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THE DEVELOPMENT OF MODERN MEDICINE

THE DEVELOPMENT OF MODERN MEDICINE

An Interpretation of the Social and Scientific Factors Involved

by

RICHARD HARRISON SHRYOCK

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1948

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IN MEMORY OF MY NEPHEW, STAFF SERGEANT
JOHN KNIGHT SHRYOCK, JR.,
400TH BOMB SQUADRON, UNITED STATES ARMY
AIR FORCE, WHO GAVE HIS LIFE IN THE SERVICE
OF HIS COUNTRY IN THE PHILIPPINE ISLANDS,
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PREFACE

THIS IS in no sense a study in the technical history of medicine, but is rather an attempt to portray certain major aspects of medical development against the background of intellectual and social history in general. Viewed in this perspective, the progress of medicine may take on new form and meaning. Medical historians have, of late years, clearly recognized the importance of this broader point of view; and they will, it is hoped, be glad to share it with general historians who have become interested in the same field. The latter realize, for their part, that the history of a vital group of arts and sciences is an essential part of cultural history as a whole.

An analysis of any science, in a given period, reveals something of the intellectual tone of that period. More than this, the story of medicine—like that of law—throws light upon social as well as upon intellectual conditions. Medicine relates to an art as well as to pure science, and medical practice concerns the most vital interests of individuals and of nations. The whole character of a society may be conditioned by the nature of the diseases common to it; and the happiness and progress of its people will depend to no little degree on the status of the medical sciences and on the extent to which they are employed for the public good. An attempt is made here to correlate these two basic themes, the history of medicine and the history of the public health.

In tracing the influence exerted by medicine upon society as revealed in the public health, it is recognized that a mutual relationship was involved. Just as medicine influenced society, so has society influenced medicine. For this reason, the social factors which entered into the making of modern medicine have received consideration, in addition to those of a strictly scientific nature.

The general conclusion reached in this study—namely, that the advancement of science was promoted by increasing resort to measurements and other quantitative procedures—is hardly a novel one. Rather obvious, also, is the fact that experimental method and the use of instrumental devices played important parts in the story. Yet it would seem that the significance of quantitative procedures, without claiming all things for it, is worthy of greater emphasis than is usually accorded it in medical historiography.

All these methods—measurement, experiment, and the use of instruments—were closely interrelated, and represented so many phases of the improvement in observation which was characteristic of modern science. The zeal for empiricism, of course, tended to go too far, and resulted at times in sharp conflict with the rationalistic traditions inherited from earlier periods. Some attention has been given here to the consequent methodological confusion which ensued in the medical sciences during the eighteenth and early nineteenth centuries, not only because of the interest the theme holds for medicine itself, but because this confusion was similar to that which had earlier obtained in physical science and which subsequently appeared in the social disciplines. In a word, an attempt has been made to suggest an historical or comparative approach to the problems of scientific method. Such an approach may prove suggestive, in view of the tendency of technicians in any given field to do battle, at a certain stage in its development, over questions of general methodology. This occurs in apparent indifference to the fact that much the same struggle went on in other fields, over a similar intellectual terrain, generations or even centuries before.

The narrative relates primarily to those lands which were major centres of scientific achievement—especially to Italy, the Netherlands, France, Germany, Great Britain, and the United States. Reference is also made to developments of special interest in certain other countries. A systematic account of the latter was prohibited by the complexity of the whole story and by the consequent necessity for setting some limits in time, place, and subject-matter.

R. H. S.

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I WISH to thank sincerely the historians, physicians, and librarians who have aided in one way or another in the preparation of this study. The debt begins with my former teachers in the University of Pennsylvania. Professor William E. Lingelbach first called my attention to the fact that the public health movement merited historical investigation. Professor Lingelbach, Professor Edward P. Cheyney, and Professor Edgar A. Singer, Jr., were kind enough to criticize my first work in this field. Professor M. P. Ravenel of the University of Missouri and Professor A. M. Schlesinger, now of Harvard University, aided me in the same manner.

I owe chiefly to Col. Edward Vedder, formerly Director of the U.S. Army Medical School, and to Dr. Joseph Willett, now of the St. Louis Department of Health, a practical instruction in certain medical subjects studied in the U.S. Army Medical School. This training proved an essential aid in later studies.

I have in later years been greatly indebted to former colleagues in the Medical School of Duke University. Dean Wilburt C. Davison, Dr. Roger D. Baker, Dr. Wiley D. Forbus, Dr. William A. Perlzweig, Dr. David T. Smith, and other members of the staff were patient in continuing my informal medical education. I am indebted in like manner to Dr. Seymour Fiske of the Lister Hospital in New York City, to Dr. Newlin Paxson of the Hahnemann Hospital in Philadelphia, and to Dr. E. B. Krumbhaar of the University of Pennsylvania. Recently it was my privilege to serve as a member of the Committee on Medicine and the Changing Order, of the New York Academy of Medicine, whose discussions and reports were most informative. I am much indebted to Dr. Malcolm Goodrich, chairman, Dr. Iago Galdston, secretary, and to other members of this committee.

Dr. John F. Fulton, of Yale University, kindly placed at my disposal materials in his library relating to seventeenth-century science in England, and made very helpful suggestions concerning the study as a whole. Mr. G. J. Drolet, of the New York Tuberculosis and Health Association, was generous in sending me the statistical charts prepared by that Association. All charts used in the present edition, unless otherwise noted, are reproduced by permission of the New York Tuber-

culosis and Health Association. Dr. Paul Diepgen, of the Institute of the History of Medicine at the University of Berlin, and Dr. Martin Müller of the University of Munich, assisted me in securing certain German materials.

A number of authorities in public health have at various times made helpful suggestions concerning the history of that field. Dr. Victor Bassett of the Savannah Health Department, former Surgeon General Hugh S. Cumming, Dr. Seneca Egbert of the University of Pennsylvania, and Dr. C.-E. A. Winslow of Yale University aided me in this way. Dr. John B. Andrews, of the Association for Labour Legislation, provided materials relating to the work of that organization.

The late Dr. Fielding H. Garrison and Dr. Henry E. Sigerist, of the Institute of the History of Medicine of Johns Hopkins University, were constantly helpful in making both general comments and in specific criticisms of the manuscript. Dr. Francis R. Packard, of the Pennsylvania Hospital, offered suggestions growing out of his wide knowledge of the history of American medicine. The late Dr. William H. Welch was always kindly and helpful in his suggestions, and I owe much to his encouragement.

I regret that it is hardly possible to list the libraries whose staffs have extended every facility. I received especial courtesies from Miss Judith Farrar of the Duke University Hospital Library, Mr. W. B. McDaniel, 2nd., of the College of Physicians of Philadelphia, Mr. F. H. Price of the Free Library of Philadelphia, Mr. F. E. Brasch and Mr. Morris Leikind of the Library of Congress, Mr. H. W. Robinson of the Royal Society in London, Miss Helen Jacob of the Medical Society of Munich, Miss Lotte Willich of the German Museum in the same city, and Miss L. Wistrand of the Royal Medical Board in Stockholm.

In conclusion, I desire to acknowledge my indebtedness to the American Historical Association for the award of the income from the Beveridge Memorial Fund in 1929, which enabled me to begin work on the present study. It was my privilege to know the late Senator Beveridge, and I take a personal pleasure in recalling the aid of a fund established in his memory.

The chapters relating to social developments are based in part on articles which appeared in the *American Historical Review*, in the *Annals of Medical History*, in the *Mississippi Valley Historical Review*, and in the *South Atlantic Quarterly*. Considerable use is made of materials edited for the *Bulletin of the Johns Hopkins Hospital* and for the Publications of the Trinity College Historical Society. The chapters on

scientific developments follow in part several series of lectures on the history of medicine given in the Medical School of Duke University, in the Medical School of the University of Pennsylvania, and at the Woman's Medical College of Pennsylvania. Last but not least, helpful suggestions concerning the present edition were provided by Dr. George Rosen, editor of *The Journal of Medical History*; and by Dr. Edmundo O'Gorman of the Archivo General de la Nación, Mexico, D.F.

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I

FIRST ATTEMPTS TO ESTABLISH A PHYSICAL SCIENCE

1600–1700

BENJAMIN RUSH, the best-known American physician of his day, remarked in 1789 that he found all schemes of physic faulty and that he was therefore evolving "a more simple and consistent system of medicine than the world had yet seen." [1] This, it soon appeared, was based upon a pathology in which all diseases were reduced to one, and all treatments likewise—a performance which greatly impressed his contemporaries, and left its author with the conviction that he had rendered medicine the same sort of service as the immortal Newton had contributed in physics. "I have formerly said," Rush declared to his admiring students in Philadelphia, "that there was but one fever in the world. Be not startled, Gentlemen; follow me and I will say there is but one disease in the world. The proximate cause of disease is irregular convulsive or wrong action in the system affected. This, Gentlemen, is a concise view of my theory of diseases. . . . I call upon you, Gentlemen, at this early period either to approve or disapprove of it now." [2]

Most of them approved, and went out over the United States practising the heroic treatments deduced from the master's doctrine. These treatments consisted chiefly of blood-letting and purging, which were supposed to reduce "convulsive action" by a process of "depletion"—euphonious name for exhaustion. They were, if good for anything, literally "good for what ails you", since by definition the same thing ailed everyone.

Nor were students the only ones impressed by the views of this medical philosopher. European physicians joined with fellow Americans in praising both his theory and practice. Lettsom, in London, declared that Rush united "in an almost unprecedented degree, sagacity and judgment", so that he had astonished Europe. And Zimmermann, in

[1] David Ramsay: *Eulogium on Benjamin Rush* (Charleston, 1813), p. 23.

[2] Rush: *Lectures on the Practice of Physic* (1796), I. No. 31, II. No. 1 (MSS., Library of University of Pennsylvania).

Hanover, wrote Lettsom that not only Philadelphia but all humanity should raise a statue to this American prodigy.[1] When Rush died, in 1813, he was widely acclaimed the greatest physician his country had known.

Three short decades passed, and an outstanding American physician of the next generation found himself revaluing Rush's medical essays. The results were rather startling. "It may be safely said," observed Elisha Bartlett in 1843, "that in the whole vast compass of medical literature, there cannot be found an equal number of pages containing a greater amount and variety of utter nonsense and unqualified absurdity."[2] A more sudden and extreme revision of scientific opinion could hardly be imagined. Here was Rush lauded by one generation and repudiated by the next.[3] The transformation was so complete that one naturally seeks some explanation. What far-reaching change must have come over the whole spirit of medical science, that the eulogy of Lettsom should have been so soon replaced by Bartlett's utter condemnation?

The truth is that Rush was so unfortunate as to be one of the last leaders of a medicine not entirely divested of the medieval tradition; while the very next generation witnessed the metamorphosis of this old art into a modern science. A similar repudiation of many of the older leaders and all they stood for occurred throughout Europe during the same period; Rush's fate was shared by Broussais in France, by Cullen and Brown in Britain, by Rasori in Italy, and by the whole *Naturphilosophie* group in Germany. Both the late date of this medical revolution—the modernization of dynamics, for instance, had occurred at least two centuries earlier—and the apparent suddenness with which it finally came, lend some interest to an analysis of the factors involved. These were many and varied, and in order to understand them one must recall briefly the circumstances attending the early development of modern scientific thought.

The development of a critical attitude during the Renaissance had not involved a complete break with the intellectual traditions of the Middle Ages. Such an attitude had long been maintained, even in Christian Europe, by alchemists and other empiricists who depended largely upon their own observations and experiments. Within the

[1] J. C. Lettsom: *Recollections of Dr. Rush* (Lon., 1815), pp. 12, 15.

[2] *The Philosophy of Medical Science* (Phila., 1844), p. 225.

[3] The condemnation was general; see e.g., O. W. Holmes: *Medical Essays* (5 ed., Boston, 1888), p. 193; W. Hooker: "The Present Mental Attitude and Tendencies of the Medical Profession," *New Englander*, X, 548–68 (n.s., IV), 1852; George B. Wood: *Medical Essays* (Phila., 1859), pp. 131, 132.

medical field, the high point of achievement was probably attained by the able surgeons of the thirteenth century who displayed some knowledge of aseptic procedures and of the use of anæsthetics. But during the next two centuries there was little further advance; indeed, there appears to have been some retrogression in science.[1]

Scholasticism, which remained dominant, involved an emphasis upon a universe of law and order and upon the possibility of understanding that order through logical thinking. It has been claimed that this afforded the very basis upon which modern science was later reared. Yet such rationalism was of a limited sort which accepted certain authorities as final, and which over-emphasized the role of logical thinking at the expense of observation and experience. During the later Renaissance the sciences finally reacted against this "rationalistic orgy". Sometimes they went all the way to the other extreme of empiricism—from an over-emphasis upon principles to an over-emphasis upon "facts".[2]

The growing zeal for observation and independent conclusions based thereon soon expressed itself in the medical as well as in the physical sciences, in anatomy as well as in astronomy, and both fields seemed simultaneously quickened by the new spirit. Vesalius respectfully questioned the authority of Galen, and Paracelsus thundered against the whole scholastic tradition in medicine, at about the same time that another physician, Copernicus, was demolishing the ancient cosmography. Both the physical and the biological sciences became fully alive to the possibilities of extending the range of observation through experimentation and also through the invention of such instrumental devices as the telescope and the microscope.[3] Experimentation not only extended the range, but tended to improve the conditions under which observation was performed, so that one could more often "control" the same in the interest of more definite and exact conclusions. Most significant was the fact that exactitude was even better served, towards the end of the sixteenth century, by increasing resort to mathematical procedures.

Scholasticism had long emphasized qualitative rather than quantita-

[1] Lynn Thorndike: *Science and Thought in the Fifteenth Century* (New York, 1929), pp. 10 ff.; Paul Delaunay: *La Vie Médicale Aux XVI^e, XVII^e, et XVIII^e Siècles* (Paris, 1935), p. 450.

[2] Cf. Alfred N. Whitehead: *Science and the Modern World* (N.Y., 1928), pp. 19 ff., 57 ff.; and A. Wolf: *A History of Science, Technology, and Philosophy in the Sixteenth and Seventeenth Centuries* (Lon., 1935), pp. 2 ff.

[3] John F. Fulton: "The Rise of the Experimental Method", *Yale Jour. of Biol. and Med.* (Mar., 1931), pp. 300–20; Wolf, op. cit., pp. 71 ff.

tive tests, classifications rather than measurements; although simple measures were of course always employed,[1] and unusual interest in mathematical procedures had been displayed by Roger Bacon and again by Leonardo da Vinci.[2] Medieval devotion to qualities may have been due to Aristotelian influence, with its emphasis upon biology rather than upon the mathematics of the Pythagorean tradition. Be that as it may, the seventeenth-century scientists in revolting against scholasticism reacted in particular against its qualitative lore, and set about measuring the things they observed. They were aided in this process by the fundamentally new mathematics which had penetrated into western Europe late in the Middle Ages—notably by the introduction of Arabic numerals, and by the use of the highly abstract conceptions of arithmetic and algebra.

It is not without significance that even prior to 1600 men began to measure time as well as spatial elements. Time, in ancient thought, had usually been conceived in a static, philosophical sense, although it was sometimes employed in astronomical calculations. Hence classical mathematics had been primarily geometrical in character.[3] When the scientific leaders of the late Renaissance came to think of time as a variable in relation to space, and consequently began to measure motion as well as form, they were expressing the same growing interest in life and activity which so transformed the fine arts during that period. Mathematicians now observed, experimented, and induced the laws of motion —the classic figure of Galilei, with his falling bodies and his pendulum, will always come to mind—and a science of dynamics, superimposed upon geometry and statics, epitomized the living spirit of the new age.[4]

Scientists gradually found that measurement afforded an effective check to reckless speculation on the one hand, and was a means of overcoming vagueness of observation on the other. In a word, quantitative procedures were calculated to obviate the chief dangers inherent in both the older methods and in the new. As a result of this, their success was

[1] Henry E. Sigerist: "Masse u. Gewichte in den medizinischen Texten des frühen Mittelalters", *Kyklos*, III (1930), 439.

[2] George Sarton: *Introduction to the History of Science*, II, Pt. II (Baltimore, 1931), 953; Robt. Steele: "Roger Bacon and the Story of Science in the Thirteenth Century", in Chas. Singer (Ed.): *Studies in the History and Method of Science*, II (Oxford, 1921), 145; H. Hopstock: "Leonardo as an Anatomist", ibid., p. 179.

[3] On the significance of the new mathematics, see Spengler: *Untergang des Abendlandes*, u.s.w., I (München, 1927), 90 ff., and the essay of Stephen d'Irsay: "Time-implied Function: An Historical Aperçu", *Kyklos*, I (1928), 52 ff.

[4] Sigerist and Pagel have emphasized this relationship between the arts and sciences of the period, with especial reference to medicine. See, e.g., Walter Pagel: *Jo. Bapt. van Helmont: Einführung in die Philosophische Medizin des Barock* (Berlin, 1930), pp. 4, 5.

at times sudden and spectacular. Kepler and Galilei found even mechanics and astronomy partly qualitative sciences; they left them mathematical disciplines. Within another generation Newton had projected terrestrial dynamics to the skies, and thus achieved in astronomy a unity and impressiveness which stimulated his contemporaries to go and do likewise in all scientific fields.[1] Small wonder that, from that day to this, there has been an increasing tendency to view a subject as "scientific" just in the degree to which it employs quantitative procedures.[2]

It is true that there have always been able men who questioned this; indeed, there were some who questioned the development of mathematical physics in the seventeenth century. It was hardly to be expected that scholasticism would succumb without a struggle to modern science, especially as the latter was indeed exasperating and at times extreme in its disdain for traditional lore. Hence Galilei was opposed and the "experimental philosophy" held up to ridicule, on logical as well as on theological grounds. In addition, both the originality and the utility of the new procedures were questioned. Had not Aristotle described these "new" discoveries centuries before? And if a few facts proved to be really novel, what of it? It was solemnly pointed out that even if telescopes *should* prove the planets to be inhabited, "things will fall out no otherwise than they do".

An interesting example of such disdain is to be found in the consistent conservatism of the English universities, which declined to recognize most of the new experimenters. As a result of this attitude, much of the most important scientific work in England during the next two centuries was performed outside their walls.[3] The universities were defended, from the first, on the ground that they prepared men well for the service of State and Church. Their academic training, it was observed, compared very favourably with the "mechanical education" of

[1] An interesting popular account of the work of Kepler, Galilei, and Newton will be found in Joseph Mayer: *The Seven Seals of Science* (N.Y., 1927), pp. 67 ff. For a more thorough analysis see A. Wolf: *A History of Science, Technology, and Philosophy*, chaps. 3, 6, and 7.

[2] "There can be little doubt, indeed", observes W. S. Jevons, "that every science as it progresses will become gradually more and more quantitative. Numerical precision is the soul of science", *The Principles of Science* (Lon., 1913), p. 273. "... the progress of science", declares J. T. Merz, depends largely "upon introducing mathematical notions into subjects which are apparently not mathematical", *A History of European Thought in the Nineteenth Century*, I (2 ed., Lon., 1904), 30, 314. See also Preserved Smith: *A History of Modern Culture*, I (N.Y., 1930), 155 ff.

[3] It is only fair to recall, of course, that Oxford had long been a centre for the older sciences; that is, for the scientific interests of late medieval and early modern times. See R. T. Gunther: *Early Science at Oxford* (2 vols., Oxford, 1923).

the moderns, "recommended with all the advantages that ariseth from Aphorisms of Cider, planting of Orchards, making of Optik Glasses, magnetick and hortulane Curiosities".[1] This was an incident in the long controversy concerning the relative merits of an academic and of a practical, scientific education; a controversy to which Herbert Spencer would contribute two centuries later, and one that echoes through the halls of American universities to this day.

The answer to scholastic conservatism during the seventeenth century was found in Newton's *Principia* (1687), which afforded so complete a vindication of quantitative physical science that it seemed to put an end to intellectual opposition outside of certain educational and clerical circles. In addition to mathematical demonstration, actual experimentation also served to overcome the critics of the new philosophy. Sometimes this experimentation was given dramatic form, with the deliberate purpose of impressing those in authority. Thus Otto von Guericke, the inventor of the air pump, arranged a demonstration of its wonders before a public gathering in Magdeburg in 1654. Before the amazed eyes of many dignitaries more than a dozen horses struggled in vain to pull apart the two halves of his metal sphere, from which the air had been exhausted. Once the air had been readmitted, these came apart—as Aristotle might have put it—of their own accord. What could have been more spectacular and, if one still harboured intellectual doubts about experimental science, more convincing?

The complete triumph for physical science, of course, came with the application of such clever devices to practical purposes. Once the new machines could meet a pragmatic test, once they actually served to lessen labour, only the most conservative academics would fail to be won over. This, too, was soon achieved; invention followed fast in the wake of pure reason.

It is sometimes forgotten that slow but definite progress had been made in various practical arts, ever since the eleventh century or even earlier. Hence the experimenters of 1600 or 1650 had available improved products in metals and glass, and many ingenious devices in wooden machinery. Notable among early technological achievements were machines devoted to the highly necessary purpose of pumping water, of which the London Bridge Waterworks afforded a good ex-

[1] Henry Stubbe: *The Plus Ultra Reduced to a Non-Plus*, etc. (Lon., 1670), pp. 10, 13. The reference above, to life on the planets, is taken from this same work. Opposition on the Continent to the new science, with particular reference to medicine, is discussed in Paul Delaunay: *La Vie Médicale Aux XVI^e, XVII^e, et XVIII^e Siècles* (Paris, 1935), pp. 453 ff.

ample. These works, begun by the German engineer Maurice, as early as 1582, proved capable of throwing a stream over St. Magnus's steeple. "Before which time", remarked a contemporary, "no such thing was known in England as this raising of water."

The seventeenth-century experimenters, taking their cue to some extent from these practical devices, displayed a marked interest in the pumping of both "airs" and liquids. Von Guericke's demonstration has just been mentioned. Others combined the interest in both gases and liquids, by experimenting with the ancient idea of a steam engine. More than twenty years before Newton first published the *Principia*, Edward Somerset, Marquis of Worcester, erected in London a steam engine capable of raising water to a height of forty feet; and in 1698 Thomas Savery patented a steam engine which was actually used to pump water out of English mines.[1] Once this stage was reached, mechanical physics was not only intellectually impressive—it worked!

The victory for modern procedures came, then, with relative ease in mechanics and astronomy. Would this hold true for the biological sciences as well? There seemed at first every reason to believe that it would. Great enthusiasm was shown for biological studies; botanical and zoological gardens were established, and a terminology of classical or modern terms was substituted for the traditional Arabic. The same groups and even the same individuals who worked in the mathematical disciplines contributed simultaneously to the sciences of life. Galilei, Boyle, Wren—great names occur to one in this connection. Descartes made the first basic attempt to envisage the animal body as a machine, and strove to describe its mechanisms as systematically as other mathematicians outlined those of the stars.

In one sense the results were similar, for just as the quantitative treatment of astral phenomena tended to destroy the more or less mystical conceptions of the astrologers, so the same treatment of biological data tended to overthrow the mystical vitalism that had dominated medical thinking since the days of Galen.[2] It appeared as though biology might abandon the traditional dualism of body and vital principle, either for the all-pervading living monads of Leibnitz, or for the out-and-out materialism of Hobbes.

In any case, the apparent revolution promised great things for medicine. The employment of mechanics in the study of the human body

[1] Wolf: *History of Science, Technology, and Philosophy*, p. 545. The quotation given just above is from the same work, p. 534.

[2] Paul Diepgen: "Vitalismus u. Medizin im Wandel der Zeiten", *Klin. Wochens.*, X (Berlin, 1931), 1434; Delaunay, op. cit., pp. 494 ff.

meant, almost by definition, an advance beyond simple anatomy. One can observe a logical progression, in the work of the Italian anatomists of the sixteenth century, from an interest in form to an interest in function.[1] Anatomy was the necessary geometry of the biological sciences —a matter of static relationships—but the quickening touch of mechanical conceptions brought it to life in the form of physiology. The problems involved in tracing the circulation of the blood related to motion in time and space, just as did those involved in charting the revolutions of the planets. And success in one field was likely to be as great as in the other.[2] It was no historical accident that Harvey followed Galilei, and that a whole "school" of iatrophysicists appeared, devoting themselves to experimental physiology.

The immediate results were interesting, sometimes brilliant. Harvey having demonstrated the circulation of the blood, Wren and others proceed to transfuse it from one animal to another, and even from man to man. One cannot read the accounts of Boyle's varied experiments,[3] or of Leeuwenhoek's amazing observations, without sensing the extraordinary vigour and brilliance of these scientific pioneers. One ingenious lens-grinder, almost unaided, laid quite definitely the foundations of all modern parasitology and bacteriology.[4]

Meanwhile, chemical as well as physical studies began to be made in medicine. Chemical therapeutics had had a long history, even before Paracelsus proclaimed that science should be diverted from the making of gold to the making of medicines.[5] The great German had emphasized the role of chemistry, not only in this connection, but in the understanding of nature as a whole. This view, though by no means acclaimed in his time, was revived by the iatrochemists of the seventeenth century. These ingenious men explained the greater part of physiology and pathology in terms of acids and alkalies, and added a therapeutics based on these principles.

Strange things these, so curious withal that in London a "Merry

[1] Arturo Castiglioni: *The Renaissance of Medicine in Italy* (Baltimore, 1934), pp. 49 ff.

[2] C. V. Daremberg pointed out, in 1850, that physiology was destined to revolutionize the traditional medicine with its four elements, etc., as no advances in anatomy could have done; *Essai sur La Determination et les Caractères des Périodes De L'Histoire De La Médecine* (Paris, 1850), p. 40.

[3] The wide range of Boyle's interests is indicated in John F. Fulton: "Robert Boyle and His Influence on Thought in the Seventeenth Century", *Isis*, XVIII (Bruges, 1932), 77 ff.; and in the same author's comprehensive bibliography of Boyle.

[4] Clifford Dobell: *Anthony van Leeuwenhoek and His Little Animals*, etc. (Lon., 1932), affords a most interesting and detailed account of this story.

[5] Ernst Darmstaedter: "Paracelsus u. die Einführung chemischer Präparate als Heilmettel", *Historische Studien und Skizzen Zu Natur—u. Heilwissenschaft: Festgabe George Sticker*, u.s.w. (Berlin, 1900), pp. 63 ff.

Monarch" deigned to incorporate a Royal Society for the pursuit of all manner of scientific investigations. This Society and similar bodies on the Continent became the foci for biological as well as physical research, and attracted the attention of the educated classes as much to one field as to the other.[1] It even became fashionable, in a few select circles, to indulge in scientific experiments.

The medical profession, it may be observed in passing, had always been influenced by the attitude of the dominating classes. Indeed, every society sets up an ideal of medical service which conditions the character of the profession therein. In early medieval times the practitioner was expected to be a priest; in late medieval days it was demanded that he be learned in academic lore. Now, under the influence of the "experimental philosophy", thoughtful men began to expect something more—physicians should investigate and think for themselves, or at least keep in touch with those who did. The satires of Molière, coming just at this time, were so many pointed indications that scholastic learning was no longer deemed an adequate medical equipment.[2] Hence a new social ideal, in addition to the immediate efforts of scientific bodies, afforded the encouragement so necessary to the furtherance of medical research.

In the first flush of the new order all things seemed possible. Could not remedies and cures be devised, as well as instruments and machines? Would not research sooner or later prove of benefit to the actual practice of medicine? True, the enthusiasm for research tended to divorce the sciences from their related arts; biological experimentation, for instance, seemed to have little relation to the medical practice of the day. Even in the latter field, however, there were manifestations of the critical spirit. The great English clinician, Thomas Sydenham, applied his objective investigations to both the treatment and to the description of diseases. Divesting his mind of much medieval tradition, he approached therapeutic problems in a relatively empirical manner. What procedures, he inquired, actually proved most effective? The results were moderately encouraging. Smallpox victims, it appeared, need no longer be half-suffocated in closed rooms in addition to their other sufferings.[3]

Even more significant, in the long run, were Sydenham's studies in

[1] Martha Ornstein: *The Role of Scientific Societies in the Seventeenth Century* (Chicago, 1928); Harcourt Brown: *Scientific Organizations in Seventeenth-Century France* (1934).

[2] Henry E. Sigerist: "Wandlungen des Arztideals", *Soz. Med.*, Sept. 1930; see also his article, "Der Artz u. die Umwelt", *Deut. Med. Wochens*, No. 25, 1931.

[3] David Riesman: *Thomas Sydenham, Clinician* (N.Y., 1926), p. 26.

the natural history of disease. Classical and medieval medicine had been concerned with the various conditions of the sick man, rather than with distinctions between different diseases as such. Their pathology had been limited to theories about the body fluids (humoralism) or to the opposing doctrine anent constricted and relaxed conditions in the solid parts (solidism). As a result of Galen's influence, the humoral pathology had usually been the more popular of the two. Medieval experience had led to the recognition of certain of the more obvious specific disorders, notably of leprosy and of smallpox; and in the sixteenth and early seventeenth centuries a number of other diseases were identified. (Fracastoro's work on syphilis is an example.) But as late as 1650 doctors still spoke largely of the sick man's humors rather than of any particular entity from which he suffered.

