratory. It was for two decades the place where young Americans went to learn the new science.

In France in the '70's there were signs of an awakening. The decade began with the publication of H. A. Taine's *De l'intelligence* (1870), the book that formed the historical background for Binet's work and that initiated the French tradition that the normal mind is to be understood by a study of the abnormal. In the same year Th. Ribot undertook to introduce English psychology to France in his *La psychologie anglaise contemporaine* (1870). Of course, experimental psychology did not yet exist in England, and Ribot was importing associationism into France, a psychology that it was already disposed to take seriously because of the tradition of Condillac and the later ideologists, like Maine de Biran. Anyhow, experimental psychology was hardly well enough established for importation in 1870. At the close of the decade, when experimental psychology had found itself, Ribot performed the same service for it in *La psychologie allemande contemporaine* (1879).

In England Darwinism was the topic of the hour. Darwin published *The Descent of Man* in 1871, and then, in the following year, brought his theory to bear upon mental evolution by his *Expression of the Emotions in Man and Animals* (1872). It would not be correct to regard this latter book simply as representing one phase of the theory of evolution, for it is really the ancestor of all evolutionary psychology. Comparative psychology, functional psychology, and, in a way, most American psychology are its descendants. Indirectly, by way of Galton, Darwin is a remote ancestor of the mental tests and of statistical measurement in psychology.

It is plain that both France and England were aware that a new psychology had come into being. In 1876 Ribot founded the *Revue philosophique* and Bain founded *Mind*. Both these journals were organs for the presentation of a new psychology, but, because of the date of their founding, they did not recognize that psychology was about to assert independence of philosophy. They were respectively the national media for experimental work until later the necessity for distinctively psychological journals made itself felt.

We should also note that James Sully, the interpreter of the new psychology to England, began his influence in this decade. His first important book, *Sensation and Intuition*, appeared in 1874, the year of Wundt's and Brentano's famous texts.

In America there was as yet nothing that was public. However, William James, then a physiologist by title, was aware of what was coming about in physiological psychology. He lectured on the new subject-matter and established at Harvard (*ca.* 1876) an informal laboratory for the drill of his class in experimental work. This laboratory of course antedated Wundt's, but it was not the same sort of institution. In fact, it was not an institution at all, but a pedagogical device. Almost no research issued from it; and we are right in saying that Wundt founded the first formal laboratory of psychological research in the world.

We may return now to the more specific developments in the experimental psychology of the '70's, and we find of course that *sensation* was the most important topic of investigation, but that other senses than vision were beginning to claim attention. The extent to which the psychophysiology of sensation advanced is shown by the content of the third volume of L. Hermann's *Handbuch der Physiologie* (the volume covers *Die Physiologie der Sinnesorgane*), which appeared at the end of the decade (1879). Here A. Fick, W. Kühne and E. Hering wrote the chapters on vision, V. Hensen the chapter on hearing, Hering and O. Funke the chapters on touch and bodily sensibility, and M. von Vintschgau the chapters on taste and smell.

In the field of *vision* the most important event of the decade was the appearance of Hering's *Zur Lehre vom Lichtsinne* (1872-1874). In his earlier work Hering had been more concerned with the physiology and physics of vision, much as Helmholtz had been. In the *Lehre vom Lichtsinne* he showed himself a psy-

chologist in the days when to be a psychologist was to be an introspectionist and to examine immediate experience as dependent upon stimulation or other psychophysiological conditions. For most introspectionists the visual experience seems to require, for its systematic organization, reference to four colors—red, yellow, green, and blue—in addition to the whites, grays, and blacks. Helmholtz's three-color theory, which asserted further the complexity of white, was physically sound as against the laws of mixture, but introspectively absurd. Hering, therefore, like Goethe long before him, proposed a four-color theory and worked out a physiological hypothesis of the retina to explain most of the facts of color mixture, adaptation, and after-image. With certain modifications by Müller and von Kries, this theory still holds its own against others, although there has never been a particle of direct evidence that the retina contains three color substances each capable of antagonistic anabolic and catabolic processes. In many other ways Hering showed that he had a gift for introspective observation as well as for the devising of demonstrational apparatus, and this book, although small, is probably, after Helmholtz's, the most distinguished classic in the psychology of vision.

There were other books on vision in the decade. Vierordt described an apparatus for working with spectral light and published some of his experimental results (1871). Aubert issued his second important work, *Grundzüge der physiologischen Optik* (1876), a book that dealt mostly with the dioptrics of the eye, eye-movement, and visual space-perception, and that thus supplemented his account of the physiology of the retina nine years earlier. Johannes von Kries began his researches on vision (1877), although his important contributions came later. F. C. Donders, the Dutch physiologist and pioneer in the compound reaction, published his discussion of eye-movements (1875), whence we now have Donder's law. There was an important study of the discrimination of hues by W. Dobrowolsky (1872). Hering, writing in Hermann's *Handbuch* (1879), added much that was new, and developed further his nativistic theory of visual space-perception.

In the field of *audition* W. Preyer, best known for his work on child psychology, appeared as an investigator. First he produced his little monograph on the determination of the limits of hearing (1876); then his *Akustische Untersuchungen* (1879), which dealt with the perception of overtones, combination tones, the tonal dif-

ferential limen, and a theory of consonance. Hensen, in Hermann's *Handbuch,* presented a theory of hearing (1879), one of the 'frequency' theories that are still in opposition to the resonance theory of Helmholtz.

Related to the topic of audition is the problem of the constitution of the vowels. Helmholtz (1863) had analyzed the vowels into sets of partials lying in certain regions and reinforced by resonance of the mouth cavity, and he had constructed a famous apparatus of tuning-forks, a fundamental and its partials, whereby vowels could be synthetized to his satisfaction. Donders (1870) obtained a similar analysis, partly by determining the resonance of the mouth cavity when formed for a particular vowel and partly by the analysis of phonograms of vowels. Rudolph Koenig (not Arthur König, the investigator of visual phenomena, but the man whose name is associated with the splendid Koenig tuning-forks and the Koenig resonators) obtained similar results by the analysis of the records of vowels with the manometric flame. The problem of vowel analysis has recurred up to the present day as new methods in experimental phonetics and psychology have presented themselves. Although attention is generally claimed by the divergence of results in these various studies, the fact remains that they all agree within rough limits as to the regions of pitch that characterize the vowels and thus in placing the vowels *ū, ō, ā, ē, ī* in an ascending order as regards their distinguishing pitches.

There was some progress in the '70's in the experimental psychology of *taste,* for M. von Vintschgau performed numerous experiments in 1875-1877 and included the results in his chapter on taste in Hermann's *Handbuch* (1879). Knowledge of the psychology of *smell,* however, did not advance, nor was there to be much progress until Zwaardemaker's work twenty years later.

The psychology of *touch* was advanced by Vierordt's research on the two-point limen (1870) and the study of cutaneous localization by R. Kottenkampf and H. Ullrich (1870). Hering, in Hermann's *Handbuch* (1879), put forward a theory of the sense of temperature patterned after his theory of color vision. He assumed the existence of opposing anabolic and catabolic processes which account for the occurrence of the sensations of warmth and cold, and, by approaching equilibrium, for thermal adaptation and negative after-image. The theory is important because there was little else in this period that was new. Advance in the knowledge

of cutaneous sensibility was waiting upon the discovery of the punctiform distribution of the cutaneous receptors, an event that occurred in the following decade.

The '70's also brought a new insight into the psychophysiology of the perception of *rotation.* Mach's monograph, *Grundlinien der Lehre der Bewegungsempfindungen* (1875), is the classical study of the problem, which had not advanced far since Flourens's day (1828). Mach invented a rotation frame (a gigantic affair which used to encumber the Clark and Cornell laboratories in their early days) whereby a human subject could be rotated about axes within or without his own body. Mach described the perception of rotation and its negative after-image, and the way in which the plane of rotation of the after-image moves with the head. From these data he argued to the familiar theory of an ampullar sense, excited by movement of the cristæ in the ampullæ of the semicircular canals. There were also two other students of this problem at work at the same time: Crum Brown (1874) and J. Breuer (1874-1875). The latter continued his reseaches (1888, 1897) and the theory is generally known to-day as the Mach-Breuer theory.

The problems of vision naturally led philosophizing psychologists into the theory of *space-perception.* In this decade the lines were being drawn between the empiricists and the nativists. Helmholtz had already espoused empiricism and Hering nativism. In 1873 Stumpf joined the camp of the nativists with his *Ueber den psychologischen Ursprung der Raumvorstellung,* the monograph that got him his chair at Würzburg and that was his first psychological work. Wundt always held with the empiricists and the *Physiologische Psychologie* (1874) reinforced this side. Helmholtz undertook to clinch his views in *Die Thatsachen in der Wahrnehmung* (1878), written several years after he had gone as physicist to Berlin, and his last important venture into the psychological domain. Hering (1879) reaffirmed his position in Hermann's *Handbuch.* It was nip and tuck between the two views.

Meanwhile *psychophysics* was continuing upon the course on which it had started. Fechner, who had only intended in the *Elemente* to settle a philosophical question, had deserted psychophysics for esthetics during a ten-year period. However, the unexpected effect of his original work upon experimental psychology —not upon philosophy, as he had hoped—forced him back into

the field. He published *In Sachen der Psychophysik* (1877), a book which is, however, much less original than the earlier *Elemente* or the later *Revision*. He had probably been too much engrossed in esthetics. More important than Fechner in the psychophysics of the '70's was the entrance of the Belgian, J. R. L. Delbœuf into the field. He published his *Étude psychophysique* in 1873 and his *Théorie générale de la sensibilité* in 1876. Beside reporting research, Delbœuf made a fundamental contribution to theory, for he developed the concept of the sense-distance which proved an effective instrument for combating the 'quantity objection' which has been pressed against Fechner. Fechner thought that if sensations are to be measured, every sensation must have an absolute magnitude measured from a zero-point. Magnitude is not introspectively obvious in sensory experience. Delbœuf showed that measurement requires only the arrangement of sensations on a measurable scale, that no zero-point and thus no absolute magnitude is necessary. At the end of the decade came G. E. Müller's *Zur Grundlegung der Psychophysik* (1878), which he had written as a criticism and revision of Fechner's methods and as an exposition and interpretation of the facts of Weber's law. The book was important enough for Fechner formally to take account of it (1882). Among the many technical matters that it contains it is perhaps most interesting to note that Müller appreciated, as Fechner did not, the fact that the limen is a variable quantity. Fechner thought of it as a fixed point determined with a variable error. Müller's notion avoids many pitfalls, of which the most obvious is that no single observation can ever alone be significant in the determination of a limen.

Other problems of the '60's continued into the '70's. Exner, following Donders, undertook an analysis of *reaction times* (1873), and, following Vierordt, an investigation of the *time-sense* (1875).

Altogether, however, it will be seen that experimental psychology had not yet advanced far beyond the topics of sensation and perception. Nevertheless, there were beginnings upon the 'higher' mental processes. G. E. Müller's doctoral dissertation with Lotze was on *attention: Die Lehre der sinnlichen Aufmerksamkeit* (1873). Titchener found it still in 1908 one of the two most important sources of information on the problem of the compulsory conditions of attention. It is also worth while to mention that W. S. Jevons, the logician, at this time reported the results of a

trivial experiment upon the range of attention (1871), a problem that was to receive experimental treatment in Wundt's laboratory in the next decade.

Just as James was working quietly in the new psychology at Harvard in the '70's, establishing a student laboratory and delivering lectures, so Ebbinghaus was making of himself a psychologist without the world being any the wiser. It must have been about 1876 that he discovered a copy of Fechner's *Elemente* in a Paris book-stall, and he immediately went to school to Fechner in those years in France and England, presumably without ever having seen Fechner in the flesh. Perhaps it was his physical contact with English associationism that gave him the idea that *memory* ought to be experimentalized as Fechner had experimentalized sensation. At any rate, he started his famous experiments in 1879, alone, a year before he returned to Germany to be habilitated as *Dozent* at Berlin.

It was also in 1879 that Galton in London sent out his famous questionary on ideational types. Here was another way of bringing a quasi-experimental method to bear upon the problems of memory and thought. The 'higher' processes were beginning to yield.

Experimental psychology proper, as we have said, was still confined to Germany. *Abnormal psychology* was, however, beginning to show life in France and to a less extent in England. In France it was Charles Richet's attestation of the genuineness of hypnotic phenomena (1875), followed by his report of experimental and clinical researches on sensibility (1877), that started the new work. Charcot had published his *Leçons sur les maladies du système nerveux* in 1873, but his most influential work at the Salpêtrière began with his adoption of the method of hypnosis in 1878. Over in England the evidence for continued interest in abnormal psychology that Maudsley began, lies in his division of his first book into two revised and extended parts: the *Physiology of Mind* (1876) and the *Pathology of Mind* (1879).

In England Galton was continuing his work on *mental inheritance.* His study of English men of science, supplementing *Hereditary Genius,* appeared in 1874. Two years later he published his very significant study of twins (1876), appealing there to the natural phenomenon of twinship for the demonstration of inheritance, in much the same way that twinship is still used as a

means for studying this problem. Much less influential but dealing with the same general topic was Ribot's first book, *L'hérédité psychologique* (1873).

During the '70's *physiology* was also making discoveries that were to have a direct effect upon the new psychology. In 1870 G. Fritsch and E. Hitzig discovered, by electrical stimulation, the supposedly precise localization of motor functions in the post-central region of the cerebral cortex. Broca (1861) had cast doubt upon Flourens's contention that there is no specific localization, and this discovery seemed finally to establish specificity. It was so remarkable that many investigations were soon under way. Perhaps the most notable were the researches of David Ferrier, whose book in 1876 appeared to establish the fact that different members of the body depend for their movement on different small areas of this region of the cortex. Recent research has thrown some doubt upon the degree of specificity that is actually to be found, but fairly exact localization was the accepted fact for several decades and the conviction had its effect upon the course of psychology. It was also at this time that C. Golgi discovered the staining method which best brings out the nervous structure of the brain (1873) and which led presently to the realization that the brain must be considered as a network of fibers. It was not until much later that the synapse was discovered and the neurone theory invented; nevertheless the cerebral physiology of the '70's meant something for psychology. It meant essentially that the brain, the organ of the mind, is composed of 'centers,' each having a specific function, and that all these 'centers' are connected in a myriad of ways by innumerable fibers. It was as if the physiologists had discovered associationism. It required but a step for the psychophysical parallelist to argue that the mind is composed of a myriad of ideas, connected for the purposes of sensation, perception, movement, memory, and thought, by innumerable associations. Had it not been for physiology, we might never have had psychological elementarism in a serious form to combat.

There remains for mention only Fechner's excursion into experimental *esthetics*. His interest and publication in esthetics were of the decade 1865-1876. His first experiments were made about 1871, for previously he had been interested in the problem of the two competing Holbein Madonnas. The culmination of his work is the *Vorschule der Aesthetik* (1876). While esthetics is not in itself

psychology, in Fechner's hands it was psychology. He originated the methods for dealing experimentally with certain problems of esthetic judgment, and he began what Wundt later called the method of impression for the investigation of feeling.