Sydenham did not reject Galen's humoral pathology, but he did devote himself enthusiastically to the differentiation of specific disorders. Where the classical physician wrote of disease, Sydenham wrote of diseases. Where the ancients had seen an inseparable connection between the patient and his malady, Sydenham saw in the patient certain pathological symptoms which he had observed in others and expected to see again. In a word, he distinguished between the sick man and the illness, and objectified the latter as a thing in itself. This was a new outlook, an ontological conception of the nature of disease which was eventually to prove of the utmost significance.

Sydenham arrived at this view because he was primarily a clinician, interested in describing the symptoms of his patients, rather than in investigations or speculations as to the inner causes of their condition. Doctors, he held, should spend their time in sick-rooms rather than in laboratories. There seemed a divergence here, almost an antagonism, between the interests of Sydenham and those of such contemporaries as Harvey or Boyle. Eventually the clinical and the laboratory types of research would be reconciled to their mutual advantage, but for a time each had to go its own way.

Sydenham left excellent descriptions of measles, dysentery, syphilis, and notably of gout—which he had the painful advantage of observing in his own person. He went farther and noted the natural history of epidemic diseases, ascribing their rise and decline to somewhat mystical "telluric influence". His writings, marked by a common sense so distinct from the scholastic verbosity still common in medical works, made an impression on the Continent as well as in England. Other physicians followed his example, and published studies of such widely

different diseases as pulmonary tuberculosis, apoplexy, and rickets. Ramazzini, in Italy, carried farther certain early attempts to describe occupational disorders.[1]

All this was most promising, since in the long run medicine could investigate neither the cause nor the cure of a disease until the disease itself was identified. There was just one essential difficulty in Sydenham's procedure. His descriptions of disease related largely to symptoms; and while these were important, they might prove endless, and consequently confusing. This was discovered to their cost by his eighteenth-century successors.

Meanwhile, it seemed that Sydenham's common-sense observations might point the way to a practical revolution in medicine. John Locke, himself a medical man, found his empirical philosophy well exemplified in Sydenham's studies, and was much impressed thereby. It seemed evident to him that both medicine and philosophy could be made over by the simple appeal to experience—experience writing on a blank page. All else was rationalistic fiction. "I wonder", he observed, "that, after the pattern Dr. Sydenham has set them of a better way, men should return to the romance way of physic." [2]

Nor was Locke the only thinker of that day who saw great promise in the medical sciences. Not only empirical philosophers were interested in this field. Leibnitz and Descartes, outstanding figures in the philosophical rationalism of the seventeenth century, viewed the new possibilities in hygiene and therapeutics as of paramount importance to mankind.[3] The latter, viewing the human body as a machine, could conceive of its continued repair and improvement. "*Si l'espèce humaine peut être perfectionée*," he declared, "*c'est dans la médecine qu'il faut en chercher les moyens.*" The French thinker, anticipating later *philosophes*, boldly envisaged a day when apparently incurable diseases would be overcome, and when the whole complexion of human life would be thereby transformed. Science, having revealed new heavens, would also provide a new earth.

[1] I have used the edition of Sydenham edited by Benjamin Rush and published in Phila. in 1815; and also the interpretations of his work given in Knud Faber's *Nosography* (2 ed., N.Y., 1930) and in H. E. Sigerist's *Great Doctors* (N.Y., 1933).

[2] Quoted in Faber: *Nosography*, pp. 16, 17.

[3] Eliz. S. Haldane: *Life of René Descartes* (Lon., 1905), p. 126. Medical opinion of the importance of Leibnitz and Descartes, in this connection, is given in Cabanes: *L'Histoire Eclairée Par La Clinique* (Paris, 1919), pp. 27–31. For the general intellectual background of this optimism, see J. B. Bury: *The Idea of Progress* (Lon., 1924), pp. 65 ff.; F. S. Marvin: "Science and the Unity of Mankind", in Chas. Singer (Ed.): *Studies in the History and Method of Science.*

II

THE PARTIAL FAILURE OF PHYSICAL SCIENCE

1700–1800

ALL THAT seemed necessary, in order to realize the hopes of philosophers, was for medical science to continue steadily along the way of the physical disciplines, and for medical practice to follow closely after. A good start had been made in this direction by the end of the seventeenth century. Then a strange and tragic thing happened. Strange, because at first glance it seemed inexplicable; tragic, because it was fraught with such dire consequences for human health and happiness. Medicine at first faltered, and then fell behind; physic failed to keep up with physics.

Looking back, one may explain this in terms of the greater complexity of the problems with which biological studies had to deal, but this was not so evident at the time. Some discouragement and consequent disillusionment ensued before this relative complexity was realized. And when it was, what then? A confused retreat to Aristotle and qualitative science, or a continued effort to push on, slowly and despite difficulties?

This remained to be seen, but meanwhile disillusionment was soon at hand. Observations and experiments, however interesting, did not fit readily together. The very multiplicity of phenomena that pressed upon the attention of the first experimenters made it difficult to bring order out of chaos. Where was the general scheme of things, whence could be deduced selective criteria? How could one distinguish essential data from the merely incidental? One is constantly impressed, in reading the early transactions of the Royal Society—which best preserve for us the scientific atmosphere of the times—with the juxtaposition therein of matters grave and matters trivial.[1] The exposition of important physical law must needs rub shoulders with a report on the latest village monstrosity. Naïve as this may seem, it was inevitable in the circumstances.

[1] Useful in this connection, for medical history, is S. Mihles: *Medical Essays and Observations . . . Abridged from the Philosophical Transactions of the Royal Society* (Lon., 1745).

Their difficulties, moreover, related not simply to the multiplicity of phenomena, but also to the methods available for studying the same. The quantitative procedures that achieved such splendid results in mechanics proved disappointing in physiology. Some phenomena did not seem measurable; and others, when measured, brought contradictory results. During continued studies of the circulatory system, for instance, Giovanni Borelli found that the heart exerted a force of one hundred and eighty thousand pounds, but Hales put it at fifty-one pounds, and Keil at only one.[1] Such discrepancies were disconcerting, to say the least. There was a natural failure, at first, to appreciate the great difficulty of the problem involved here.[2]

At the same time, the various "schools" of medicine inherited from classical authorities found little to aid them in composing their differences—no *principia* appeared upon which all could agree. Hence quarrels continued between the advocates of a humoral pathology and those who supported solidism; between those who believed in the healing powers of Nature and those who insisted upon "interference";[3] between those who favoured mineral remedies and those who demanded the botanical. Having no factual basis for a systematic pathology or therapeutics, physicians must continue to depend largely on speculation. And to the speculative mind a final logical solution, admitting no exceptions, was far more attractive than half-true empirical generalizations, which might hold to-day and then be abandoned to-morrow.

Something of the old scholastic spirit survived, in this connection, among some of the most critical investigators of the early seventeenth century. They still sought for final causes or ultimate realities, and consequently confused their objective findings with all sorts of metaphysical abstractions. It is rather startling to find that even Harvey, after all his splendid work on the circulation of the blood, felt it necessary to philosophize on the ultimate origin of the heart's motion. This he found in innate heat, which was ascribed to the soul or spirit of the blood; and this in turn was described as "identical with the essence of the stars". No less surprising, to the modern mind, was the concept employed in his work on generation; that is, the "transcendental notion of the impregnation of the female by the conception of a 'general

[1] Sir Gilbert Blane: *Elements of Medical Logic* (Lon., 1819), pp. 73, 98, 99. Cf. Sir Michael Foster: *Lectures on the History of Physiology* (Cambridge, 1924), pp. 73 ff.

[2] Cf. Paul D. White: *Heart Disease* (N. Y., 1931), for modern methods of measuring the force of the heart pump.

[3] Each of the schools, of course, had a long history behind it. See, e.g., Max Neuburger: *Die Lehre von der Heilkraft der Natur im Wandel der Zeiten* (Stuttgart, 1926), *passim*.

immaterial idea' ". Here was medieval realism very much alive indeed.[1] Yet, after all, one should not expect the intellectual habits of centuries to be overcome within a few generations.

It must be admitted that final solutions were more attractive to lazy, as well as to medieval minds. It required effort to find *how far* one theory might hold true and *how far* another; to what extent, for instance, one could trust to Nature's healing, and to what extent one must interfere with Nature. Hence the continued devotion to one theory *or* the other. Such mutually exclusive alternatives represented so many over-simplifications of the real problems involved—so many attempted short cuts across fields of still unrecognized complexity. Not until more was known about the medical sciences could men learn how little they really knew.

While the sciences were involved in such confusion, things fared no better with medical practice. Old treatments and remedies persisted, even as old theories and dogmas, the one being logically connected with the other. The traditional depleting procedures continued in vogue—bleeding, purging, blistering—and were associated with an amazing collection of drugs which was not substantially improved until well into the eighteenth century. This was a period of excrementory therapeutics as well as of excrementory humor. The London Pharmacopœia of 1618 listed such pleasing items as blood, bile, hair, perspiration, saliva, and wood-lice. Other remedies which were not repulsive were absurd and sometimes dangerous. Cotton Mather, writing to the Royal Society in 1724, observed that physicians in Boston advised the swallowing of "Leaden Bullets" for "that miserable *Distemper* which they called the *Twisting of the Guts*". He admitted that some difficulties had followed upon this treatment when a bullet entered a lung; and added cannily enough: ". . . from which . . . unhappy experiments, I think, I should endure abundant, before I tried such a remedy".[2]

His account of the treatment of "Thankful Fish, a young woman of Falmouth in New England" who was cured of frequent faintings by submersion in a mash of barley malt, relates to what might be termed the lighter side of therapeutics. So, too, did the adoption of certain American plants into the pharmacopœia. The optimistic employment of tobacco as a cure-all, for instance, can scarcely have been a great

[1] Thomas C. Allbutt: *Science and Medieval Thought* (The Harveian Oration . . . 1900. Lon., 1901), pp. 44, 45.

[2] Mather to John Woodward, Sept. 28, 1724; MSS. in the Royal Society's *Guard Books*, M, 2–3, No. 53.

hardship. And one is almost reconciled to the physic of the time upon discovering the statement of another Massachusetts worthy, who wrote that "Maple Sugar exceeds all others in its medicinal value, in the opinion of the New England physician".[1]

The general picture of early eighteenth-century therapeutics, however, is bizarre and sombre, and presents an unfortunate contrast with the light already thrown by Sydenham on sensible and humane practice. Despite his example and Locke's plain warning, men did "return to the romance way of Physick"—if, indeed, the majority of them had ever left it.

Since no practical benefits seemed to result from the pursuit of pure medical science, questions about the utility of research were naturally raised and were difficult to answer. Of what avail was it after all "to know that the pancreas has a duct"?[2] It is easy to observe that science, in the long run, was bound to enrich practice, but this was not obvious at the time. In any case this thought would have afforded small comfort to the sick, who could hardly wait a century or more for cures. Small wonder men complained that physic was a backward science; and that such criticisms were voiced for more than a century, until an open meeting of the British Association would hear medicine referred to as "the withered arm of science".[3]

Continued failure and disagreement among physicians naturally led to intellectual confusion. Some returned to the view that there was something peculiarly subtle about living phenomena which made physical methods inapplicable in this field. Perhaps the old dualism of body and spirit, long dominant in medical thought, was after all the soundest philosophy. Even Descartes—possibly with an eye to the theological windward—had reserved a site in his animal-machine for an immaterial moving principle. And now Georg Ernst Stahl (1660–1734) elaborated his rather mystical idea of the "sensitive soul" in the human body. This conception, when made more abstract and of wider application, became the "vital principle" of later eighteenth-century thought. A vitalistic philosophy again prevailed at certain medical

[1] Paul Dudley to John Chamberlayne, Boston, June 20, 1719; MSS. in ibid., D–1, No. 72. It may be observed, in this connection, that the influence of the Royal Society reached to America, as well as throughout Europe; see Frederick E. Brasch: "The Royal Society of London and Its Influence upon Scientific Thought in the American Colonies", *Scientific Monthly*, XXXIII (1931), 336 ff.

[2] Quoted in Victor Robinson: *The Story of Medicine* (N.Y., 1931), pp. 301, 302.

[3] An early instance of interest to Americans is Cadwallader Colden's observation, in 1720, that medicine was falling behind astronomical science; see E. B. Greene and R. B. Morris: *Guide to the Principal Sources for Early American History (1600–1800) in the City of New York* (N.Y., 1929), p. 323.

centres; for instance, at Montpellier under such leaders as Bordeu and Barthez, and can be traced well down into the nineteenth century in both France and Germany.[1]

This does not mean that the mechanistic philosophy was entirely abandoned. Indeed, its most complete medical formulation was that of Friedrich Hoffmann (1660–1743), who viewed the body as a machine operated primarily by a combination of muscles and nerves. Pathology, he held, related largely to muscle-nerve tone. Spasms at one extreme and *atony* at the other resulted in disease. This theory revived the general view of the ancient methodists (solidism), and it eventually made a deep impression in both Europe and America.

It is possible to view all the later eighteenth-century medical systems as adhering either to that of Hoffmann or to that of Stahl, according to their acceptance or rejection of the mechanistic philosophy.[2] This distinction is not always so significant as might at first appear. Most modern readers need to be warned that the vitalists were not necessarily a more speculative group than the mechanists. The latter were associated with the rather extreme speculations of the Brunonians; while vitalism on the other hand obtained among some of the ablest men, virtual founders of modern medicine.

Yet even those biologists who have revived the vitalistic conception during the past fifty years admit that it was of small service to medicine in the eighteenth century.[3] It is chiefly of interest here, in that its development was one indication of a natural reaction against physical ideas and principles. This reaction involved, in addition to the renewed interest in vitalism, other and more practical consequences. Indeed, it went all the way to a complete repudiation of physical and chemical studies in medicine. Jonker, one of Stahl's disciples, decided that chemistry was of no use to medicine whatever; while Bordeu and many other leading physicians in France came, during the latter part of the century, to the same sweeping conclusion.[4] Dutch and British schools maintained their interest in chemistry, and it was no accident

[1] Paul Diepgen: "Vitalismus u. Medizin im Wandel der Zeiten", *Klin. Wochens.*, X (1931), 1434 ff.

[2] Georg Honigmann: *Geschichtliche Entwicklung der Medizin*, u.s.w. (München, 1925), pp. 52 ff.

[3] J. S. Haldane: *The Sciences and Philosophy* (N.Y., 1929), pp. 66 ff.; Enriques Federigo: *Problems of Science* (transl. by Katharine Royce, Chicago, 1914), pp. 385, 386; Albert Vilar: *A Propos de Doctrines Médicales*, etc. (Paris, 1921), pp. 166 ff.

[4] F.-J.-V. Broussais: *Examen des Doctrines Médicales*, etc., II (Paris, 1821), p. 346; T. C. E. Edouard Auber: *Traité de Philosophie Médicale*, etc. (Paris, 1839), pp. 55, 56; Bordeu's views are analyzed in C. G. Cumston: *Introduction to the History of Medicine* (N.Y., 1927), pp. 351–6.

that Rush (trained in Edinburgh, under leaders influenced by Leyden) published the first American text on the subject. Yet the trend set in the other direction on the Continent.

The reaction against iatrophysicists was similar to that against the iatrochemists. The rare enthusiasm displayed by seventeenth-century experimenters was succeeded by a strange indifference, by a curious lag in the employment of their improved means of observation and measurement. Santorio had measured the "insensible perspiration", and Galilei the pulse and temperature; and about two generations later, Leeuwenhoek had so improved his lenses as to discover protozoa and bacteria. It is even possible that he used, in this connection, the dark-field method of illumination. Yet Leeuwenhoek lived to see his methods almost ignored; and neither pulse-counting nor temperature-taking was generally practised during the eighteenth century or the first half of the nineteenth century. Texts used in English-speaking countries as late as 1830 spoke of microscopy as an "art now almost forsaken". An eminent surgeon has testified that there were probably not half a dozen clinical thermometers employed in the largest Union army throughout the American Civil War.[1] And this was some two hundred and fifty years after their first introduction!

The long delay has been accounted for by difficulties met in the development of the instruments involved.[2] This doubtless does explain much, particularly in the case of microscopy. On the other hand, it is hard to believe that the problem of practical time-keepers could not have been solved, had there been sufficient will to do so. Indeed, at the very outset of the eighteenth century Sir John Floyer produced his "physician's pulse watch", only to have it ignored by the profession at large.[3] Nor did Stephen Hale's early success in measuring blood pressure (1733) and his other interesting quantitative studies make any more impression on general practice.[4] Apparently the reaction against the mechanical and mathematical schools involved a rejection of quantitative procedures as such.

[1] Oliver Wendell Holmes: *Medical Essays* (5 ed., Boston, 1888), p. 225; W. W. Keen: *Medical Research and Human Welfare* (Boston, 1917), p. 18.

[2] Charles Singer: *Short History of Medicine* (Oxford, 1928), p. 159.

[3] S. Weir Mitchell: "History of Instrumental Precision in Medicine", *Univ. Med. Mag.* (Phila.), Oct., 1891, reprint, pp. 11 ff.; see also review of J. L. Forney's *Versuch einer Würdigung des Pulses* (Berlin, 1823), in the *Dublin Jour. of Med. and Chem. Sci.*, II (1833), 97. On the thermometer see M. E. Renow: *Histoire de Thermomètre* (Paris, 1876). The general history of scientific instruments is treated in C. A. Crommelin-Leiden: "Over de Kunstzinnige versiering van wetenschappelijke instrumenten", *Elseviers Maandschrift* (1935); and in A. Rohde: *Die Geschichte der Wissenschaftlichen Instrumente* (Leipzig, 1923).

[4] Note, e.g., his observations on the diameter of the capillaries, included in J. F. Fulton: *Readings in the History of Physiology* (Springfield, Ill., 1930), pp. 75 ff.

This became most obvious in Germany, where the realistic *Aufklärung* was succeeded, at the end of the eighteenth century, by a wave of philosophical romanticism. Although not without its merits, this spirit was naturally opposed to prosaic processes of measurement and mathematical analysis. It preferred to soar on the wings of scientific insight and imagination. The general point of view found expression, at its best, in the scientific writings of Goethe; who, it will be remembered, opposed Newton even in the physical realm of colour-theory. Mathematical methods, the great poet-scientist believed, could be applied to nothing more complicated than purely physical phenomena. Biological, psychical, social phenomena—all these, he held, elude the profane hands of those who would reduce them to quantitative abstractions. A lover, for instance, might indeed measure his fiancée's *dôt*, but what scale could he apply to the more important matter of her emotions? [1]

While this reaction was widespread in Germany, it was not entirely limited to that country. As late as 1839, a French authority condemned mathematics as having done more harm than good in medicine.[2]

The abandonment of quantitative methods in medicine, or the failure ever to follow the pioneers who introduced them, necessarily meant a continued use of qualitative ones. This was consciously recognized by the medical leaders of the time. At the very end of the eighteenth century, for instance, Cabanis observed that "*En médecine tout, ou presque tout dependant du coup-d'oeil et d'un heureux instinct, les certitudes se trouvent plutôt dans les sensations mêmes de l'artiste, que dans les principes de l'art*". Ignoring the mathematical work of the seventeenth century, he went back to Hippocrates as authority for the view that no measurements could be employed, other than the physician's own sensations.[3] All this doubtless suited the inclination of most practitioners; for in the absence of proof to the contrary, qualitative distinctions usually seem the most natural ones to employ.[4] What happened can be well illustrated again in the case of pulse studies; for the age that ignored the

[1] Paul le Gendre: "Goethe et les Sciences Médicales", etc., *Bulletin de la Société Française d'Histoire de la Médecin*, XXVI (1932), 276, 277; see also Honigmann, op. cit., p. 61; and Paul Diepgen: "Goethe und Die Medizin", *Klin. Wochens.*, XI (1932), 1611 ff., for a favourable view of Goethe's medical interests.

[2] Auber: *Traité de Philosophie Médicale*, pp. 87, 88.

[3] *Du Degré de Certitude de la Médecine* (Paris, 1798), pp. 23, 117.

[4] Hence the fact that sciences go through a qualitative before a quantitative stage of development; F. C. S. Schiller: "Hypothesis", in Charles Singer (Ed.): *Studies in the History and Methods of Science*, II (Lon., 1921), 422. This does not necessarily mean that the quantitative stage entirely changes the nature of a science, or even that "it admits of reasoning of a high character"; on this point see Jevons: *Principles of Science*, p. 273.

pulse-watch witnessed the elaboration of a qualitative pulse lore, with its talk of the "critical" and "non-critical" pulse, the renal and the uterine pulse, and even of the pulse in terms of musical tones.[1] It is hardly necessary to point out the subjective and more or less speculative character of such learning.

Quantitative methods were neglected or abandoned just when their use might have checked a renewed growth of speculation. The eighteenth-century bent for medical speculation may be interpreted as a continuation of, or reversion to, earlier theoretical tendencies. But it was also the direct product of exactly the opposite procedure. The very success of observation and experiment prior to 1700 led, perhaps inevitably, to the subjective and fanciful medicine so popular for more than a century thereafter.

The explanation of this seeming paradox is really simple enough. The brilliant example set in mechanics had not only encouraged mathematical method as a means to an end, but had also demonstrated the end itself. This was the organization of new knowledge into a co-ordinated whole. Newton had brought nearly all known mechanical phenomena into harmonious relationship in this manner, and in terms of relatively simple mathematical law. He *systematized* mechanics and astronomy as no one had done before. Medical leaders were tempted to go and do likewise; and here they were, after a manner, led astray.

To begin with, physicians had the same reasons for desiring a grand synthesis as had Newton. In the biological field, as in the physical sciences, there had been a rapid accumulation of new data—often of apparently unrelated data. There was real need to bring order out of this chaos in medicine as well as in physics, if the former was to serve any other interest than mere curiosity. "For, when many new facts had been acquired," wrote Cullen in the eighteenth century, "it becomes requisite that these should be incorporated into a system, whereby not only particular subjects may be improved, but the whole may be rendered more complete, consistent, and useful." [2]

It seemed evident, then, that the mere accumulation of facts would

[1] Baas: *History of Medicine* (transl. by H. E. Handerson, N. Y., 1889), p. 625; S. Weir Mitchell, op. cit., pp. 14, 15. There is an amusing burlesque on pulse lore in Molière's *Le Malade Imaginaire*, scene 9.

[2] *First Lines of the Practice of Physick*, I (Edinb., 1796), 34. This "plethora of facts" is recognized as the chief cause of systemization by most medical historians; e.g., Baas, op. cit., p. 603; Victor Robinson: *The Story of Medicine* (N.Y., 1931), p. 334; S. G. B. Stubbs and E. W. Bligh: *Sixty Centuries of Health and Physick* (N.Y., 1931), p. 203. Singer observes (*Short History of Medicine*, pp. 138, 139) that the eighteenth-century physicians "must have been well-nigh hopeless of reducing their vast accumulation to order, when they contemplated the beauty and symmetry" of Newton's accomplishments in astronomy.

continue to confuse medicine, unless a rational synthesis were developed. No better illustration could be found than the confusing state of things in nosography, to which Cullen himself contributed. The effort to identify and classify specific diseases, which Sydenham had encouraged, went to unexpected lengths during the eighteenth century. The chief basis upon which Sydenham and his successors could distinguish diseases was that of clinical symptoms. This was well enough in the case of a few relatively clear-cut conditions; but as the study of symptoms proceeded with enthusiasm there seemed to be no end to these phenomena. Physicians tried to classify all symptom-diseases, in much the same way that they were classifying plant and animal species. When their nosographies listed as many as eighteen hundred presumably different illnesses, it became apparent that good intentions had made confusion worse confounded.

What was obviously needed here was some simpler principle of identification. Bewildering multiplicity must be reduced in terms of some inner relationships. Perhaps the many symptoms reflected only a few real pathological changes in the body of the sick man. Could students of pathology explain what these changes might be? Unfortunately the actual knowledge of pathology available prior to 1760 was anything but equal to this occasion, and even after that date was not entirely adequate. Systematizers had therefore to make the most of such clues as anatomy and physiology—wherein most of the new data had accumulated—could give them, and then to piece things out with theory where actual information was lacking.

Under such circumstances every genuine inductive advance in these sciences seemed to lead only to further speculation. Haller's great work on muscle-nerve functions ("sensibility" and "irritability") encouraged a vast amount of theorizing relative to nervous tension and relaxation. Perhaps all diseases could be reduced to forms of muscle-nerve tension or relaxation, just as the *strictum et laxum* philosophers had always claimed. As Hoffmann's system had already stressed a muscle-nerve pathology of this sort, Haller's work gave it a certain impetus, and was a factor in its further elaboration by John Brown. The latter's teacher, Cullen, had listed hundreds of diseases; Brown reduced them all to forms of two conditions, to excessive tension or to equally extreme relaxation. Rush then carried Brown's logic to its final conclusion, and recognized only the one condition of hypertension—the "system" proclaimed in the opening paragraph of the present study.

Current developments in chemistry, meanwhile, exerted a similar in-

fluence, despite the reaction against that science in some medical quarters. When oxygen and nitrogen were finally differentiated, for instance, each gas was promptly glorified as the basis of all pathology.[1] Nor could the claims of physics be entirely resisted, when some outstanding contribution like Galvani's experiments appeared. "Galvanism" was soon carried over into medicine. Current electricity so impressed the public that when regular physicians found small use for it, it became the basis for the most amazing cults of the next half-century —"Perkinism" and "Mesmerism".[2] Thus each forward step in objective science apparently led to an elaboration of medical systems, wherein old theories were simply restated in terms of new facts.[3]

It is unnecessary to enter into a detailed description of any of these wordy syntheses. English-speaking readers will find excellent examples in the works of Cullen and John Brown; or, for the United States, in those of Rush and in Mace's commentaries thereon.[4] The usual characteristic of a "system" was its monistic pathology. Most systematists assumed one "proximate cause" of all morbid phenomena, one pathological condition which "explained" all disease. Thus Rush held that every illness was a form of hypertension in the blood vessels; Broussais later explained everything in terms of gastroenteritis; and Hahnemann finally reduced nearly all pathology to its lowest common denominator in the form of psora or "the Itch". Such over-simplification, absurd as it seems to-day, was an ancient procedure, and was probably inevitable in early attempts at synthesizing morbid phenomena still too mysterious and complicated to admit of a more critical treatment.

Associated with this monism were secondary characteristics, most of which were also inevitable in the premises—imperfect observation, use of too-selected and otherwise inadequate data, loose reasoning (especially the careless use of analogies "to bridge the gaps"), mere assertion, haste, mysticism, and unchecked speculation in general.[5]

[1] Baas, op. cit., pp. 632–3.

[2] Favourable interpretations of Mesmerism have been published, as in Stephan Zweig's *Heilung durch den Geist* (Leipzig, 1931), pp. 66 ff.; but before being accepted should be compared with Victor Robinson's comments thereon.

[3] An almost contemporary account of these developments will be found in Cullen's *First Lines of the Practice of Physic*, I, 9 ff.; a generation later is that of William Whewell's *Philosophy of the Inductive Sciences, Founded upon Their History*, I (Lon., 1847), 548–71 (first ed., 1837)—based on Sprengel; see also F. H. Garrison: *History of Medicine* (4 ed., Phila., 1929), pp. 106, 107, and other standard histories.

[4] John Mace: *Proximate Cause of Disease* (Phila., 1802), pp. 55 ff. Rush's MSS. are more interesting, in this connection, than his published works.

[5] An excellent analysis of these logical weaknesses was given by Louis in his work on blood-letting (P.-Ch.-A. Louis: *Recherches sur les effets de la saignée*, Paris, 1835, pp. 87 ff.). Another interesting critique is that of Louis's pupil, Oliver Wendell Holmes, in his

Lest such comments seem too severe, it is well to offer brief illustrations of the sort of reasoning involved. In the case of therapeutics, it was quite common for systematists to base their conclusions upon mere assertions of the most dogmatic sort. Freteau, a French authority of the early nineteenth century, solemnly asserted that leeches were always to be employed in visceral diseases, upon no other ground than the fact that this was a common practice. Even such an explanation may be preferred to the rationalistic argument, so often used, that a particular treatment was indicated because it agreed with a particular theory. Thus because Brown dogmatically announced that a disease was due to lack of vital tone, it followed *a priori* that a stimulant must be employed. When a systematist did deign to mention evidence in the form of actual cases, these were often limited to two or three that seemed to prove the thesis in hand—"the mode of inference *per enumerationem simplicem*". So Freteau, again, insisted upon the necessity for venesection in certain conditions, on the ground of *two reported cases*. Each of these, incidentally, was susceptible of other interpretations than those he accepted.[1] Innumerable examples of this sort might be offered; indeed, it is amazing what little evidence actually supported some of the most rigorous treatments employed.

Illustrations taken from pathology, which was nearer to the heart of medical systems, are more suggestive than are those relating to therapeutics. The former are, if anything, also more bizarre from the modern point of view. A single instance—the chain of reasoning by which Rush reduced all diseases to one—may serve the present purpose. Granting the obvious fact, that Rush was influenced by the whole tradition of a *laxum* and *strictum* pathology extending clear back to classical times, it still remained for him to give some explanation of his sweeping conclusions. The argument, somewhat abridged and simplified, ran as follows. He had observed that fever is associated with a flushed skin; it was believed that this was caused by distended capillaries; he therefore concluded that the "proximate cause" of fever itself must be an abnormal condition ("convulsive action") in these vessels.

So far, so good. Passing then from fever in general to "fevers" in particular (from a general symptom, to what would to-day be viewed as so many disease entities),[2] Rush deduced that all fevers resulted from a common disturbance of the capillaries. Passing also, by easy analogy,

"Currents and Counter-Currents in Medical Science", *Medical Essays* (5 ed., Boston, 1888), pp. 185, 186.

[1] Louis, op. cit., pp. 85 ff.

[2] Or, at least, so many "clinical pictures".

from capillaries to veins, and from veins to arteries, he concluded that a hypertension of the whole circulatory system was involved. Finally, by a sweeping philosophical gesture as impressive as it was obscure, he included other pathological conditions in the category of fevers, and so reduced them all to varied forms of the one disease.[1] The reader, who may by this time have drawn his own conclusions, should be reminded that this represented the most popular American system as late as 1820.

In addition to their feeble logic, systematists exhibited characteristic personal failings. These were egotism, pride in a system as a sort of artistic creation, zeal to establish it as one spreads a new gospel, and a notable desire to defend it against all comers.[2] Such traits accounted for the bitter professional controversies in which some physicians indulged. Obviously, if one philosopher was right, the others were all wrong, and in such cases it was difficult to restrain one's feelings. Dogmatism in medicine was no more inclined to toleration than was dogmatism in theology. All the medical world knew what Cullen and Brown thought of each other!

Even where no definite controversy was involved, the general tone of medical writing was often more personal and pugnacious than would be conceivable to-day. Here, for example, is a passage taken at random from Broussais, written while he was still a dominant figure at Paris early in the nineteenth century. "Not content with having proved you of ignorance on this first point," he observed to his opponents in general, "we return to the valuation of those symptoms of fevers for which you have reserved the title of essential; and this new examination proves to us that these symptoms belong to these same inflammations of the digestive canal, which you have already misunderstood under other forms." Thus authorities who differed from him simply "misunderstood", or were guilty "of ignorance"! The same somewhat choleric Broussais was wont, during his continuous controversy with Louis and other clinicians, to refer sneeringly to "the maladies which are the creations of M. L——."[3] It is difficult to imagine a modern pathologist indulging in such a tone.

It is only fair to the systematists to repeat that each one hoped to bring order out of confusion, but as each was inclined to differ so militantly from the others, the confusion tended to increase. It was typical

[1] This analysis is based on Rush's lecture notes, and those of his students, as well as on his published works.

[2] How a system actually spread is described in the case of that of Broussais, in an anonymous work: *Conversations on the Theory and Practice of Physiological Medicine*, etc. (transl. from the French, Lon., 1825), pp. 297 ff.

[3] *Examen des Doctrines Médicales*, II (Paris, 1821), 726.

of the times that the keenest criticisms of any given system were often expressed by rival systematists, who were themselves guilty of exactly the same logical blunders as those they condemned.[1] The great Boerhaave essayed a synthesis of the better elements in several doctrines, but the resulting eclecticism tended to become just another school. The situation was becoming serious, for by 1800 not only physicians but the better-educated public were becoming increasingly alarmed over medical controversies and uncertainties.[2] What efforts could be made by medical men to clarify the whole situation?

It would be a mistake, in emphasizing eighteenth-century confusion, to overlook the contemporary protest against this confusion. Despite its persistence, rationalism did not entirely dominate the medicine of the period. The tradition of observation and experiment "carried on", though at times in a somewhat obscure and uncertain manner. More will be said of this in an ensuing chapter; but meanwhile it is to be noted that physicians who opposed speculation sometimes reacted to the other extreme, and demanded that *all* reasoning be banned from medicine. One should simply observe, they insisted, and in due time generalizations would stand out of themselves. They appealed to Hippocrates, to Sydenham, and especially to Francis Bacon as their authorities for this stand; though it was not clear that any of these great figures had gone to this extreme in method.

Indeed, some of the confusion concerning scientific method may be ascribed to continued uncertainty as to what Bacon's method was after all. Although frequently cited by those claiming to be pure empiricists, Bacon had condemned both the "empirics" and the "dogmatists", and had even observed that "the Empiric kind of philosophy produces views more mis-shapen and monstrous than the Sophistic or rational school". Nor had he limited his logic to induction, as was sometimes assumed.[3] Nevertheless, the illustrations of scientific procedure he offered simply suggested a piling up of all the facts relating to a given topic, and this hardly afforded a definite guide to methods of research.[4]

[1] For example, the excellent exposé of the shortcomings of the French schools prior to 1820, given in Broussais, op. cit., II, 359–68. This whole ironical situation was clearly analyzed by J. Gregory: *Observations on the Duties and Offices of a Physician*, etc. (Lon., 1770), pp. 109–11.

[2] R. H. Shryock: "Public Relations of the Medical Profession in Great Britain and the United States, 1600–1860", *Ann. of Med. Hist.*, n.s., II (1930), 314, 315.

[3] *Novum Organum* (transl. by G. W. Kitchen, Oxford, 1855), pp. 35, 78.

[4] Notably his "method of instances", which he illustrated in the case of heat. Hence he has been severely criticized by later authorities. J. S. Mill seemed to share his distrust of deduction (see his *System of Logic*, N.Y., 1846, p. 129); but this distrust has been condemned by later logicians, and few scientists have taken Bacon's actual procedures seriously.

It may be that Bacon's emphasis upon observation had exerted a laudable influence on all the sciences during the seventeenth century;[1] but his specific instructions were not such as to aid physicians caught in the methodological difficulties of the next century.

Nor could medical men find assistance in the formal logic of the period. It is true that one phase of the general revolt against scholasticism had been a sceptical reaction against traditional logic. The famous Port Royal School set this example during the seventeenth century.[2] The view there entertained, that scientific errors were usually due to false procedures rather than to false reasoning *per se*, might well have been taken to heart by some of the systematists. It is also true that such a methodological sequence as Bacon seems to have had in mind—"observation, induction, deduction, and verification"[3]—can now be stated with fair simplicity, but it was not always so clear as this.

The essential logical error of both the rational systematists and the empiricists, strangely enough, was the same. Both failed to appreciate the full significance of verification.[4] It is obvious that the rationalists were misled because they failed to verify their hypotheses. On the other hand, the empiricists rejected potentially valuable hypotheses and deduction in general, simply because they did not realize that proper verification could render the use of such methods reliable.

One phase of the empirical reaction of special interest was the ancient protest of art against science, which has been mentioned in another connection. It had been observed from the first that most of the "experimental philosophy" bore no direct relation to bedside practice. It was pointed out in early issues of the *Edinburgh Medical and Surgical Journal*, one of the first all-medical publications, that new discoveries in anatomy and physiology were not really aiding medical practice. Conversely, the great practitioners like Mead and Fothergill were not cultivators of "minute anatomy".[5] It was but a step from this to the con-

See, e.g., Jevons: *Principles of Science*, p. vii; Whitehead: *Science and the Modern World*, p. 63; Walter Bagehot: *The Postulates of English Political Economy* (Lon., 1835), p. 17.

[1] Max Neuburger thinks, therefore, that unfavourable criticism of Bacon has been carried too far—"Lord Bacon's Relations to Medicine", *Medical Life*, XXXIII (N.Y., 1926), 149 ff. Severe criticism, however, continues to appear; e.g., Jonathan Wright: "The Lights and Shadows Bacon Threw on Science in the Novum Organum", *Scientific Monthly*, XXVI (N.Y., 1928), 34 ff.

[2] Robert Blakely: *Historical Sketch of Logic* (Edinb., 1850), pp. 253–8, 476.

[3] Wm. McDougall: "Psycho-Physical Method", in T. B. Strong (Ed.): *Lectures on the Method of Science* (Oxford, 1906), p. 113. See also Thomas Case: "Scientific Method as a Mental Operation", ibid., pp. 15–19.

[4] On the role of verification, note A. Wolf: *Essentials of Scientific Method* (N.Y., 1925), p. 9.

[5] Cited by Elisha Bartlett, in his *Philosophy of Medical Science* (Phila., 1844), pp. 114, 115—an able presentation of the empirical view.

clusion that empirical observation was the only procedure of any real use to suffering mankind. Was not the one empirical product, cinchona (picked up, as it were, by the Peruvian Indians), of more comfort to the sick than all the elaborate "systems" put together? Physicians, said the empiricists, should therefore abandon logic and tradition, and simply observe, test, and try all things. Sooner or later, by trial and error, remedies would be found.

Shocked by such "raw empiricism", the theorists condemned it as an abandonment of reason—"man's noblest faculty"—and as a method leading to nothing but the meaningless accumulation of facts.[1] (It was such scorn that had long tended to debase the very term "empirical" in medical circles, while it continued to circulate at par in philosophical usage.) The rationalists then proceeded to the other extreme, and would recognize no good in their opponents, even though the latter succeeded where they had failed. A Scottish editor admitted that empiricism sometimes brought results in practice where rational methods had failed, but he denied indignantly that the mere curing of patients afforded any excuse for resorting to the despised procedure.[2]

An example of the practical results of such disdain is afforded in the case of the disease *la gale* (scabies). Empirical workers had long known of its parasitic origin and knew also how to eradicate it; but orthodox medicine had insisted upon interpreting it in terms of a humoral pathology and the like. This last was philosophically more impressive, but unfortunately it had no effect upon the disease.[3] Not until Bonomo took the empirical lore seriously towards the end of the seventeenth century did regular medicine recognize the real cause. Indeed, it is a question whether most physicians treated it intelligently until after Hebra's day in the nineteenth century.

The controversy concerning methods was of course age-old in medicine. Galen, in the second century, had quite clearly contrasted the rationalists and empiricists of his time.[4] Even the tendency to insist upon one procedure *or* the other, which the great Greek physician criticized severely, was one of those mutually exclusive simplifications—like the choice between mineral and vegetable remedies—which have

[1] See, among other examples, Cullen's analysis of Lieutaud's works, in the former's *First Lines of the Practice of Physic*, I, 35–7.

[2] *Edinb. Med. and Surg. Jour.*, XXI (1824), 160.

[3] S. Mihles: *Medical Essays and Observations . . . Abridged from the Philosophical Trans. of the Royal Soc.*, I (Lon., 1745), 337; Ugo Faucci: "Contributo alla Storia Della Scabba", *Riv. d. Stor. della Sci. Med. a Nat.*, ser. 4, XXII (Florence, 1931) Nos. 5 ff.; Reuben Friedman: "The Story of Scabies", *Med. Life*, XLI (1934) No. 8 ff.

[4] These interesting passages from Galen are included in A. J. Brock: *Greek Medicine* (Lon., 1929), pp. 130 ff.

already been noted. During the Middle Ages this choice had at least possessed the merit of being quite a clear one; no one could then be uncertain as to the crudity of the one procedure or the dogmatism of the other.[1] The very progress made during the seventeenth century tended later to confuse the issue. For each school had been impressed to some degree by the experimental philosophy, and each wished to claim the best results as its own. This was commendable in itself, but made distinctions difficult. The very terms "rationalism" and "empiricism" came to have a relative meaning, and their constant employment with no exact denotation must have been almost as confusing to contemporary physicians as it is to modern historians.

The Baconians, for example, did not really stand for "raw empiricism". By the eighteenth century this survived only in folk-medicine or among the quacks. In a word, the empiricists of 1800 were not at all the "empirics" their opponents accused them of being. On the other hand, the rationalists were so far from being pure theorists that they usually claimed to be the actual inheritors of the Baconian tradition. It is significant that they rarely displayed that awe for ancient authority *per se* which had so characterized the scholastics. Even the most theoretical of the eighteenth-century writers would have agreed in principle, on this point, with the protests of Paracelsus or of the Lord Chancellor. And the later systematizers claimed to be more than independent; they apparently went all the way with Bacon, and were forever condemning speculation and praising "true induction". Thus Cullen observed that his views represented "only a generalization of facts, or conclusions from a cautious and full induction". At another point he added: "I flatter myself that I have avoided hypothesis and what have been called theories".[2] Yet his system, as has been noted, was destined to influence the Brunonians, and so helped to spread a veritable plague of speculation across Europe and America. During this visitation an American systematist actually hailed the speculative Brown as the first true follower of Baconian principles![3]

Despite such inconsistencies, neither group involved in the controversy need be accused of "lip-service" or insincerity. Both faced difficulties that made inconsistencies inevitable. To all that has been said about the inherent complexity of biological phenomena, the need for a synthesis, and the confusion concerning the proper method of reaching

[1] Max Neuburger: *History of Medicine*, II (transl. by Ernest Playfair, Edinb., 1925), Pt. I, 72, 73.
[2] J. Thomson (Ed.): *Works* (Edinb., 1827), I, 469 (from ed. of 1784).
[3] John Mace: *The Proximate Cause of Disease* (Phila., 1802), pp. viii ix.

it, one must add that, paradoxically enough, there was still a lack of factual knowledge essential to further progress. So many facts were already known that it was difficult to handle them; yet more must be acquired before a synthesis could be achieved.

Viewed calmly and in retrospect, one can see that this period was one of protracted struggle between the old and the new. Certain aspects of medieval rationalism lingered in those sciences in which it was most difficult to establish modern methods. The same conflict had occurred in the physical sciences during the seventeenth century; but there it had been relatively short and incisive because of the relatively prompt triumphs of Newtonian mathematics. Rationalism in medicine was partly replaced, but in part simply disguised by the new philosophy.

III

SOCIAL FACTORS IN MEDICAL LAG AFTER 1700

BEFORE ATTEMPTING to explain the rapid progress finally made in medicine during the later eighteenth and the early nineteenth centuries, it is well to consider one other cause of the apparent lag during the preceding period. Complicated as the analysis already attempted may seem, it constitutes by no means the whole story. It so happens that medicine, in the ordinary sense, represents a group of biological sciences, but that medicine as an art is closely related to the social sciences. This is true, at least, if one may employ the term "art" in the rather broad sense of including both medical practice and the social relations which this involves.

Most sciences relate to corresponding arts, but few of the latter have such important social implications as has the practice of medicine. It is also noteworthy that the relationship between medical science and professional service is a peculiarly intimate and vital one—more so, for instance, than that obtaining between astronomy and the corresponding art of navigation. It was in many ways unfortunate that a close connection existed between so difficult a science and so vital an art.[1] This circumstance alone accounts for much of the delay in medical progress.

In tracing the influence exerted by medical practice upon medical science one may distinguish between, first, those social factors which influenced all sciences (and which may therefore be taken largely for granted here); second, those which involved the relations of physicians to their patients and to the public; and third, those which concerned the relations of physicians to each other.

The retarding factors involved in the public relations of the profession were often obvious enough; yet their significance looms larger the more one thinks about them. There is the fact that medical men were

[1] The first clear analysis of this situation I have found is in Auguste Comte's *Cours de Philosophie Positive*, III (Paris, 1908. First ed., 1830), 148–50. The subject of social factors in scientific work in general is analyzed in A. De Candolle: *Histoire des Sciences et des Savants depuis Deux Siècles* (Genève, 1885), pp. 262 ff. For a later discussion see Alfred E. Cohn: *Medicine: Science and Art* (Chicago, 1931), pp. 170 ff.

forbidden by public opinion to experiment with, or even to examine, the very material in which they were most interested. The human body could hardly be handled in the same manner as were test-tubes and pendulums. Roger Bacon observed as early as the thirteenth century that:

> "The operative and practical sciences which do their work on insensate bodies can multiply their experiments till they get rid of deficiency and errors, but a physician cannot do this because of the nobility of the material in which he works. . . . Wherefore physicians are to be excused for their defects more than are other workers in the sciences."[1]

It would be difficult to find a better statement, to this day, of a situation which has ever been a great handicap to medicine. Consider, for instance, the long opposition to dissection, which actually survived in Anglo-Saxon countries until after the middle of the nineteenth century. While British anatomists had resorted to illegal methods of procuring bodies long before this, the difficulties involved were considerable. Only after the search for "subjects" had led to a series of ghastly murders in Edinburgh was the British Parliament persuaded to pass its first "anatomy act" (1832).[2] Massachusetts, the first English-speaking commonwealth to legalize dissection, did so just before this, in 1831; but most American States failed to take action until well after the Civil War. It is thus fair to say that prior to 1830 there was never, in Britain or in British America, a proper supply of material for anatomical research. There was, to be sure, the opportunity to dissect animals, but the limitations of such a procedure were all too well known by this period.

Physiological experimentation on the human body was usually held unthinkable. When occasionally suggested, as in London in 1743, it was always severely condemned.[3] Much as one may justify this view on moral grounds, it was a serious scientific handicap. (Imagine Galilei's difficulties had the local mores prohibited the profane handling of pendulums and falling bodies!) Here again there was the possibility of using animals, and it is sometimes forgotten to what an extent this idea was exploited during the seventeenth and eighteenth centuries. During

[1] E. T. Withington's transl. of the MS. "On the Errors of Physicians", in Charles Singer and H. E. Sigerist (Eds.): *Essays on the History of Medicine: Presented to Karl Sudhoff* (Lon., 1924), p. 149.

[2] There are many accounts of the illicit trade in cadavers; but see especially James M. Bell: *The Sack-'Em-Up Men: An Account of the Modern Resurrectionists* (Edinb., 1928); and Alan F. Guttmacher: "Bootlegging Bodies", *Bull. of the Soc. of Med. Hist. of Chicago*, IV, No. 4 (1935), 353 ff.

[3] The proposal was to practise vivisection on a condemned criminal, in order to search for a canal between the mouth and ear.

the nineteenth century popular opposition in the form of the anti-vivisection movement developed even to this type of work.

There was, inevitably, much trial-and-error experimentation in therapeutics; but it is doubtful whether public opinion would have tolerated this had it possessed anything more than an individual and rather haphazard character. The suspicion that one was likely to be experimented on in hospitals was one reason for a popular fear of these institutions that survives until the present time. Popular feeling, even to-day, might conceivably interfere with therapeutic experimentation, whenever this necessitates withholding some treatment the public desires. (Readers of Sinclair Lewis's *Arrowsmith* will recall its fictitious but plausible dramatization of this theme.)[1] Physicians could and occasionally did experiment upon their *own* bodies, as when John Hunter inoculated himself simultaneously with gonorrhœa and syphilis. Or individual laymen might give permission for such a procedure. But the opportunities so afforded were rather limited.

To the extent that public feeling prohibited experimentation, medical men had to depend on passive observation in many phases of their work. Now, controlled observation may be as effective as the best experimentation, but here again medicine was in difficulty. For the problem of "control" was a complicated one. This was not, strictly speaking, a social factor, but it was similar to the difficulties faced by the social sciences. There were so many variables involved even in apparently simple clinical situations—in the common cold, for instance—that it was often impossible to isolate any one item for research purposes. The situation was analogous to that which confronts a sociologist when he attempts to evaluate the factors involved in so complex a matter as "the race problem".[2]

Not only were medical materials inherently difficult to handle, but clinical and pathological data were for the most part hard to collect. The astronomer found his materials ready at hand, and the chemist usually had no great difficulty in this regard; but the physician might never be able, in the course of his entire life's work, to collect adequate data relating to any particular disease. Despite the advent of the first scientific societies during the seventeenth century, the great majority of

[1] Popular pressure, on the other hand, may encourage the use of therapeutic measures which research workers consider of little or no value. For an actual illustration of the present time, see H. G. Bull: "As to the Use of Convalescent Serum in Infantile Paralysis", *Amer. Med.*, n.s., XXVII (1932), 4.

[2] Cf. the discussion of this factor in Singer: *Short History of Medicine*, p. 333, with that in W. I. Thomas: "The Relation of Research to the Social Process", in *Essays on Research in the Social Sciences* (published by the Brookings Institute, Wash., 1931), pp. 182 ff.

physicians had few means for observation beyond what their own practice supplied. Except for a few national bodies, there were, prior to about 1750, neither professional societies nor journals to bring them the observations of others. In England, for instance, even the Royal Society does not seem to have been associated with much medical work of importance during the first part of the eighteenth century. Not until the founding of the Warrington Academy in 1757 does a society appear which encouraged first-rate contributions to the biological sciences.[1] Local doctors no doubt compared ideas and experiences; but the use of even this simple procedure must have been hindered by the professional jealousy then common among practitioners. Medical men could, like other scientists, resort to correspondents; but the experience of the latter was also limited.[2]

Anatomists, to be sure, had less difficulty than clinicians in finding material, if they happened to reside where there was no public opposition to dissection. The same was true of physiologists, in so far as they could rely upon animal experimentation. The animals then used for this purpose were neither expensive nor rare. These facts, perhaps, had something to do with the relatively rapid progress made in anatomy and physiology, in comparison with other medical fields. Such uneven development had unfortunate consequences; since some of the confusion in medical "systems" was due to the necessity of co-ordinating the progressive medical sciences with the backward ones.

Hope here lay in the development of some institution which would insure a supply of clinical material, adequate for the needs of systematic research in pathology and in therapeutics. The modern clinic had its origin at Padua about the middle of the sixteenth century; and a realization of its values for research and education spread thence to Leyden, and finally to Vienna, Edinburgh, and other medical centres.[3] The establishment of successful clinics proved, however, rather slow work. In the first place, it was easier to talk about clinics than it was actually to administer them—just as to-day it has proved easier to advocate "observation and practice" in training teachers than it has been to organize

[1] John F. Fulton: "The Warrington Academy (1757–1786) and its Influence Upon Medicine and Science", *Bull. of the Inst. of the Hist. of Med.*, I (1933), pp. 50 ff. (Supplement to the *Bull. of the Johns Hopkins Hosp.*, LII).

[2] The first medical society in the English North American colonies, e.g., was founded at Boston in 1735, followed by others in New York (1749) and in Philadelphia (1765). Substantial organizations did not appear until after 1780, and the first real medical journal in 1797. A Mexican journal was established in 1772.

[3] Julius Petersen: *Hauptmomente in Der Älteren Geschichte der Medicinischen Klinik* (Copenhagen, 1890), pp. 62 ff.; David Riesman: "The Rise and Early History of Clinical Teaching", *Ann. of Med. Hist.*, II (1919), 136.

actual "practice schools". More important was the fact that an adequate supply of clinical material depended on the development of very large hospitals with clinical facilities, and this in turn had to wait upon the growth of great towns. Hence the whole trend of economic and social evolution between 1750 and 1850 exerted, in this connection, a direct and peculiar influence on the medical sciences.

Continued commercial development, and the whole series of changes conveniently summed up under the phrase "early industrial revolution", inaugurated during this period that amazing urbanization of population that has continued down to our own day. Cities appeared, first in England, later in other parts of western Europe and in America, where only towns or villages had existed before. Large towns doubled and trebled in size. Many thousands moved from the land into crowded urban centres, and there found themselves facing new problems and new dangers. Not the least of these related to health and disease. Historians have differed as to whether the lot of the poorer classes did or did not become the worse for this change in habitat, but no one can doubt that disease and other miseries were at least made more obvious. Voices once lost in rural isolation now swelled to an unmistakable wail from out the noisome tenements of Manchester and Glasgow. Who would be the first to heed?

To their credit be it said that physicians often led in the demand for relief. The more far-sighted among them saw the need of institutions to care for the sick poor, and set about finding means to establish them.[1] Christian tradition had long supported hospitals, but the old Church institutions were hardly equal to the emergency created by new conditions. Fortunately the same social changes which had led to this emergency also provided, to some degree, for its solution. The Industrial Revolution that apparently brought misery to the working people, also brought wealth and leisure to other men; and thus placed the latter in a position where—if they were so inclined—they could afford to take a benevolent interest in those less fortunate than themselves. They were the more likely to be so inclined because other influences of the day, such as increasing travel and a growing literature, made them more familiar with the conditions of the poor. It is at least a plausible explanation of the origins of eighteenth-century humanitarianism to say that this familiarity led the prosperous to identify themselves with a great number of unfortunates, and therefore to extend to them the

[1] The role of physicians in the humanitarian movements will be considered again in relation to early public health activities under Chapter V.

instinctive sympathy hitherto reserved for immediate associates.[1] Not only the "parish poor" but the poor in general—whole social classes—might become the object of one's altruism.

Men who in former times would have been just kindly souls now found themselves humanitarians and reformers. The concentrated suffering of the slums made an insistent claim upon them. They supported physicians who called for hospitals, perhaps subsidized the same, or urged local authorities to do so. Old hospitals were reorganized and enlarged, and new ones were established. In English-speaking lands, especially, institutions began to limit themselves to the care of the sick, rather than to continue the medieval practice of providing for unfortunates of all kinds and conditions.[2]

This "hospital movement" was a phenomenon of the larger cities. Transportation facilities were not such as to permit smaller places to care for the sick of surrounding areas, even if there had been a desire to do so. Hospital expansion was especially notable in the great capitals—London, Paris, and Vienna afford the best examples—but the same development was under way in nearly all places of any size. Americans will recall the growth of Philadelphia and the founding of the Pennsylvania Hospital (1751) as a familiar illustration in the New World.[3]

These hospitals meant more to medical science than might at first appear. In the Philadelphia institution, for example, was established not only the first dispensary, but also the first real medical library in British America. A century later this still contained the largest collection of its kind in the United States. But most important, to return to the original point, was the fact that the hospitals provided for the first time a supply of clinical material adequate for real research. Thousands of patients passed annually through the wards of St. Bartholomew's in London, the *Charité* in Paris, the *Allgemeines Krankenhaus* of Vienna, and other institutions of this type; and any attending physician could study as many of these poor people as his time and inclination permitted. No wonder that as hospitals grew, so grew systematic investigations into the phenomena of disease, and eventually into methods of

[1] The thesis is elaborated in Maurice Parmalee: *Poverty and Social Progress* (N.Y., 1916), pp. 233 ff.

[2] See R. M. Clay: *The Medieval Hospitals of England* (Lon., 1909), or Dorothy L. Mackay: *Les Hospitaux et la Charité à Paris au XII^ième Siècle* (Paris, 1923). The development of eighteenth-century British hospitals is told in A. D. Evans and L. G. R. Howard: *The Romance of the British Hospital Movement* (Lon., c. 1930), and in J. D. Comrie: *History of Scottish Medicine*, II (Lon., 1932), 449 ff. For an excellent brief account with definite figures, see G. T. Griffith: *Population Problems of the Age of Malthus* (Cambridge, Eng., 1926), pp. 217 ff.

[3] Francis R. Packard: *History of Medicine in the United States*, I (2 ed., N.Y., 1932), 179 ff.

treatment. Yet these institutions had been built in the first instance for philanthropic reasons. Thus did humanitarianism make possible the progress of science.

It was slow work at first, and constructive results of clinical and pathological studies could hardly be expected for some decades at best. One reason for this deserves special attention; namely, the simple matter of the time available for research. Here was a professional problem that affected all types of medical inquiries. How many hours per day could a medical man devote to original investigations? It was inevitable, in a period when there were few foundations or patrons to subsidize research, that the physicians who worked in hospitals should also continue their private practice. They were rarely in a position to give their full time to pure science. Young men just launched in practice doubtless had adequate time, but they often lacked the necessary funds, ability, or hospital connections. Moreover, if the beginner did find patients, he could not afford much research if he were to live; and once a living was assured, there was still a fortune to be made.[1]

These difficulties doubtless exerted a salutary check on mediocre investigations, but even the born research man faced similar handicaps. He must perforce serve two masters, science and mammon, and so was constantly distracted from the one by the necessity of pursuing the other. It was not only John Hunter who was worried by the eternal chase after "that damned guinea". (Hunter found it necessary to rise at four every morning, in order to work among his specimens before beginning the day's routine.)[2] Even the professor was likely to be the busiest of practitioners, in contrast to his colleagues in the chairs of mathematics or physics, who could give their time largely to science for its own sake. And routine practice bore little relationship to any special problem the physician might investigate. He must therefore overwork himself if he was to accomplish anything original, and many could not stand the pace. Those who could did not always stand it long.

Another way of explaining this situation would be to say that the need for specialization in research appeared earlier than did the recognition of specialities in practice; and this made it all the more difficult to combine the two interests.[3] One might wish to study the eye, but only quacks could confine their practice to ophthalmology. In a word, pro-

[1] Robinson in *The Story of Medicine*, p. 336, gives an interesting quotation from Weikard of Fulda on this point.

[2] S. D. Gross: *John Hunter and His Pupils* (Phila., 1881), pp. 18, 19.

[3] On the relations of increasing specialization to this problem, see Candolle: *Histoire des Sciences et des Savants depuis Deux Siècles*, pp. 265 ff.

fessional usage required that a doctor see all types of cases, regardless of whether he wished to concentrate on any one or not.

Many promising proposals must have proved fruitless because their authors were too busy to follow them through. Fordyce, and "probably many other physicians" of the eighteenth century, wished to use a system of clinical observations and post-mortem reports similar to that so effectively employed by Louis at a later time; but "the difficulties attending the plan in private practice" discouraged them.[1] Practice also prevented busy doctors from following the contributions of others—a situation which handicapped research as well as daily service to patients.