The Third Decade of Experimental Psychology: 1880-1890

The infancy of experimental psychology was in the '60's. Its character was formed in the '70's. In the '80's it became of age, still quite inexperienced but ready to be treated as a man. The perspective of the decade shows Wundt's laboratory in full swing in its attempt to bring other topics than sensation and perception under the experimental method. In this effort Leipzig opened up the fields of attention and reaction, and, by way of the reaction experiment, the chronometric method of the analysis of mind, the compound reactions and the subtractive procedure. Even more important, because more successful, was Ebbinghaus's invention of methods of measuring association and memory. Külpe described Ebbinghaus's research as initiating the third and most recent period of experimental psychology, the period in which the 'higher' processes succumbed to the experimental method. In this decade we find America busy with the importation and interpretation of the new German psychology, and starting rapidly in its attempt to overtake Germany. In England it was the period of Darwinism in psychology and thus of the beginnings of animal psychology. These were the important occurrences. There was a great deal of research and theorizing in Germany and America and also of the writing of books. The James-Lange theory of emotion was formulated, and the psychophysiology of the skin opened up by the discovery of the sensory spots on it.

We may begin with a list of the important books that appeared in Germany. More than half of them came from the prolific Wundt. The second edition of the *Physiologische Psychologie* came out in 1880, and the third in 1887, the *Logik* in 1880-1883, the *Ethik* in 1886, and the *System der Philosophie* in 1889. We have seen elsewhere how Wundt the philosopher was still the psychologist. There was much psychology in the *Logik*, and the *System* was meant to be a science of philosophy. The new editions of the *Psychologie* were expansions, but the important changes

came later. Wundt founded the *Philosophische Studien* in 1881 and thereafter all the researches from his new laboratory were published in it. It was for eight years the sole organ in Germany devoted exclusively to the new psychology.

Besides Wundt, Lipps was the only psychologist writing systematic texts in Germany in the '80's. Ebbinghaus started late; Stumpf and Müller never came to this sort of work; Brentano was not prolific; men like Külpe were too young. Th. Lipps published his *Grundtatsachen des Seelenlebens* in 1883, and in 1885 his *Psychologische Studien.* Lipps was at best, however, only on the periphery of the new psychology. Hugo Münsterberg, from Wundt's laboratory but not shaped to the Wundtian mold, issued his *Beiträge zur experimentellen Psychologie* from his new laboratory at Freiburg in 1889-1892, after which he was called to Harvard. Mach's *Analyse der Empfindungen* came out in 1886. and Richard Avenarius's difficult *Kritik der reinen Erfahrung* in 1888-1890; these were the books upon which Külpe and Titchener later leaned for epistemological support in their definitions of psychology. The *Analyse* was in addition a work that included many interesting psychological observations, for Mach, the physicist, had almost as much of the gift for psychological observation as did Hering, the physiologist.

The very end of the '80's and the beginning of the '90's saw great activity in the founding of new psychological *laboratories* in Germany, a wave of laboratory-founding that was coincident with a similar wave in America. There seem to be no exact records readily available for Germany, but at any rate there were in 1892 laboratories at Göttingen (G. E. Müller), Berlin (Ebbinghaus), Freiburg (Münsterberg), and Bonn (Lipps); at least localized collections of apparatus at Munich (Stumpf) and Prague; and some opportunity for experimental work at Heidelberg, Strassburg, Zürich, and Halle. By 1892, however, America, the land of organization, had outstripped Germany in laboratories, for at this time it had 'founded' at least fifteen.

America was then in the '80's the second most active nation in the prosecution of the new psychology. Stanley Hall founded the Hopkins laboratory in 1883, the first in America unless we count the student-laboratory of James at Harvard. The Hopkins laboratory was intended to be a research laboratory like Wundt's, but it never succeeded in becoming so important. For one thing, Hall

soon left Hopkins to go to Clark, and the laboratory lapsed for a time. By 1890 the Universities of Indiana, Wisconsin, Nebraska, Michigan, Iowa, and Pennsylvania, and Columbia University all had laboratories. Cornell and Wellesley followed in 1891, Harvard formally in 1892. By 1900 there were twenty-six altogether.

In 1887 Stanley Hall founded the *American Journal of Psychology*. It was in a sense a new kind of journal. It was strictly limited to the 'new' psychology as the *Philosophische Studien* had not been, for some philosophical articles crept into the *Studien*. The *American Journal* had some degree of catholicity at the start, although 'outsiders' complained that it was made to serve primarily the interests of Hall and his associates in other universities. The *Psychological Review* was later begun to offset this limitation, just as the *Zeitschrift* was founded in Germany because the *Studien* was dominated by Leipzig. The *American Journal*, however, showed from the start how conscious was America of its new endeavor; it printed book reviews, minor experimental studies, and notes and news. The *Studien* had carried major articles only.

The most important American book of this decade was G. T. Ladd's *Elements of Physiological Psychology* (1887), a direct effort to 'get up' the new psychology for American consumption, and an effort that succeeded admirably. John Dewey's *Psychology* (1886) was not very important, for it was suspect of philosophy; nevertheless it had its place and foreshadowed Dewey's later contribution to functional psychology. J. M. Baldwin published the first half of his *Handbook* in 1889, but he really belongs to the '90's.

England, engrossed in Darwinism and animal psychology, brought out little of general import in this decade. James Sully published his *Outlines of Psychology* in 1884, and James Ward wrote his first article on psychology for the *Encyclopædia Britannica* in 1886. To Galton's and Romanes's work we shall come presently.

France was absorbed in abnormal psychology, and Ribot was publishing his books. The most important event in experimental psychology was the foundation of the first laboratory, at the Sorbonne, by Henri Beaunis and Alfred Binet in 1889.

If we turn now to the field of sensation, we find that experimental psychology was at last beginning to outgrow its preoccupation with this topic. There has always been a great deal of re-

search in sensory psychophysiology, but in the '80's there were not many highly significant developments when viewed from the historical perspective. In the field of *vision,* von Kries published his first book on the analysis of visual sensations in 1882, and Aubert published his last book on visual orientation in 1887. Neither is so important as some of the other work of its author. Ebbinghaus turned from memory to some visual experiments; he made a study of brightness contrast (1887) and a determination of the applicability of Weber's law to brightness (1889). His color theory came only a little later (1893). By far the most striking publications in the whole field of sensation in this decade were the two volumes of Carl Stumpf's *Tonpsychologie,* which were issued in 1883 and 1890. There had been nothing of equal moment about *hearing* since Helmholtz in 1863. Of course these volumes of Stumpf's were not entirely on auditory sensation and perception. They dealt with the psychology of music and also with many systematic matters as remote from sensation as Stumpf's theory of attention. However, they marked Stumpf out as the *Tonpsychologe* of Germany, and his data and theory on tonal fusion (1890) still go into most textbooks of psychology. Mention should also be made here of W. Rutherford's 'telephone' theory of hearing (1886-1887) and E. Aronsohn's two papers on *smell* (1886, 1894). Nothing more needs to be said about the psychology of the special senses in the '80's.

In the middle of the decade there occurred the interesting case of the independent and simultaneous discovery of the sensory spots on the skin by three different investigators, M. Blix, A. Goldscheider, and H. H. Donaldson. Blix was a Swede, Goldscheider a German, and Donaldson the American neurologist, then a student in Stanley Hall's laboratory at Hopkins. Up to this time there had been no satisfactory classification of the *cutaneous sensations* and research had languished for lack of one, just as it has always for the same reason failed to develop in the field of smell. There were names for perceptual experiences: hot and cold, wet and dry, hard and soft, rough and smooth, and so forth; but these led nowhere. Presumably no one had thought of exploring the skin with small stimuli, just because one does not work with the retina in this way. It was actually Johannes Müller's incorrect theory of the specific energies of nerves that led Blix to hunt with small temperature points for specific receptors in the skin, and he was of course

rewarded with the discovery of the warm and the cold spots. He published in 1883. Meanwhile Goldscheider had made the same discovery and also found that pressure and pain show a punctiform distribution of end-organs. He published in 1884. Donaldson had been getting the same results. He read the papers of the other two men and published confirming them and emphasizing experimental technique, also in 1885. The way for research was now open. Psychologists knew that there are four fundamental modalities of tactual sensation instead of an indeterminate number, and they could proceed to investigate them.

There was little else in this decade of importance in the field of cutaneous sensibility. The two-point limen was still being investigated, this time by W. Camerer (1883, 1887). Goldscheider's researches on the skin actually began in 1881 and continued into the '90's, when they were brought together in his *Gesammelte Abhandlungen* (1898). He also went into the field of kinesthetic sensibility (1887-1893), and the conventional classification of *kinesthetic sensations* into muscular, tendinous, and articular is due to him. We should mention further G. E. Müller's monograph on muscle contraction (1889) and his better-known research on lifted weights with F. Schumann (1889). The latter paper gained in importance because it is part of the classical literature of psychophysics, where lifted weights have been, ever since Fechner, the preferred medium of research.

In *psychophysics* proper we must note that Fechner replied to Müller's criticisms of 1878 with a partial revision of his views: *Revision der Hauptpunkte der Psychophysik* (1882). Delboeuf published his *Examen critique de la loi psychophysique* (1883) and reprinted his earlier papers under the title *Éléments de psychophysique* (1883). In America, Joseph Jastrow and C. S. Peirce published their study of small differences in sensation (1884), the study in which they eliminated the category of equality from the judgments, thus eliminating also the possibility of computing a limen, and substituting as a corresponding measure the probable error of observation. This is the view that was emphasized later by Fullerton and Cattell (1892). The conception is an early example of the American tendency to mistrust introspection and to rely on behavior, because the establishment of the category of equality is difficult and depends solely upon introspection for evidence of its reliability.

Perception was not an important topic of research in the '80's. Sully's little book on optical illusions came out early (1881). G. Martius's paper on the relation of visually perceived size to distance (1889) was important. F. C. Müller-Lyer first published an account of the illusion that bears his name (1889), but this paper really belongs with all those others on optical illusions that came out in the '90's.

It was, however, in the '80's that *attention* came under experimental investigation, for the most part in Wundt's laboratory. Hamilton and Jevons had had something to say about the range of attention. J. McK. Cattell, with the tachistoscopic method, measured it at Leipzig (1885-1886). G. Dietze, also Wundt's student, published the classic paper on rhythm and the range of attention for successive stimuli (1885). W. von Tschisch took up the complication experiment, which Wundt had salvaged from the astronomers, and published the standard paper on prior entry (1885). The view that attention has a limited duration also gained ground because very faint stimuli give rise to fluctuating sensations. This discovery was first described by V. Urbantschitsch (1881-1882) and the facts were studied further by Nicolai Lange (1888). All those papers are experimental. A. Pilzecker published his *Lehre der sinnlichen Aufmerksamkeit* (1889), the companion monograph to G. E. Müller's of the same title, that is to say, an examination of the compulsory conditions of attention. In France there was Ribot's *Psychologie de l'attention* (1889). Certainly by the end of the '80's it seemed as if the new psychology had triumphed over the difficult problem of attention, and researches of this type continued in the '90's. Of course we are no longer so sure that it was really 'attention' that was being investigated, but all we need to accomplish here is to picture the enthusiasm of the time.

It was largely in the '80's that the *reaction experiment* as the means to a mental chronometry had its vogue. The period for it is really the decade 1883-1892. These were the days when it was possible actually to measure the times of apperception, cognition, discrimination, association, and will. If measurement is the test of successful experimental science, then experimental psychology had indeed come of age—or at least so it seemed. The method of the subtractive procedure was simple. The sensorial reaction, Wundt held, differs from the muscular by the time required for apperce-

tion. Cognition and discrimination, which occur in slightly different situations, require each an increment of time over the sensorial reaction. Association takes an increment in addition to cognition, and will an increment in addition to discrimination. The important papers, all from the Leipzig laboratory were these: on apperception time, M. Friedrich (1883), L. Lange (1888); on cognition time, J. McK. Cattell (1885), E. B. Titchener (1892); on discrimination time, E. Tischer (1883); on association time, M. Trautscholdt (1883); on will time, J. Merkel (1885); on various of these times, G. Dwelschauvers (1891), G. Martius (1891-1892), O. Külpe (1891-1892). It was Külpe, however, who effectively criticized the subtractive procedure on the ground that the increment of difference in time represents no separate psychophysiological process, since the whole consciousness is changed in the different situations. Trautscholdt's paper has perhaps to-day more significance than any of these others, because he was actually measuring association times, and the association reaction has become standard. Coming before Ebbinghaus, Trautscholdt nevertheless missed the best approach to association, and contented himself on the qualitative side with the logical classification of verbal associations—the 'statistics of association,' Wundt called it.

Cattell was Wundt's most energetic and prolific student at this time. He published many papers in the study of reaction, and his *Psychometrische Untersuchungen* (1885-1886) contain in them the beginnings of the effort to apply psychological methods to the problem of individual differences, an idea which Wundt said was "*ganz amerikanisch*," as indeed it proved to be.

Preëminent among all these papers was Ludwig Lange's discovery that there are two types of simple reaction, the sensorial and the muscular, as he called them, and that the difference between them depends upon the predisposing direction of attention (1888). Actually it proved later that he had been anticipated in this discovery by S. Orschansky (1887), but it was Lange's experiment that brought the matter into the proper setting. Not only did this finding give Wundt the chance to claim that the difference between the two types, about a tenth of a second, was due to the presence of apperception of the stimulus in the sensorial reaction; the discovery also set new standards of control in the interests of precision, brought attention in a new way into the laboratory, and

provided a plausible explanation for the absolute personal equation that the astronomers had found.

For all the activity of Wundt's laboratory, there is no doubt that the most important single research of the decade was Ebbinghaus's investigation of *memory*. As we have seen, he began the experiments when he held no university post; he completed them at Berlin; and he published *Ueber das Gedächtnis* in 1885. The application of statistical measurement to associations, the invention of the nonsense syllable and of the methods of complete mastery and savings, and the working-out of a large number of the laws of association was no mean accomplishment. It would perhaps have been no more important to measure memory than to measure attention, cognition, and will; but the historical result was that the Leipzig experiments led to relatively little, whereas Ebbinghaus's work initiated a great mass of research that still continues, and at one time seemed about to become the basis for an applied psychology of education. Perhaps, as many now think, Ebbinghaus was wrong in considering frequency as the sole condition of the formation of associations; nevertheless we should never have arrived even at that conclusion without the work which he began.

It should be observed that *feeling* and *emotion* had not yet in the '80's entered the psychological laboratory to any considerable extent. Still there were, outside of Germany, signs of concern about these topics. William James first formulated his theory of emotion in 1884, amplified it in 1890, and modified it in 1894. C. Lange published his similar theory in 1885 in Danish, and it was translated into German two years later. The theory is, of course, that the consciousness of emotion is nothing other than the consciousness of bodily states that are automatically induced by the perception of the situation that proves to be emotional. James's theory was quite general as to the variety of bodily states that might be induced and that might thus contribute to the emotion; Lange emphasized the vasomotor changes. The James-Lange theory is still popular, and was responsible for Cannon's important research thirty years after its original statement.

In France there was C. Féré, who became interested in feeling and its bodily expression (1887) and who had something to do with the discovery of the psychogalvanic reflex (1888). In Italy there was A. Mosso, whose work on the expression of feeling

began about 1884. However, the development of this topic waited upon the research of Lehmann and the pronouncements of Wundt in the succeeding decade.