If, in spite of all obstacles, a physician succeeded in accomplishing original work he was open to attack by both laymen and "the faculty", on the ground that he was cultivating science at the expense of his patients. Harvey was subjected to considerable criticism of this sort. The question involved continued to worry research men well through the nineteenth century. When Broussais, for instance, could not deny Louis's advances in diagnosis, he complained that these possessed little value, since they had not resulted in cures.[2] Auguste Comte answered such criticism by pointing out that pure science is certain in the long run to benefit the practical arts; but it is not clear that his views exerted any influence. Comte, incidentally, emphasized the difficulties involved in leaving medical research to busy practitioners, and in order to drive the point home employed an interesting analogy taken from the physical sciences. What, he inquired politely, would have become of astronomy if its development had been left entirely to the sea captains?[3]

Even after questions of time and materials have been discussed, one has not yet penetrated to the centre of the problem of social pressure on medical science. This lay finally in the following circumstance. Physicians were the only scientists who, because they were also practitioners of a vital art, were constantly being pushed to hasty and careless conclusions. Other research men, uncertain in the face of new problems, could suspend judgment and proceed with due caution. Practitioners confronted with dying patients did not dare to wait; they must act quickly and, if necessary, "take a chance". Even during hours stolen

[1] James Jackson, in the preface to Louis's *Researches on the Effects of Blood-letting* (transl. by C. G. Putnam, Boston, 1836).

[2] *Examen des Doctrines Médicales*, etc., II (Paris, 1821), 736.

[3] "*Ceux qui rejetteraient comme absurde la pensée de confier aux navigateurs la culture de l'astronomie finiront probablement par trouver étrange l'usage d'abandonner, d'une manière analogue, les études biologiques aux loisirs des médecins; car l'un n'est pas, en soi, plus rationnel que l'autre*", *Cours de Philosophie Positive*, III (Paris, 1908, I ed., 1830), 149.

for research they were still under pressure to get practical results *as soon as possible*. Such circumstances played an obvious role in encouraging the medical systems discussed above, for their authors were motivated not only by scientific interests, but also by the practitioner's dire need of short cuts to cures. Treatments must be given and, lacking sufficient facts, they had to be drawn from partial evidence or even from pure theory.

Much may be forgiven Benjamin Rush, for instance, if his dogmatic statements are recalled only in connection with the actual circumstances of his career. One should go with him through the desolate streets of Philadelphia during the appalling yellow fever epidemic of 1793—past the dead-carts with furtive figures walking well in the rear, into houses in which only children remained to greet the doctor silently, and then back to his own home to find both mother and sister desperately ill. Would there be no end to the heat, the dreadful fever, and the dying? Surely something could be done: some remedy *must* be found! Would it not have seemed sheer frivolity to have spoken then of "suspended judgment" or of "scientific caution"? One grasped at any hint in such emergencies, worked it up into a complete treatment, and tried desperately to believe that all recoveries which followed were due to its employment. Soon one had a cure, and hastened to announce it to a public which also wished to believe. In a word, while true medical science often waited upon progress in other sciences, medical practice could rarely afford to wait.[1]

And here again the plot thickened. Just because the systematists claimed so much for their treatments, just because some of them offered panaceas, they long received popular encouragement. Only towards the end of the period under consideration did there appear a growing suspicion that these claims were not well founded. System-makers were impressive and promised much; while genuine research men were more or less isolated and promised nothing. Popular favour, therefore (in addition to the need for a synthesis, and the desire to cure), tempted medical leaders of the seventeenth and eighteenth centuries to speculate and systematize. "I should have gained more reputation", observed Sydenham, "by advancing some trifling and useless speculation" than

[1] This situation was analysed by Max Simon: "De l'influence dangereuse, que les théories médicales peuvent exercer sur la société", *Déontologie Médicale* (Paris, 1845), pp. 414 ff.; and again by Theod. von Bischoff: *Ueber den Einfluss des Freiherrn Justus Von Liebig auf die Entwicklung der Physiologie* (München, 1874), pp. 60 ff. What would have happened, asks the latter, if medicine had proceeded as cautiously as physical science? "*Durften wir da nur einen Bissen in den Mund nehmen, oder einen Schnupfen anders als mit Angst und Zittern behandeln?*"

by writing a history of diseases.[1] Shortly after his time a physician of Paris, M. François, made the same complaint concerning French opinion. He referred to "the little Regard which the Public always pays to Observers; the high Esteem on the contrary, bestowed upon the Inventors of Systems, and upon their Followers"; and he thought the effect of this so serious as largely to account for the uselessness of the contemporary medical literature.[2] The insidious influence of popular favour continued to be a matter of protest among critical physicians for more than a century after this. Oliver Wendell Holmes noted, for instance, that as late as 1860 the physicians most praised by the American public were "showy talkers rather than . . . sagacious observers".[3]

Popular feeling occasionally interfered directly with medical progress. This was notably the case in obstetrics, in which field moral and religious questions complicated the purely scientific ones to an unusual degree.[4] Opposition to "man-midwives" in English-speaking countries, prior to about 1800, will be recalled as a case in point. Physicians themselves, moreover, have been accused of permitting their moral—or immoral—prejudices to interfere with scientific obligations in this field. The whole profession has been accused of sadism, because of the long delay in employing anæsthesia in obstetrics.[5] Be this as it may, the laity has itself interfered with the alleviation of suffering in childbirth. Protestants have glorified its pain on Biblical grounds; while Catholics have refused to save the life of the mother, in certain special cases, as a matter of religious principle.

The public patronage of quacks and charlatans may have been another factor which exerted an unfortunate influence upon regular medicine, though this is difficult to determine. Quackery was, if anything, on the increase during the eighteenth century. This was largely due to the appearance of a new form, the patent remedy, which was made possible by the development of patent law and by the availability of the first newspapers as effective advertising media.[6] The resulting success

[1] Preface to *Works*, Benjamin Rush (Ed.), (Phila., 1815), p. xxxiv.

[2] Quoted in Benito C. Feijoo: *An Exposition of the Uncertainties of Physic* (transl. from the 7th Spanish edition, Lon., 1751), pp. 6, 7. This is a most interesting work by the great Spanish advocate of the Enlightenment.

[3] "Currents and Cross-currents in Medical Science", *Medical Essays*, 5 ed., p. 194.

[4] See H. W. Haggard: *Devils, Drugs and Doctors* (N.Y., 1929), pp. 3 ff.

[5] Bertrand Russell: *The Scientific Outlook* (N.Y., 1931), p. 15.

[6] R. H. Shryock: "Public Relations of the Medical Profession in England and America, 1600–1870", *Ann. of Med. Hist.*, n.s., II (1930), 312. Much has been written on the history of quackery and charlatanism; for brief discussions see W. R. Steiner: "The Conflict of Medicine with Quackery", ibid., VI (1924), 60 ff.; [Paul] Diepgen: "Kurpfuscherei und wissenschaftliche Medizin im Wandel der Zeiten", *Gesundheitslehrer*, XIV (Berlin, 15 Juli, 1933).

of bombast and bluff hardly afforded an edifying example to physicians, but it is to the credit of the majority that they declined to indulge in similar tactics.

The degree to which regular physicians resisted quackery depended largely on the extent to which they themselves had attained to professional standards and solidarity. There were some signs of progress in this direction during the late seventeenth and early eighteenth centuries, notably in the growing condemnation of scientific secrecy. The effort of the Chamberlen family to conceal their development of the first obstetrical forceps occurred during this period; and although temporarily successful, it represents one of the last well-known attempts of the kind. One may, perhaps, interpret the development of patent remedies as a survival of medieval secrecy, but the significant thing here was their repudiation by reputable physicians.

At the same time, certain characteristics of the medical profession itself were not such as to further the interests of research. Like other learned groups, it was inherently conservative, and its institutions adapted themselves but slowly to new needs and opportunities. While the local medical societies and the first medical journals, which grew rapidly in numbers after about 1800, were often most progressive, it was precisely the lack of such institutions that delayed improvement prior to that time. Typical of the older organizations was the London College of Physicians—originally the product of the youthful enthusiasms of the Renaissance, but by 1750 a right ancient and honourable body. It continued through the eighteenth century to be more concerned with questions of precedence and privilege than it was with promoting scientific advance. If only plebeian apothecaries could be kept out of general practice, and if upstarts with Dutch or Scottish degrees could be kept from poaching on sacred ground, then the interests of the College would be well served. Hence much potential energy and influence, which might well have been spent in supporting research, was wasted; for in the long run the outlanders and the apothecaries were bound to have their way.[1]

It was well indeed that the apothecaries did succeed, for there were perhaps fifty times as many people in London as the fellows and licentiates of the College could possibly attend. William Farr once estim-

[1] Interesting accounts of this story will be found in the *Edinb. Rev.*, LXXXI (1845), 237 ff., and in the *Dublin Rev.*, XVIII (1845), 174 ff. A similar struggle between physicians and apothecaries also went on in Paris during the eighteenth century. The social history of the French profession is described in Albert Babeau: *Les Bourgeois D'Autrefois* (Paris, 1886), pp. 107 ff.

ated that there was a ratio in London, during the seventeenth century, of over thirteen hundred serious cases of illness per day to every single fellow of the College.[1] Either the apothecaries must look after folk, or no one would do so. It appeared that the unpretentious apothecaries—the quasi-irregulars—were the people's friends; while the lordly fellows seemed concerned only with their income and with their monopoly. What did *they* care, inquired the cynics, about the welfare of the masses? Some critics went further, and claimed that the whole regular profession conspired to keep the public in ignorance and to deprive them of the services of other practitioners, simply for the sake of the doctors' purses. Why were books and prescriptions written in Latin but to keep trade secrets; and what were codes of "professional ethics" but trade rules?[2]

This controversy concerning the private interests and public duties of physicians was by no means new in the seventeenth and eighteenth centuries, nor was it to cease with this period. Certain phases of it have obviously entered a rather acute stage in recent years. It had, presumably, no direct bearing on medical research at any particular time; but it did lead to varying degrees of popular suspicion of the regular profession. One would expect, *a priori*, that this was one of the factors which eventually withheld some public support from medical work.

Professional pressure was applied by physicians, not only against rival groups, but also against regulars who attempted innovations not meeting with general approval. The early medical societies occasionally disciplined their own members. This was often a salutary procedure, and was by no means peculiar to the medical profession; yet there were times when the effect upon research was unfortunate. The professional taboo on specialization, mentioned above, affords an excellent illustration. S. Weir Mitchell declared, in 1891, that he could remember the day in Philadelphia when "older physicians refused to recognize socially a man who devoted himself to the eye alone".[3]

More serious was the professional divorce which had long obtained between two major phases of medical science and practice; that is, between general medicine and surgery. Medical faculties had tended since medieval days to disdain surgery as a mere craft. Occasional efforts to unite the two fields, such as the attempt made in London in 1423, had either failed or succeeded only in a few institutions. Hence physicians

[1] Wm. Farr: "History of the Medical Profession", *Brit. Med. Almanack* (Lon., 1839), pp. 169 ff.

[2] For specific accusations along this line, during the seventeenth century, see Nicholas Culpepper: *Galen's Art of Physick*, etc. (Lon., 1662), preface.

[3] *University Med. Mag.* (Phila., Oct. 1891), p. 1.

had remained blind to the great values of surgery, which in the very nature of the case had remained a less speculative subject than medicine proper. Not only did medicine miss the salutary empirical influence which surgery might have exerted upon it, but surgery on its part must have suffered by the diversion of talent into fields of greater prestige.[1]

Prior to the eighteenth century this disdain of surgery as a craft also applied to experimental procedure, to what would now be termed "laboratory work" in general. No real professional man should work with his hands. It is somewhat startling to recall that Robert Boyle was criticized by his Oxford colleagues as "one given up to base and mechanical pursuits".[2] To examine the composition of the air and its relation to respiration was "base and mechanical", since it involved the handling of pumps and other vulgar contrivances. Such conservatism, however, should be ascribed to universities as a whole, rather than to their medical faculties in particular; and it was directed at one science as much as another.

As has already been noted, the universities often remained strongholds of medieval tradition. They were inclined to keep aloof from the experimental science of the seventeenth century, and to hold fast to the faith in Galen and Aristotle. It is important here not to review the history of medical education as such, but to note its relation to research. The character of a medical student's training presumably had some influence upon his future interests and accomplishments. Until real clinical instruction was introduced there was little in this training that would arouse interest in original investigations. Rather was it calculated to have quite the opposite effect. The continued effort to present all medical knowledge in final, logical form probably encouraged professional complacency in generations of young doctors who might just as well have been trained to think for themselves. It is hardly necessary to add that faculties who failed to interest their students in original work were not themselves likely to be notable in this connection. Hence they failed to realize the value of combining teaching and research in the modern manner.

One should pause to qualify here. The remarkable scientific work accomplished in the seventeenth-century Dutch universities, and especially the development of the medical school at Leyden, set an example to the rest of Europe. That this exercised some influence, as the eighteenth century advanced, is attested by the establishment of a

[1] Sir Thomas C. Allbutt: *The Historic Relations of Medicine and Surgery* (Lon., 1905), pp. 51, 107 ff.

[2] Allbutt: *Science and Medieval Thought* (Lon., 1901), pp. 59–62.

similarly progressive school at the University of Edinburgh.[1] The situation described above, however, obtained in many of the Continental institutions through the better part of the century, and was particularly true of those in England. Medical teaching at Oxford was still essentially scholastic in character as late as 1770.[2]

Partly because of this, the best English training developed in connection with the hospitals of London, and thus had no university affiliation. These London schools exercised a monopoly until 1821, and not until three years after that was the first provincial medical school set up at Manchester.[3] Whether this had any effect upon the progress of research in England is not apparent, but it is quite possible that it exerted an unfortunate influence on the United States. Here the original impetus in medical education had been of the best, deriving as it did directly from Edinburgh and indirectly from Leyden. Medical schools were established in Philadelphia and in New York City more than half a century before the first were founded in Manchester, Liverpool, and Leeds. These early American schools were in some ways superior to most of the European institutions of the period.[4] During the half century between 1780 and 1830, however, the influence of the London schools became dominant in American medicine, and this encouraged the establishment of independent medical colleges like those of the metropolis. In an expanding and commercially-minded country such schools became more concerned with turning out graduates than with the progress of science. University affiliations might have exerted a restraining influence upon them.[5]

It is unnecessary to follow further the story of medical education in particular, or of social and professional factors in general. Perhaps too sombre a picture has been drawn. There were undoubtedly some social factors which proved beneficial to medicine. It will be pointed out below, for example, that a public distaste for over-dosing and other heroic treatments exerted a salutary check on such measures after 1830.[6] This

[1] J. D. Comrie: *History of Scottish Medicine*, II (Lon., 1932), 449 ff.; Sir A. Grant: *Story of the University of Edinburgh*, I (Edinb., 1884), 306 ff.

[2] Gregory: *Observations on the Duties and Offices of a Physician* (Lon., 1770), pp. 159 ff., contains a severe indictment of the English university education of that period.

[3] An interesting account of the origin of one of the first provincial schools is given in an *Address by Professor John Hay, D.L., M.D., F.R.C.P.: on the Liverpool School of Medicine* (Liverpool, 1934).

[4] See Sigerist: "Medical Societies, Past and Present", *Yale Jour. of Biol. and Med.*, VI (1934), 360. The relations between British and American medicine in this period are described in Francis R. Packard: "How London and Edinburgh Influenced Medicine in Philadelphia", *Trans.*, Col. of Phys. of Phila., LIII (1931), 151 ff.

[5] William H. Welch: "Some of the Conditions which have Influenced the Development of American Medicine, especially during the Last Century", *Johns Hopkins Hosp. Bull.*, XIX (1908), 33–40.

[6] See Chapter XIII.

was an instance in which men felt that the medical profession had done those things which it ought not to have done, and that there was literally no health in it. There may have been other occasions when physicians have left undone those things which they ought to have done, and it is possible that public pressure has been useful at such times. It can scarcely be denied, finally, that such major social developments as the humanitarian movements exerted a stimulating influence upon the medical sciences.

Yet the fact remains that most social forces favourable to medicine were simply those that aided all the sciences; while those peculiar to medicine usually exerted a retarding influence upon it. This must be remembered, in addition to the purely technical difficulties, if one is to understand the long lag in the field.

IV

RENEWED PROGRESS TOWARDS AN OBJECTIVE SCIENCE

1750–1800

LOOKING BACK over all the scientific and social difficulties involved, the lag in medical advance seems natural enough. The really remarkable thing is that these sciences were finally modernized so early as the first half of the nineteenth century. How and why did the modern spirit finally establish itself? Obviously, no one influence can explain so momentous and far-reaching a transformation. At times one factor followed upon another; but usually they were concomitants, and their arrangement in an analysis is more a matter of logical than of chronological order.

The general intellectual climate of the later eighteenth century was in marked contrast to that of the earlier decades. From the point of view of scientific men, it became distinctly fair and warmer. Modern experimental science, having developed rapidly during the seventeenth century, came into its own in the eighteenth. At first on trial, it proved itself worthy in certain fields and gave promise in many more. Most successful among the sciences were mathematics and physics; and especially impressive were the achievements of Newton and other English scientists in these fields. Notable, too, had been the explanation and justification afforded their work by the English philosophers of the time; by Bacon's condemnation of rationalism, and by Locke's more comprehensive analysis of the human understanding. British empirical philosophers and British mathematicians were influential in establishing throughout Western Europe the new "scientific point of view". In its essence this was the conviction that the ultimate truths of Nature transcended human understanding, but that man could learn through experience to control Nature to his own ends. This meant that less concern was to be manifested about the rationalistic metaphysics of a Descartes or a Spinoza, and that more interest was to be displayed in Locke's programme for the pursuit of knowledge through experience.

In order to understand the exact meaning of this concept of "experience" one would have to enter into the philosophical problems of the period. Empirical thinkers held that reason and all the "faculties" were based ultimately upon sensation; but this did not mean that they under-estimated the role of reason (once it was established) in dealing with the data afforded by further sensations.[1] In a word, "experience" in the larger sense was the product of reason as well as of sensation. One must reason, but reason about the data afforded by the senses, rather than about the absolute and the transcendental. If one would thus keep his feet on the ground he might get somewhere—he could expect practical results. In this utilitarian sense the eighteenth century was more optimistic than had been any preceding age. The *philosophes* were convinced that through a science of observation, and of reasoning based strictly thereon, man could go forward to great things.

Past experience suggested that the more mathematical this science could be made, the more successful it was likely to become. Even the partial failure of mathematical method in the biological sciences did not overcome this conviction. Indeed, some of the more striking demands for the development of quantitative procedures were to come from physicians, who felt that measurement was essential to further progress in medicine. So influential a leader as Haller declared that ". . . more exact divisions on measuring instruments are more important than all of Descartes, all of Aristotle".[2]

The eighteenth century was not only the "Age of Reason". It was also the era of the Enlightenment, of the *Aufklärung*. The very fact that science was becoming optimistic about results, was beginning to envisage "progress", helped to render it popular among the educated classes. The same end was served by contemporary improvements in publishing and in postal service, which provided a circulating medium for scientific as well as for other types of literature. Merchants and professional men, as well as aristocrats, began to read, to patronize, and even to observe for themselves. Americans will recall Franklin with his kite, and Jefferson's enthusiasm for fossil remains. The belief became current that social as well as physical problems could be solved by science—"the appeal to reason" was heard in politics as well as in philosophy. For the first time, science set the general intellectual tone,

[1] John Locke: "An Essay Concerning Human Understanding", *Works*, I (Lon., 1913), 205 ff.

[2] Quoted by Stephen d'Irsay: "Scientific Thought and Enlightenment", *Kyklos*, III (1930), 139.

which in consequence was naturalistic and optimistic in the extreme. The Middle Ages, with their despair of this world and their hope for perfection in the next, were at last replaced by an era which rather despaired of the next world, but looked forward to perfection in this. There have been reactions since 1800, it is true, in the name of piety, of beauty, and of romance; but no general return to the prescientific outlook has persisted long. In a very real sense, the eighteenth-century *Aufklärung* marked the advent of the modern world.

It was a natural consequence that the same era witnessed progress towards modern medicine. There will be occasion to refer below to the manner in which the late eighteenth-century fondness for a critical empiricism expressed itself in medical method; and also to the way in which the optimism of the Enlightenment revealed itself in the general outlook in the medical sciences. There was some reason for this optimism, despite all the difficulties of the period. Even the confusion of medical "systems" was a sign of progress, was real evidence that new facts and ideas were accumulating rapidly. Data were not lost just because the first imperfect attempts to interpret them proved misleading; men could and did come back to the facts and start over again. Nor was the example of seventeenth-century method ever entirely forgotten. During the height of theoretical confusion, and when there seemed the least hope of attaining useful results, various workers continued patiently to experiment and to observe. Such men stood in marked contrast to the speculative system-makers who flourished at the same time. It is hard to believe, to-day, that the observing John Hunter and the speculative John Brown were countrymen and contemporaries.

To begin with, considerable progress was made in eliminating the intellectual debris of earlier centuries. This was important, in that it cleared the way for more positive advances in the future. In looking back on the therapeutic blunders of the age, for example, one should not forget its reform of the *materia medica*. This was done primarily by purging the pharmacopœia of many absurd and repulsive preparations. The "shot-gun prescriptions" and revolting animal concoctions still employed in 1700 were no longer in vogue among sensible practitioners a hundred years later.[1]

It was a distinct advance, also, to divorce therapeutics from the occult lore with which it had long been encumbered. The rationalism of medieval medicine could and did pass over at times into what appears

[1] A. C. Wootton: *Chronicles of Pharmacy*, II (Lon., 1910), 65 ff. The first pharmacopœias, which grew out of the herbal literature, were published in the sixteenth century. Progressive changes may be traced in the many editions of the London manual.

to our modern eyes as mere superstition and mysticism; and the respect paid to astrology as late as the last half ot the seventeenth century is an excellent example of this. One finds a royal physician of Paris solemnly proclaiming, in 1663, that astrology is absolutely essential to medical practice; and as late as 1688 another authority of that city published a book to defend the same thesis.[1] After about 1700 medical authorities paid less and less attention to such occult matters—no doubt because the development of Newtonian astronomy led most educated men to repudiate astrology altogether.

Astrological ideas survived, as usually happens in such cases, only on the lower cultural levels of folk-medicine and folk-lore. A partial exception was to be found in a lingering belief that great epidemics might be associated with solar or planetary phenomena. An apparent example will be found in an American work, Noah Webster's *Epidemic and Pestilential Diseases* (1799), which contained much of "researches among revolving planets, blazing comets, exploding volcanoes", and the like. But an examination of Webster's study reveals that he approached such phenomena in a really empirical manner, and that he deplored mysticism of any sort.[2] The general repudiation of astrology was significant, in that it indicated the rejection of quasi-supernatural elements in medicine. This was necessary, as in all other fields, if the way was to be cleared for a truly "natural" science.

The positive trends which finally modernized the medical sciences are not easy to distinguish, since every phase of scientific progress possessed more or less significance for the whole. Probably most essential was continued achievement in the basic sciences of anatomy and physiology, and the distinction made, in the course of this, between normal and pathological phenomena.

Considerable progress in anatomical studies was made possible by a new technique for injecting the vessels with coloured fluids. This originated largely with the Dutch school towards the end of the seventeenth century, but the art underwent further improvement after 1700.[3] Its service to anatomy was obvious, and somewhat analogous to that rendered bacteriology more than a century later by the discovery of

[1] See Lussauld: *Apologie pour les Médecins* (Paris, 1663), p. 138.

[2] C.-E. A. Winslow: "The Epidemiology of Noah Webster", *Trans.* Conn. Acad. of Arts and Sciences, XXXII (1934), 76 ff.; Alfred Scot Warthin: "Noah Webster as Epidemiologist", *Bull. of the Hist. of Med. Soc. of Chicago*, III, No. 2 (1923), 205 ff.

[3] Leonardo da Vinci was the pioneer here, as in so many other fields, but his work remained largely unknown; H. E. Sigerist: "Hollands Bedeutung in der Entwicklung der Medizin", *Deutsche Med. Wochen.* (1928), nr. 36. See also, on the general significance of Dutch medicine in the eighteenth century, C. A. Crommelin-Leiden: "Die holländische Physik im 18. Jahrhundert", u.s.w., *Sudhofs Archiv*, XXVIII (Leipzig, 1935), 129 ff.

various staining methods. The development of the art of engraving, the making of wax figures, and the advent of the first large collections of anatomical materials—all of great value to anatomical and pathological studies—were products of the eighteenth century. Even instruments of observation, which suffered considerable neglect between 1720 and 1820, were improved in minor respects, and Lyonet was given credit as "the first contriver of the anatomical microscope".[1]

Perhaps the best picture of anatomical research in this period is afforded by the systematic, objective studies of John Hunter in London. His famous anatomical museum, including some thirteen thousand specimens, was the pioneer collection of this sort. Nor did his work appeal only to intellectual curiosity, for his contributions to surgery were most significant. His demonstration that aneurysms due to arterial disease must be tied high up in the healthy tissue, by a single ligature, subsequently saved "thousands of limbs and lives". Hunter was practically the founder of experimental and surgical pathology, and his influence did much to improve the whole status of the surgical profession in Britain. As one of his colleagues observed: "He alone made us gentlemen". It is probably no exaggeration to say that he found surgery still a mechanical art, and left it an experimental science.[2]

Equally promising were the advances made in physiology after about 1740. Although seventeenth-century investigators had encountered great difficulties in this field, the immediate fruitfulness of their mechanical conceptions was not forgotten. There was delay and hesitation, but no real return to the naïve teleology of Galen. The work of Stephen Hales in measuring the blood pressure (1733) illustrates the persistence of Harvey's interests and methods during the least progressive decades that followed. And the final justification of the mechanical approach to physiology was the work of the presiding genius of eighteenth-century medicine, Albrecht von Haller.

Trained at Leyden, Haller carried back to Switzerland, and later to Hanover, the critical and progressive standards of the great Dutch centre—much as Van Swieten did to Vienna and Monro to Edinburgh. Haller proved the most versatile and productive of these mid-century leaders, and the unusual character of much of his work has only recently received due recognition. He was, for instance, the chief founder of modern medical bibliography, and has been claimed as the first

[1] David Ramsay: *A Review of the Improvements, Progress and State of Medicine in the XVIII Century* (Charleston, S. C., 1800), pp. 8–10.

[2] Garrison: *History of Medicine*, 4 ed. (1929), p. 347.

modern medical historian.[1] It is especially his work in experimental physiology which requires attention here. Inspired by a direct study of Newton, and sharing the latter's concept of a world mechanically ordered, Haller based his great study of muscular contraction upon dynamic principles.[2] As a result of this, he emphasized the distinction between contraction (irritability) and nerve-impulse (sensibility); a distinction which was much misused by the systematists, but which also became the basis for all further studies in the physiology of muscle and nerve.

Haller was thus influenced, like the earlier physiologists, by the example of physical mechanics and by a desire to apply its principles to biological phenomena; but he so widened the field in which they had worked, and lent it such prestige by his systematic researches, that he has sometimes been termed the founder of modern physiology. It is perhaps more accurate to say that he served as a vital connecting link between the iatrophysicists of Harvey's day, and such physiologists as Claude Bernard and Johannes Müller in the nineteenth century.

Significant as discoveries in normal anatomy and physiology were in themselves, they were of especial interest in their relation to the first systematic work in pathology. For it was the study of pathology that would eventually bring order out of nosographical confusion; and thus make possible the basic advances in ætiology, preventive medicine, and therapeutics that were to follow in the nineteenth century.

Anatomists had always noted morbid as well as normal phenomena, but had reported them in a rather uncritical and incidental fashion.[3] The seventeenth century witnessed an increasing interest in the same field, and the possibility of employing the microscope therein received at least passing recognition. Dutch, Danish, and English physicians made valuable individual contributions during the half-century between 1640 and 1690, especially with reference to apoplexy, tuberculosis, and such conspicuous phenomena as tumours and dropsies. Giovanni Riva (1627–1677), physician to Pope Clement IX, was instrumental in organizing a society for the study of morbid anatomy; and towards the end of the century Théophile Bonet published the first large collection of pathological observations. His work ran into seven-

[1] See Paul Diepgen: "Albrecht Haller u. die Geschichte der Medizin", in *Historische Studien u. Skizzen zu Natur—u. Heilwissenschaften, Festgabe Georg Sticker* (Berlin, 1900), pp. 100 ff.; and John Fulton: "Haller and the Humanization of Bibliography", *New Eng. Jour. of Med.*, CCVI (1932), 323 ff.

[2] Stephen d'Irsay: *Albrecht von Haller; eine Studie zur geschichte der Aufklärung* (*Arb. d. Inst. f. Gesch. d. Med.*, I. 2, 607–704, Leipzig, 1930).

[3] The earlier story is well told in the first three chapters of E. R. Long: *History of Pathology* (Baltimore, 1928).

teen hundred pages, and included nearly three thousand necropsy protocols drawn from many sources.[1] These, unfortunately, were ill assorted and uncritical in character, so that they were of little significance save as an ineffective codification of the subject, in which numerous data already accumulated were preserved from oblivion.

More significant than the work of Bonet was that of Italian physicians, who continued to work in the great tradition of anatomical research already established in that country. Quite an intensive study of morbid anatomy was conducted at Padua and Bologna during the first half of the eighteenth century. Such notables as Malpighi, Valsalva, and Morgagni were leaders of successive generations in these centres. Employing the best technique then available, the Italians gradually acquired both new facts and new viewpoints. They learned that there was a difference between the merely abnormal and the pathological, and that the latter had much the greater significance of the two. It was also realized that, even within the limits of the truly pathological, the common phenomena were ordinarily of more importance than those of spectacular rarity.[2] Equally significant were the first efforts to distinguish between appearances due to different diseases in the same subject. These distinctions cleared the way for an economical concentration upon the truly serious problems, rather than upon those of a trivial or only occasional character.

Most important, finally, were the first attempts at a correlation between clinical and post-mortem observations. In Sydenham's day the two fields had been cultivated quite independently, often by different men. While one authority was listing symptoms unrelated to any known body changes, another was describing lesions unrelated to any particular symptoms. One man, working at the bedside, described a symptom-complex involving coughing and fever, known as consumption; another man, at his dissecting-table, noted in the lungs the small nodules known as tubercles. The symptomatic conception of "consumption" could be made more specific and meaningful only when it was associated with the tubercles discovered by the pathologist.

It is not strange that the first efforts to correlate clinical and pathological findings were made in the period that witnessed both the development of the clinic at Leyden and of the more intensive studies of morbid anatomy at Padua and Bologna. Boerhaave, Haller, and others

[1] E. R. Long: *History of Pathology* (Baltimore, 1928), p. 98.

[2] Even Haller's treatment of pathology included much attention to monsters and the like; see Albert Haller: *Pathological Observations*, etc. (London, 1756), pp. 171 ff.

paid the matter some attention, but it was a great Italian who devoted himself systematically to this task. Some eight years after the publication of Haller's work on irritability (1753), Morgagni of Padua brought out his classic work on the seats and causes of disease. Convinced of the practical services which his science could render to diagnosis, prognosis, and even therapy, Morgagni emphasized the relationship between disease and post-mortem phenomena whenever possible. Proceeding with the utmost patience, he refrained from publishing until able to present the findings of nearly fifty years of research along this line. His work, in consequence, clearly laid the basis of modern pathology—an anatomical, rather than the traditional humoral or tension pathology, would henceforth prove most promising. The whole subject was treated critically and genetically, and outlined as systematically as was consistent with the best canons of inductive research.[1]

For the present purpose it is the matter of Morgagni's method that is of more interest than any specific contributions that he made. The latter, such as his splendid descriptions of aneurysms and other lesions of the vascular system, related largely to gross anatomy; since, like his contemporaries, he made little use of the microscope. The critical character of his method is well illustrated in his clinical-pathological procedure. He seems to have realized that there should be some distinction between the bedside and the post-mortem report—lest the one even unconsciously influence and perhaps distort the other. Eventually, with later pathologists, this developed into a routine in which the clinician responsible for a fatal case reported his data and interpretations to the pathologist, often in ignorance of the latter's findings, and knowing the while that every statement was subject to correction from this source.

Nothing could have been better calculated to inhibit the effusiveness of the systematists than this knowledge that each guess was subject to check, and each hypothesis to confirmation. It was disconcerting to speculate about the cause of an individual's death, only to be told by the pathologist that it was clearly quite a different condition which was responsible. At last a procedure had been evolved which could deflate the enthusiasm of medical speculation. It was one destined to prove of increasing value during the next two generations; indeed, there are to-day

[1] I have used an English translation by Benj. Alexander, published at London in 1769. For recent appreciations of Morgagni, see A. Castiglioni: *A History of Medicine* (N.Y., 1941), pp. 601–8; Paul Diepgen: "Giovanni Battista Morgagni und die Pathologie", *Zeits. für ärztl. Fortbild.*, XXIX, nr. 5 (1932); Sir Wm. Osler: *The Evolution of Modern Medicine* (New Haven, 1922), p. 189; H. E. Sigerist, *The Great Doctors* (N.Y., 1933), pp. 229 ff.

few finer demonstrations of critical method than this divorce and subsequent comparison of clinical and pathological findings.

Valuable as Morgagni's methods were, there is no reason for claiming that there was anything unique about them. Associated with advancing knowledge in all fields, both as cause and effect, was a growing recognition of the best means of attaining it. This was true not only of the immediate technique of special subjects, but also of logical procedure as a whole. The old controversy between a speculative rationalism on the one hand and a radical empiricism on the other had tended, it will be recalled, to come to a head in the eighteenth century. The impact of experimental procedure forced the issue, but such were the difficulties involved that the outcome was long delayed. Slowly there developed a growing condemnation of both extremes. There was an increasing recognition of that sensible compromise which Galen had advocated in classical days, and which a few medieval seers had envisaged during the thirteenth century. Rationalistic speculation was fanciful and dangerous; but then, too, pure empiricism with its trial-and-error gropings was blind and stupid. One must indeed think, as well as observe, but think critically withal.

Since pure empiricism had usually been condemned by physicians, it was the increasing criticism of speculation that was most indicative of improving methodology. This trend may be illustrated by the views of three medical leaders whose careers overlapped, but who represented respectively three successive generations between 1660 and 1760—Sylvius, Boerhaave, and Haller. Sylvius claimed to depend only on his senses in his studies in anatomy and physiology, and some of his actual observations were of great value. Yet in his iatrophysical enthusiasms he actually indulged in much "arbitrary and false reasoning".[1]

Boerhaave, who followed him at Leyden, was somewhat more cautious and sophisticated, and went over the whole question of method in a careful and convincing manner. The search for facts, he began, was undoubtedly the main thing. Medicine must also be advanced by "just Reasoning", but this should be indulged in only with the greatest caution. Mere conjectures should be rejected, "retaining only what results from Observation or what follows of consequence". When, therefore, a problem presents itself "not accountable for but upon Hypothesis, we should restrain our Judgment, and leave the Doubt to be solved by Posterity, when they shall have obtained Light enough from Experi-

[1] J. H. Baas: *History of Medicine* (transl. by H. E. Handerson, N.Y., 1889), pp. 490, 493; F. H. Garrison: *History of Medicine* (4 ed.), pp. 261 f.

ments which have escaped us. It therefore behooves us to defer our Opinion about the uses of the Spleen . . . the Causes of Contagious Diseases from Poisons, etc., till Time shall bring the Truth to Light. By this means Physic, 'tis true, will be reduced to a small Compass, but then it will be true, certain and always the same." Such careful procedure was necessary, not only for medical science, but for medical practice as well—not only for research men, but for the sake of mankind as a whole. For speculation might influence practice, and hence "false theory in a City might turn out to be of the most fatal Consequence to its Inhabitants".[1]

More satisfactory statements, from the modern point of view, could hardly be desired. It was, of course, one thing to preach such excellent procedure and another thing to practise it. This fact was well illustrated in Boerhaave's own conclusions. After all his admonitions, one finds him indulging in the very sort of conjectures he had just condemned. How absurd, he notes, was the classical view of the heart as "the Spring of all heat". Knowing so little, the ancients should have attempted no explanation of the matter; whereas we to-day, with all our facts, are now able to show that bodily heat is simply the mechanical result of "the velocity of the blood" in its circulation. Alas! Like others, the great Dutch physician found it difficult to leave all uncertain matters to posterity.

The earlier eighteenth century doubtless witnessed much such preaching of good method, by men who commonly failed to practise it. Yet this by no means denies all significance to their words. As preaching became more common and insistent, it was the more likely to be related to actual procedures. So it was with medical method. What Boerhaave preached, his pupil Haller would practise. The latter's studies of irritability and sensibility, for instance, were strictly objective in character and were based upon nearly six hundred experiments, of which he himself performed about two hundred.[2] Haller's ideas on method were not, in principle, so different from those of Boerhaave before him; but the painstaking character of the younger man's actual researches lends greater significance to his views. "We should relate", Haller observed in his work on physiology, "those things which experience shows to be true; and then add those hypotheses by which learned men have endeavoured to supply such things as are not evident from the subject itself. How few things are ascertained on this subject,

[1] Boerhaave: *Institutes* (transl. into English, Lon., 1751), pp. 42, 50, 51 (based on the 2 ed.; first ed. 1708).

[2] Garrison, op. cit., p. 318.

and how difficultly they are ascertained, I have learned by too much experience." [1] The modesty of his claims here, in contrast with the omniscience of speculative contemporaries, is striking. In medicine, as in other fields, the confession of ignorance was to prove the beginning of wisdom.

The general trend towards a critical empiricism, gaining headway during the latter part of the century, was peculiar to no one country. It spread from one centre to another, via generations of migratory students and an increasing number of professional publications, and was to be observed in London and Manchester as well as in such Continental centres as have been mentioned. Indeed, it has been claimed that good method became especially characteristic of England, where not only the general philosophical background was favourable, but where a mystic something known as "common sense" was said to inhibit speculative enthusiasms. It was doubtless no accident that American medical students abroad—ever seeking the best—were usually to be found in London after 1780.

Here they observed not only "common sense" practice, but also a conscious recognition of the whole trend in methodology. A number of English works, published between 1760 and 1800, were devoted entirely to this subject. Eminently sane was Lettsom's *On the Improvement of Medicine in London* (1775); so, also, was Henry Manning's *Modern Improvements in the Practice of Physic* (1780). But perhaps the clearest formulation of modern principles was that in Gregory's *Observations on the Duties and Offices of a Physician* (1770). After an analysis of the shortcomings of both speculation and of pure empiricism, Gregory proceeded to a statement of principles that might well have served as a working programme for the century that was to follow. Although condemning unchecked theorizing, he reminded his readers that men could not advance one step in the pursuit of knowledge without reasoning. "It should be seen, upon the whole," he continued, "that all physicians must reason, and that the only difference among them consists in this; that some reason better than others. Some search into the cause of diseases and the effects of remedies. Deeply sensible of the difficulties of enquiry, and of the various ways in which they may be deceived, they collect and arrange all the facts relating to the subject; when they have got a remote view of a leading principle, they attempt to bring a direct and conclusive proof by experiment. . . . If the proof

[1] Albertus Haller: *First Lines of Physiology* (Eng. transl., Edinb., 1779), p. 449 (first ed., 1747).

turns out against it, they see and candidly acknowledge that there is an error somewhere. . . . These", he concluded, "I think have a just claim to the title of rational physicians." [1]

The sensible character of the best English work received due recognition in Europe as well as in America, and the need for following English examples was freely admitted at times.[2] Speculation flourished longer in several Continental countries; notably in Germany, where Hume's empiricism became but a stepping-stone to Kantian idealism, and where the resulting *Naturphilosophie* cast a spell over German medicine which will be discussed later. It is also true that Brunonianism, a most exuberant and almost the last recognized medical system, exerted less influence at home in Britain than it did abroad.

Yet the prompt and severe criticism to which Brown's theories were subjected after 1800, even in Germany, Italy, and the United States, indicated a growing distrust of speculation in all progressive countries. Able German physicians like J. C. Stark and Hufeland had no use for Brunonianism, and J. P. Frank considered the whole system absurd.[3] Goethe, for all his romantic and philosophical bent, had too great a mind to be impressed by this school.[4] It was becoming clear that genuine philosophical synthesis was one thing, while premature and shabby imitations were quite another.

Some enthusiasm for Brown's theories was displayed in Italy, where Rasori popularized his general doctrine. Yet there was no lack of critical workers there, as in Germany, to point out its absurdities.[5] Relatively unaffected, moreover, by the romantic idealism of the *Naturphilosophie*, such able Italians as Corti and Rolando continued to labour in the tradition of Morgagni and his contemporaries.[6] As far as the former were concerned, the speculative tempest would pass and blow itself out.

Much the same could be said of the more critical physicians in such northern nations as Holland, Denmark, and Sweden. One needs only to picture Linnæus, working in his gardens at Upsala, to recall what

[1] J. Gregory: *Observations on the Duties and Offices of a Physician and on the Methods of Prosecuting Enquiries in Philosophy* (Lon., 1770), pp. 110–12.

[2] See, e.g., Kurt Sprengel: *Histoire de la Médecine* (traduite de l'allemand . . . par A. J. L. Jourdan, Paris, 1815), V, 441 ff.; P. V. Renouard: *History of Medicine* (transl. from the French by C. J. Comegys, Cincinnati, 1856), pp. 567, 568.

[3] Leona Baumgartner and Elizabeth M. Ramsay: "Johann Peter Frank", etc., *Ann. of Med. Hist.*, n.s. VI (1933), 69 ff. Frank is sometimes cited as a supporter of Brunonianism.

[4] Paul Diepgen: "Goethe und die Medizin", *Klin. Wochens.* XI (1932), 16 f.

[5] Note, e.g., the criticisms of Brunonianism, in Vicenzo Lanza: *Novum Organum Mediocorum* (transl. by C. Stormont, Lon., 1826), pp. 15 ff.

[6] Castiglioni: *Italian Medicine* (Clio Medica ser., N.Y., 1932), p. 77.

objective work was accomplished by Swedish medical men in this period. Brunonianism and other systems flourished in Spain, but here also they were ridiculed by such intelligent physicians as P. Rodriguez and Martin Martinez. Experimental and critical procedures became popular after 1770.[1] Overseas in the United States, the potent influence of Benjamin Rush popularized medical speculation among a generation of students who attended his lectures in Philadelphia between 1790 and 1812. Rush performed some real services to medicine, and represented an abler type of systematist than did his erstwhile classmate, John Brown; but like the latter, he was subject to prompt criticism in his own land. His colleague Kuhn, while in no sense a brilliant man, had been well trained under Linnæus, and expressed a somewhat more empirical point of view. And some of Rush's contemporaries in the northern States, notably David Hosack of New York, condemned the whole rationalistic approach to medicine which the Philadelphian represented.[2]

In France, strange to say, rationalism was dominant until almost the end of the century. The mystical vitalism of the leading school of Montpellier has already been mentioned. One searches in vain for a Haller, a Hunter, or a Morgagni in the French faculties of 1750 or 1770; and it is only necessary to read Broussais's *Examen des Doctrines Médicales* (1821) to appreciate the weaknesses of the French leaders of that period. The great lay critic, Voltaire, held saner views on medicine than did most of his professional compatriots.[3]

Yet conditions were shaping up in France for an era of amazing progress. Those familiar with the general history of the "old regime" will recall how the spirit of enlightenment stirred in the Paris of the eighteenth century. English philosophy and science, as well as English political institutions, inspired the thinkers of the day. Paris tended to become the scientific, as well as the political and social centre of Western Europe.

More must be said of this below; but it is obvious, in passing, that in such an environment medical method was bound to improve. At about the turn of the century, Bichat, Barthez, and other prominent physicians voiced the same warnings against speculation that were becoming com-

[1] Eduardo García Del Real: *Historia de La Medicina En España* (Madrid, 1921), pp. 360 ff. Feijoo was the great influence here; see Rafael Altamira: *Historia de España y de la Civilización Española* (Barcelona, 1914), pp. 353 ff.

[2] A MS. copy of Adam Kuhn's lectures is preserved in the Library of the University of Pennsylvania. Hosack's views on method are found in his *Introductory Discourse on the Theory and Practice of Medicine* (1813), p. 12.

[3] Pearce Bailey: "Voltaire's Relation to Medicine", *Ann. of Med. Hist.* I (1917), 54 ff.

mon in neighbouring nations.[1] Meanwhile the gradual decline of extravagant theories removed the temptation to an equally extravagant rejection of reasoning.[2] As the charge was removed from one pole, it automatically disappeared from the other.

In France, as in all progressive nations, medical literature assumed a more hopeful tone as the eighteenth century came to an end. This was not due to the extreme claims of some particular systematist, nor to those occasional pronouncements that perfection was at last attained; but resulted rather from the conviction that an improvement in scientific method was really under way. The vision of a natural science comparable to physics had never been lost, and the belief grew that some great leader would soon attain it. Boerhaave was at first thought to have led medicine out of the wilderness; just as Broussais was later believed to have attained the promised land of true science.[3] But whoever the leaders were to be, there was increasing confidence that the children of Æsculapius were almost there.

A prime factor in the growing recognition of intelligent empiricism was the simple fact that it worked. It was bringing results, not only in pure science, but in medical practice as well. The most substantial services were rendered by empirical studies, inspired in some cases by so humble a source as lay medicine. The introduction of digitalis into the *materia medica* was suggested by old wives' remedies, and was then tested by critical observation and experiment. In somewhat the same manner the use of cinchona had been borrowed long before from the Peruvian Indians; and this drug, although condemned at times, had made a profound impression upon the medical profession.[4] It was truly surprising that these "untutored heathen" should have found a specific against the very fevers which had baffled the most learned of the faculty throughout the ages.

Equally surprising was the demonstration that certain old, popular conceptions could be employed to develop a whole new branch of

[1] Interesting critical comments along this line, from these physicians, as well as from Broussais, the Italian Giocomini, and others, are given in P. V. Renouard: *Lettres philosophique et Historiques sur la Médecine au Dix-Neuvième Siècle* (2 ed., Paris, 1857), pp. 6–14.

[2] One of the last extremely empirical writers was Joseph Lieutaud: *Précis de Médecine Pratique* (2 ed., Paris, 1761).

[3] Thus Manning, whose London work of 1780 was noted above, declared in his preface that: "The age of Boerhaave forms a memorable epoch in the history of physic. Theory, which before had been entirely conjectural, now assumed a more plausible and scientific appearance", Manning, op. cit., preface. A detailed analysis of Broussais's work by De La Sarthe (Paris, 1854), was actually entitled: *Histoire de la Révolution Médicale du XIX^e Siècle.*

[4] See, e.g., the long section devoted to the story of cinchona by Kurt Sprengel, op. cit., pp. 413–53.

practice—preventive medicine. The common observation that a diet of fresh foods prevented scurvy became the basis for critical, empirical studies that proved the peculiar value of citrus fruits in this connection. All readers of sea stories will recall "the terrible ravages of scurvy among the crew"; and will also remember the final demonstration on Captain Cook's voyages that the simple use of lime or lemon juice would prevent the disease.

Perhaps more important were the first attempts at artificial immunization against contagious diseases. Classical medicine had tended to ignore the concept of contagion; but the idea, probably of Old Testament origin, was widely accepted during the Middle Ages. Coming to the surface of things at times of crisis, as in the isolation regulations inspired by epidemics, the popular belief was also the basis of the Near Eastern practice of inoculation with smallpox. The employment of this practice in Europe and America, after Lady Mary Wortley Montagu and others had carried it from Constantinople to London, involved a definite idea of infection. It even resulted, in the hands of a few research men, in a conscious attempt to attenuate the virus involved.[1]

It is sometimes stated that inoculation was useless as well as dangerous,[2] and there was no lack of bitter opposition at the time of its introduction. But, to claim the least for it, it so impressed many of the ablest physicians and laity as to be adopted widely, and it led eventually to Jenner's great discovery at the end of the century. For vaccination was essentially an improved form of inoculation. Here, again, it was a hint given by dairy-maids that led Jenner to make his classic observations and experiments. He avoided speculation; and did not even reason, as Pasteur and Ehrlich were later to do, about the nature of immunization. Nevertheless Jenner employed simple hypothesis and check—in a word, something more than pure empiricism—and he elevated popular report to the level of scientific certainty.[3] The results were immediate and impressive. All the values of protection against smallpox were achieved, with almost none of the risks hitherto associated with inoculation. Some appreciation of the fear then inspired by this disease, as well as a glance at subsequent declines in mortality, is necessary to appreciate what this discovery meant to the world of 1800. Once again empirical procedure had justified itself in such a manner as to impress all civilized society; vaccination was as tangible and as useful an achieve-

[1] Arnold C. Klebs: "The History of Infection", *Ann. of Med. Hist.*, I (1917), 171.
[2] E.g., Albert H. Buck: *The Dawn of Modern Medicine* (New Haven, 1920), p. 109.
[3] A good brief description of Jenner's work is given in Sir Arthur Newsholme: *Evolution of Preventive Medicine* (Baltimore, 1927), pp. 175 ff.

ment as was the steam engine or any other product of the physical sciences.

Inoculation and vaccination not only vindicated empirical procedures, but opened up a new prospect in preventive medicine—a prospect as alluring as it was dangerous and obscure. For if immunization could be artificially secured against smallpox, by use of an attenuated or related virus, was it not possible that similar results could be obtained with other contagious diseases? This did not become a common point of view, even among medical scientists; but here and there research men continued to display an interest in it, and thus kept such ideas alive. Experiments were made, before 1800, with the plague and other diseases,[1] and were continued during the next century. Success was claimed in the case of measles;[2] and a long series of French experiments with syphilis culminated in a serious proposal to inoculate the entire youth of the nation with that disease.[3] Hence it is misleading to imply, as is sometimes done, that the possibilities of immunization were forgotten after 1800, until Pasteur revived interest in them some seventy years later.

Meanwhile hygiene, which, like surgery, had always been more or less empirical in nature, found itself projected from the individual to the public plane. Early interest in public health work was stimulated, not only by work on occupational and contagious diseases, but by a growing consciousness of public health problems.[4] French and German physicians began writing on "State medicine" about 1650;[5] and the subject was at least theoretically organized in Germany, during the next century, in the works of such authorities as Süssmilch and J. P. Frank. Some progress in actual administration was made in Central Europe by 1800.[6] While English-speaking peoples remained rather backward in this field, both the need and the way to reform were preached to them by individuals before that date, and the first signs of improvement were already to be discerned. The nature of these improvements will be considered in the next chapter.

[1] David Ramsay: *Eulogium on Benjamin Rush* (Charleston, S. C., 1813), pp. 28, 29.

[2] See the *Prov. Med. and Surg. Jour.*, VII (1844), 436, re Dr. M. von Katona's experiments in Hungary, 1841.

[3] Vaillard: "Rôle de L'Academie de Médecine dans L'Evolution de L'Hygiène Publique", *Bull. de L'Acad. de Méd.*, 3 s., LXXXIV (1920), 409. See, re continued experiments with syphilis, S. J. Otterburg: *Das Medizinische Paris*, u.s.w. (Carlsruhe, 1841), pp. 144–6.

[4] The most thorough history of the whole subject of hygiene, up to about 1800, is a work now often forgotten; John Sinclair: *Code of Health and Longevity* (4 vols., Edinb., 1808).

[5] Baas: *History of Medicine*, pp. 541, 706.

[6] Baumgartner, op. cit., *passim*.

The optimism of physicians concerning their science was for a time paralleled by popular appreciation. Laymen were especially interested in achievements in preventive medicine and in public health. Descartes had dreamed a century before of the development of medicine as a great civilizing influence; now there seemed evidence that the dream might come true. It was remarkable how many of the world's worst scourges—smallpox, plague, leprosy, scurvy, and malaria—seemed on the decline in Europe, or were at least subject to considerable control.[1] Even so persistent a disease as "summer diarrhœa" sharply declined in London after about 1780.[2] Humanitarian attempts to better the living conditions and hygiene of the masses, such as the highly successful infant welfare movement of the later decades, promised further improvements in the immediate future.

The more imaginative of the social reformers, much impressed by these "conquests of modern medicine," began seriously to contemplate a society freed from disease as well as from poverty and other ills. French *philosophes* of the Revolutionary era, in particular, found such visions congenial to their social enthusiasms. Thus Condorcet, in reviewing the whole pageant of human progress, envisaged as its goal an era in which science would free man from all disease, and in which both old age and death would be indefinitely postponed.[3] Here at last was a seer who predicted an approach to immortality in *this* world. *L'esprit humain* had travelled far since the thirteenth century.

[1] As examples of the current medical optimism see, for French literature, Hallé et Nysten: "Hygiène", *Dict. des Sci. Méd.*, XXII (Paris, 1818), 542; for British literature, Sir Gilbert Blane: *Select Dissertations on Medical Science* (Lon., 1822), p. 174; C. F. H. Marx and R. Willis: *Decrease of Disease Effected by Civilization* (Lon., 1844); for American literature, David Ramsay: *Review of the Improvements . . . of Medicine in the XVIII Century* (Charleston, 1800).

[2] John Brownlee: "The Health of London in the Eighteenth Century", Royal Med. Soc. *Proceeds.*, XVIII (1925), Pt. 2, pp. 78–81.

[3] *Esquisse d'un Tableau Historique des Progrès de L'Esprit Humain* (*Oeuvres complètes*, Paris, an IX, VIII, 372–5). Benjamin Franklin and doubtless other *philosophes* made similar predictions.

V

EARLY CONTRIBUTIONS OF PHYSIC AND PHYSICIANS TO THE PUBLIC WELFARE

1750–1800

So SIGNIFICANT is the state of the public health, both in itself and as the ultimate test of medical science, that the subject merits primary attention. What influences had been at work during the eighteenth century to bring about the apparent improvement in disease conditions just prior to 1800? To what extent were physic and physicians involved in this story, and in what degree were social rather than scientific factors of prime significance? It was to be expected that medical men would exert some influence on public health, and that the condition of the latter would in turn react upon the medical profession.

The health conditions of European peoples, which had been bad enough during the greater part of the Middle Ages, improved little during the early modern centuries. Generally insanitary surroundings remained the rule for both town and country. A revival of interest in classical hygiene and gymnastics during the Italian Renaissance was of brief duration;[1] and certain factors seem to have made living conditions more unsatisfactory after 1500 than before. The spread of syphilis, for instance, was a factor in the gradual abandonment of the public baths often found in medieval towns, and bathing became a lost art.[2] Certain medieval diseases, notably leprosy, definitely declined, but were replaced by smallpox and syphilis; and the plague again became pandemic in the seventeenth century.[3] No exact comparisons with earlier periods

[1] H. E. Sigerist: "Die ärztliche Kosmetik im Wandel der Jahrhunderte", *Die Medizinische Welt*, 1931, nos. 47, 48, 49 (sonderabdruck), pp. 10 ff. Lynn Thorndike believes that medieval towns were more sanitary than has usually been assumed. See his *Science and Thought in the Fifteenth Century*, pp. 10 ff., and also his article in *Speculum* (III, 192 ff.) on the same subject.

[2] On the medieval bath, see H. Peters: *Der Arzt u. die Heilkunst in der Deutschen Vergangenheit*, u.s.w. (Leipzig, 1900), pp. 49, 50, 51; Karl Sudhoff: *Internationale Hygiene Austellung, Historische Abteilung* (Dresden, 1911), pp. 161, 198.

[3] The literature on the history of syphilis alone is very large. See especially Karl Sudhoff: *Der Ursprung der Syphilis* (Leipzig, 1913), pp. 6–17. The general history of epi-

are possible, but it is well known that death rates continued generally high.

Actual living conditions and mortality rates naturally varied with time and place. The seventeenth century witnessed the nadir of health and happiness in a Germany torn by the Thirty Years War;[1] while there is evidence that the low point of things in Great Britain was reached early in the eighteenth century. These were years of fair economic opportunity for the British people, yet disease and death rates—the final criteria for the measurement of social welfare—rose in ominous fashion. Cumulative data of all sorts pointed to an extreme degree of poverty, drunkenness, and general depravity, although some of this may have been exaggerated by over-zealous reformers.[2]

It is hardly necessary to describe the insanitary surroundings of the poor. Filth and overcrowding were common enough, and modern ideas relative to pure air, food, and water were largely lacking. The London slums were most written about at the time, and afforded the best "horrible examples"; but there is reason to believe that other towns were about as bad and almost as unhealthy.[3] As a matter of fact, London had more than its share of relatively new dwellings, because of the great fire and the rapid expansion of its population. It also possessed a superior sewage-removal system, inasmuch as the development of market gardens encouraged the employment of efficient "night soil men". The gradual extension of private water-pumping facilities and the invention of an improved water-closet, however, led after 1780 to the contamination of sewers originally intended to serve only drainage purposes. This unfortunate consequence of early "modern improvements" was later associated with the famous stench of the Thames, and with the coming of the cholera.[4]

The British Government provided the poorer classes with almost no protection against the "underlying unwholesomeness" of their sur-

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demic diseases is traced in A. Hirsch: *Geographical and Historical Pathology* (transl. by C. Creighton, Lon., 1885). Creighton's own study of English epidemics is also a classic.

[1] Alfons Fischer: *Geschichte des Deutschen Gesundheitswesens*, I (Berlin, 1933), 275, 297 ff.

[2] See, for a strong statement on the degree of public drunkenness, Sir George Newman: *Health and Social Evolution* (Lon., 1931), p. 50; note also G. T. Griffith: *Population Problems of the Age of Malthus* (Cambridge, Eng., 1926), pp. 198 ff.

[3] William Farr held this view, as well as some present authorities; see John Brownlee: "The Health of London in the Eighteenth Century", Royal Soc. of Med. *Proceeds.*, XVIII (1925), Pt. 2, p. 75. Creighton believed that London was more unhealthy than other cities, though he admitted that some market towns were bad enough; see H. D. Traill (Ed.): *Social England*, IV (Lon., 1902), 471.

[4] M. C. Buer: *Health, Wealth, and Population in the Early Days of the Industrial Revolution* (Lon., 1926), p. 108.

roundings. Statute laws relating to such matters as street cleaning and sewers had been enacted occasionally since medieval times, and during the sixteenth century an elementary health code was developed and apparently enforced.[1] But there were no permanent boards of health in English towns until almost the end of the eighteenth century; and by this later period the general authorities seem to have lost interest in the enforcement of such statutes as existed.

The Elizabethan poor-law system, locally administered, provided for a poor-law doctor in each "union", but this official came to be overworked and underpaid as the towns grew, and his functions were palliative at best.[2] He certainly did not serve as a health officer. No redress could be secured against insanitary conditions in Scotland, save through an expensive private suit. English common law provided for indictments against common nuisances by inquest juries, but this practice gradually ceased to be an effective one.[3] It would seem likely that the gradual acceptance of *laissez-faire* principles inhibited governmental control of health, as well as of business, during the eighteenth century.