In general, France still held to *abnormal psychology*. Th. Ribot's books on the diseases of the memory (1881), of the will (1883), and of the personality (1885) show the trend. C. Richet published *L'homme et l'intelligence* (1884). Binet published *La psychologie du raisonnement* (1886) and thus began the line of research that was to make his name known practically throughout the world.

In England Francis Galton was continuing his study of inheritance and of *individual differences*. The *Inquiries into Human Faculty* appeared in 1883 and *Natural Inheritance* in 1889. The former book stands as much at the beginning of mental testing in general as Binet's work stands at the head of the intelligence testing.

It was in the '80's that comparative or *animal psychology* began, under the influence of Darwinism and the need of the theory of evolution for demonstrating mental continuity in man and animals. G. J. Romanes was the leader and originator of what later was called the 'anecdotal method.' His three books were: *Animal Intelligence* (1882), *Mental Evolution in Animals* (1883), and *Mental Evolution in Man* (1888). There was also important work on the psychology of insects in progress in England and France; for example, Sir John Lubbock (1882), J. H. Fabre (1879-1904), and A. Forel (1887). Binet published his little book on the psychic life of protozoa in 1888.

There remain in this decade only two incidental events for mention. Preyer published his *child psychology*, the first that had ever been written, in 1882. Galton founded the modern *statistical methods* that psychology uses, by formulating his theory of regression in 1886 and his theory of correlation in 1888.

The Fourth Decade of Experimental Psychology: 1890-1900

The '90's were the period of the democratization of experimental psychology in Germany and America. Its control passed from the few to the many. Müller and Stumpf were becoming forces quite independent of Wundt. Wundt's pupils were scattering in both Germany and America, and were not always continuing to follow

Wundt's lead. As we have seen in the last section, there was a wave of laboratory-founding that began in the late '80's and extended through the '90's in both Germany and America. The *Zeitschrift* was founded in Germany to offset the dominance of Leipzig and to be a more catholic organ of the 'new' psychology than was Wundt's *Studien*. The *Psychological Review* in America had somewhat the same motivation. Many books were written from the new point of view as each author interpreted it. Americans who had studied in Germany were still attempting to expound German psychology in English, by texts or translations or reviews. America was also learning to stand alone, and the character of its psychology was becoming vaguely defined.

The decade saw the decline of the reaction experiment as a method of mental chronometry. Experimental research on sensation and attention continued, with distinctly increased vitality in the field of vision. There arose a great interest in optical illusions, so much so that this decade has been called the decade of the illusion. Ebbinghaus's pioneer work in the experimental psychology of memory bore fruit in a vigorous development of this field, a development which Müller dominated. Experimental work on feeling began, and Wundt's new theory of feeling led to a general interest in the method of expression. However, research on feeling did not reach its mode until the next decade. Animal psychology passed from the anecdotal to the experimental method, and from a concern with the validity of the theory of evolution to an interest in the animal mind for its own sake. The problem of objectivism appeared in animal psychology. Individual psychology and the mental tests were getting under way in America and France, but the modern concept of intelligence was lacking to give direction to the work. Such was the decade. We may now turn to the details.

In Germany there were a number of important books. Wundt published the fourth edition of the *Physiologische Psychologie* (1893), somewhat changed from the other editions, but not so radically as the fifth was later to be. He published his *Grundriss der Psychologie* (1896), with the new tridimensional theory of feeling in it. Külpe, while still at Leipzig, published his *Grundriss der Psychologie* in 1893. It was the clear, direct exposition of the younger man. Wundt's thought was becoming cumbersome; he was involved in his elaborate past. Psychology needed the

fresh attack of youth, and Külpe gave psychology a text that was in many respects an experimental handbook, for he set out to expound only those topics where there was experimental foundation for his writing. Ebbinghaus, more popular in his style than Külpe, but less systematic, the William James of Germany, began his *Psychologie,* the first part of which came out in 1897. The book really belongs, however, to the next decade. Th. Ziehen and Th. Lipps were less influential in experimental psychology than these others, but their books helped to complete the constellation of the decade. Ziehen wrote *Leitfaden der physiologischen Psychologie* (1891), and Lipps the *Raumaesthetik* (1893-1897).

In America the decade was introduced by the publication, after twelve years in the writing, of William James's *Principles of Psychology* (1890). No other psychological treatise in the English language has in the modern period had such a wide and persistent influence. James accepted experimentalism in psychology only with some reservations, although he faithfully presented the researches of the laboratory as far as they had gone. His influence was not upon experimental psychology directly, but upon psychology in general. The roots of American functionalism lie, in part, in him. Ladd and Baldwin were prolific in books. Ladd published *Psychology, Descriptive and Explanatory* in 1894. His other books were less important, and none had the influence of the *Physiological Psychology* of 1887. James Mark Baldwin began with his *Handbook of Psychology: Senses and Intellect* (1889) and *Feeling and Will* (1891). Then there followed his works on evolutionary psychology: *Mental Development in the Child and the Race* (1895) and *Social and Ethical Interpretations in Mental Development* (1897). Stanley Hall was not yet writing books, but was struggling with the problem of the inadequate support for Clark University. Titchener, who went to Cornell from Leipzig in 1892, was busy translating Wundt and Külpe into English, and published in 1896 his *Outline of Psychology,* a book similar to Külpe's *Grundriss* and not so good.

In England, Sully and Stout were the more active psychologists. Sully's *Human Mind* came out in 1892; Stout published the *Analytic Psychology* in 1896 and his *Manual* in 1899.

Meanwhile the new experimental science was waking up to the need for laboratory courses in the universities, such as were to be found in the older sciences. E. C. Sanford at Clark began the

publication of a *Course in Experimental Psychology* in 1891, although the completed volume did not appear until 1898. Titchener was by then working on his *Manuals,* which did not appear, however, until after the turn of the century. Binet, with a new laboratory at the Sorbonne, felt the same need, and published *Introduction à la psychologie expérimentale* in 1894.

Psychological journals were being founded: the *Zeitschrift für Psychologie* in 1890 by Ebbinghaus and Arthur König, the *Psychological Review* in 1894 by Cattell and Baldwin, and *L'année psychologique* in 1895 by Binet and Beaunis. There was also a series of *Yale Studies* edited by E. W. Scripture (1892-1902).

The decade saw the raising of several important *systematic issues.* It is the decade of the school of *Gestaltqualität* (form-quality). Chr. von Ehrenfels began the criticism of sensationism and 'founded' the school (1890). A. Meinong (1891, 1899) and H. Cornelius (1892-1893) continued it, and Stephan Witasek presently came to be numbered among its supporters (1897). The movement was a protest against simple sensationism, but, in so far as the form-quality was conceived as a new dependent element of higher order, it failed as a protest against elementarism. It also failed because it was largely an unexperimental movement in an experimental age. In general, the Austrians preferred argument to apparatus.

There was a similar impatience of sensationism in Mary Whiton Calkins's argument that attributes, and not sensations, are the conscious elements (1899), an argument that had been opposed in advance by Ellen Bliss Talbot (1895), and that was resolved by Margaret F. Washburn's rejoinder that the answer to the question all depends on the definition of an element (1902-1903).

The '90's also saw the beginning of the American functional psychology. William James (1890) really began the school without ever evincing enough party loyalty to belong to it. John Dewey's article on the concept of the reflex arc is, therefore, the formal beginning (1896). Titchener, borrowing the word *structural* from James, defended the morphological view of mind as structural psychology (1898-1899). The study of the reaction experiment by J. R. Angell and A. W. Moore (1896) illustrated the functional view, but the explicit and self-conscious development of the school belongs to the succeeding decade.

Psychophysics, as a separate field of research, seemed in this decade to be on the wane. Fechner was dead. The psychophysical

methods were becoming tools for research rather than objects for improvement or controversy. In the traditional school there was the monograph of Lillien J. Martin and G. E. Müller (1899) on the differential sensitivity for lifted weights, one of the classics of psychophysics and of the topic of lifted weights which has, ever since Fechner, lent itself as material for the study of the psychophysical methods. In America G. S. Fullerton and Cattell published the most important study in what might be called American or functional or behavioral psychophysics (1892), a work that is in line with the suggestions of Peirce and Jastrow (1884). Cattell also amplified his contention that psychophysics studies errors of observations of stimuli rather than changes in sensation as conditioned upon a stimulus (1893). Fifteen years later Urban, working at Pennsylvania with the apparatus that Fullerton and Cattell had used, took at first the same point of view.

The year 1896 represents the beginning of what the psychologist generally refers to as *statistical method;* that is to say, Karl Pearson established the method of correlation in its conventional form of products-moments. Galton had invented the method (1888) and Bravais had prepared the mathematical foundation (1846), but it was Pearson who made it available in its present form and who, beginning at that time, has continued to contribute more notably than any one else to the development of the methods of statistical measurement.

We have seen in the last section that the decade of the *reaction experiment* as a means to the measurement of different mental processes was 1883-1892. Külpe's criticism (1893) approximately marks the end of active research in mental chronometry. The distinction between the sensorial and muscular types of reaction was not affected by Külpe, but there was a famous and vigorous controversy about the matter between Baldwin, who denied it, and Titchener, who upheld it (1895-1896). It was the paper of Angell and Moore (1896) that brought the two views into reconciliation. C. S. Dolley and Cattell used the reaction experiment in an attempt to measure the times of conduction of the sensory nervous impulse (1894, 1896), but the method failed in general to satisfy because there is great variability in the results, arising, presumably, from numerous effective factors that cannot be controlled.

The psychology of *vision* prospered in the '90's. Two of the more important investigators were A. Kirschmann (1889-1898) and C. Hess (1889-1902). Kirschmann (1891) and H. Pretori and M. Sachs (1895) published important works on color contrast. Kirschmann worked out the laws that are still current in the textbooks. Hess and Pretori published their well-known study of brightness contrast in 1894, the study which describes an apparatus in which two including fields and two fields undergoing induction can be independently controlled and a measurement made by a subjective equation. Color adaptation has always furnished more difficulty to investigators than color contrast, but there were at this time Martius's study (1896), and the work of Sherrington (1897), who devised certain crucial situations in the form of color disks.

Interest in the retinal theory of color was growing. Christine Ladd-Franklin's theory was first advanced in 1892. Ebbinghaus put out a theory in 1893. Von Kries formulated in 1894 the duplicity theory—that is to say, the theory of dual vision with the retinal rods as the organs of twilight vision and the retinal cones as the organs of daylight vision. G. E. Müller was studying the problems of vision (1896-1897) and put forth his conception of the cortical gray to explain the fact that, although under Hering's theory there can be equilibrium of all the retinal color substances, nevertheless in such cases one sees gray and not nothing. Müller also shifted the emphasis of the Hering theory from metabolism to chemical changes, and substituted assimilation and dissimilation for anabolism and catabolism in describing the hypothetical retinal processes.

Helmholtz had revised the *Optik*, and the new edition came out in 1896, two years after his death. Arthur König had appended to it his bibliography of almost 8,000 titles in the literature of physiological optics, thus showing what a huge field this department of sense had become.

The '90's also saw a great deal of research on *cutaneous sensibility*, mostly in the revival of E. H. Weber's problems of cutaneous localization and the two-point limen. On cutaneous localization there were the studies of W. Barth (1894), W. B. Pillsbury (1895), W. Lewy (1895), and C. S. Parrish (1897). On the two-point limen there were Victor Henri and G. Tawney (1895), C. H. Judd (1896), and Tawney and C. W. Hodge (1897). Henri

brought all this material together in his *Ueber die Raumwahrnehmung des Tastsinnes* of 1898, and by a comparison of the different methods worked out an elaborate analysis of the parts played by vision, touch, and kinesthesis in these perceptions.

The classical papers of Max von Frey on cutaneous sensation appeared in 1894-1895. Von Frey assigned different nervous receptors in the skin as organs functioning for the four fundamental modalities, warmth, cold, pressure, and pain, which had been discovered a decade earlier. This correlation of organ with quality has had acceptance, in spite of the meager grounds upon which it was based, until almost the present day, although recent research has at last discredited it. It is probable, however, that von Frey's association of the pressure spots with the hairs of the skin still remains valid. Von Frey had a theory of the pressure sense, based on the fact that his psychophysical research on the limens for pressure indicated that the true unit of pressure is neither force nor pressure per unit area, but the quotient of force by the linear dimensions of the stimulus. Such a unit applies to the physical situations of which surface tension is an example, and von Frey concluded that the stimulus to the quality of pressure is tension within the skin, not pressure upon it. He established the existence of paradoxical cold—that is to say, of the arousal of the quality of cold on a cold spot by a warm stimulus—and thus prepared the way for the theory of S. Alrutz that the sensation of heat is distinct from warmth (1897) and that heat is a fusion of warmth and cold. In general, von Frey was regarded as the physiological authority on cutaneous sensibility for long after this period, although Goldscheider was still conducting research in this field and published his *Gesammelte Abhandlungen* in 1898.

There was not much of importance in the '90's on the psychology of *hearing* after Stumpf's second volume of the *Tonpsychologie* (1890). A. D. Waller (1891) and J. R. Ewald (1899, 1903) published theories of hearing, theories of the 'pressure-pattern' type, as they have been called. L. Hermann put out his formant theory of the vowels (1895).

Regarded absolutely, there was little work in *smell*, but viewed relatively it was great, for H. Zwaardemaker, the Dutch psychophysiologist, published *Die Physiologie des Geruchs* in 1895. This was the first book in this field that contained much scientific in-

formation about smell. It gave the facts of adaptation and mixture, and Zwaardemaker's rearrangement of the old classifications of odors. It described psychophysical apparatus for working on smell. Much less important were J. Passy's researches on smell (1895-1896).

Taste as well as smell came in at this time for research. H. Oehrwall (1890) and F. Kiesow (1894-1896) were the investigators. They established beyond question the four qualitative modes of taste—sweet, sour, salt, and bitter—worked out some of the psychophysics, and indicated inconclusively some principles of mixture and contrast. Kiesow claimed the existence of a fifth quality, insipidity, that arises from a mixture of sweet and salty substances, much as heat arises from a mixture of warm and cold excitations.

In general it is interesting to note that the mental chemistry of sensationistic elementarism of that day was attempting, like chemistry proper, to count the tale of its elements. There were four in taste, and insipidity would have been a fifth. There were four in the skin, and heat might be a fifth. Organic and visceral sensations, as well as smell, represented an indefinite number. It was already believed that there were more smell sensations than visual sensations. In hearing, Helmholtz's method of dividing the range of pitches by the differential limen was accepted as indicating about 11,000 tonal sensations, though the number of noises remained indeterminate. There were various attempts to estimate the visual sensations by way of their relationships and the differential limens, but the results varied from a few thousand (Donders) to several million (Aubert). Both Külpe (1893) and Titchener (1896) gave lists and attempted a total. Titchener gave 44,435 plus an indefinite number, as the total number of sensations. We cannot criticize this view here. The immediate fallacies were different as applied to the different sense-departments. The fact, however, that elementarism of this sort has proved both useless and false makes it interesting to see how it was regarded in its heyday in the '90's.

The experimental problems in the study of *perception* were fairly numerous in this decade. The work on cutaneous localization and the two-point limen is really research in space-perception, although the distinction between perception and sensation has been less clearly drawn in the tactual than in the visual

field. A great deal was done about the visual perception of the third dimension, partly because a controversy arose. Wundt had of course published results on the functions of accommodation and convergence in his *Beiträge* (1862). Now F. Hillebrand (1894, 1897) disagreed with these findings. E. T. Dixon (1895) tended to confirm Hillebrand, although he drew different conclusions. Then M. Arrer, at Wundt's suggestion, entered the field and found results that supported Wundt's original report (1896-1897), and Wundt himself published (1898). The main fact that convergence is very much more effective than accommodation in mediating this perception was established by every one; the controversy hinged upon the question of the limens of recession and approach for objects moving from or toward the eye when only accommodation is effective, and upon certain theoretical implications based on such a difference. We must also mention B. Bourdon's book on space-perception (1898), a product of the second oldest French laboratory.

Temporal perception has never presented such interesting problems as has spatial perception, nevertheless it had in the '90's, as it has had at other times, a modicum of attention. E. Meumann, in Wundt's laboratory, completed his classical researches on the time-sense (1893, 1894, 1896), best known to-day because they contributed a general chronometric instrument to the laboratory, the 'Meumann time-sense apparatus,' as it is called. Rhythms were investigated by Meumann (1894) and by T. L. Bolton in the Clark laboratory (1894). The facts of rhythm bear on the successive range of attention, as Dietze showed (1885), but it was even more interesting to discover that rhythm is seldom perceived without a kinesthetic accompaniment which 'carries' or marks off the rhythm.

All these perceptual problems—visual depth, rhythm, the time-sense, cutaneous localization, the two-point limen—were important in the experimental psychology of the '90's, but they loomed less large at the time than the question of *optical illusions*. This period was in a sense the decade of the optical illusion. The researches went on in Germany, Austria, Belgium, Holland, and America. The list is as follows: F. C. Müller-Lyer (1889, 1896), W. Laska (1890), J. Jastrow (1892), F. Brentano (1892-1893), Th. Lipps (1892, 1897), Ch. Brunot (1897), J. Delbœuf (1893), F. Auerbach (1894), A. Thiéry (1895), G.

Heymans (1896), W. Einthoven (1898), W. Wundt (1898), C. H. Judd (1899). Wundt wrote a book, *Die geometrisch-optischen Täuschungen* (1898). There was controversy about the rôle of eye-movement in the perception. Judd photographed the moving eyes and established the fact of the occurrence of relevant movements without proving conclusively that they are the sole condition of the illusions. There were very many theories, all of which attempted to show how the perception of the primary stimulus is affected by the presence of the *Nebenreizen,* the secondary stimuli that induce the illusion. Most of the theories had the effect of assimilating the primary stimulus to these secondary ones, although not all: Brentano tried to reduce many illusions to the simple illusion of the overestimation of acute angles and the underestimation of obtuse angles.

The problems of *attention* just about held their own in the '90's after the activity of the '80's. The experiments on the fluctuation of faint stimuli continued with researches by H. Eckener (1893), E. Pace (1893), and K. Marbe (1893). There was a controversy between Titchener and his student, H. C. Cook, on the one side, and W. Heinrich on the other, over the fluctuation of pure tones (1898-1901). Heinrich held that pure tones do not fluctuate, and such a result placed doubt upon the question as to whether the fluctuations when they occur with impure tones should be ascribed to the instability of attention. The work of Benno Erdmann and Raymond Dodge on reading (1898) provided further information about the simultaneous range of attention, and also furnished valuable data about the rate and nature of eye-movements. J. R. Angell and A. H. Pierce published an important study of prior entry (1892). Külpe wrote a theoretical paper on attention (1897).

All these matters were the conventional problems of attention as established in the '80's. In addition to them there arose in America a question about the effect of distractors. Münsterberg and N. Kozaki seemed to find that a 'distraction' enhanced discriminatory capacity (1894). Alice J. Hamlin verified these experimental results (1896), but showed that they came about because the 'distractors' do not always distract, but may act by their presence as spurs to attention, a conclusion that has been supported by various more recent studies.

It was in the '90's that Ebbinghaus's original research led to the extensive experimentalization of the problems of *memory.*

G. E. Müller took over the topic, and the period begins with Müller and Schumann's well-known study (1893-1894) of the method of complete mastery that Ebbinghaus had invented. They made out lists of nonsense syllables and improved the technique in numerous ways. Thereafter new methods began to multiply. J. A. Bergström invented the method of interference (1893); Münsterberg and J. Bigham the method of reconstruction (1894); J. M. Baldwin and W. J. Shaw (1895) the method of recognition and the method that Bentley later called the method of production (they called it ambiguously the method of reproduction); and A. Jost the method of right associates (1897). The decade closed with Müller and Pilzecker's monograph on the method of right associates and times (1900). All this work led to a greatly increased knowledge of the laws of mechanical learning. 'Jost's law,' that, of two associations of equal strength but of different age, the older is strengthened more by a single repetition than is the younger, is an example. Müller was developing the principles of the mechanical interaction of associations in terms of reinforcement, effectual inhibition, associative inhibition, terminal inhibition, and retroactive inhibition. M. W. Calkins (1896) established effects for primacy, recency, repetition, and vividness as conditions of recall. Frank Angell worked on the psychophysics of the tonal image (1899) and Madison Bentley on the psychophysics of the image of color (1899). Bentley showed that an image could be compared by a psychophysical method with a sensation, provided the original stimulus to the image were known, and that the tendency of the image to change could be thus determined. In France, Binet and Henri (1895) were also performing memory experiments, from which some of the earlier knowledge of the effect of meaningful grouping in a memorial material was obtained.

Elementaristic psychology had never done much with the image as an element. It was there, along with the feelings and sensations, but there was little to do about it, although Bentley's psychophysical method suggested a means of approach. Nobody knew whether the images were a class of elements distinct from sensations or not, and there had been considerable systematic argument about the matter. For this reason it is important to note that Külpe (1893) pronounced in favor of what is now the modern view, namely, that images are centrally excited sensa-

tions, and differ from sensations, not as conscious data, but only in their method of arousal.

It is also appropriate at this point to mention the fact that the synapse had been discovered by S. Ramon y Cajal in 1889 and that W. Waldeyer had inaugurated the neurone theory in 1891. At first thought, the neurone theory seems to have no relation at all to the work which Ebbinghaus's experiments on memory began. However, the connection is intimate. The theory of mechanical association that was favored by the mnemonic research is a close parallel to the picture of the nervous system that the neurone theory provides. Nonsense syllables and nerve cells, associations and synapses, the comparison was irresistible. Nobody ever made so naïve a comparison and was taken seriously, and yet the effect of the elementarism of neurones was directly to support the elementarism of experimental associationism. Otherwise there would have been no need for Dewey's condemnation of the reduction of psychology to patterns of reflex arcs (1896), nor of Titchener's understanding Dewey to include the conscious data of the mind.

Just as the '80's prepared the way for the research on memory in the '90's, so the '90's opened the problem of *feeling* to experimentation. In 1892 Alfred Lehmann's *Hauptgesetze des menschlichen Gefühlslebens* was translated from the Danish into German. This book stands at the beginning of the serious attempt to use the method of expression for the experimental investigation of feeling. It presented many correlations of feeling with bodily functions like changes of the pulse and of breathing. Binet and Henri also worked with the method in Paris a little later (1895). Almost at the same time, Fechner's method of impression for the investigation of feeling was developed. Jonas Cohn (1894, 1899) invented the method of paired comparisons and worked out many results for affective judgments on simple stimuli like colors. D. R. Major in Titchener's laboratory (1895) developed the serial method of impression. Then in 1896 Wundt published his *Grundriss* with his new tridimensional theory of feeling in it. He elaborated it later in the fifth edition of the *Physiologische Psychologie* (1902-1903). By this time Wundt's prestige was enormous, and the theory burst without warning on a surprised psychological world. Some of Wundt's grounds for the theory were hardly more than argumentative analogies, but there was one aspect of

the new view that challenged experimental verification. Wundt gave for each of the six primary feelings—pleasantness, unpleasantness, strain, relaxation, excitement, depression (calm)—a statement of physiological correlates in terms of rate and amplitude of breathing and pulse. Wundt based his correlations upon a study of Lehmann's records. Immediately, then, at the start of the new century, there was a wave of research upon the method of expression, an activity to which we may more properly refer in the next section. It is enough to say here that the '90's made available both the method of expression and the method of impression for the study of feeling, and that Wundt's pronouncement on the matter so placed the problem that immediate research was inevitable.

Outside of what is nowadays called experimental psychology proper, there were the tests. In the '90's the *mental tests* were getting under way, but the final direction that they were to take was as yet undetermined. Galton had had tests. Cattell was interested in individual differences. In 1890 he first used the phrase *mental test*. In 1896 he and Livingston Farrand tested a large number of Columbia University students and analyzed the results. There was a great deal of incidental work going on. Committees that never did anything were formed in America to develop mental tests. Binet and Henri were working on in France at tests of complex mental capacities. Ebbinghaus, in an effort to aid the school authorities at Breslau, invented the completion test (1897). America, however, clung to the tests for simple sensory and motor capacities, such as Galton had used. Nevertheless, the decade ended with the practical failure of the simple tests to yield reliable information about the human capacities for which the tests were wanted, and thus left the field open for the more complex tests that Binet favored.

The '90's were also the decade in which *comparative psychology* came to be studied for its own sake and not merely for the light it would throw on the validity of the theory of evolution. The reaction away from Romanes was made in England by C. Lloyd Morgan, who published *Animal Life and Intelligence* in 1890-1891, and then his much more valuable *Introduction to Comparative Psychology* in 1894. In the swing of the pendulum away from the anthropomorphism of Romanes, Morgan adopted the principle of parsimony in imputing consciousness to animals on the basis

of their behavior, and this canon of his has, at least until recently, remained the accepted rule. The reaction from Romanes was aided still further by Jacques Loeb, who formulated his theory of the tropism in 1890. Morgan had left room for animal consciousness when parsimoniously assigned. Loeb, however, denied consciousness to the lower animal forms and ascribed their behavior to mechanical tropisms. He held that associative learning is the proper criterion of consciousness, and that on such grounds consciousness can belong only to the higher animal forms. His *Vergleichende Gehirnphysiologie und vergleichende Psychologie* (1899, Eng. trans., 1900) had a very great influence in comparative psychology. Others went even farther than Loeb. For instance, A. Bethe (1898, 1900) raised the question of consciousness in insects, and he with Th. Beer and J. von Uexküll argued (1899) against the use of any conscious terms at all in comparative psychology. This defense of objective psychology is one of the several forerunners of behaviorism. However, not all students of animal behavior felt the same way. H. S. Jennings, whose numerous researches began in 1897, found it useful to impute mind to protozoa.

E. L. Thorndike is generally said to have been the first to apply the experimental method to animals in his experiments upon cats, dogs, and chicks (1898). Of course Loeb had been experimenting, but upon the lower forms. Everybody who had artificially altered situations for animals and had then observed the results had in a sense experimented. However, it is true that Thorndike was the first to apply to the higher forms artificial situations of sufficient complexity to be described as 'apparatus,' and there is no doubt that his work was regarded as a new application of the experimental method in psychology and for that reason immediately stimulated a great deal of similar research.

In France there was little during this decade that concerns us. We have mentioned the work of Binet and his associates that led up to the intelligence tests in the following decade. *Abnormal psychology* was progressing. Pierre Janet published *L'état mentale des hystériques* in 1892. Ribot was writing books (1896, 1897, 1900), and had turned from strictly abnormal psychology to general psychology as exhibited in part through the abnormal.

Thus the century closed. If psychologists in 1890 by their very eagerness betrayed some fear lest the new science fail, their

unexpressed misgivings had vanished by 1900. They were convinced by the amount and variety of research that the 'new' psychology was firmly established, even though philosophers and the public might still doubt.

The Fifth Decade of Experimental Psychology: 1900-1910

With the new century, the 'new' psychology, no longer very new, passed beyond the limits of such brief review as we have already given of the final decades of the nineteenth century. Even in these decades we have been obliged to plot, rather than sketch, the patterns, putting in only enough points to define the general form of the chief constituents. In the twentieth century, even plotting becomes impracticable. All we can do is to list some of the significant events, indicate the form of some of the most important components, and confine ourselves even more closely to the experimental core of modern psychology.

It is best to begin with a general orientation for the first decade of the twentieth century, and for this purpose the author lists below what seem, from the point of view of this book, to be the nine major features of the decade; and he ventures further to present the list in what seems to him to be the decreasing order of importance of the various items.

1. The development of the experimental psychology of thought —Külpe's Würzburg school and its immediate effects elsewhere, especially at Cornell. Thought was the one important remaining aspect of the normal human mind that was generally recognized as requiring consolidation within the 'new' psychology and that had resisted invasion by the experimental method.

2. The development of intelligence tests at the hands of Binet and under his influence. The tests may or may not belong to experimental psychology, for everything depends on the definition of terms; nevertheless, the course of the decade placed them finally in a position to dominate what may be a full half of American psychology.

3. The formulation of the functional point of view at Chicago, for this was in a way the only sophisticated philosophy of American psychology that we have ever had.

4. The development of experimental animal psychology in

American laboratories, with leadership in comparative psychology passing from England to America and with the resultant increase of emphasis upon animal behavior instead of animal consciousness.

5. The development of the experimental psychology of memory, primarily in Germany and secondarily in America, with further multiplication of methods and an excellent start in the accumulation of laws. It was mostly because of this work that it seemed for a time that the 'new' psychology must become the basis of the new scientific study of education.

6. The reaction, in physiological psychology, against the theory of exact localization of cerebral function, a change in which the research of Franz was of the utmost importance.

7. The revival of psychophysics, which had languished since 1882, with the critical and constructive résumés of Müller and Titchener, and the new approach established immediately afterward by Urban.

8. The extension of the experimental psychology of space-perception to the field of hearing in the researches on the localization of sound. The two opponent theories became clear—that localization might be dependent upon dichotic difference of intensity or upon dichotic difference of phase.

9. The measurement of the degree of attention by Geissler and Wirth, for these results were almost the only ones among all those pretending to be laws of attention in which it was quite clear that attention, and not phenomena of sensation or memory, was being investigated.

Before we are through, we shall have returned to all these topics. We must enter now into more detail, and begin with the more important general events of the decade.

In Germany Wundt published the fifth edition (1902-1903) and the sixth edition (1908-1911) of the *Physiologische Psychologie*. The fifth edition was much changed. It was half again as large as the fourth and yet, so rapidly was psychology increasing in bulk, it gave up the task of being a complete handbook of psychology. It contained Wundt's new theory of feeling, which, giving into his hand a wealth of affective material quite as extensive (in theory) as the sensory material, led him to change his pictures all the way through. The sixth edition was much like the fifth, partly because Wundt was becoming absorbed by

the writing of his *Völkerpsychologie,* of which the first volume appeared in 1900 and the tenth in 1920. Wundt was claiming the right of age to specialization. He discontinued the *Philosophische Studien* in 1903, and it was replaced in the same year by the more catholic *Archiv für die gesamte Psychologie* under Meumann's editorship. However, Wundt, not schooled in coöperative enterprises, soon started the *Psychologische Studien* (1905) as the independent organ of Leipzig.