The lack of administrative or judicial control over health conditions left that field largely in the hands of private medical men. This very fact made it incumbent upon governments to give some heed to the qualifications of practitioners, and even to measures for extending their services to the poor. This latter interest was illustrated in England, for example, by Daniel Defoe's suggestion (*Essay Upon Projects*, London, 1697) for a sort of sickness insurance system. Some years later (1714) John Bellers proposed a national health service that would have extended medical care to the poor. The provision of poor-law doctors mentioned above, imperfect as it was, represented a first step towards this goal of "social medicine".[4]

Most European governments felt the need, at least by the sixteenth century, for some regulation of medical practice. The College of Physicians in London, the *collegium medicum* of German cities, the *tribunal del protomedicato* in Spain, were all granted some control over licensing practitioners, the inspection of drugs, and the like. The creation of such medical councils in Prussia in 1661, and the grant to them of detailed and effective powers, exerted a considerable influence there-

[1] S. V. Larkey: "Public Health in Tudor England", *Amer. Jour. of Pub. Health*, XXIV (1934), 1099 ff.

[2] Edwin Chadwick, in the *Report on the Sanitary Condition of the Labouring Population* (Lon., 1842), p. 354.

[3] Edwin Chadwick, op. cit., pp. 40–77, 300–3.

[4] See George Rosen: "An Eighteenth-Century Plan for a National Health Service", *Bull. of the Hist. of Med.*, XVI (Dec. 1944), 429 ff.

after in the rest of Germany.[1] The English Government experimented with a system of licensing by episcopal authorities, but then turned these powers over to the London College of Physicians. This body, as was noted in a preceding chapter, proved more concerned with maintaining a narrow monopoly of practice than with an effort to provide English society with reliable practitioners. Hence even this branch of medical police was marked by considerable confusion in England until after 1850.[2]

To all that has been said about administrative indifference to health conditions, one important exception must be made. Attempts to control epidemics had long been taken more seriously than the regulation of either sanitation or of medical practice. Here the influence of both folk-medicine and of rational science was marked, although the two were sometimes at variance. The popular belief in contagion, strengthened by medieval experience with leprosy and with the "Black Death", had been further intensified during the seventeenth century by renewed visitations of the plague. Administrative authorities in all European countries, aroused by fear, expressed this contagion doctrine in a series of stringent regulations. Quarantine laws, first developed in Italian ports against ships from the Levant, were copied by the northern nations. Local as well as national quarantines were sometimes provided, and both were supplemented by isolation and notification procedures similar in principle to those once employed against leprosy. Provisions were also made for the destruction of materials and of animals suspected of carrying the "poison" of contagious disease. Unfortunately only the larger animals, such as dogs and cats, seem to have been much feared; while the sinister rat went his way largely unmolested.[3]

What did rational medicine have to say on this important problem of contagion? This was the query which the British Government submitted in 1720 to Dr. Richard Mead, of St. Thomas's Hospital, when an outbreak of plague at Marseilles threatened England with a renewed invasion. Mead recommended the usual quarantine and isolation in "pest

[1] Fischer: *Geschichte des Deutschen Gesundheitswesens*, I, 331. Full text of the edict creating these bodies is given on pp. 340 ff.

[2] See "Medical Reform", *Edinb. Rev.*, LXXXI, 248 ff.; R. H. Shryock: "Public Relations of the Medical Profession in Great Britain and the United States", *Ann. of Med. Hist.*, n.s., II (1930), 308 ff.

[3] An excellent description of prevailing views and procedures may be found in Richard Mead: *A Short Discourse concerning the Pestilential Contagion, and the Methods Used to Prevent It* (2 ed., Lon., 1720). A brief but interesting history of quarantines is provided in Wilson Jewell et al.: "A History of Quarantine", in the *Proceedings and Debates* of the Third National Quarantine and Sanitary Convention (N.Y., 1859), pp. 247–331.

houses", but objected to the closing of infected dwellings. Rather, he held, should such buildings and the whole neighbourhood surrounding them be kept open and cleanly, and the houses "fumigated" in order to destroy all traces of disease.[1] A parliamentary Act embodying these suggestions was adopted at once, but was repealed the next year. Despite such governmental indifference, Mead's book went through several editions, and called attention to the need for sanitary reform as well as for quarantines in checking the spread of disease. He even urged the creation of a national health council, but this interesting suggestion does not seem to have been taken very seriously.

A tendency to make quarantines, isolation, and the destruction of goods a little less severe was already apparent in Mead's recommendations. The protests of commercial interests, hampered by long quarantines, became more insistent as the fear of plague declined. Those who objected to quarantines could always claim that a given disease, even though of an epidemic character, was not spread by contagion. There was the orthodox classical view that epidemics could be traced to noxious airs, decaying materials, and the like—a view that called for sanitary reform rather than for quarantines. To the extent that emphasis shifted from the popular concern with contagion to the older interest in noxious airs and waters, there was a corresponding shift in health administration from isolation procedures to sanitary measures.

This was notably the trend in hygienic thought and practice after 1800. Yet such a change was not to take place without intense controversy as to the contagious or non-contagious character of certain diseases.[2] It was particularly unfortunate for the public health that, amid all the social and legal difficulties of the early eighteenth century, medical science was also marked by such confusion as was involved in this contagion controversy.

The history of obstetrics and of pediatrics affords other illustrations of the way in which inadequate medical science affected the public health. Maternity cases were left, in English-speaking lands, almost entirely to midwives. Despite the general coarseness of the age, the attendance of men upon maternity cases was held to be most indelicate. The apparent paradox may possibly be explained in terms of matrimonial jealousy and suspicion. Whatever the explanation, the fact

[1] Mead, op. cit., pp. 21–47. See also C.-E. A. Winslow: "A Physician of Two Centuries Ago: Richard Mead and His Contributions to Epidemiology", *Bull. of the Inst. of the Hist. of Med.* of Johns Hopkins Univ., III (1935), 509 ff.

[2] See, e.g., Chas. Maclean, M.D.: *Evils of Quarantine Laws and the Non-Existence of Pestilential Contagion*, etc. (2 ed., Lon., 1825), pp. 216 ff.

remains. And since midwives lacked any scientific training, obstetrics proceeded on the level of folk practice, and with consequences which may be easily imagined.

The medical literature of earlier centuries contained considerable material relating to children's diseases,[1] but there was no recognition of pediatrics in the modern sense. Physicians, indeed, tended to ignore this type of practice even as they did maternity cases. Since babies could not describe their own symptoms, was it not mere guesswork for an adult to attempt it? There really was not much to do but to leave infants to the "old grandmothers" who had been tending them since time immemorial. And how they tended them! "The general practice is," observed Dr. Cadogan of London, "as soon as a child is born to cram a dab of butter and sugar down its throat, a little oil, panada, caudle or some such unwholesome mess. . . . It is the custom of some to give a little roast pig to an infant, which, it seems, is to cure it of all the mother's longings." Once weaned, on the other hand, the child was denied fresh fruits and vegetables for fear that these were dangerous.[2] Tight wrapping in swaddling clothes may have been picturesque, but was far from wholesome. All in all, folk practice of this sort was as bad or worse than folk practice in obstetrics.

Even children of the prosperous classes were cared for in this fashion. In addition, they were often turned over to the more or less tender mercies of wet nurses, some of whom were of questionable character. Children of poorer parentage faced additional hazards. If contemporaries can be trusted at all, illegitimacy was common, and great numbers of unwanted babies died of systematic neglect or were deliberately murdered and then reported as "overlaid".[3] All accounts of "the streets of London" refer to the many "dropt" infants, who were abandoned to the parish authorities or to such other persons as might care to preserve them. Most appalling in this tale of horrors was the fate of the infant parish poor. New-born babes were put out to nurse by overseers whose chief concern was to drive a good bargain. The result was a foregone conclusion. In several of the London parishes, about the middle of the century, the mortality of all children received ran from 80 to 90 per

[1] F. H. Garrison: "History of Pediatrics", in I. A. Abt (Ed.), *Pediatrics*, I (Phila., 1923), 65 ff.; John Ruhräh: *Pediatrics of the Past* (N.Y., 1925), pp. 382 ff.; Heinrich Fasbender's *Geschichte der Geburtshülfe* (Jena, 1906) is standard in that field.

[2] W. Cadogan: *An Essay upon Nursing and the Management of Small Children*, etc. (9 ed., Lon., 1769), p. 18.

[3] Common neglect and even infanticide were by no means limited to England. A similar situation in Germany, where severe measures were employed to control it, is described in Fischer: *Geschichte des Deutschen Gesundheitswesens*, II, 17 ff.

cent; while that of those received under one year of age was declared to be 99 per cent.[1]

It was realized before 1750 that the whole disease situation in London was becoming more serious. The death rate rose after 1720 to a maximum in 1741, and remained very high for some twenty years thereafter.[2] Throughout this period the deaths greatly exceeded the births. After the bills of mortality began to give ages, in 1728, it became apparent that infant mortality was the chief cause of the increasing rates. It was estimated that for certain years 74 per cent of all children born in the city died under two years of age, and that this loss accounted for about 50 per cent of the total death rate.[3]

It was hardly to be expected that British authorities, so careless as to health control at home, should manifest any great concern about it in their expanding empire overseas. Disease was a most serious problem in the early American colonies and threatened the first plantations with extermination. New clearings were usually decimated by "fevers"—chiefly malaria, dysentery, and typhoid. Both Europe and Africa contributed diseases to the American scene, as when the English brought smallpox, and Negro slaves arrived with hookworm and yellow fever. The death rate of the three races, whites, Negroes, and Indians—each exposed to unfamiliar infections—was certainly very high.[4] Medical science seems to have been of a relatively progressive nature in Latin America during much of the Colonial period; but it eventually deteriorated and had to be reformed after 1830 under French influences.[5] In the English colonies medicine at first sank below the European level, but improved during the eighteenth century as Americans returned to Europe for professional training.[6]

[1] Ernest Caulfield: *The Infant Welfare Movement in the Eighteenth Century* (N.Y., 1931), p. 140.

[2] One estimate, based on parish registers rather than the bills, gives the death rate in 1700 as 1 in 25; in 1750, 1 in 21; from 1797 to 1801, 1 in 35. By 1821 it had dropped to 1 in 40; cited in George, *London Life in the Eighteenth Century*, p. 25. On the reliability of the bills, see ibid., p. 32.

[3] Newman: *Health and Social Evolution*, pp. 42 ff.

[4] References to the literature are suggested in R. H. Shryock: "Medical Sources and the Social Historian", *Amer. Hist. Rev.*, XLI (Wash., D. C., 1936), 458 ff. A detailed account of disease in the Canadian colonies is given in John J. Heagerty: *Four Centuries of Medical History in Canada*, I (Bristol, Eng., 1928), 3 ff.

[5] Considerable interest has developed in recent years in Spanish colonial medicine. See, e.g., Giuseppe Mazzini: "La Medicina Nel Cile Prima Della Conquista Spagnola", *Riv. d. Stor. d. Sci. Med. e Nat.*, iv s., XXVII (Florence, 1936), 27 ff.; and the account of other papers in the same field given in MM. Laignel—Lavastine et Marcel Fosseyeux: "Le X• Congres International D' Histoire De La Médecine", *Bull. de la Soc. Française d'Hist. de la Méd.*, XXIX (Paris, 1935), 314 ff. There are a number of medical histories of particular Latin American countries, such as the able work of Carlos Martínez Durán, *Las Ciencias Médicas en Guatemala: Origen y Evolucion*, 2 ed. (Guatemala, C.A., 1945).

[6] Francis Packard: *History of Medicine in the United States*, I (2 ed., N.Y., 1931), Chaps.

Social control in the English colonies was largely limited, prior to about 1760, to local or provincial quarantine laws, and to occasional Acts concerning medical practice.[1] Some of this colonial legislation became fairly elaborate on paper. The health codes of Massachusetts and Connecticut, for instance, provided by 1732 for quarantines, isolation of cases of contagious disease (under guard if necessary), the impressment of "tenders" (nurses) in emergencies, the killing of dogs, the destruction or fumigation of materials, and the flying of white notification signals.[2]

It was naturally difficult to enforce such legislation in the little colonial towns of that day. This is well illustrated in the case of one John Rogers, Sr., who returned to New London from Boston, in October o 1721, suffering from smallpox. As a result, both the selectmen of his own town and the governor and council of Connecticut were troubled all one autumn and winter with this case. The town authorities quarantined the Rogers house and twenty people found in it. Some disorder ensuing, they set a guard, and ordered all the dogs there killed. Various people, however, insisted upon going in and out, especially those "who have several times discovered much stupidity and stubbornness" in refusing to obey orders. A month later sickness was still increasing at the unruly Rogers place, and the town became alarmed. John Rogers, Sr., having meanwhile died, his son was forced to give £100 bond that none who were not properly recovered (and they and their belongings "purged of all dregs of the said distemper") should come into town. Finally, as late as April 1722, the governor had to take action to make the New London selectmen pay their share of the expenses of this vexatious quarantine.[3]

Apart from such efforts at isolation or quarantine, there was little that could be called medical police in the English colonies. Laws relating to medical practice were sporadic and of doubtful effect. No health councils or other administrative units were established. Lest this be set down

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1 and 2. See also Edward Eggleston: *The Transit of Civilization from Europe to America in the Seventeenth Century* (N.Y., 1901), pp. 48 ff.; Carl Bridenbaugh: *Cities in the Wilderness* (N.Y., 1938), 88 ff.; Carl and Jessica Bridenbaugh: *Rebels and Gentlemen* (N.Y., 1942), 263 ff. There are a number of state histories of medicine covering the Colonial period. Outstanding is Wyndham B. Blanton: *Medicine in Virginia in the Seventeenth Century* (Richmond, 1930)—later volumes cover the succeeding centuries. For American Indian medicine, see the anthropological journals.

[1] Packard, op. cit., Chap. 3. Records of special local quarantines will be found in the journals of the Assembly of Massachusetts Bay, and doubtless in other provincial and town records.

[2] C.-E. A. Winslow: *Health Legislation in Colonial Connecticut* (New Haven, n.d.), p. 11 ff.

[3] Winslow, op. cit.

to the spirit of the times, it is well to recall that from the sixteenth century on, the Spanish Government had provided a fairly comprehensive system of health officers and councils for its American colonies. The functions of the *Tribunal del Protomedicato General* of Peru (established in Lima, 1569) related to medical licensing, the inspection of drugs, and the investigation and reporting of new remedies. The medical code of Castile was, in all such matters, applied to the Spanish American viceroyalties.[1] Whatever this system actually achieved, it at least indicated a concern for public welfare apparently lacking in the English-speaking lands. Fortunately for them, however, the English colonists possessed certain advantages over the people of the mother country. The former rarely lived in congested areas, and it is a plausible assumption that they gradually benefited from the higher standards of living possible in a new continent.

Fortunately, also, the British scene itself was transformed after the middle of the eighteenth century. Certain economic and technical changes associated with industrial development began to improve the surroundings of the poor. The advent of cheap cotton clothing and of iron bedsteads eventually promoted cleanliness. The rebuilding and expansion of London early in the eighteenth century impressed the provinces, and efforts were made to imitate the superior Georgian brick houses of the metropolis. In the fifteen years between 1785 and 1800 alone Parliament passed over two hundred Acts for paving and parochial improvements.[2] At the same time, the gradual extension of drainage operations tended to decrease the malaria of rural districts. Even the smaller villages witnessed a decided improvement in general living conditions during the period.[3]

In this way the Government took the first steps towards the sanitary reform advocated by medical authorities. The general humanitarian trend of the eighteenth century moved in the same direction. Clergymen and physicians were more likely to appreciate the sufferings of the poor than were other representatives of the upper classes, and both groups took a leading part in the ameliorative movements which ensued. Thus the Church of England established various welfare societies from 1690 on, and some of its more earnest spirits displayed a growing interest in the poor which culminated in the Wesleyan movement after

[1] H. Valdizan: *La Facultad de Medicina de Lima*, I (Lima, 1927), 21; *Recopilación de Leyes de los Reynos de las Indias* (2 ed., Madrid, 1756); libro V, título VI, ley V (1535).

[2] Buer: *Health, Wealth, and Population in the Early Industrial Revolution*, pp. 78 ff.; G. T. Griffith: *Population Problems of the Age of Malthus*, pp. 235 ff.

[3] John Sinclair: *Code of Health and Longevity*, I (Edinb., 1808), 124, 125.

1750.[1] Clerical reformers, however, were naturally preoccupied with problems of religion, morals, and education; and in many cases it remained for physicians to demand an improvement in the health and general well-being of the masses.

Socially minded practitioners were shocked by conditions which seem to have escaped other observers. Their training enabled them to appreciate the seriousness of disease situations, and also helped them to envisage a remedy. Dr. Cadogan and Dr. Trotter of London, as well as Wilberforce, condemned the evils of drunkenness. The London College of Physicians urged Parliament to restrict the sale of strong drink, and some response followed this early temperance movement. A number of "gin acts" seeking to limit or prohibit the use of that beverage were passed by Parliament after 1729.[2] In like manner it was a physician, Benjamin Rush, who inaugurated the temperance movement in America;[3] and physicians took a leading part at the same time in establishing the first *Verein gegen den Missbrauch geistiger getränke* in Germany.[4]

During the last half of the eighteenth century the rapidly growing British manufacturing towns reproduced the sordid surroundings of the metropolis. Poverty and disease became more obvious, whether or not they actually increased at first in an urban environment. Public-spirited physicians in Manchester and Glasgow added their voices to those of London, in urging both the prevention and cure of disease among those least able to care for themselves. Noteworthy were the services of Thomas Percival (1740–1804) and John Ferriar (1761–1815), who served in the Manchester Infirmary, investigated homes and factories, led in the establishment of the Manchester Fever Hospital, and preached both general sanitary and factory reform.[5]

It would be over-flattering to think of such men as typical of their profession, but they were in a sense typical of an age in which the new industrialism was combined with a new striving after "enlightenment". It was significant that the lay reformer, Robert Owen, was associated with the "Manchester group" of physicians before he inaugurated his humanitarian experiments in Scotland. The American counterpart of

[1] George Newman claims that "out of Wesley's work came a widespread health movement, personal and communal and covering the country", *Health and Social Evolution* (Lon., 1931), pp. 61, 62. Other historians are inclined to think that the primary emphasis in early Methodism was laid on personal salvation rather than on social reform.

[2] Newman: *Health and Social Evolution*, pp. 50, 55.

[3] See Nathan Goodman: *Benjamin Rush: Physician and Citizen* (Phila., 1934), pp. 274 ff.

[4] Georg Meyer: *Die Sociale Bedeutung der Medizin* (Berlin, 1900), pp. 20 ff.

[5] See John Ferriar: *Medical Histories* (2 vols., Lon., 1810).

public-spirited British practitioners was again Benjamin Rush, who was not only the outstanding pioneer in temperance agitation, but also led the prison reform movement in the United States. Somewhat similar in his activities was another American scientist, Benjamin Thompson (Count Rumford), who, after living some years in London, did much to improve the lot of the poor in Munich. There he established, for example, the first school lunches in order to improve the diet of underprivileged children. The attractive English Garden in Munich remains to this day a memorial to his interest in city planning.

The outstanding achievement of medical humanitarians was the establishment of hospitals and dispensaries for the poor.[1] Although the general condition and management of many of the English hospitals of 1800 were unfortunate, there were exceptions to the rule. Especially notable was the founding of a number of institutions concerned entirely with maternal and infant welfare. English obstetrics was inferior to that of France and Holland until the time of William Smellie (1697–1763). This remarkable physician was trained in Paris, began practice in London as a "man-midwife" in 1740, and exerted thereafter a great influence in improving this phase of English medicine. He introduced an improved technique, and trained more than nine hundred men, as well as many women, in his new methods. Despite opposition from midwives and prudes, professional obstetricians gained ground and began to improve the whole character of practice. Most remarkable was the work of Dr. Charles White of Manchester and of Dr. A. Gordon of Aberdeen, whose "common-sense" demand for care and cleanliness in obstetrics anticipated the later work of Holmes and Semmelweis.[2]

Conscious that maternal mortality was rising, the early British obstetricians realized the need for institutions in which their improved methods could be placed at the service of the poor. Before 1739, when Sir Richard Manningham established the first lying-in ward, there had been no regular provision in any of the London hospitals for obstetrical cases. After this there was a rapid growth of special wards and hospitals. The Middlesex Hospital, for example, set aside a ward directed by an "accoucheur" in 1747; the British Lying-In Hospital was founded in 1749, the City of London Lying-In Hospital in 1750, Queen Charlotte's in 1752, and so on. Other specialized as well as general hospitals were established in this period—almost always, in English-speaking lands, as a result of private charity rather than of Government support. But

[1] See Chap. III.

[2] Sir Arthur Newsholme: *Evolution of Preventive Medicine* (Baltimore, 1927), pp. 182, 183.

none of the new institutions was more significant than these first maternity hospitals.

Some idea of the remarkable improvement in obstetrics which accompanied their early growth may be had by comparing the mortality rates reported for different periods. The average figures for the British Lying-In Hospital of London, for instance, were as follows: [1]

	1749–1758	1779–1788	1789–1798
Maternal Mortality Rate per thousand births	24	17	3·5
Infant Mortality Rate per thousand births	66	23	13

This record was no doubt much better than the average, even in the best hospitals, but it illustrates the truly remarkable reduction in maternal mortality which ensued when "midwifery" was raised from a level of folk practice to the plane of scientific procedure. Like surgery and dentistry, obstetrics had long suffered from the age-old taint attached to actual work with the hands. It was no accident that all of these specialties, once little more than trades, attained professional status during the later eighteenth century.

Paralleling the maternity welfare work of this period was a most interesting and effective infant welfare movement. Foundling hospitals were established at the same time as the lying-in institutions. In 1747, Dr. William Cadogan published his famous *Essay upon Nursing* for the governors of the London Foundling Hospital. In this forerunner of our later "child-care" literature, Cadogan sought to clear away the accumulated rubbish of folklore and to replace it with a sane, empirical hygiene. His book went through ten editions between 1747 and 1772.[2] In it he extolled the value of clean linen and fresh air, and even insisted that fresh fruits and vegetables were not unhealthy for children. Present readers may recall that the succulent banana has only come into its own, in this connection, during the last generation.

Cadogan was not the only London physician of his day who was interested in child welfare. George Armstrong published a work on the diseases of children which went through three editions between 1767 and 1778. In 1769 he established a dispensary for poor children, and here no fewer than 35,000 children were treated during the next twelve

[1] Buer: *Health, Wealth, and Population in the Early Days of the Industrial Revolution*, pp. 143 ff. The maternal mortality rate here reported for 1789–1798 (3·5) is almost as good as the best rates reached to-day. It is to be remembered, however, that a hospital might run for some years with no outbreak of puerperal fever, only to have its mortality suddenly increased by that disease.

[2] Buer, op. cit., p. 150.

years.[1] This and subsequent dispensaries were the forerunners of out-patient departments in hospital service; and the dispensary doctors who served them were, from a social point of view, more significant figures than the contemporary "gold-headed cane".[2] Meanwhile, mothers' pamphlets were issued, and the manuals on home medical care began to give more adequate attention to children's ills. So, in various ways, both physicians and laymen came to demand that something be done about the prevailing waste of child life.

The same demand was current on the Continent. Definite concern about child mortality was expressed, during the later seventeen hundreds, by governmental authorities in Spain and in the Spanish colonies.[3] One finds a certain Francisco Laguna publishing, in distant Lima, an essay on the welfare of the unborn (1781).[4] The child-welfare work of the French reformer, St. Vincent de Paul, antedated that of outstanding British humanitarians in the same field. A generation before the appearance of Armstrong's work André published in France his book on the prevention and correction of bodily deformities in children, in which a plea was made for health and freedom.[5] Most notable of the French reformers was Rousseau, whose *Émile* contained much that was sensible about child hygiene, amidst no little nonsense about physicians. The romanticism he represented called men "back to nature", and advocated the liberation of both adults and children from artificial limitations and restraints. Romanticism also tended to sentimentalize the relations of parents and children, and so made for greater tenderness towards "the little ones". Such sentiments led eventually to the famous decrees of 1793, in the French National Assembly, for the protection of mothers and children.

Rousseau's appeal, like that of English physicians, was for the voluntary co-operation of parents and others who came into contact with children. At the same time in Germany, Spain, and Italy the tendency had been to promote health reform directly by administrative authority. This tendency was best expressed in both the writings and actual administrative reforms of Johann Peter Frank. The most minute details of healthy living for children and adults were to be regulated by law; the exact hour at which young people were to stop dancing, for in-

[1] Garrison: "History of Pediatrics", in Abt, *Pediatrics*, p. 72.
[2] This is Buer's observation.
[3] M. L. Amunategui: *Los precursores de la independencia*, I, 321.
[4] *El zelo sacerdotal para con los niños no nacidos* (Lima, 1781).
[5] Sigerist: "Die Arztliche Kosmetik im Wandel der Jahrhunderte", *Die Medizinische Welt* (1931), nos. 47, 48, 49 (sonderabdruck), pp. 12 ff.

stance, was to be fixed and final.[1] Yet the appeal to popular co-operation also appeared in Germany, as is illustrated by the interesting little health catechisms for children which attained a considerable vogue both there and abroad.[2] The same tendency is to be observed in Adalbert Zarda's attempt (1792) to afford the German people instruction in "first aid" procedures.[3] Some instruction in hygiene was given in German schools from the seventeenth century on.[4] Such a programme was rarely attempted in English-speaking countries until after 1850, unless the interest in sports may be viewed as an equivalent.

Other forces beside romanticism and humanitarianism were at work to improve the health of children and adults after 1750. Both the infant welfare movement and sanitary reform were but special aspects of an increasing concern for all phases of hygiene. Such a hygienic renaissance was to be expected in any civilization in which there had been economic and cultural progress for several centuries. Hygiene, in the broadest sense of the term, was more than a branch of the medical sciences; it was an aspect of civilization in itself.

There had long existed a considerable European literature on the rules of daily living. After the fifteenth century the number of books on hygiene increased by a sort of geometrical progression. The seventeenth-century list was longer than the sixteenth; while the eighteenth-century publications far surpassed those of the two preceding eras combined. Most of these works were written by physicians or by those who made a claim to that status. The eighteenth-century literature represented a phase of "the Enlightenment",[5] and as such began to attract the attention of scholars. Mackenzie, who wrote one of the first histories of hygiene,[6] overlooked a large part of it. Continental bibliographers, notably Haller and Plouquet, were more careful; and their lists became the basis of the comprehensive bibliography published by John Sinclair of Edinburgh in 1808. Sinclair was able to note nearly 1900 items which had been published up to that time; and this number

[1] Leona Baumgartner and Eliz. M. Ramsey: "Johann Peter Frank", etc., *Ann. of Med. Hist.*, n.s., VI (1933), 69 ff.

[2] E.g., Bernhard C. Faust: *Gesundheits-Katechismus: zum Gebrauche in den Schulen u. beym häuslicher Unterrichte* (Buckeburg, 1794). A translation of this popular little book appeared in the U.S.A.

[3] Georg Meyer: *Die sociale Bedeutung der Medizin* (Berlin, 1900), p. 19. There was considerable opposition, on the part of physicians, to this or any sort of *Samariterunterricht.*

[4] Fischer: *Geschichte des Deutschen Gesundheitswesens*, I, 310 ff.

[5] The German literature on hygiene, e.g., seems to have been stimulated by the general spirit of the French Enlightenment, see Fischer, op. cit., II, 121.

[6] J. Mackenzie: *The History of Health and the Art of Preserving It* (Edinb., 1758).

presumably attests to the interest which had been aroused in the subject.[1]

A considerable percentage of Sinclair's list related to new viewpoints and methods in hygiene; for contemporary advances in physics, chemistry, and other sciences were bound to exercise an influence in this field as well as on other phases of medicine. Classical concepts and terminology relating to the "naturals" and "non-naturals" which influenced health were abandoned, and in their place appeared the modern distinction between hereditary and environmental influences—the latter in turn divided between those relating to personal and to public hygiene.[2] While there was still much quoting of classical authorities, after the scholastic manner, it was evident that the experimental method was impinging on hygiene even as it had on other sciences. The subject ceased to be of a purely empirical nature. The American physicists, Franklin and Count Rumford, explored the problems of ventilation, partly as a result of Lavoisier's work on combustion and respiration. And the great Frenchman himself made experimental studies of chemical changes in sewage, and on the means of rendering such materials harmless.[3]

A notable feature of eighteenth-century hygiene was the attempt to project it from a purely personal to a public plane. Interest gradually shifted from individuals to special groups; then, from the latter, to neighbourhoods or classes; and finally to the population as a whole. A number of works on the diseases of miners were published during the seventeenth and early eighteenth centuries. Dazille, a French authority, made observations on the diseases of Negro slaves, in his general work on health conditions in colonies. About the middle of the century and thereafter, British and French physicians wrote on military and naval hygiene; the contributions of Lind, Pringle, and Poissonnier were notable in this connection.[4] Captain Cook's famous voyages, already mentioned, clearly demonstrated the value of citrus diet as a preventive of scurvy, which was practically an occupational disease of sailors. And John Howard, in the course of his famous "winter's journey" through English prisons, called attention to the evils of typhus as a "gaol fever".

[1] Sinclair: *Code of Health and Longevity* (Edinb., 1808). There was some duplication in his lists. See also Hallé and Nysten: "Hygiène", *Dictionnaire des Sciences Médicales*, XXIII (Paris, 1818), 510–588; L. Simon, "Recherches Historiques sur l'Hygiène", *J. d. prog. d. sc. et inst. méd.*, IX (Paris, 1928), 104–55.