Ebbinghaus completed the first volume of his *Psychologie* in 1902, and this date marks the beginning of its influence. He never got far with a second volume. Lipps published his *Leitfaden der Psychologie* in 1903 and S. Witasek his *Grundlinien der Psychologie* in 1908. These two were the general texts most definitely in the tradition of Brentano. Münsterberg's *Grundzüge der Psychologie* (1900) was published from America, but the book belongs in the German tradition as representing a psychological system tempered with philosophy.

In America the extremes were represented by Angell and Titchener. Angell's *Psychology,* representing the functional point of view, appeared in 1904, but it was his papers (1903, 1907), not his book, that show clearly what he intended functional psychology to be. Titchener was at first occupied with his experimental handbooks. The *Qualitative Manual* came out in 1901, the *Quantitative* in 1905. They were models of care and erudition, and went far toward fulfilling Titchener's purpose of establishing psychology firmly as an experimental science. Titchener's *Text-Book of Psychology* (1910) was his only complete systematic treatise and still stands as representative of the introspective school in America.

In England experimental psychology of Galton's kind gained some recognition in the inclusion of psychologists in the Cambridge Anthropological Expedition to Torres Straits at the end of the century, and the reports of this work were issued early in the new decade (1901-1903). The expedition had also, however, the deterrent effect upon psychology of turning these psychologists in the direction of ethnology. Nevertheless, the *British Journal of Psychology* was founded in 1904, so that Great Britain at last joined Germany, America, and France in having a psychological periodical to represent (as *Mind* did not) a psychology that had nothing to do with philosophy. McDougall published his *Physio-*

logical Psychology in 1905, and Myers his *Text-Book of Experimental Psychology* in 1909. They and Rivers, who had been the psychologists of the expedition to the Torres Straits, were all more or less distracted by ethnological work.

In the field of *mental measurement* there were two important events. C. Spearman invented the method of analyzing the results of tests into specific factors and a common factor (1904) and thus founded the mathematical conception of intelligence. F. M. Urban attacked psychophysics from a new point of view and shifted the emphasis therein from the calculation of limens to the description of psychometric functions. This latter term he invented. His first work had been preceded by G. E. Müller's new evaluation of the methods of psychophysics (1903) and by Titchener's *Quantitative Manual* (1905), which gave psychophysics a new importance and made the methods readily available to English-reading students of psychology. We may also mention here Cattell's work on the method of order of merit, which he developed in his study of the eminence of scientific men (1903-1915). It is also worth pointing out that F. Schumann (1904) finally disposed of the current belief that successive psychophysical judgments depend on the persistence of a memory image of the first stimulus for simultaneous comparison with the second stimulus.

Organization of the knowledge of *sensation* was greatly aided by the publication of the volumes on the psychophysiology of sensation in Schäfer's *Text-Book of Physiology* (1900) and Nagel's *Handbuch der Physiologie des Menschen* (1905). The authors of articles in these volumes are in general the men who have contributed notably to the psychology of sensation: C. S. Sherrington, W. H. R. Rivers, J. G. McKendrick, A. A. Gray, J. B. Haycraft, W. Nagel, J. von Kries, K. L. Schäfer, T. Thunberg.

In *vision* the collection (1903) of Arthur König's researches since 1888 was a notable event, supporting the more physical approach of Helmholtz to visual problems, rather than the more phenomenal approach of Hering. There was a great deal of important research which can only be illustrated by the mention of J. W. Baird's work on indirect vision (1905), W. Lohmann's on light adaptation (1906), H. Piper's on dark adaptation (1907), and the beginning of a long series of studies by C. E. Ferree (1909).

In the field of *audition* there was Ebbinghaus's theory of hearing (1902), Max Meyer's theory (1907), and W. Köhler's analysis of the tonal attributes (1909-1915). Zwaardemaker published on *taste* (1903). Henry Head and W. H. R. Rivers propounded their theory of the division of *cutaneous sensibility* into protopathic and epicritic systems (1905, 1908), and were negatively criticized by the experiments of W. Trotter and H. M. Davies (1909), as they also have been by later investigators. E. Meumann (1907, 1909) and E. Becher (1908, 1909) contributed the first experimental work on *visceral sensation.*

In the field of *space-perception,* the outstanding problem concerned the localization of sound. There came to be two opposing theories, which have only recently been resolved into one. A. H. Pierce (1901) and J. R. Angell and W. Fite (1901) were the first to present the theory of the binaural intensive ratio, that is to say, the theory that the left-right localization of sound is dependent upon its relative intensity at the two ears. Lord Rayleigh (1907), supported presently by the experiments of H. A. Wilson and C. S. Myers (1908), accounted for localization in the case of tones by the relative phase of a tone at the two ears, that is to say, the tone that leads in phase determines the localization toward itself. There has been a great deal of research on this problem in the last twenty years, and there seems to be no doubt that both theories are correct, and that both sets of facts must be considered in some more general theory.

In visual space-perception, especial mention must be made of F. Schumann's classic analysis of the factors involved (1900, 1902, 1904). F. Hillebrand (1902) and J. W. Baird (1903) furthered the problem of the visual perception of the third dimension. B. Bourdon published *La perception visuelle de l'espace* (1902).

On cutaneous space there were the papers of A. Brückner (1901), von Frey's pupil, and von Frey and R. Metzner (1902). These studies investigated the simultaneous and successive two-point limens, and, beside describing methods and apparatus, reported certain important facts of the reinforcement and inhibition of one stimulation by the other.

The decade saw the culmination of the experimental work on *memory.* The methods multiplied, for P. Ephrussi invented the method of prompting (1904), E. Ebert and E. Meumann extended Jacobs's method of the memory span (1905), A. Pohlmann orig-

inated the method of retained members (1906), F. Reuther, working in Wundt's laboratory, initiated the method of identical series (1906), and Eleanor A. McC. Gamble developed Münsterberg and Bigham's method of reconstruction (1908). Lottie Steffens published her study of learning by wholes and by parts (1900). R. M. Ogden published his investigation of the effect of the rate of learning upon retention (1903). P. Radossawljewitsch repeated Ebbinghaus's research on the forgetting curve and obtained a new and more exact function (1907). The mass of accumulated information was really much greater than it has been possible to indicate in these pages, and Meumann brought it all together as of 1903 in his *Oekonomie und Technik des Lernens*. Meumann was making the facts available for scientific education, and he wrote at the height of the period in which it was believed that educational psychology would have to be the chief propædeutic to pedagogy. As regards the work in the field of memory, the difficulty with this latter view seems to be that the experimental psychology of memory, dominated by Ebbinghaus's conceptions, has dealt principally with the formation of mechanical associations, and that the laws obtained in this way are not always applicable with precision to the learning of meaningful material. Moreover, even the question of whether mere frequency of temporal contiguity ever, quite alone, establishes associations has been questioned.

For the most part research on the topic of *feeling* was limited to the application of the method of expression, a situation for which the promulgation of Wundt's theory of feeling was directly responsible. The important papers of this sort during the decade were: P. Zoneff and E. Meumann (1901), M. Brahn (1901), W. Gent (1903), F. G. Bonser (1903), H. Berger (1904), H. C. Stevens (1905), M. Kelchner (1905), and J. F. Shepard (1906). This list includes papers on the bodily expression of attention, for the fact was that changes in the direction of attention proved to vitiate some of the correlations which seemed to be found for feeling. Wundt's theory was not verified in detail when the total mass of research is considered, and in fact there was so much conflict of result that E. Leschke in summarizing the research (1911, 1914) was obliged to resort to a statistical method, polling the researches and determining scores for and against a given correlation, in order to determine a general trend. Such a situation is an unsatisfactory showing for a scientific method. Titchener

employed the method of impression (1902) to disprove Wundt's new theory, and his students, S. P. Hayes (1906) and W. S. Foster and K. Roese (1916) carried this line of attack further. Thus Wundt got the psychology of feeling nowhere in the end; it was his prestige and the boldness of his pronouncement that excited all the research.

We should also note at this point that J. Orth in *Gefühl und Bewusstseinslage* (1903) indicated that the explanation of affection might be cognitive after all, and that Stumpf put out his own theory of feeling as concomitant sensation (1907, 1916).

The experimental psychology of *attention* was beginning to show diminished vigor as against the two preceding decades. J. P. Hylan (1903) showed that the phenomenon of the visual range of attention, tachistoscopically determined, might be a measure of the persistence of the memory after-image. M. Geiger (1903) and H. C. Stevens (1904) worked on the problem of prior entry, and Geiger constructed a complication clock that was a great improvement upon Wundt's instrument. L. R. Geissler published on the fluctuation of attention (1907). However, in the author's opinion, the important researches that stand out from all the rest were the experiments in measuring the degree of attention. This work was done by W. Wirth at Leipzig (1902, 1907, 1908) and L. R. Geissler at Cornell (1909). These studies had the advantage of being more nearly a direct attack. Fluctuations of attention might be due to eye-movements or to movements of the muscles of the middle ear; prior entry might be an artifact due to eye-movements (Dunlap, 1910), for the visual department of sense was always used; range might be a matter of memory after-image or of rhythmical grouping. The measurement of degree, on the other hand, involved in part the conscious direction of attention by the subject toward one task or object and away from others, and it thus brought under investigation the very datum which leads to the belief that there is such a thing as attention to be investigated.

We must also note that McDougall gave the only important physiological theory of attention when he promulgated his drainage theory (1903). This view has been pronounced against by physiologists, but it was for a time a useful view. Psychologically attention is drainage, whatever it may be physiologically.

The literature on attention was admirably summed up by

W. B. Pillsbury in *Attention* (1908) and by Titchener in his *Psychology of Feeling and Attention* (1908). Titchener in his book founded his own doctrine that attention is phenomenal and exists in consciousness as variations in an attribute of clearness (called later, vividness or "attensity") that all sensations have.

The new topic in experimental psychology was *thought.* Wundt had excluded it from experimental psychology, but Külpe was dissatisfied with this situation. At Würzburg he and his students brought thought directly into the laboratory, subjected it to "systematic experimental introspection," and arrived at the conclusion that it was phenomenal and could thus be introspected upon, but that it was not made of the same palpable stuff as sensations and images. There resulted the Würzburg school of imageless thought, which used such concepts as *Bewusstheiten* and *Bewusstseinslagen* for description. The importance of the movement is not in the least diminished by the fact that its central tenet proved to be false, and that many of the critical factors in thought were shown to be, not only imageless, but also unconscious, to be taken into account as *Aufgaben* and *Einstellungen.* It was the Würzburg school that contributed most to this positive conclusion. The work began with the study of association by A. Mayer and J. Orth (1901). Then came K. Marbe's study of judgment (1901). Orth's *Gefühl und Bewusstseinslage* (1903) belongs indirectly in the series. H. J. Watt attacked thought directly (1904) and N. Ach approached it as involving decisions, thus linking it with conscious action (1905). A. Messer furnished an elaborate introspective study (1906), and K. Bühler, other studies, in which he related thought to memory (1907-1908). Messer's *Empfindung und Denken* (1908) is the only book approaching a summary written from within the school.

Külpe himself did not publish directly on the topic of thought. He was throughout the guiding influence, and his paper on the subjectification of visual and cutaneous impressions (1902) and his experiments on abstraction (1904) show how his mind was running. At some time during the decade he came greatly under the influence of E. G. Husserl's *Logische Untersuchungen* (1900-1901), a fact which shows how the Würzburg school was turning from the camp of Wundt toward the camp of Brentano. Külpe must be thought of as still proceeding with these problems in his writing of *Die Realisierung* (1912).

One of the most effective criticisms of the Würzburg school was Titchener's in his *Experimental Psychology of the Thought-Processes* (1909), where he also reviewed the movement and undertook to support a sensory-imaginal account of thought. Titchener's well-known context theory of meaning was formulated with this interest in mind. The interesting fact about this whole development is that, while Külpe was working toward a theory of conscious thought composed of imageless elements and Titchener toward conscious thought composed of images and sensations, both were really stressing the effect of an *Aufgabe* operating unconsciously by way of a determining tendency. The most important thing about Titchener's context theory of meaning is that habitually the context lapses and the meaning is 'carried' unconsciously. Thus the result of bringing thought into the laboratory was to leave it in the laboratory, but to declare that it could be only imperfectly studied by the method of introspection.

During this decade the *animal psychology* of the laboratory developed rapidly in America. H. S. Jennings published *The Behavior of the Lower Organisms* (1904). Small's invention of the method of the maze was capitalized by numerous investigators. John B. Watson completed a very thorough study of the white rat in the maze (1907), in which he showed that the behavior of the rat was little influenced by the elimination of the use of most senses and concluded that it was guided by its kinesthetic sense, which he could not of course eliminate and leave the animal free to run. M. F. Washburn signalized the coming of age of animal psychology by producing a handbook, *The Animal Mind* (1908). Shortly afterward the *Journal of Animal Behavior* was founded (1911).

The most notable contributions to *physiological psychology* were made by S. I. Franz and I. P. Pavlov. Franz employed the extirpation method with animals to study cerebral localization of functions. There is a long series of papers and monographs (1902-1921). His primary conclusion tended to throw the problem back toward the position of Flourens and away from the more recent belief in exact localization and centers. Such results, while in themselves largely negative, accord well with the general trend of psychology and psychophysiology and lead away from a simple mechanics of association. Pavlov discovered and developed the conditioned reflex (1903-1928), and thus made possible an objec-

tive psychology of animals or human beings which could take account of the problems in which association had earlier been the effective concept. Perhaps mention should again be made here of McDougall's drainage theory (1903) and of his *Physiological Psychology* (1905).

In the field of the *mental tests* the great advance was, of course, under Alfred Binet in France. Binet published *L'étude expérimentale de l'intelligence* in 1903. With Th. Simon he put out the first form of his scale of intelligence in 1905 and a revision in 1908. He developed the conception of the growth of intelligence and the concept of mental age. Spearman, as we have seen, began the analysis of intelligence into specific factors and a common factor (1904), but for the most part his work came later. In America psychologists were busy with the tests, but the crucial work did not lie there at this time. G. M. Whipple published a *Manual of Mental and Physical Tests* (1910), which summarized the field as Washburn's book had done for animal psychology and Titchener's *Manuals* for experimental psychology.

This was not all, but it is enough to state. The reader will see that the new psychology had in the new century got out of hand for any purposes of complete exposition because of its mass and volume. Moreover, a true account of this decade cannot be written until a considerably increased perspective of time has reëvaluated the work and has furnished better guidance than exists at present as to what really mattered.

The Present: after 1910

If the beginning of the twentieth century is too close for a just historical evaluation, the last two decades are not yet history at all. Of them it might be better at this time to say nothing, except for the fact that the rounding-out of this general survey requires some termination. We shall content ourselves, then, with a brief mention of the more important events of the last twenty years, without any attempt at detailed explication.

Gestalt psychology has been the important German movement from the point of view of experimental psychology. Max Wertheimer, Wolfgang Köhler, and Kurt Koffka are its best-known advocates. It began with Wertheimer's paper on seen movement (1912). It was retarded by the World War, but furthered by

Köhler's research on the apes (1917). That the movement was to have the magnitude of a school hardly became apparent before Köhler's remarkable book on static physical *Gestalten* (1920), a book which helped to clinch his call to Stumpf's chair at Berlin. Koffka's book on child psychology (1921) lent further support, and in the same year the *Psychologische Forschung* was founded as an organ for the new school. Immediately there were theoretical papers by Wertheimer (1921) and Koffka (1922), laying down the principles of the new approach. The school became known in America and England, largely by way of visits from Köhler and Koffka, and the principles of *Gestalt* psychology soon came to have wide acceptance even where adherence to the school was scouted. The movement was most impressive, however, not because of any argument for it, but because it produced a mass of experimental research that went on in part under its direct auspices. Most of this research lay in the field of perception, the field in which the movement started with Wertheimer. Most of it, while rejecting formal analytical and sensationistic introspection, employed phenomenological description, and experimental phenomenology thus became a method of *Gestalt* psychology.

In this newly quickened field of perception, the outstanding problem concerned the phenomenon of seen movement. As we have just said, *Gestalt* psychology began with Wertheimer's paper on this topic (1912). His work was supplemented by A. Korte (1915), by P. Cermak (1921), and by many recent investigators in America. V. Benussi (1913, 1917) carried out the first analogous experiments for movement on the skin. The notion that perceived movement could not be described in analytical sensory terms, but could be identified for experimental purposes as itself and designated as the *phi*-phenomenon, was a justification of experimental phenomenology.

Gestalt psychology also led to considerable investigation of visual space-perception, as many articles in the *Forschung* testify. Of especial importance was E. Rubin's study of figure, ground, and contour (1915), work done in Müller's laboratory but assimilated by the *Gestalt* school.

There has also been a great deal of activity in the investigation of the phenomena of the localization of sound. For the most part this work has not been stimulated by *Gestalt* psychology, but rather by physicists and by the research on the detection of

submarines in the World War. Of the psychologists whose attention has been engaged by the problem we may mention O. Klemm (1913, 1918, 1920), E. M. von Hornbostel and Wertheimer (1920), C. E. Seashore (1922), H. M. Halverson (1922, 1924), and H. Banister (1925-1928).

Another event of importance in Germany was the founding of the Marburg school by E. R. Jaensch about 1920. This school of psychology is based upon the discovery of eidetic imagery in children and on the subsequent development of correlations of this imagery with physiological types of individuals.

Just as *Gestalt* psychology stormed Germany, so behaviorism, a synchronous movement, attacked, and to some extent captivated, America. John B. Watson promulgated his views in 1913 and explicated them in his later books (1914, 1919). Quite early E. B. Holt lent to the view a philosophical respectability and sophistication (1915). In the last fifteen years the American journals have been riddled with papers criticizing, modifying, amplifying, or defending behaviorism.

The effect of *Gestalt* psychology was to stimulate experimental research upon perception. The effect of behaviorism was to stimulate experimental research upon animals. The importation of Pavlov's method of the conditioned reflex did much to make the problems of behaviorism general.

There has also been, quite independent of behaviorism and for the first time, competent research upon the anthropoid apes. Köhler was the first to attack this problem (1917), but R. M. Yerkes and his associates are now the well-known investigators in this field (1927 *et seq.*).

Physiological psychology has prospered. Woodworth's revision of Ladd's treatise (1911) made a useful start. Behaviorism has always led over into physiological research, and the conditioned reflex is a physiologically formulated concept. K. S. Lashley (1920 *et seq.*) has continued Franz's work on cerebral localization with notable results. Henri Piéron in Paris has contributed to the same problem (1923). Also the James-Lange theory of emotion passed at last from the argumentative to the experimental stage in the hands of W. B. Cannon (1915), who induced emotions in cats and dogs and reported the physiological effects in terms of excitation of the sympathetic nervous system and the liberation of adrenin in the blood.

The inheritance of Wundt can be found in *Gestalt* psychology, and of James in behaviorism, even though the children are so unlike their ancestors. In the same way we can find the inheritance from Brentano still alive. In England it appeared most explicitly in James Ward's systematic psychology (1911, 1918). William McDougall brought it with him to America (1923). In another form it appeared in Germany with Messer (1914) and in Külpe's posthumous book (1920). However, act psychology seems never to lead primarily to experimentation.

There has been, of course, in the last twenty years a great deal of research in the field of *sensation.* Vision has, as usual, dominated the field. C. E. Ferree, G. Rand, L. T. Troland, H. E. Ives, P. G. Nutting, and P. Reeves are only a few of the names of the persons whose many papers form this literature. D. Katz (1911) and K. Bühler (1922) have written on the phenomenology of color, under the new German influence. W. de W. Abney published his important book on color vision (1913); J. H. Parsons summarized the literature of color vision (1915); and Hering's *Grundzüge der Lehre vom Lichtsinn* was published posthumously (1920).

There was comparatively little important work on hearing except the researches on the localization of sound which we have mentioned above. In the field of smell, Hans Henning published (1916) the most important work since Zwaardemaker, and it was thought for a time that his classification of odors would lead at once to much research in this field. The belief has not been justified for the reason that the basic theory has been found to hold only approximately. The testing of aviators in the World War led to a great deal of research on the static sense, or, as it has now come to be called, vestibular sensibility. The work of Raymond Dodge and C. R. Griffith requires especial mention in this connection.

Finally there have been the tests and the research on *intelligence.* B. Hart and C. Spearman invented the hierarchical method of analyzing a battery of tests into specific factors and a common factor (1912). William Stern devised the 'mental quotient' (1912), which Terman later named the 'intelligence quotient.' R. Pintner and D. G. Paterson invented the performance tests (1917) and M. Lobsien the group tests (1914). L. M. Terman and his assistants revised the Binet scale and issued the Stanford Revi-

sion (1916), which is now the standard form. Then America entered the World War, and the psychologists undertook to test almost 2,000,000 recruits for intelligence, primarily by way of the group tests. This work yielded many valuable data about intelligence, as the tests test it, in relation to the methods of testing and to social factors, but the most striking effect of the work was the popular advertising that it gave to psychology. The many ramifications of intelligence testing in the last decade are too numerous to mention. The method has found its way into educational systems and into industry, and has made applied psychology a reality.

And thus we come at last to the place where the past meets the future. Of the future we shall have no word to say. It can very well look after itself. However, from the past we can gather certain generalizations which may perhaps be of use to a psychologist living in the present, and to these we turn in the next, and last, chapter.

Notes

The clerical mind is tempted here to append the several hundred references that are indicated in the text by name and date. Such a list, however convenient it might be to the reader, would hardly be justified. More than half of the citations are to be found elsewhere in this book, and in many cases the index is a ready guide to them. The full list would be in no sense complete except as against the particular content of this chapter, which is dependent upon all the evaluative idiosyncrasies of the author. Moreover, nearly all the references, both those that are printed elsewhere in this book and those that are not, are readily available in two other sources. The reader, bent on verification or further knowledge, should consult for the nineteenth century and by subject and author, B. Rand's bibliography in J. M. Baldwin's *Dictionary of Philosophy and Psychology*, III, 1905. For the twentieth century or for any date later than 1893, he can refer by date and author to the volumes of the *Psychological Index*. When in doubt, the author himself has always verified the dates of this chapter from these two sources. There are only a few of the older references that Rand does not give.

Perhaps then no notes are needed. However, it has seemed advisable to give in this place some of the references to those accounts of research in certain important fields of experimental psychology, in so far as they have come to the author's attention.

General History

Certainly the best book now is G. Murphy, *Historical Introduction to Modern Psychology*, 1929, which includes about 900 references to the literature.

Systematic History

The other histories stress more the systematic issues: O. Klemm, *History of Psychology*, Eng. trans., 1914; G. S. Brett, *History of Psychology, III. Modern Psychology*, 1921, esp. 89-309;

W. B. Pillsbury, *History of Psychology*, 1929. None of these books gives the flavor of experimentalism. Murphy's book, *op. cit.*, also deals with systematic issues, especially the supplement on Contemporary German Psychology, by H. Klüver, 417-455.

Experimental History

On the history of the formal development of experimental psychology and the founding of laboratories, see, for the German laboratories, O. Krohn, Facilities in experimental psychology at various German universities, *Amer. J. Psychol.*, 4, 1892, 585-594; 5, 1892, 282-284. The later German history has presumably not been brought together, nor is there any such account for Great Britain. For America, see E. B. Delabarre, Les laboratoires de psychologie en Amérique, *L'année psychol.*, 1, 1894, 209-255; C. A. Ruckmick, The history and status of psychology in the United States, *Amer. J. Psychol.*, 23, 1912, 517-531; *idem*, Development of laboratory equipment in psychology in the United States, *ibid.*, 37, 1926, 582-592. For some incidental discussion about the dates of coming into being of the laboratories at Clark, Harvard, Yale, Toronto, and Columbia, see *Science*, N. S. 2, 1895, 626-628, 734 f. For a brief general discussion of all these matters in the entire world, see, as of its date, E. W. Scripture, *The New Psychology*, 1897, 463-473.

For general summaries of research in experimental psychology during certain periods, see E. B. Titchener, The problems of experimental psychology (covers *ca.* 1882-1905), *Amer. J. Psychol.*, 16, 1905, 208-224; *idem*, The past decade in experimental psychology, *ibid.*, 21, 1910, 404-421; Ruckmick, The last decade of psychology in review, *Psychol. Bull.*, 13, 1916, 109-120.

The best summaries of research in different fields are those in the *Psychological Bulletin*, esp. since vol. 7, 1910. They are the *raison d'être* of the *Bulletin*. They are readily accessible and should not be listed here, because they are so numerous, because, being sometimes detailed reviews of the research of a single year, they lack the perspective that we seek, and because many of the topics are narrowly defined as against the broad scope of our treatment.

Psychophysics and Measurement

On the history of psychophysics, see Titchener, *Experimental Psychology*, II, pt. ii, 1905, esp. pp. xiii-clxxi. There is available no good history of statistical measurement in psychology, but on the history of the use of the normal law of error see E. G. Boring, *Amer. J. Psychol.*, 31, 1920, 1-29.

Sensation in General

While there are no histories of research in the field of sensation, the handbooks are historical and thus extremely valuable. See, therefore, E. A. Schäfer, *Text-book of Physiology*, II, 1900, 920-1258; W. Nagel, *Handbuch der Physiologie des Menschen*, III, 1904-05, 806 pp. Reference is made below to specific authors in these texts. A very great deal of historical information is to be obtained from Titchener, *Experimental Psychology*, I, pt. ii, 1901; II, pt. ii, 1905, and from G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911.

Vision

There is no history, but there is of course H. Helmholtz, *Handbuch der physiologischen Optik*, 3d ed., Eng. trans., 1924-1925, which contains bibliographical lists that can be used in conjunction with A. König's huge bibliography in the 2d German ed., 1896. The handbooks are useful; see W. H. R. Rivers, in Schäfer, *op. cit.*, 1026-1148; and F. Schenck, W. Nagel, J. von Kries, and O. Zoth in Nagel, *op. cit.*, 30-475. In the same way, see

J. H. Parsons, *Introduction to the Study of Colour Vision*, 1915. For an excellent account, giving the immediate background at a still later date, see L. T. Troland, The present status of visual science, *Bull. Nat. Res. Council*, 5, 1922, no. 27.

Audition

We are even more poorly provided with historical accounts of research in the field of hearing. Here again there is Helmholtz, *Sensations of Tone*, Eng. trans., 1875 *et seq.* There are the handbooks: J. G. McKendrick and A. A. Gray in Schäfer, *op. cit.*, 1149-1236, and K. L. Schäfer in Nagel, *op. cit.*, 476-588. There is a little in R. M. Ogden, *Hearing*, 1924.

Taste and Smell

There are only the handbooks. For taste, see J. B. Haycraft in Schäfer, *op. cit.*, 1237-1245, and Nagel in Nagel, *op. cit.*, 621-646. For smell, see Haycraft in Schäfer, 1246-1258, and Nagel in Nagel, 589-620.

Somesthesia

On cutaneous sensation, see C. S. Sherrington in Schäfer, *op. cit.*, 920-1001, and T. Thunberg in Nagel, *op. cit.*, 647-733. On kinesthetic sensation, see Sherrington in Schäfer, 1002-1025, and Nagel in Nagel, 734-806. There is, however, one striking exception to this general paucity of historical material, the excellent history of research on the 'static' sense by C. R. Griffith, Historical survey of vestibular equilibration, *Univ. Illinois Bull.*, 20, 1922, no. 5.

Perception

No one has attempted to write a general history of the theory of perception as dependent upon experimental work, although Klemm, *op. cit.*, 317-345, approaches it. The work of the school of the *Gestaltqualität* has been admirably summarized by M. Bentley, The psychology of mental arrangement, *Amer. J. Psychol.*, 13, 1902, 272-288. On optical illusions, there are excellent summaries of the literature by V. Henri, Illusions et hallucinations, *L'année psychol.*, 3, 1896, 495-513; 4, 1897, 538-542; E. B. Titchener, *Experimental Psychology*, I, pt. ii, 1901, 303-328. The research on the visual perception of the third dimension is well summarized by J. W. Baird, *Amer. J. Psychol.*, 14, 1903, 150-170. On tactual space-perception there is, excellent for its date, V. Henri, *Die Raumwahrnehmung des Tastsinnes*, 1898.

Feeling

The history of both theory and research, except the results of the method of expression, is admirably presented by Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 33-168, 328-351. There is no similar text for emotion. The list of reviews of the results of the method of expression is sizable: A. Binet and J. Courtier, *L'année psychol.*, 3, 1896, 65-126; J. R. Angell and H. B. Thompson, *Psychol. Rev.*, 6, 1899, 33-43; H. C. Stevens, *Amer. J. Psychol.*, 16, 1905, 464-483; J. F. Shepard, *ibid.*, 17, 1906, 522-530, 559; E. Leschke, *Arch. f. d. ges. Psychol.*, 21, 1911, 435-463; 31, 1914, 27-37.

Attention

There are excellent texts for the history of research on attention. They are W. B. Pillsbury, *Attention*, 1908, and Titchener, *Feeling and Attention* (*op. cit.*), 171-282, 352-384.

Association and Memory

The best text is E. Meumann, *The Psychology of Learning*, Eng. trans., 1913. Murphy, *op. cit.*, 189-206, 245-262, devotes special chapters to the history of the study of memory and of

the acquisition of skill. There is a good chapter on experimental studies of association in H. C. Warren, *History of Association Psychology*, 1921, 213-257.

Thought

Here again Titchener, always the critical encyclopedist, is the source. See his *Lectures on the Experimental Psychology of the Thought-Processes*, 1909.

Reaction Experiment

Distinctly the best historical account of the reaction experiment is V. A. C. Henmon's, *Arch. Psychol.*, no. 30, 1914, 1-32. E. C. Sanford has written the classic history of the personal equation, *Amer. J. Psychol.*, 2, 1888-89, 3-38, 271-298, 403-430.