[2] See Sinclair, op. cit., I, 20.

[3] A. Proust: "Les progrès de l'hygiène à travers les âges", *Le Prog. Méd.*, 2 s., XVII (Paris, 1893), 394.

[4] English-speaking writers usually overlook the French contributions, but see Hallé et Nysten, op. cit., pp. 548, 549.

The necessity for a permanent control of the public health, however, was but slowly realized in English-speaking countries. Quarantine regulations were thought of primarily as emergency measures. While a few English writers, notably Defoe and Bellers mentioned above, had envisaged systematic social control, this concept received little serious attention. It has been noted that there was almost no conception of comprehensive medical police in British cities, although the Manchester group may be said to have been working towards it by about 1780.[1] German authorities on medical police were convinced that the British attached little importance to the regulation of medical practice, and that the Americans did nothing about it at all.[2]

It is true that increasing attempts were made, as the eighteenth century advanced, to popularize personal hygiene among the British peoples. Cadogan's work illustrates this, although it related only to the care of small children. At the same time, the general literature on personal hygiene was increasing in England as well as abroad. Some of it still appeared in didactic verse, as did John Armstrong's *The Art of Preserving Health: A Poem in Four Books*, which was dedicated to Richard Mead. A desire to instruct the public was expressly stated in the dedication of this long poetical treatise. Much that had a modern ring—we ought to say our "modern" work has an ancient sound—was included in Armstrong's treatise. On exercise, for example, he observed tersely that:

> Of Exercises *Swimming's* best,
> Strengthens the Muscles of the Chest.
> As 'tis the best, so 'tis the Sum
> Of *Exercises* all in one,
> And of all Motions most complete,
> Because 'tis vi'lent without *Heat*.[3]

It is a natural assumption that such attempts to improve individual well-being represented an effort to better the public health as a whole.

Most British physicians who wrote on personal hygiene were conscious of the value of sports in this connection. The English gentry, and to some extent the country people in general, had long been devoted to outdoor life of one sort or other. Hunting, fishing, racing,

[1] An interesting description of the work of Percival and Ferriar along this line, in Manchester, is given in Sir Gilbert Blane: *Select Dissertations on . . . Medical Science* (Lon., 1822), p. 174.

[2] R. V. Mohl: *Die Polizei-Wissenschaft Nach den Grundsätzen des Recht staates*, I (Tübingen, 1833), 135. Some indication of the German interest in medical police, before 1800, may be had by consulting C. F. Daniel, *Entwurf einer Bibliothek der Staats-arzneikunde*, u.s.w. (Halle, 1784).

[3] 2 ed. (Lon., 1774).

boxing, and golf all had their devotees.[1] It seems strange to-day to find Sinclair lamenting, back in 1800, the decay of the grand old game of golf in Scotland; but it is likely that some other sport had temporarily replaced it. Meanwhile, the British manifested an almost unique interest—among Europeans—in team games. Football of some kind can be traced well back into the Middle Ages;[2] and those famous "Pilgrim Fathers" who—not so old at the time—planted a New England Plymouth in 1620, used to play with "ball and bat" along its single thoroughfare. Football and cricket were cultivated in English public schools, and so became a feature of the education of the ruling class.

Although team games temporarily declined in the United States under the stress of frontier conditions, baseball and other sports were developed after 1840;[3] and the same general interest in athletics eventually asserted itself on one side of the Atlantic as on the other.[4] This was well illustrated in Canada, where cricket survived side by side with baseball, and where the Indian lacrosse was added to games of Anglo-Saxon origin. The pursuit of sports in all English-speaking lands involved, even if unconsciously, some cultivation of hygienic ideals.

A few British observers became interested, soon after 1800, in public health control of a really comprehensive character. Influenced in part by contemporary German work, but also forced to face the facts of urban development, they began to call for a systematic medical police. One of the first of them was John Roberton, who published his work on *Medical Police* in Edinburgh in 1808. In this he called attention to the importance of German work in public hygiene, and to the limitations of typical English investigations into particular problems or particular diseases. "It is to be regretted", Roberton observed of John Howard, "that he confined his observations almost entirely to the gaols he visited." Why did he not investigate the surrounding country as well? Only then, observed Roberton, would he "have rendered his work complete".[5]

Having depicted the seriousness of the disease situation in England, and something of the misery this entailed, Roberton went on to point out how much of it was preventable. Science, he noted, was making

[1] E. D. Cuming: "Sports and Games", in A. S. Turberville (Ed.): *Johnson's England*, I (Oxford, 1933), 362 ff.

[2] Francis P. Magoun, Jr.: "Scottish Popular Football, 1424–1815", *Amer. Hist. Rev.*, XXXVII (1931), 1 ff.

[3] There are still, at least in name, a few cricket clubs in Philadelphia.

[4] The history of American sports is illustrated in John A. Krout: *Annals of American Sport* (New Haven, 1929). This is Vol. XV in Ralph H. Gabriel (Ed.): *The Pageant of America.*

[5] John Roberton: *Medical Police*, etc. (2 ed., Lon., 1812), p. xliii.

such progress in England that it seemed strange greater efforts had not been made to improve the public health along scientific lines. Overcrowded tenements in industrial centres and the unhealthy fens of rural regions should both be systematically removed. (Marked success had already been claimed for drainage operations directed towards reducing malaria in France and Italy.)[1] If unnecessary disease were not prevented, continued the British author, the statesman must consider its effect upon the "productive powers" of all classes. "Let him calculate the decrease of a country's wealth from this subtle, active, wide-wasting destroyer." Authorities should think, too, of the future. Malthus had already pointed out that a rapid increase of even an active population would be bad for Britain; how much worse would be the increase of an unhealthy people! What a drain such a people would make upon the economic resources, as well as upon the virility of the nation. How much more economical as well as more humane would it be for physicians, magistrates, and philanthropists to prevent all this unnecessary tragedy.[2]

So argued a forward-looking British sanitarian as early as 1808, but his ideas made relatively little impression at the time. Similar opinions had been expressed before by Johann Peter Frank;[3] and more than a generation later essentially the same views would be stressed by Chadwick and by Southwood Smith. Statesmen would give some heed in this later period. For the time being the chief improvement in medical administration was to be found in the establishment of the first permanent local boards of health in Great Britain and in the United States between 1790 and 1830.[4] These boards had at first no full-time medical officers attached, although quarantine officers, who had long served in major ports, were sometimes associated with them. The first permanent boards were suggested by the example of the temporary organizations set up in times of emergency. An illustration is afforded by the special board created in Philadelphia during the yellow fever of 1793, when the Franco-American merchant, Stephen Girard, so courageously directed its activities.[5]

Yet it would be unfair to judge the significance of the early British public-health movement simply in terms of administrative developments. Progress in obstetrics, pediatrics, and hygiene could be made

[1] Hallé et Nysten: "Histoire de l'Hygiène publique", *Dict. des Sci. Med.* (Paris, 1818), XXIII, 549.
[2] Roberton, op. cit., pp. xxx, xlii, xlviii.
[3] J. P. Frank: *System einer vollständigen medicinische Polizey*, I (1779), 9.
[4] J. M. Toner: "Boards of Health in the United States", Amer. Publ. Health Assoc., *Reports and Papers*, I (1873), 499 ff.
[5] J. B. McMaster: *The Life and Times of Stephen Girard*, I (Phila., 1918), 212 ff.

available in other ways than by official mandate. A voluntary campaign of popular education may, in certain circumstances, exert as much influence upon health conditions as could the development of an administrative programme. The final test of any means to an end must be stated in terms of results; in the case of an attempt at health control, in terms of vital statistics. Judged in this manner, the early British health movement seems a most significant one. As a result of some of the circumstances noted, and doubtless of other influences less easily recognized, the English infant mortality rate began to fall in striking fashion after 1780 and continued to decline until about 1815. The average rate for 1731–1740, under the most deplorable conditions, is estimated to have reached 437 per 1,000 births, and that for the next decade was practically the same. Between 1751 and 1760, however, the rate had dropped to 360, and by 1791–1800 it was down to 240—a fall of almost 50 per cent within fifty years![1]

Such a decrease in the deaths of small children was bound to affect the crude death rate of the entire nation. This is estimated to have averaged 35·8 per 1,000 in 1731–1740; by 1751–1760 it had fallen to 30·3; in 1771–1780 it rose slightly to 31·1; but thereafter fell again, until it reached 26·9 in 1791–1800. The rates for London alone during the same decades were usually higher but displayed a similar downward trend.[2] That the decrease was due almost entirely to the improvement in children's health is made evident by an examination of the death rates and life expectancy of older age-groups during the same period. The average life expectancy at age ten, for instance, is estimated to have been 36·9 for the period 1730–1739. This remained almost stationary until about 1780, when a slight improvement brought it up to 38·2 in the last decade of the century. The life expectancy at age twenty showed no improvement whatever in the whole period between 1730 and 1800; standing at 29·1 in 1730–1739, and actually below that (28·4) in 1790–1799.[3] Life expectancy at ages thirty and forty was also approximately stationary throughout the century.

[1] For children under two years. Estimates are based upon bills of mortality, parish records, etc., and are of a complicated character. They are never exact—e.g., births have to be estimated from christenings, for a time when nonconformism and other factors tended to decrease the ratio of christenings to total births. Modern statisticians, however, consider that careful estimates are significant for general trends. The figures here cited are from Brownlee: "The Health of London in the Eighteenth Century", Roy. Soc. of Med. *Proceeds.*, XVIII (1925), Pt. 2, p. 74. See also Buer: *Health, Wealth, and Population*, p. 224; and T. H. Marshall: "The Population Problem during the Industrial Revolution", *The Econ. Jour.* (Econ. Hist. Ser. No. 4, Lon., Jan., 1929), pp. 429 ff.

[2] Brownlee, op. cit., p. 76.

[3] As compared with expectancies in London, for 1910–1912, of 42·35 (males) and 46·71 (females), ibid.

It would seem, then, that contemporary efforts to improve adult health, such as inoculation with smallpox or the preaching of personal hygiene, did not result in any significant decline of the adult health rate. This is interesting in view of the fact that the apparent control of such diseases as smallpox, scurvy, and others led to that general medical optimism which was described in an earlier chapter. Was this optimism entirely unfounded, so far as those over ten years of age were concerned? It would seem more likely that the decline or disappearance of certain diseases after 1700 or 1750 did indeed give ground for a sound optimism; but that a contemporary and more or less unrecognized increase in tuberculosis or other diseases prevented the fall in adult death rates which would otherwise have occurred.

Why did certain diseases increase and others decline? This is the great conundrum of demography. There would as yet seem to be no entirely satisfactory answer, although a number have been suggested.[1] A Scottish medical authority of the late eighteenth century was convinced that the two phenomena were related as cause and effect; that is, that the decline of one disease reacted automatically through some obscure biological mechanism to increase another. This really expressed a deep-rooted popular impression that still obtains to-day—"if it isn't one thing, it's another!" Perhaps divine Providence had something to do with it—perhaps this was God's way to prevent such an excessive increase in population as was already worrying the Malthusians. For, with the crude death rate falling rapidly, and with a birth rate which had not reacted to this and which continued high, the danger of overpopulation seemed real enough.

It was, of course, no accident that a serious birth-control movement began in England about two decades after Malthus published his famous *Essay* (1798). While some agreed with his suggestion of "moral restraint", a small group of able men soon decided that artificial control was more feasible. Francis Place, "the radical tailor of Charing Cross", published the pioneer work on birth control in the usual sense in 1822.[2] He eventually secured the aid of John Stuart Mill and other eminent utilitarians in behalf of this cause. Shortly thereafter, the Anglo-American Robert Dale Owen began to advocate the same idea —largely independently—in the United States. He in turn influenced an American physician, Dr. Charles Knowlton; and it is claimed that

[1] There is a most interesting discussion of the natural history of diseases in Hans Zinnser: *Rats, Lice, and History* (N.Y., 1935).

[2] Graham Wallas: *The Life of Francis Place* (N.Y., 1919), pp. 164 ff.; Norman E. Himes: *Medical History of Contraception* (Baltimore, 1936), pp. 212 ff.

the latter's book on birth control was ultimately of more influence in promoting actual decline in the English birth rate than was the original English movement.[1]

It is a common view that a falling infant death rate tends to result in a falling birth rate. The theoretical reasons for this are obvious enough, particularly if this decline in infant deaths is followed by wide urbanization and such social reforms as child-labour laws and compulsory education. Children become an economic liability and parents are more and more pressed to resort to prudential checks—delayed marriage, abortion, and the use of contraceptives. If the official statistics can be relied upon, however, the major decline in the British birth rate did not occur until after about 1870. This suggests a considerable lag in such reactions of birth rates to death rates as may have been due to the application of these checks. It is conceivable that the British birth rate actually began to decline somewhat after 1820, but that this was concealed for some decades by the increasing accuracy of the statistical reports.

The crude birth rate in the United States declined almost steadily after 1810, with a decided drop between 1840 and 1850. This last coincided with the writings on birth control noted above, with the first wide advertising of abortifacients, and with definite protests in medical journals against the growing scandal of family limitation.[2] This phase of the early "Victorian" era has been overlooked by later generations, which have prided themselves on their greater realism and sophistication in such matters.

So far as the death rate was concerned, it is likely that partly unrecognized environmental factors promoted the increase of certain diseases, despite the general trends towards hygienic improvement. Perhaps the appearance of the factory system, with its attendant evils of long hours in badly ventilated mills, overcrowding in unsanitary tenements, and so on, made the masses more subject to pulmonary tuberculosis. This disease did develop ominously during the early industrial revolution in England.[3]

[1] Norman E. Himes: "John Stuart Mill's Attitude Toward Neo-Malthusianism", *The Eco. Jour.* (Supplement, the *Quart. Jour. of the Roy. Ec. Soc.*, Ec. Hist. No. 4, Jan. 1929), pp. 459 ff.; and the same author's "Robert Dale Owen, The Pioneer of American Neo-Malthusianism", *Amer. Jour. of Soc.*, XXXV, 529 ff., 546 ff.

[2] On the history of the American birth rate, see Warren S. Thompson and P. K. Whelpton: *Population Trends in the United States* (N.Y., 1933), pp. 267 ff. (Monographs on Recent Social Trends in the U.S.). The early advertising and the medical protest is noted in *The Med. and Surg. Reporter*, n.s., I (Phila., 1858), 127.

[3] It was fairly well recognized as a clinical picture ("consumption") at the time, in contrast to the nosographical confusion obtaining for most other diseases.

After about 1810, the crude death rates in England again began to rise. Cumulative disturbances due to the Napoleonic wars, as well as the growth of congested manufacturing towns and other circumstances may explain this, but the story must be left to a subsequent chapter. The very fact of such a rise in mortality makes it seem strange that the early nineteenth century did not witness a continued development of the public-health movement. The need for such a movement, and both the scientific and governmental means to attaining it, had been in part pointed out by this time. Yet there seemed a relative lag in the progress of public hygiene during the period between 1860 and 1830, which reminds one of the lag in the other medical sciences during the preceding century.

Perhaps a temporary delay was inevitable. In welfare work, as in pure science, a new trend or new method may suddenly achieve certain things easily—only to find, sooner or later, that other desiderata are far more difficult to attain. The fine zeal of pioneer discoverers or reformers must suffer some disillusionment sooner or later. Those who are disillusioned are often discouraged; or, again, those who come after them may see less hope in "carrying on." It is quite likely, for instance, that the maximum decrease in infant mortality possible under conditions necessarily obtaining in 1800—with regard both to the state of society and to the state of medical science—had already been attained by about that time. Not until the underlying evils of industrialism had been to some extent corrected, and not until fundamentally new medical procedures had been discovered, could there be another drop in the infant death rate comparable to that of 1770–1800. Such circumstances were not to be realized for another century.

It has also been suggested that there ensued in England an actual loss of interest in health and in other phases of the public welfare. Reaction swept over England in protest against the excesses of the French Revolution. The dominant, conservative classes came to see in all appeals for social reform a trend towards the radical and the subversive.[1] Then again, the wars with France distracted attention from domestic to foreign problems, and so made it difficult for humanitarians to arouse an interest in the troubles at home.

The Continental nations suffered a somewhat similar experience during the Napoleonic wars. French arms carried social reform into Italy and the Rhineland; but increasing disease also followed in their train,

[1] W. T. Laprade: *England and the French Revolution, 1789–1797* (Johns Hopkins Univ. Studies in Hist. and Polit. Sci., XXVII, 1909), pp. 152 ff.

and standards in health and in medical police were sometimes lowered in consequence. War brought the plague into France from the Near East, and yellow fever from Santo Domingo. Typhus and typhoid became common among the troops and spread to some extent among the civilian population. In the face of such dangers, an imperial order directed the Academy of Medicine of Paris to report to the Government on all matters pertaining to the public health.[1] But the powers of the Academy remained purely advisory; and meanwhile, in consequence of growing need, men with only preceptorial or hospital training were authorized to practise medicine.[2] The advent of peace, in 1815, left the impression in France that the worst danger from disease was past, and there was no notable effort to advance health control during the next decade. Reaction against social and political liberalism then ensued on the Continent, even as it had in Great Britain before this time.

So far as the public health movement was concerned, the indifference and inertia of European peoples were not to be overcome until the early thirties. A general concern about disease reappeared only when humanitarianism was suddenly supplemented by the old appeal to fear. The terrifying spectre of Asiatic cholera, advancing slowly but relentlessly upon an unprotected Europe, revived the demand for social control as no disinterested humanitarianism could ever have hoped to do.

[1] Vaillard: "Rôle de L'Académie de Médecine dans L'Evolution de L'Hygiène Publique, 1820–1920", *Bull. de l'Acad. de Méd.*, 3 S., LXXXIV (1920), 401, 402.

[2] A. H. Buck: *The Dawn of Modern Medicine* (New Haven, 1920), pp. 260 f.

VI

SCIENCE IN A ROMANTIC AGE
1800–1850

WHILE MEDICAL optimism was aroused by real achievements in practice, it was also encouraged by progress in the related sciences. The more general causes and consequences of this progress, and of its popularization during the so-called Enlightenment, were reviewed in a preceding chapter. Much that was there said with regard to the social and intellectual forces making for the advance of science holds true, though not to the same degree, for the early nineteenth century.

It was clear that science was at work remaking society before 1800. Also obvious, and most encouraging, was the fact that in the process of modifying social conditions science often made possible an acceleration in its own advance. Consider, for example, the indirect influence of the steam engine upon the medical sciences. First patented late in the seventeenth century, occasional improvements in this type of engine brought it to a stage where it could be applied to various practical purposes. Citizens of Paris were alarmed, a generation before the French Revolution, to see a great steam-wagon lumbering down their medieval thoroughfares. Similar monsters—primitive automobiles—now and then astonished the people of other towns in Europe and America during the ensuing decades, and suggested to the more imaginative the revolution in transportation that was to follow.

More or less effective steamboats were in operation after 1790, and the locomotive ran successfully upon a road of rails about 1825. By the latter date, moreover, the steam-engine had been applied to the operation of "steam-presses". These machines speeded up and cheapened printing, just as the steam-boat and locomotive performed a similar service for transportation.

Since cheap transportation, postage, and printing were essential to the development of scientific societies and their publications, these bodies were the immediate beneficiaries of the new order. It became relatively easy to go to meetings at some distance, to correspond with

those one met in this way, and to publish articles, books, and proceedings. The old barriers to professional co-operation were breaking down. And since physicians always represented the most numerous group of scientific men, they were among the first to make the most of these opportunities. There was a rapid increase in the number of medical societies after 1790, and a corresponding growth of medical literature. Provincial and even national bodies, holding periodical meetings and sponsoring important publications, appeared in all progressive nations. Such bodies laboured with some success to facilitate the exchange of ideas, and to encourage improved standards in research, in practice, and in professional ethics. The establishment of the State medical societies and of the American Medical Association in the United States (1847) afford pertinent illustrations.

Behind the progress in transportation and printing, of course, lay the whole material advance of the Western nations during the period of the early Industrial Revolution. Associated with this was an accelerated accumulation of wealth in the hands of the middle classes, which in turn benefited the arts and sciences in varying degrees. The era of special endowments for medical research was as yet far off; but meanwhile some indirect aid was obtained through the establishment of additional university chairs, and from an increased public and private expenditure upon hospitals.

It is true that the intellectual climate of the new age did not seem so favourable to the growth of objective science as had that of the preceding era. There gradually appeared, in apparent reaction against the critical spirit of the Enlightenment, a romantic tendency that profoundly influenced the philosophy and art of all Western nations. This reaction may be explained, in part, as a result of a natural desire for variety and change; men had been taught so long to look at life in a cold-blooded and rational manner that they turned with relief to a warmer and more colourful *Weltanschauung*. They tired of "the cold grey light of dawn" and yearned rather for "the moon tints of purple and pearl".

To be more specific, the romantic reaction may be ascribed to the potent influences set in motion by the French Revolution and by the Empire of Napoleon. These influences may be interpreted in two ways, one of which is the antithesis of the other. To many conservative people it seemed as if the reasoning of eighteenth-century thinkers had been of a merely destructive character and had been responsible for the excesses and carnage of the Revolution. Rationalism itself seemed dis-

credited, and men longed for a nobler and more romantic outlook on life. They sought to escape from the realities of war and desolation. If philosophers and scientists could offer nothing but destructive criticism, they had best stand aside for those who could once more thrill mankind with positive ideals.

In contrast with this, it may be that it was the glory of the Revolutionary era rather than its destruction which promoted a romantic reaction. Perhaps the events which spelled disillusion to some inspired others with a sense of adventure and of achievement. To the dramatically minded the long planning of the *philosophes* had at last led to action; their ideas had finally been translated into deeds, and what deeds! A ragged but glorious France, fighting for the rights of man, succeeded in holding off the tyrants of all Europe. Their battle cry—"Liberty, equality, fraternity"—was no mere appeal to reason. Lyric in the form of *La Marseillaise*, the call has lost none of its stirring power to this day. Mankind was thrilled with a new hope and dreamed a new destiny.

Even when the Republic gave place to the First Empire there was still hope, and even greater grandeur and display. These were indeed romantic times, and it is little wonder that those who lived through them looked at life in the romantic manner. Art, religion, politics, and even philosophy became suffused with emotion. Pope, with his calm, didactic verse, gave way to the mystical Coleridge, the impassioned Shelley, and the love-lorn Keats. The classic dignity of Georgian mansions was replaced by the romantic eccentricities of a "Gothic revival". And a nostalgia for the half-forgotten Middle Ages expressed itself in the other arts as well as in architecture. Tennyson wrote of the Arthurian legend, and Scott of *Ivanhoe*; the while Wagner composed his *Meistersinger* and the *Nibelungen Ring*. This nostalgia eventually permeated all the literate classes, and degenerated at times into sentimentalism and romantic nonsense. So it was that in Charleston, South Carolina, young men spoke much of "chivalry", engaged in tournaments, and dreamed themselves back into the days of the Round Table.

Other young men, no less romantic, devoted themselves to a political ideal known as nationalism. A Germany, an Italy, or an American "Union" must be saved or reborn! Here again an abstract Platonic idea—"the nation"—was suffused with the emotion of the times, and issued forth as a vivid, romantic ideal for which men could live and die. Meanwhile the romantic spirit captivated the gravest philosophers. German idealism, despite its obscurity, was permeated after 1800 with

a moral zeal quite foreign to the cool scepticism of the *philosophes*. Somewhat humanized in the versions of Carlyle and Emerson, this idealism reached overseas and breathed new life into the decadent Calvinism of America. The resulting "transcendentalism" was as truly a romantic adventure, albeit in the moral and intellectual field, as were the more mundane excursions of a Lord Byron or a Garibaldi. Historians, who may have been thrilled by such contemporary drama, began to present the story of the past in a more colourful manner. Historiography, which had shown signs of becoming "scientific" during the eighteenth century, now backslid into literature. Many volumes were devoted to thrilling accounts of the personal and the dramatic. Scholars lost interest in general trends, and became enamoured of the "unique fact".[1] The ironic Gibbon, with his distaste for "enthusiasm" of any sort, would hardly have approved of a Motley, a Macaulay, a von Treitschke or any of the decidedly enthusiastic historians of this age.

How would scientific men react to the prevailing romanticism? Would they be swept off their feet along with philosophers and historians, or would they go ahead against the current? There was real danger in the situation; for scientists, like other men, may become enamoured of metaphysics and even of emotion. Indeed, it was barely two centuries since science, for the first time in modern history, had become more or less independent of the older philosophical modes. Something has been said of the long struggle of the medical sciences to divest themselves of the clinging traditions of medieval thought. Had they triumphed in this struggle only to be ensnared anew?

This was indeed their fate east of the Rhine. For Schelling and other German philosophers, inspired by Kantian idealism, evolved in their minds that *Naturphilosophie* to which reference has already been made, and which exerted so potent an influence upon scientific work in Germany between 1810 and 1840. These thinkers prepared another grand systematization, not simply of medicine, but of the natural sciences as a whole. Ultimate reality was found in the universal Mind, and all natural phenomena were fitted into this metaphysical pattern. As in the medical systems, much was made of the latest scientific ideas; for instance, of the current interest of biologists in the concept of evolution, and of the studies of physicists in polarity and other electrical phenomena. In this manner a scientific façade was thrown across the front of an imposing edifice which was really speculative in character.

[1] Hence historians may still be found who limit historical research to a study of unique facts—a definition accepted enthusiastically by sociologists, who define their field in turn as a search for social laws transcending the unique and the individual.

Evolution was conceived as an activity in the divine Mind, rather than as an actual process among living plants and animals. All Nature was thought, in the ideal sense, to have evolved through cycles and in various forms; and in this scheme of things organic life represented the apex of natural evolution, and human life the last word in things organic. Hence medicine, dealing with the "highest" of all natural forms, must be *the* science of life—must become the crown of all the natural sciences.[1]

This was most impressive. Implicit in such views was a solution for the whole problem of logical method which had perplexed physicians since classical days. Why worry about the conflicting claims of reason and experience? For the "pure reason" in man is a spark of that divine Mind which creates all natural phenomena; that is, which creates all that man experiences. Hence reason, as a part of the divine, actually creates experience; and between the creator and the creature there can be no conflict. Reason creates Nature according to its own categories—for instance, in terms of time and space—and so will recognize its own when it contemplates natural phenomena. The conclusion of such abstractions was that medicine should work both through reason and observation; but that while observation might prove faulty, logical reasoning could never be mistaken, since it was itself the source of all experience.[2]

Obscure as such idealism seems to most English readers, the manner in which it encouraged medical speculation is obvious enough. In theory, at least, medicine became a branch of philosophy—science was absorbed in metaphysics. Medicine in the higher sense was simply the use of philosophy in explaining health and disease. The titles of many of the works of the period reflect this view; for instance, Troxler's *Elemente der Biosophie* and the same author's *Ideen zur Grundlage der Nosologie und Therapie.*

An analysis of general works of this sort discloses, as might be expected, a poverty of actual observation and a wealth of speculation. An example may be found in the *System der Medizin* of Kieser (professor of pathology at Jena), which was published at Halle in 1817. Of 130

[1] Ernst Hirschfeld: "Romantische Medizin. Zu einer künftigen Geschichte des naturphilosophischen Ära", *Kyklos*, III (1930), 6–9. Hirschfeld appends an exhaustive bibliography to this article. See also F. H. Garrison: "The Romantic Episode in the History of German Medicine", *N.Y. Acad. of Med. Bull.*, 2 ser., VII (1931), 841 ff.; Dampier-Whetham: *A History of Science*, p. 324. Paul Diepgen, *Deutsche Medizin vor 100 Jahre: Ein Beitrag zur Geschichte der Romantik* (Leipzig, 1923), is authoritative.

[2] This point of view was not entirely new, even in medical theory—it will be found, for instance, in the earlier English work of Gregory cited in the preceding chapter. Its classic formulation, of course, was achieved in Kant's *Kritik der Reinen Vernuft* (1781).

pages devoted to general diagnosis, only twenty-four related to genuine observations; the remaining 106 were given to theories about the meaning of various symptoms in relation to the general nature of disease. Taking his cue from Schelling's emphasis on the "polarity" of all natural phenomena—that is, on the attraction of such opposites as male and female, or of positive and negative electrical charges—Kieser found that health represented a balance of positive and negative spheres of life. Disease resulted when some cause aroused the negative factor, and so disturbed the normal equilibrium between these mystical biological principles.[1] How much service this sort of reasoning performed for actual practice may be readily imagined.

While the speculative spirit usually soared on an idealistic level, it descended at times to mysticism and to demonology. Heinroth and others formulated a "Christian pathology", and Kerner wrote of demons as the causes of disease. Worthy of more serious consideration was Mesmer, whose work antedated the romantic period, but whose theories about the "magnetic fluid", "animal magnetism", and the like developed in a romantic setting.[2] Mesmerism never secured professional recognition, since the scientific atmosphere at Vienna and Paris was unfriendly to romanticism of any sort; but what physicians rejected, the populace applauded. Fashionable Parisians flocked to this German healer, and had themselves "mesmerized" amid subdued lights and soft music. How much of this was real "mental healing", and how far it was mere charlatanism, would still seem a matter of opinion.