Physiological Psychology

There is no history of physiological psychology, but a great deal of historical information can be extracted from Ladd and Woodworth, *op. cit.*

Animal Psychology

In animal psychology, M. F. Washburn's *Animal Mind*, 1926, is a handbook with such an historical orientation as to be almost a history of the subject; and there are C. J. Warden's historical articles: *Psychol. Rev.*, 34, 1927, 56-85, 135-168; or *A Short Outline of Comparative Psychology*, 1927, a reprint of the first with the bibliography missing; *Quart. Rev. Biol.*, 3, 1928, 486-522; and with L. H. Warner, *Psychol. Rev.*, 34, 190-205.

Mental Tests and Intelligence

There is a splendid history of both the tests and intelligence (for the two cannot be separated historically) by J. Peterson, *Early Conceptions and Tests of Intelligence*, 1925. Murphy, *op. cit.*, 347-372, has a section on intelligence.

Chapter 24

REVIEW AND INTERPRETATION

IN closing, it is worth our while to look back over the entire history of experimental psychology, so that we may see how modern psychology came about and, while leaving prediction to others, may venture a few interpretations of the past.

Science and philosophy began together as the result of a curiosity about the natural world. The two are essentially inseparable; nevertheless they became separated and the advance of science seems even to have depended upon the establishment of a practical separation. The paradox of the separation of inseparables has been realized because neither the whole of science nor the whole of philosophy can any longer be the property of any one man. Each and both belong to nothing less than civilization. We have, therefore, in understanding these matters, to take account of personal equations and of differences in human temperament. The rationalistic method of philosophy and the experimental method of science have tended to appeal to different men. There is, it is true, no necessary mutual exclusion of the two within a single mind. Aristotle was for his day both the philosopher and the scientist. Descartes was also both, although he was more the philosopher. Many men nowadays straddle the line. Nevertheless, these persons are but exceptions to a rule. In general, science, as viewed in terms of persons, has separated itself from philosophy, and the separation has become greater as the limitations of the capacity of a single human being have forced other separations, often quite arbitrary and artificial, between the different sciences. In the third century before Christ it was possible for an educated man to cope with the entire range of knowledge. To-day a competent philosopher is forced to take his science at second hand; he cannot be an expert experimentalist. A competent scientist is forced to take his scientific philosophy naïvely and at second hand. With both the range of attention and the span

the life limited, the individual becomes smaller and smaller as chap. 20 against the scope of knowledge.

It seems probable, however, that the cleft between philosophy and science is caused by a more fundamental human individual difference than are any of the clefts between the different sciences, and that science split off from philosophy, in this personal way, largely because of the rise of the experimental method. In the first chapter of this book we have said little of the great Aristotle and more of the lesser Aristarchus, who measured the distances to the sun and the moon, and of the lesser Eratosthenes, who measured the size of the earth. These two men were experimentalists, and the contrast of the experimental with the rationalistic method shows clearly in the simple beginnings of the former. Both philosopher and scientist require imagination, but the imagination of the scientist is limited by the scope of the experiment. The crucial test of empirical truth that the experimental method yields requires that the scientist be intimately concerned with small affairs, with the material objects upon which the precision of his results depends. Often mere manual dexterity is a factor in successful experimental work; the scientist must think in part with his hands, whereas the philosopher need not.

After science, in the hands of persons, separated from philosophy, physics split off from astronomy, biology from physics, physiology from biology, and then came scientific psychology. Whence did it come? It did not merely separate itself from physiology. It grew out of the fusion of a philosophical psychology that had been going on within philosophy and a physiological psychology that had been developing within physiology.

On the side of philosophy there had been a continuous tradition. Philosophical psychology became most explicit about the time of Locke, when philosophy became psychological, for then it was recognized that all knowledge is in some way a function of the mind. The tradition of Locke developed associationism, which became eventually all ready to be assimilated into an experimental psychology. Herbart recognized that psychology should be scientific, although he was not ready to accept the experimental method for the mind. Later, Lotze and Bain, just before Wundt, were trying sincerely to write physiological psychologies, and they failed, not through a mistaken intent, but because of these temperamental differences between persons: neither man possessed

the endowment to be a competent physiologist of the generation in which he lived.

On the side of physiology, the oldest experimental tradition is the research in optics and acoustics that was vivified by Newton. The age of this tradition has had much to do with the fact that to-day, in psychology, sensation is the most highly developed subject-matter, and vision and audition are the most thoroughly worked regions within the field of sensation. In the nineteenth century, physiology had become separated from the rest of biology and was turning to the problems of the nervous system. Bell discovered that the nerves can be divided into two classes, sensory and motor. Phrenology localized the mind in the brain. Flourens worked out some of the functions of the brain. Marshall Hall and Johannes Müller discovered reflex action, showing that movement in response to stimulation does not need always to be conscious. However, the sensory nerves balked the physiologists because they ended, as it were, 'in the mind.' Müller endoctrinated the theory of the specific energies of nerves as a physiological theory of mental qualities. E. H. Weber performed numerous experiments in which introspective report was one of the terms, and came out with, among other things, Weber's law. Thus there already was by the middle of the nineteenth century an experimental psychology, both introspectional and behavioral, within experimental physiology.

Then Wundt founded experimental psychology by fusing physiological and philosophical psychology. Wundt was a philosopher by nature and a physiologist by circumstance. It was a lucky accident that Fechner, a first-rate physicist turned into a second-rate philosopher by a crisis in his life, invented an experimental philosophy of the problem of mind and body, and so created psychophysics and a new set of experimental methods, all ready to Wundt's hand.

Wundt's maturity belongs to the days when philosophers not only believed that philosophy is dependent upon science, but actually tried to put their beliefs into practice by personal experimental work. Brentano, for instance, sent Stumpf to the chemical laboratory in order to make a better philosopher of him. As a consequence of this view, there developed in Germany, along with the new physiological psychology of Wundt, a very natural paradox. The new psychologists courted the experimental method

and eschewed metaphysics. Still they believed that all philosophy is scientific, and thus that all science is philosophical. Hence we find them rejecting philosophy with one hand and holding on to it with the other. We find Wundt, the philosopher, being convinced that Wundt, the psychologist, should avoid philosophy, and we find Wundt, the psychologist, writing both philosophical psychology and the psychology of philosophy. The contradiction was never resolved in Germany, partly because, as we have said, philosophy and science are essentially inseparable, although they may have to be separated in practice.

Wundt succeeded in founding the new psychology because he was a man of considerable power, with a stupendous capacity for the assimilation of information and for written exposition, and because he brought his weight to bear in the right direction at the right time. Not only had he Fechner for his purposes, but he had also Helmholtz, who, exactly at the right moment, was psychologizing the old Newtonian fields of optics and acoustics, making them over into the topics of 'vision' and 'audition.' The astronomers also helped with the personal equation, which was even then being studied independently by Donders as a method for mental chronometry. So Wundt took the nervous system from the physiologist, vision and audition from Helmholtz, certain fragments of experimental physiological psychology from Weber, psychophysics and Weber's law from Fechner, the reaction experiment from the astronomers, associationism from the British psychological philosophers; he bound them together with a systematic logic that he himself contributed, and he made the new psychology.

The new psychology was not quick to find either a personal or a physical home. The latter came in 1879 when Wundt founded the laboratory at Leipzig, but it is doubtful if at this date there were in the world any men beside Wundt who thought of themselves as being exclusively psychologists. Müller may have; Stumpf and James did not. Nevertheless people knew that the 'new' psychology had been born. James knew it in America, when he gave a course in psychology and started his great book. Bain knew it in England, when he founded *Mind*. Ribot knew it in France when he founded the *Revue philosophique*. Stumpf, Müller, and Ebbinghaus all knew it. Of course Brentano knew it, though he was not to be of the new movement. The '60's and '70's were really a period of preparation for later activity, when

Wundt wrote his *Psychologie* and when all the other lines from which he drew continued to develop independently, in the hands of philosophers or physiologists, as the case might be.

After 1880, however, the child began to do things for himself. The problems of space-perception, which Wundt had attacked in 1862, continued. The reaction experiment came into the Leipzig laboratory and flourished until 1893. Attention came into the laboratory for an equal span of life in the '80's and early '90's. Ebbinghaus experimentalized memory in 1885, and began a line of research that has never been terminated. Cutaneous sensation joined the other sense-departments as a profitable field of research at about the same time. The optical illusions enjoyed a most unusual popularity in the '90's. Feeling became the object of experimental work in the '90's, and even more so in the following decade. Külpe brought thought into the laboratory at the beginning of the new century. Not all the attempts to experimentalize mind succeeded, but on the whole the 'new' psychology was prospering. Moreover, the experimental results did not always die with the theories upon which they were based, but often turned out to have significance for some other theory.

Meanwhile America was importing the 'new' psychology from Germany and blending it with the Darwinism that America had from England. The Americans were relatively free from the internal personal conflict between philosophy and psychology. American psychology was never highly philosophical in any technical sense. Of necessity this situation was both an advantage and a disadvantage. The Americans were free to concentrate on the experimental problems, except as they were hindered by administrative union with philosophy in the universities. On the other hand, they imported the philosophical bias with the German psychology, and have never since ceased to be concerned with philosophical systematic matters in an inexpert and, therefore, ineffective way. However, the chief philosophical presupposition of American psychology did not require much attention because it came ready-made. It was the theory of evolution. Out of it came explicitly the functional psychology of Ladd, James, Dewey, Baldwin, and Angell, and implicitly the capacity psychology of Stanley Hall, Cattell, and a great many others. American psychology was practical, for it dealt with life, the adaptive value of the mind for the organism against its environment—an ap-

proach to psychology that would not have been possible but for Darwin. The rise of the mental tests in America in the '90's, and again later when Binet had pointed the way, is a concrete illustration of how Darwin affected Wundt on American soil. The development of animal psychology in America is another instance.

In the twentieth century the gains of the nineteenth were consolidated and further progress was made. In Germany, the laboratories, which had already multiplied, settled down to a period of productivity. The connection with philosophy was never severed, but was emphasized by the fact that the psychologists held chairs of philosophy in the universities. The act tradition survived as well as the experimental, and Külpe's Würzburg school eventually proved to have played into the hands of the act psychologists in spite of its experimentalism. There is to-day a general reaction against the elementarism and sensationism of Wundt, in which the school of *Gestaltpsychologie*, centered at Berlin, leads. In America, which equaled if it did not excel Germany in the founding of new laboratories of psychology in the '90's, experimental work on the classical problems continued, functional psychology grew up as a school and was replaced by behaviorism, the mental tests flourished, applied psychology developed, and animal psychology was brought into the laboratory for a healthy existence until it was in part absorbed by behaviorism. In England, where Galton had just missed starting a movement in mental tests founded upon his interest in evolution, the history of America repeated itself more slowly. Some laboratories were begun and there was a proportionate amount of research. Recently, applied psychology has prospered in Great Britain. In France the interest remains, as it has always been, primarily in abnormal and physiological psychology.

* * * * * *

It is now proper for us to ask ourselves the question: To what extent has the new psychology justified itself? There is a criticism of modern psychology, often spoken and sometimes written, that the new science has not quite succeeded, that it has been, as compared with its ambitions, relatively sterile, that it set out to study mind by the experimental method, and that it has gained a mass

of knowledge about sensation (which the physiologists might have gained), a little else, and nothing of great moment about the rational mind, the personality, and human nature. This criticism, when expressed by philosophers, may be suspected of reflecting the disappointment of philosophers at the turn which the new psychology took, or even of their reaction to the negativism of psychologists toward philosophy. However, it is not at all impossible that psychologists themselves might express some dissatisfaction with the advance of psychology, were they relieved from the necessity of defending themselves. Has psychology been relatively ineffective in attacking its problems?—we may ask, now that the perspective of seventy years is before us.

The reader can formulate his own answer to this question, but he is entitled to the author's opinion. Here it is.

The author believes that the application of the experimental method to the problem of mind is the great outstanding event in the history of the study of mind, an event to which no other is comparable. He believes that the person who doubts that the results have justified the importance that has been attached to the invention of experimental psychology must be either ignorant or influenced by a disappointment that the progress of experimental psychology has not aided in the solution of his own particular problems. On the other hand, the author confesses to a certain disappointment of his own that experimental psychology has not accomplished more than it has in its seventy years of life. It started with such high hopes that all that was needed was the willingness to experiment, patiently, honestly, and industriously, and it has found that mere faith in experimentalism is insufficient for great and rapid progress, unless that faith is accompanied by some flash of insight as to method. There have been perhaps many little flashes, in addition to the tremendous amount of careful, painstaking research, but there has never been in the history of experimental psychology a dazzling light. Psychology has progressed and developed in a manner similar to the course of any science under ordinary circumstances; but there has not, except for the initial inspiration in the thought of experimentalizing mind, been any great idea or discovery that has revitalized the science—for a science it is—opening up new fields, releasing new energy and removing hidden doubts. Why?

Perhaps the answer lies in the fact that there has not yet been time, but—if we are to follow the author's opinion further—there are at least two explicit reasons.

The first reason is fairly obvious. There have been no great psychologists. Psychology has never had a great man to itself. Wundt was not a great man of the order of Helmholtz or Darwin. He 'founded' experimental psychology, but in that he was more the instrument of the times than an originator. The rest of his influence was due to his persistence and to the sheer mass of his production, and perhaps also to the fact that he was senior and that no other psychologist was ever any greater than he. The influence of Helmholtz was considerable, and the influence of Darwin in America and England was profound, but both of these men affected psychology only from the outside, as it were. There are signs that psychologists are ready for a great man or a great event, for they seize eagerly upon every new movement that aspires to greatness; but the great event has not yet occurred.

The second reason which the author would assign for the less than maximal advance of experimental psychology is an internal conflict within psychology itself, a conflict that is the natural outgrowth of its history. Psychology has never succeeded in taking philosophy to itself nor in leaving it alone. Such a diagnosis is not founded upon the necessary relation of science to philosophy, for this conflict is not to be found in the natural sciences. In psychology it lies very near the surface. Often the men who cry out most loudly against philosophy in psychology are the men who regard psychology as a system and who write of epistemological matters. All the movements in psychology that have led to self-conscious schools—Wundt's physiological psychology, introspectionism in America, functional psychology, *Gestalt* psychology, behaviorism, but not animal psychology nor the mental tests—have been philosophical movements, conducted, for the most part, by men who would eschew philosophy and rely solely upon the experimental method. The degree to which the text of this book has entered upon quasi-philosophical and systematic matters, while purporting to be an account of the history of experimental psychology, is a measure of the admixture of philosophy to experimentation within psychology. Ever since their foundation, the journals of psychology in all countries have been weighted down with 'theoretical' papers that are really the expositions of psychol-

ogists, untrained to philosophy but writing on philosophical matters. Incompetent work can be ignored, but a division of the mind within psychology is not healthy. Inevitably it must hinder work in the individual and thus the most rapid progress in the science. There is too much to psychology now for psychologists to master their own material and philosophy too. Psychology ought to fare better when it can completely surrender its philosophical heritage, in fact as well as in voiced principle, and proceed, unimpeded by a divided soul, about its business.

APPENDIX

Appendix

FRENCH PSYCHOLOGY

THE exposition of this book is built about the fact that the genetic intellectual structure that is experimental psychology has been a scientific process focalized in Germany and Austria, in America, and to a lesser degree in England. Outside of these loci, work has been scattered and sporadic, and laboratories have been few. It seems, however, desirable to support this statement with an appendix which makes some mention of the psychology of France, the one other nation where the new science may be said to have prospered. Such neglect requires a word of explanation.

France has played an important rôle in psychology, but it has never been at the core of experimental psychology. We saw in chaps. 3 and 4 that, in the first part of the nineteenth century, France took the lead in experimental physiology and in the physiology of the nervous system and the brain. Paris was then the intellectual and scientific center of the world. Mesmerism, forced out of Austria, had flourished and been condemned in Paris. Phrenology, also driven out of Austria, had also flourished and been condemned there. Paris gave the cults a hearing and fostered the research that persisted. If there was in existence any center of experimental psychology before the days of Johannes Müller, that center must have been Paris.

The movement for experimental psychology belongs at first exclusively to Germany, because Germany specialized then, as now, in movements, and made way for them with massive steam-rollers of encyclopedias and systematic tomes. The trend in France was quite different. The physiology of the nervous system led toward a medical psychology, and France has become the first nation in abnormal psychology and psychopathology, while failing ever to develop a national experimental psychology in the slightly restricted sense of this term that excludes psychopathology.

A review of the French psychology of the last fifty years takes us into the history of hypnotism, physiological psychology in the most physiological sense, psychopathology, feeble-mindedness and intelligence, mental abnormalities in general, the psychology of genius and exceptional minds, psychic research, and social psychology. The books are theoretical or clinical for the most part. The great men, except Binet, were theorists or clinicians. The lab-

oratories for the normal human mind have been few and far between. Beaunis and Binet founded the first laboratory at the Sorbonne in 1889. Bourdon had one at Rennes in 1890. There have been some others, for instance Foucault's at Montpellier.

We have recounted in chap. 7 the rise and fall of mesmerism in France, and the rediscovery of hypnotism by the Académie des Sciences and by Liébeault about 1860. It was in 1875 that Richet attested the genuineness of the phenomena, in 1878 that Charcot began his demonstrations of hypnosis and started the school of the Salpêtrière, and in 1882 that Liébeault converted Bernheim and thus initiated the Nancy school.

Jean Martin Charcot (1825-1893) is the father of French clinical psychology. His connection with the Salpêtrière began in 1862 and continued throughout his life. His most important single work is his *Leçons sur les maladies du système nerveux* (1873, 4th edition, 1880, English trans. 1877); but his many scattered papers have been brought together in the nine volumes of his *Œuvres complètes* (1886-1890). For the references to his first demonstrations of hypnosis, see the notes to chap. 7. Binet was a pupil of Charcot's, and Freud studied with him for a year, and his place at the head of French psychopathology has since been taken by Janet.

Charles Richet (1850-) has been a most prolific writer. He has written on metaphysics, on the history of culture, on education, on thought-transference and psychic research, and on the problems of war and peace. He has with collaborators prepared a *Dictionnaire de physiologie* (1895). His favorable pronouncement upon the phenomena of hypnosis is in his paper *Du somnambulisme provoqué* (cf. notes to chap. 7). His books that are closest to psychology in the French sense are: *Recherches expérimentales et cliniques sur la sensibilité* (1877); *L'homme et l'intelligence* (1884, 2d edition, 1887); *Essai de psychologie générale* (1887, 10th edition, 1919).

Pierre Janet (1859-), Charcot's pupil, after a period in the provinces, was placed in charge of the course in psychology at the Sorbonne in 1895, and became professor in the Collège de France in 1902. Many books show his authorship, but he is best known for his *L'état mentale des hystériques* (1892, English trans., 1901), and the shorter book of Harvard lectures, *The Major Symptoms of Hysteria* (1907). He is now, as we have said, the dean of psychopathology in France, and his work takes on a wider significance as the line between the normal and the abnormal, especially in hysterical phenomena, becomes obliterated with the advanced study of the normal personality.

Binet himself dated the beginning of experimental psychology in France from the publication by *Hippolyte Adolpe Taine* (1828-1893) of *De l'intelligence* (1870, 7th edition, 1895, English trans.,

1871). Taine was of course primarily a philosopher who continued the French tradition of positivism from Auguste Comte (1798-1857). In general on the history of modern French psychology, as Binet saw it in 1897, see Binet's accounts printed in E. W. Scripture's *New Psychology* (1897, 464-469).

The actual advent of a new psychology in France was, however, marked by the early work of a young man, *Théodule Armand Ribot* (1839-1916). Ribot's first accomplishment was to introduce English psychology to France in *La psychologie anglaise contemporaine* (1870, 3d edition, 1892, English trans., 1874, 1892). A little later, when the German psychology was becoming definitely formed, in fact in the year that Wundt started the Leipzig laboratory, he performed the same service to France for the German movement: *La psychologie allemande contemporaine* (1879, 2d edition, 1885, English trans., 1886, 1892). In the meantime he had founded the *Revue philosophique* in 1876, the same year that Bain founded *Mind*. The *Revue* resembled *Mind* in being an organ for both psychology and philosophy until the more strictly psychological journals came to supplant it.

We see in Ribot the effectiveness of the French tradition. Ribot, approaching scientific psychology from philosophy and from the interpretation of English and German psychology, had no reason for believing that the general approach to mind should lie through the study of the abnormal mind, and yet such a conviction is the primary characteristic of his entire professional work. In part he helped to fix the general character of French psychology by his line of attack, but it must also be true that he found the method in the French atmosphere, in the French physiological tradition, and in the interest in psychopathology already being stamped in by Charcot. Moreover, Taine's important book on intelligence had been grounded upon the study of abnormal cases. The immediate evidence for this view about Ribot lies in the titles and dates of his books between 1873 and 1885, a period which antedates the foundation of most of the laboratories in Germany or America and precedes in a large part even the publication of Wundt's *Philosophische Studien*. These are the books: *L'hérédité psychologique* (1873, English trans., 1875); *Les maladies de la mémoire* (1881, English trans., 1882); *Les maladies de la volonté* (1883, 2d edition, 1884, English trans., 1884); *Les maladies de la personnalité* (1885, English trans., 1887).

In 1885 Ribot was placed in charge of a course of experimental psychology at the Sorbonne, and he was given a chair of experimental and comparative psychology at the Collège de France in 1889, the year in which Beaunis and Binet founded the laboratory at the Sorbonne. With these events his faith in the abnormal as the approach to the normal somewhat diminished, as his books

show: *La psychologie de l'attention* (1889, 2d edition, 1894, English trans., 1889, 1896); *La psychologie des sentiments* (1896, English trans., 1897); *L'évolution des idées générales* (1897, English trans., 1899); *Essai sur l'imagination créatrice* (1900, English trans., 1906); *La logique des sentiments* (1905); *Essai sur les passions* (1907); *Problèmes de psychologie affective* (1910); *La vie inconsciente et les mouvements* (1914).

On the other hand, it must be said that Ribot never abandoned the belief that the study of abnormal mental states is one of the most important ways of studying psychology, but rather that he, in his later years, combined this conviction with a belief in the evolutionary or genetic approach. His knowledge of psychopathology fixed upon him the belief in the importance of the subconscious mind, and would thus also have diverted him from purely introspective psychology had he had any leaning toward it. What happened, however, was that his faith in the rôle of the subconscious and his exposure to the physiology of the physiologist (for Wundt's introspective physiological psychology was never thoroughly the physiological psychology of physiologists) led Ribot into a motor psychology, in which movement, incipient movements, tendencies to movement, and kinesthetic consciousness dominate the picture.

Henri Beaunis (1830-1921), although about nineteen years older than Ribot and twenty-seven years older than Binet, comes into this picture mostly as Binet's older associate and as a psychological physiologist. It was Beaunis who, in 1884, supported Bernheim's thesis that hypnosis is a function of suggestibility and is not necessarily associated with hysteria, as Charcot had supposed: *Le somnambulisme provoqué* (1884). The physiological character of his work can be seen from the titles of his most important books: *Nouveaux éléments de physiologie humaine* (1875, third edition, 1888); *Recherches expérimentales sur les conditions de l'activité cérébrale et sur la physiologie des nerfs* (1884-1886); *Les sensations internes* (1889). As we have said, he was associated with Binet in the founding of the psychological laboratory at the Sorbonne in 1889. In 1895 he joined with Binet in founding *L'année psychologique,* thus giving France its exclusively psychological journal a few years after the founding of the *Zeitschrift* in Germany and the *Psychological Review* in America. Ribot, of course, was continuing the *Revue philosophique.* Beaunis retired from active work at about the close of the century.

Alfred Binet (1857-1911) was certainly France's greatest psychologist from our point of view. He was much more of an experimentalist than was Ribot. With Beaunis he began the first French psychological laboratory and founded the first French psychological periodical. He was very productive, and his work was careful and sound, as the subsequent history of the mental

tests has shown. There is no ground for grudging him his fame, even though it has been accentuated by the accident that the practical value and consequent adoption of the Binet tests have made his name almost a household word.

L'année psychologique contains some experimental articles by Binet, articles that resemble in subject-matter the output of the German laboratories and that show the freedom from philosophy in the interests of experimentation so characteristic of the 'new' psychology. However, even this work of Binet's was largely motivated by the problem of intelligence. There is available an excellent account of Binet's work by Joseph Peterson (*Early Conceptions and Tests of Intelligence,* 1925, 117-214), and we need do no more than indicate the general trend here. Peterson also gives a bibliography of thirty-three articles by Binet alone and twenty-one more by Binet in collaboration with other authors. Binet's life has been sketched by E. Claparède (*Arch. de psychol.,* 11, 1911, 376-388) and by Th. Simon (*L'année psychol.,* 18, 1912, 1-14). The reader may refer to chap. 21 for the relation of Binet's work to mental testing in America.

The way in which Binet came at the problem of intelligence is interesting because it shows the French tradition, already quite clearly established, reacting to the 'new' psychology. Taine's book on intelligence and Ribot's work on the abnormal personality already lay in the background. Binet's first book, *La psychologie du raisonnement* (1886, English trans., 1899), set the topic for his life-work, although it stressed the abnormal in being based upon hypnotic experiments. Binet made an excursion into the psychic life of micro-organisms in 1887 and of insects in 1894. *Les altérations de la personnalité* came out in 1891 (English trans., 1896). There was also his general *Introduction à la psychologie expérimentale* in 1894, published when the new laboratory was getting under way. However, what interested Binet most was the problem of personality, and this subject took him ultimately, not into the clinic as it did Charcot and Ribot, but to stupidity and genius. Thus in 1894 we find his studies of great calculators and chess-players and of blindfold chess. He recognized the fact that intellectual ability is developing in childhood and at the same time he sought to measure these changes by laboratory methods, the study of visual illusions and of tactile sensibility, and especially of esthesiometry for the discrimination of two points (1895-1903). *La suggestibilité* appeared in 1900. Meanwhile, with his pupil, Victor Henri, Binet had been undertaking a series of investigations of the mind of the child, and especially of memory (1894-1898). From this amorphous background there issued Binet's crucial book, *L'étude expérimentale de l'intelligence* (1903). The 'Binet scale' for the measurement of intelligence followed later with collaboration by Th. Simon, the first

scale in 1905, the revisions in 1908 and 1911. (Cf. chap. 21; also Peterson, *op cit.*)

A brilliant and effective life was closed unexpectedly and prematurely by Binet's death at the age of fifty-four in 1911.

The names of the other French psychologists in this period are less important. Victor Henri is known best for some of his articles in *L'année,* for his work with his teacher, Binet, and for his work with his other teacher, G. E. Müller: *Ueber die Raumwahrnehmungen des Tastsinnes* (1898), a study related to Binet's work on the tactile sensibility of school-children. Marcel Foucault is best known for his book, a doctoral thesis, *La psychophysique* (1901). As has been said, he had later a small laboratory at Montpellier. Benjamin Bourdon (1860-), who founded the second psychological laboratory in France in 1890 at Rennes and remained there for the rest of his life, is known particularly for *La perception visuelle de l'espace* (1902). He has also published *L'expression des émotions et des tendances dans le langage* (1892), and *L'intelligence* (1926). Georges Dumas (1866-) has published widely in the fields of psychopathology, sociology, and philosophy. More recently he has touched upon psychology also; e.g., *La tristesse et la joie* (1900); *Le sourire* (1906); *Traité de psychologie* (1923-1924); *Troubles mentaux et troubles nerveux de guerre* (1919). He is not an experimentalist, but a writer of broad interests and an encyclopedist. The *Traité* is a large handbook of psychology by numerous authors which Dumas edited. At present the leader of experimental psychology in France is Henri Piéron (1881-), who inherited Binet's laboratory at the Sorbonne and his editorship of *L'année* and was appointed to the Collège de France in 1923, to Ribot's old chair. He represents the French physiological tradition in psychology, a tradition that in a sense began with Flourens in 1824: *L'évolution de la mémoire* (1910); *Le problème physiologique du sommeil* (1912); *Le cerveau et la pensée* (1923, English trans., 1927); *Psychologie expérimentale* (1927).

French sociology and social psychology, and the work of such men as Gustave le Bon, find, of course, no place in this book.

Other Nations

At Geneva in Switzerland, on the periphery of France, and coming under the French influence, is *Édouard Claparède* (1873-). He has been the editor of the *Archives de psychologie* since its beginning in 1902. He is best known for his *L'association des idées* (1903), and his *Psychologie de l'enfant et pédagogie expérimentale* (1905, 6th edition, 1916, English trans., 1911). He has published on psychopathology, hypnosis, animal psychology,

and numerous other topics. Most of his papers are to be found in his *Archives*.

A. Michotte of Louvain in Belgium is on another French frontier. His book on the experimental psychology of space-perception, *Les signes régionaux* (1905), is well known, as is also his study of thought and action with E. Prüm: *Étude expérimentale sur le choix volontaire et ses antécédents immédiats* (1910). This latter study bears a close relation to the work of Külpe's Würzburg school (cf. chap. 17).

For a very brief statement of the early status of psychology in Italy, Russia, Switzerland, Japan and China in 1897, see E. W. Scripture, (*The New Psychology*, 1897, 469, 473). A fairly full account of experimental psychology in Italy in 1904 has been given by G. Chiabra (*Amer. J. Psychol.*, 15, 1904, 515-525) and G. C. Ferrari (*ibid.*, 16, 1905, 225-227). The fifth International Congress of Psychology met in Rome in 1905 under the presidency of G. Sergi, the sixth at Geneva in 1909 under Th. Flournoy, and the eighth at Groningen in Holland in 1926 under G. Heymans. There is an Italian journal of psychology, *Rivista di psicologie*, which was begun in 1905 and is edited by G. C. Ferrari. There are psychological laboratories in Italy, Spain, Switzerland, Holland, Denmark, Scandinavia, and Russia, and individual investigations and investigators are well known in Germany and the English-speaking countries. There are, however, in these countries no national trends or general bodies of research that have greatly affected psychology as a whole, and a superficial mention of men and laboratories would serve no useful purpose here. In so far as these investigators do not publish in German, French, or English, the language-barrier is apt to leave the work unknown and without the recognition which it deserves. An account of present-day Russian psychology has been given by A. L. Schniermann (*J. Gen. Psychol.*, 1, 1928, 397-404). In Japan there is considerable psychological work going on, and there is some of it in China. For the most part it consists in the assimilation of Western psychology, and whatever is strikingly original in it is still largely undiscovered in Europe and America.

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