Thus one species of romantic medicine did reach over into France, although along the lower level of a popular cult. Its influence eventually ramified even farther. Years later an itinerant lecturer on Mesmerism travelled through New England and inspired a well-intentioned Mr. Quimby to develop a system of "spiritual" healing. Quimby, in turn, preached his gospel to a remarkable woman, who was to become the most successful of modern health cultists. In the hands of Mary Baker Eddy—or of Quimby—the "magnetic fluid" lost all claim to an electrical nature and became a purely spiritual phenomenon, while "animal magnetism" descended to the same level as Kerner's demonology.

The American version of romantic healing became known as "Christian Science", somewhat as some of the Germans had earlier referred to "Christian Pathology". Unlike the latter, however, the American cult went all the way to an extreme idealism. Since its original leaders were

[1] Honigmann: *Geschichtliche Entwicklung der Medizin*, pp. 66, 67.
[2] Ibid., p. 66.

quite incapable of understanding the subtleties of German philosophy, notably the relationship obtaining between Mind and Nature, they simply abandoned Nature altogether. Matter, which with Schelling was the product or expression of infinite Mind, became for Mrs. Eddy simply an "error" of finite, mortal mind. She viewed both the body and its ills as essentially unreal. This doctrine promised much to the sick, and at the same time put no great tax upon their understanding. Hence it was well adapted to secure what the transcendentalists—the original American idealists—could never hope to attain; that is, a large popular following.

The occasional flirtations between Christian Scientists and transcendentalists, between Mary Baker Eddy and such leaders as Alcott and Emerson, were natural enough and not entirely devoid of significance.[1] For these contemporary New Englanders had all been inspired, to some degree, by the same German romanticism; albeit the more intellectual group had borrowed directly from the best sources, while Christian Scientists had been influenced only by mystical phases of romantic medicine. The latter were, philosophically speaking, the "poor relations" of true transcendentalists.

In Germany itself the general speculative trend did not necessarily demoralize ordinary medical practice. Certain medical leaders continued to display a "common sense" opposition to cults and theories. Christian Hufeland (1762–1836) is an example of the practically minded physician who opposed such movements as Brunonianism (which naturally continued to flourish in Germany) and such new romantic cults as Mesmerism and phrenology. No doubt, too, many an unimaginative but faithful *arzt* continued to minister to his patients in an empirical manner.

To decide whether German romantic medicine should be interpreted as reversion, or whether it should be viewed as a continuation of speculative trends never broken, would require a more careful analysis than is here possible. Certainly it seemed reactionary in comparison with the science of the Western world as a whole. This is not to say that the latter was entirely uninfluenced by idealism of one sort or another. In the United States, for example, the French Revolution was followed by a sweeping religious revival—the later "Great Awakening"—which almost smothered the rationalism of Jefferson's generation. The enthusiasm of the masses affected the seats of learning as well as the frontier.

[1] This rather entertaining story is well told in E. S. Bates and J. V. Dittemore: *Mary Baker Eddy: The Truth and the Tradition* (N.Y., 1932).

Rationalism survived in the unitarian movement, but orthodoxy became the dominant force, and even pervaded the ranks of physical scientists.[1] The professor of "natural philosophy" in 1830 was a much more religious person than had been the *philosophe* of 1770; a difference which is symbolized by the contrast between the orthodox Silliman of New Haven and the sceptical Franklin of Philadelphia.

This does not necessarily mean that physical science was distorted by theology. Teleological interpretations encouraged grandiose theorizing at times, but at the worst they do not seem to have led research men far astray. Silliman is usually credited with having done as much to advance natural science in America as did the critical Philadelphians who were his original tutors.

It is possible that American theology, which was largely of British origin, was a less heady influence than was German idealism. It is also probable that the medical sciences were less responsive to either of these than were the physical disciplines. Astronomy revealed a grandeur in Nature that lent itself readily to religious interpretations; while such a study as pathology revealed aspects of Nature which were hardly so edifying. No one can blame the pathologists if they did not care to think of what *they* found in teleological terms. And pathology was the medical science *par excellence* in this period; in the Western nations it determined the whole tone of the medical world.

Hence when theologians and philosophers appealed to French- and English-speaking peoples, the medical men were largely unmoved. Or, to put it conservatively, those who were doing original work were not moved sufficiently to affect the character of their investigations. When such apostles of German thought as Coleridge, Carlyle, and Emerson sought to convert the English-speaking peoples, it was largely the religious leaders or the artistically minded who responded. Medical workers proved indifferent, or regarded the idealists with cold and quizzical eyes. Such men as Skoda and Magendie stand out, in retrospect, as quite cold-blooded and hard against the background of a sentimental age. Poets might lament the pathos of early death, but to French and Austrian clinicians—as to the doctor of *Reading Gaol*—death was but a scientific fact.

When medical men really waxed romantic, as did John Keats or Thomas Holly Chivers—the poetic shadow of Edgar Allan Poe—they were usually ones who did not happen to work at their profession. On

[1] Howard Mumford Jones: "European Ideas in Nineteenth-Century America", *American Literature*, VII (Durham, N. C., 1935), 255.

the other hand, in such a rare case as that of Oliver Wendell Holmes, who was at once an interesting physician and a popular poet, scientific objectivity does not seem to have been compromised by artistic enthusiasms. Although a Bostonian of the best days, he displayed little interest in transcendentalism or in the other romantic isms of his native New England.

If a prominent physician did indulge in sentimentalism or speculation he was likely to be brought up short by critical colleagues. An interesting example is found in the case of the American obstetrician, Charles D. Meigs; the same who denied, during a famous debate with Holmes, the merits of aseptic procedure. Meigs wrote of his patients as "the dear little ladies" (a nice Victorian touch); and the preface to his popular textbook was positively gushing in its regard for "genteel females" in general. He relished the speculative as well as the sentimental, and indulged the former taste to the limit in his book on *Certain Diseases of Young Children.* No sooner had this appeared than it was formally condemned by a committee of the American Medical Association, on the ground that it dealt "over-much with attractive, almost transcendental abstractions, or theories which are the plausible refinements of erudition swayed by fancy".[1]

[1] A. M. A. *Trans.* (Phila., 1851), p. 485.

VII

MEDICINE AND "THE BASIC SCIENCES"

THE INDEPENDENCE of the medical sciences from a prevailing romantic mode was a fact of cardinal importance. For it enabled medicine to benefit by the various social developments already noted, and thus to proceed all the more rapidly along the path of progress first laid down during the two preceding centuries. What this meant for medicine between 1800 and 1850 may be made clear by first reviewing the history of the natural sciences, to which medicine stood, as ever, in a mutual cause and effect relationship.

This relationship was of a relatively intimate character. There had always been, of course, outstanding men in the physical sciences who had little or no contact with medicine; yet a surprising number of those who contributed to physics, chemistry, and biology during the nineteenth century were medical men. To name but a few, Helmholtz, Du Bois-Reymond, and the Webers in physics, Davy, Wöhler, and Berzelius in chemistry, Johannes Müller, Huxley, and Haeckel in biology, and Fechner, Wundt, and Binet in psychology—all were physicians, or at least received primary medical training.[1] Such training, which had been common among scientists during the preceding centuries, was not so usual after 1800; but it still served as one of the chief preparations for a career in any scientific field. And research in biology and medicine continued to contribute to the physical sciences, though hardly to the same degree that the latter now aided medicine. The manner in which Galvani's experiments in muscular contraction opened up the way to the study of current electricity is a notable example. When the undoubted dependence of medicine upon the "basic sciences" is considered, it should not be forgotten that the latter were themselves long indebted to medicine, or at least to the medical profession.

[1] A list of such names is given by F. H. Garrison: "What Medicine Owes to Science and What Science Owes to Medicine" (reprint of lecture given before the Mayo Foundation, 1928); and also in the earlier paper of W. H. Welch: "The Interdependence of the Medical and Other Sciences", *Papers and Addresses*, III (Baltimore, 1920), 315 ff. Sarton feels that too much has been claimed for medicine in this connection; see his editorial "The History of Science Versus the History of Medicine", *Isis*, XXIII (1935), 313 ff.; and the reply thereto by Sigerist in the *Bull. of the Inst. of the Hist. of Med.* of Johns Hopkins Univ., IV (1936), No. 1.

The history of physics during the eighteenth century presents some analogies with that of medicine. Progress was not so rapid after 1700 as it had been during the preceding period; it is even possible to say that speculation revived to some extent and was less restrained by experiment. The process never went so far in physics, however, as it did in medicine—no doubt because of the success of mathematical procedures in the former field—and so can hardly be viewed as a serious reaction. Little progress was made in mechanics; but, then, mechanics had been developed to a relatively high level before 1700. In the field of light Newton's corpuscular theory was generally adopted, and proved a retarding influence; but Euler (1707–1783) supported Huygen's wave theory and explained colour diversity in terms of the duration of vibrations. Euler suggested that "the different media of the eye had the property of preventing the dispersion of colours, and suggested that lenses be prepared out of two different substances, with the view of removing chromatic defects". This aroused the interest of Klingenstierna, of Upsala, who experimented on achromatism, and who cooperated with John Dollond, a London optician, when the latter finally succeeded in making achromatic telescope objectives (1758).[1]

Since the removal of colour effects had never been effectively achieved before, the development of Dollond's achromatic telescope was of great service to astronomy. There was, obviously, a need for an analogous type of compound microscope, but here technical difficulties long delayed the making of successful instruments. After more powerful telescopes had made possible the work of William Herschell (1738–1822) in a further plumbing of the macrocosm, the first achromatic microscopes were finally constructed, and soon made possible renewed investigations of the microcosm. Once more, as in the days of Kircher and Leeuwenhoek, men peering through lenses were to see what human eyes had never seen before.

No exact date for the "invention" of this modern type of microscope can be set, since the work of various lens-makers in Paris, Berlin, and other cities was involved; but the new instruments became generally available between 1830 and 1840.[2] This was also the period when studies in light and lenses made possible the first photography (1827)—a pro-

[1] Cajori: *A History of Physics*, pp. 102, 103; W. T. Sedgwick and H. W. Tyler: *A Short History of Science* (N.Y., 1917), p. 313.

[2] Charles Chevalier: *Des Microscopes et De Leur Usage*, etc. (Paris, 1839), pp. 13 ff., 199 ff. On the long history of the microscope, see Charles Singer: "Steps Leading to the Invention of the First Optical Apparatus", *Studies in the History and Method of Science*, II, 385 ff.; S. H. Gage: *The Microscope* (Ithaca, N. Y., 1932), pp. 521 ff.

cess not without potential significance to medicine. It may be observed, in passing, that more refined anatomical research probably led to a demand for better instruments of observation; somewhat as improved travel arrangements led, about the same time, to a demand for the further advantages of the railway and the locomotive. For this reason, microscopy was probably indebted to medicine, even as medicine was to be indebted to microscopy.

Studies in the theory of heat went astray, during the eighteenth century, much as did those relating to the nature of light. The earlier view that heat was due to molecular motion was superseded by the theory that it consisted of material particles which were given off by hot bodies and absorbed by cold ones. The corpuscular hypothesis, however, did not prevent continued improvements in the measurement of heat; and this branch of physics benefited by the development of quantitative methods, somewhat as had dynamics more than a hundred years before. As early as 1659 Boullian had modified Galilei's air thermometer by introducing mercury in a glass tube; but the major improvements in these instruments came after 1700. Gabriel D. Fahrenheit (1686–1736), originally of Danzig, first brought about the general use of mercury thermometers; and at the same time he, Réaumur, Celsius, and others experimented with different scales of measurement. As many as thirteen of these were in use about 1740, but only the three bearing the names just mentioned survive into our own day. That which eventually came into general scientific use—one of several "centigrade" scales—was developed about 1742 by Celsius and Strömer of Upsala. Since the boiling point of water was taken as a fixed point in this and most other thermometers, the discovery that this point varied with atmospheric pressure contributed greatly to exact measurements.[1]

The use of reliable thermometers enabled Black of Edinburgh, during the latter part of the eighteenth century, to note the "latent heat" involved in the melting and boiling of water, and also to measure the "specific heat" of different substances. His work led on the one hand to Watt's practical improvements in the steam engine, and on the other hand to Count Rumford's revival of the theory of molecular motion. From this it was possible to proceed, by a series of researches between 1800 and 1850, into the whole field of thermodynamics. Of chief significance here is the fact that after 1750 medical men had available (whenever they could be persuaded to use it) an instrument well adapted to

[1] Cajori: *A History of Physics*, pp. 105 ff.

measuring the heat of the human body as well as that of inanimate objects.

The most striking advances in physics during the eighteenth century were made in the relatively new field of electricity and magnetism. While an Englishman, William Gilbert, had published (1600) the first important study in this field, he was largely ignored by contemporary scholastics, and little was added to his work during the next hundred years. Shortly after 1700 friction machines were developed for producing static electricity, which was thus made available for systematic study. Attempts to electrify water in a bottle resulted in the invention (1746) of the "Leyden jar". This made it relatively easy for almost anyone to experiment with the fascinating "electrical fire", and such experiments increased the interest which educated people were taking in science as a whole during this age. By about 1750 Benjamin Franklin had published his theory of the material nature of electricity—analogous to contemporary theories about the corpuscular character of light and heat—and had demonstrated the identity of lightning and electricity. He also aroused interest in the possible utility of studies in electricity, when he suggested the construction of the first lightning rods, and even wrote of electrical cooking.

Meanwhile Henry Cavendish of London, and Coulomb of Paris, were taking the first steps towards the exact measurement of static electricity. The former measured in terms of "inches of electricity"; that is, in terms of the diameter of a sphere of equivalent capacity whose charge was known.[1] Thus quantitative procedure, limited largely to mechanics prior to 1750, was developed simultaneously thereafter in studies of both heat and electricity.

Of immediate interest to medicine were the early researches in current electricity associated with the labours of two brilliant Italians, Galvani and Volta, at the end of the eighteenth century. Galvani was professor of anatomy at Bologna, and became interested in "animal magnetism" as a result of experiments with the muscles and nerves of a frog's leg. When a bent metal rod touched muscle and nerve at the same time, convulsions ensued. Galvani decided that the rod played the role of a conductor for a charge inherent in the nerve; but Volta showed that the current originated in the rod, and was due to a difference of temperature at its two ends. More powerful currents were produced by the contact of two different metals. This observation led to

[1] Cajori, op. cit., p. 129; Henry Crew, *The Rise of Modern Physics*, 2 ed. (Baltimore, 1935), pp. 175 ff.

the construction of the first voltaic cell.[1] By 1810 there was a widespread interest in current electricity, and a demand arose that "galvanism" be employed in some way in medical practice. Early attempts to do so led to uncertain results, and regular physicians tended to ignore the possibilities of electrotherapy; but popular hopes were thereupon exploited by the various cults and quackeries already mentioned.[2] It has also been observed that "polarity" and other electrical and magnetic phenomena played a leading role in the speculations of the *Naturphilosophie* school.

Studies in current electricity proved of immediate value in chemistry, since they opened up the field of electrolysis. Sir Humphry Davy, in 1810, discovered sodium and potassium by the aid of this process. Such were the possibilities here that much effort was expended between 1800 and 1850 on the development of improved voltaic cells. The first "storage battery" was invented in 1803, but this type of apparatus did not attain to commercial value until after 1880. The relation of currents to magnetism was investigated, after 1819, by Oersted of Copenhagen; and the field of electromagnetism was rapidly advanced between then and about 1840 by the great researches of Ampère, Ohm, Faraday, and Henry. As early as 1829 Henry had assembled at Albany, New York, the apparatus essential to the electric telegraph, but later turned his interest into other channels. Further work in electromagnetism led, during the next two decades, to the successful development of this instrument. Thus physics added the telegraph to the steam engine, in its contributions to modern civilization.

Chemistry hardly differentiated itself from physics on the one hand, and from medicine on the other, prior to 1775. Like the other sciences, chemistry seemed to lag during the first half of the eighteenth century. There was a general neglect of quantitative methods, and speculation continued in consequence. The whole subject was dominated by Stahl's theory of phlogiston, which was supposed to be a fire-substance which escaped from burning material. The analogy with contemporary theories re heat, light, and electricity is obvious enough. All were looked upon as particles rather than as processes—as forms of matter, rather than of energy. While the ultimate nature or relation of matter and energy is still a debatable question, the existence of a fire-material could be and was shortly disproved. So long as it was postulated, the

[1] George Sarton: "The Discovery of the Electric Cell (1800)", *Isis*, XV (1931), 124 ff.
[2] See also W. J. Turrel: "Three Electro-Therapists of the Eighteenth Century, John Wesley, Jean Paul Marat, and James Graham", *Ann. Med. Hist.*, III (1921), 361 ff.

real nature of combustion, and therefore of the whole subject of chemical combinations, was hidden from view.

Alchemy, however, was definitely discredited after 1700, and for the same general reason that astrology had suffered a similar fate. There was a genuine desire to discover the natural laws relating to the chemistry of all substances, and when quantitative work began to be employed, progress was made in this direction. Joseph Black, the Scottish physician whose work on the measurement of heat has been mentioned, employed similar methods in studying the difference between the mild and caustic alkalis (1756). How elementary a science chemistry was at this time may be realized when it is recalled that potash and soda were still confused as one substance, and as an elementary substance at that. A similar confusion of different gases as so many "kinds of air" marked the able work of Joseph Priestley, who actually discovered (1768–1774) not only oxygen, but also ammonia, nitric oxide, nitrous oxide, and several other distinct gases. Priestley failed to advance beyond Boyle and other seventeenth-century workers, in distinguishing various compound or elementary subjects; nor did he attain to an understanding of the nature of chemical combination as involved in combustion.

This understanding was established by Lavoisier, and it is not without significance that he and Black were among the first systematically to employ quantitative methods in chemistry. By an unremitting use of the balance, Lavoisier demonstrated that metals burnt in air gained in weight and therefore did not lose any mystical phlogiston. By further weighings, he found that the air lost, during combustion, just the amount which the metals gained; while when a burnt metal (oxide) was decomposed, it lost in weight what the air in this case regained. Following a similar procedure, Lavoisier then burned charcoal in oxygen, and secured "fixed air" or carbonic acid, which he analyzed quantitatively into seventy-two parts of oxygen and twenty-eight of carbon.[1] The way was thus opened to a study of the composition of chemical compounds; and in consequence new viewpoints and terminology were adopted and various materials were analyzed for the first time. Certain known substances, which resisted all attempts at analysis, were therefore viewed as final elements.

About two years after publishing his paper on the calcination of

[1] Sedgwick and Tyler, op. cit., p. 309; Sir William A. Tilden: *Famous Chemists* (Lon., 1921), pp. 69 ff. On the hitherto little-known work of the Russian chemist, M. W. Lomonosoff (1711–1765), who anticipated Lavoisier in some ways, see J. M. Stillman: *The Story of Early Chemistry* (N.Y., 1924), pp. 511–13. Note also such recent, interesting accounts as those in Bernard Jaffe: *Crucibles* (N.Y., 1934); and W. R. Aykroyd: *Three Philosophers* (Lon., 1935).

metals (1775), Lavoisier noted that expired air was similar to the air used in burning charcoal. It had lost oxygen, but contained "fixed air" or carbonic acid gas. He concluded that in this case the oxygen had combined with the blood, just as it had with the charcoal in earlier experiments. Perhaps it was a similar combination with oxygen, which lent the bright red colour already known to result when blood came into contact with air. (Did not oxygen often produce a red tone in its combinations with other elements; for instance, with iron or mercury?) It looked as though respiration products could be explained in terms of combustion in the body, and this led to an attempt to solve the problem of body heat in these terms. Working still in a mathematical manner, and co-operating now with Laplace, the chemist proceeded to measure the amount of carbonic acid given out by an animal in a given time, and also the amount of heat produced by it in this period. He then demonstrated (1780) that this amount of heat was identical with that given out by a quantity of burning carbon sufficient to produce the same amount of carbonic acid as did the animal noted. In other words, a respiring animal reacted chemically with the air exactly as did burning carbon. Respiration evidently involved a slow combustion similar to that observed in charcoal.[1] An essential physiological process was at last explained along chemical lines, and further progress in this direction was to be expected in the near future.

Although Lavoisier was executed during the Terror of the French Revolution, recognition had been accorded his genius before that time. Cuvier declared, a few years later, that it was in the work of Lavoisier that Europe "first beheld with astonishment the whole system of modern chemistry". A prime factor making this system possible, added Cuvier, was the introduction—not without some opposition—of the mathematical spirit.[2]

Lavoisier himself prepared a table of thirty-three elements, of which twenty-three are still regarded as such. It already seemed incredible that men had accepted, not so long before, the classical thesis of a world composed simply of earth, air, fire, and water! Other elements were now discovered rapidly by such leading chemists as Berzelius of Stockholm (noted for his careful analyses) [3] and by Sir Humphry Davy (in

[1] Sir Michael Foster: *Lectures on the History of Physiology* (Cambridge, 1924), pp. 242 ff.

[2] Cuvier: *Rapport historiques sur les Progrès des Sciences naturelles depuis 1789* (Paris, 1810), p. 90. For Lavoisier's general career see Mary L. Foster: *Life of Lavoisier* (Northampton, Mass., 1926), pp. 3 ff.

[3] An account of the work and career of Berzelius is included by Victor Robinson in his *Pathfinders of Medicine* (2 ed., N. Y., 1929), pp. 285 ff. Interesting are Berzelius's *Autobiographical Notes*, transl. by Olof Larsell for the History of Science Society (1934).

the course of his studies in electrolysis). By 1830 the number known had grown to over fifty. Meanwhile John Dalton had revived the ancient atomic theory, and had shown that it offered an excellent explanation of the observed facts of chemical combination. This theory was put in mathematical form; and while the formulæ expressing it were at times erroneous because of an ignorance of molecular structure—the formula for water, for instance, was sometimes written H + O = HO —the theory made potentially for a much more exact description of chemical phenomena.[1] This fact in itself encouraged further research and facilitated additional discoveries.

Three factors may therefore be said to have modernized chemistry by about 1820: first, the repudiation of alchemy, and the consequent effort to observe and differentiate ordinary materials; second, the advent of a quantitative analysis of compounds, plus, in this connection, the discovery of apparently irreducible elements; and, third, the theoretical expression of these results in approximately mathematical form. The modernization of chemistry was also indicated, as had been the case with physics, by utilitarian as well as by purely intellectual triumphs. While the chief products of "creative chemistry" are of relatively recent date, it is significant that the early experiments with "airs" resulted in the commercial manufacture of illuminating gas, by an English firm, before 1800.

The revolution in chemistry proved stimulating to the related medical sciences. Contacts were close, because so many physicians were still leaders in chemical research. A notable example is afforded by the case of Sir Humphry Davy. After medical training with a preceptor, young Davy worked under Dr. Beddoes at the Bristol Pneumatic Institute. Attempts were being made there to investigate the possible medicinal effects of the new gases. Davy experimented with the inhalation of nitrogen, nitrous oxide, and other materials. He wrote that the use of nitrous oxide "absolutely intoxicated me", and suggested (1800) that "it may probably be used to advantage in surgical operations. . . ." Shortly thereafter he was persuaded by Count Rumford to devote himself to systematic chemical research in the latter's Royal Philosophical Institution in London. From that time on he devoted himself primarily to chemistry rather than to medicine.[2]

The suggestion that gases be employed as anæsthetics was not entirely forgotten. Ether was used to relieve asthma, and Davy's assist-

[1] John Johnston: "Chemistry", in L. L. Woodruff (Ed.): *The Development of the Sciences* (New Haven, 1923), pp. 82 ff.
[2] Tilden: *Famous Chemists*, pp. 86, 87.

ant, Faraday, demonstrated that it also produced insensibility to pain (1818).[1] Scholarly observations on the effects of these gases appeared occasionally during the next two decades, and some popular knowledge of them was spread by itinerant lecturers. Public demonstration of ether intoxication proved "both entertaining and instructive"—it was amusing to see men tumble against tables without sensing any pain. Such demonstrations afforded a hint that later played its part in the introduction of modern surgical anæsthesia.

While Davy's suggestion of a gaseous anæsthetic received little attention from most medical men, this did not mean that the latter were uninterested in the new chemistry. Of peculiar concern to them was the advent of organic chemistry, and the special development of biochemistry. Lavoisier, Berzelius, Gay-Lussac, and others had been interested in the analysis of carbon compounds, and had made some beginning in that direction by burning them in oxygen and measuring the products of combustion. Their early apparatus and procedure, however, left much to be desired; and it remained for two Germans, Friedrich Wöhler and Justus Liebig, to make possible a systematic and relatively exact analysis of highly complex organic materials.

Both Wöhler and Liebig, as young men, found that the Germany of the *Naturphilosophie* could not afford adequate instruction in the natural sciences. They accordingly went abroad for training. The former studied under Berzelius in Stockholm, and the latter under Gay-Lussac in Paris. Returning to the Fatherland, Liebig established at Giessen (1824) the first systematic laboratory instruction in any of the sciences (excepting anatomy), and so organized what became the most famous school of chemistry in the world. Here he developed an improved technique for analyzing organic compounds, and in collaboration with Wöhler proceeded to investigate the chemistry of metabolism, respiration, fermentation, and animal heat.[2]

Wöhler, in 1828, was able to produce urea by chemical means independently of animal life; and while the full significance of this discovery was not at once apparent, it eventually encouraged the view that all life processes are chemical processes, differing only in complexity from other chemical phenomena. It appeared as if no final distinctions could be made between the "organic" and the "inorganic", although the terms continued to be used in chemistry as a matter of convenience. Chemists began to view the animal body not so much as a machine but

[1] Francis Packard: *History of Medicine in the United States*, II, 1075, 1076.

[2] Dampier-Whetham: *History of Science*, pp. 270 ff.; Eduard Färber: *Geschichtliche Entwicklung der Chemie*, pp. 98–100; Tilden: *Famous Chemists*, pp. 194 ff.

rather as a laboratory, and so thought of physiology as the study of the chemical reactions that went on therein. While Wöhler and Liebig did not go all the way to this extreme view, evidence supplied by other workers encouraged it. An American army surgeon, William Beaumont, published in 1833 his remarkable observations (made possible by a case in which a permanent opening existed in the walls of the stomach) proving that gastric digestion was truly a chemical process.[1] And about the middle of the century the biochemical point of view came to full fruition in the brilliant investigations of Claude Bernard. Shades of Paracelsus, van Helmont, and Sylvius—a new iatrochemistry was at hand!

Of less interest to medicine than the progress of chemistry, but equally illustrative of the trends of the time, was the development of geology during the fifty years between 1780 and 1830. This science emerged from the controversies of rival "systems", which had emphasized respectively the part played by volcanic action and by sedimentary deposits in the formation of rock structures. Geologists came to realize, somewhat as pathologists did during the same period, that no one explanation would suffice for all phenomena.

Some rocks were evidently of an igneous nature, but many others displayed strata which pointed clearly to a sedimentary origin. In the course of studying the latter, increasing attention was paid to fossil remains found therein. The modern view of fossils had been suggested by a number of early thinkers, but it received scant attention until the naturalism of the eighteenth century made men more receptive to historical interpretations. As the Biblical chronology was abandoned, geologists came to see in rock strata and their fossils a clue to an entirely new and enormously extended history of the earth. This interested biologists, who pondered the relationship between fossils and living species. The first systematic comparisons along this line were made between 1790 and 1810, by Cuvier and Lamarck. The publication of Sir Charles Lyell's *Principles of Geology* (1830–1833) well expressed the newer interests and points of view.

At the same time, mineralogy was differentiated from geology on the basis of Häuy's studies in forms of crystallization. These afforded a system of classification, analogous to that which Linnæus had built up for botany, Cuvier for zoology, and which Cullen and others had attempted to provide in nosography. It has already been observed that such a classifying stage was a necessary prologue, in the biological

[1] Francis Packard: *History of Medicine in the United States*, II, 1058 ff.

sciences, to the main performance that would come after the middle of the nineteenth century. New species were rapidly being found, and had to be related to a general scheme of things. Linnæus had recognized but 8,000 plant species, while Cuvier knew some 50,000 by 1824.[1]

It is obvious that the more one knew about an increasing number of biological species, the more insistent became the question of the origin of these species. Why such diversity, and at the same time such apparent relationships? Philosophers had long pondered the possibility of evolution. Now, as data suggesting this accumulated in such diverse fields as embryology, geology, and stock-breeding, the problem was at last approached from an inductive angle. Erasmus Darwin, Goethe and others felt that there was now real evidence supporting this ancient theory, though they offered no satisfactory explanation of the process by which evolution had been carried on. Lamarck, early in the century, proposed his thesis that species were modified by the inheritance of acquired characteristics—a view that nas never been entirely abandoned. William Charles Wells, an English–American physician, presented to the Royal Society in 1808 the theory of inherent variations and the survival of the fittest of these in the struggle for existence. Here was the view which eventually received general acceptance. Other naturalists, in increasing number, expressed the conviction that there must have been an evolution rather than an independent creation of related forms. Charles Darwin later noted the work of no fewer than thirty-four such authorities who wrote between 1794 and 1859.[2]

In the latter year there appeared the first edition of Darwin's *Origin of Species*, as well as Alfred Russel Wallace's essay on the same subject. The two Englishmen, after observing both fossil and living forms in distant parts of the world, had come independently to the conclusion that biological evolution had resulted from inherent variations (mutations) and the subsequent "survival of the fittest" in the course of the struggle for existence. They marshalled the cumulative evidence and reasoned so effectively that many outstanding scientists were soon convinced of the soundness of their view. Sir Charles Lyell found that it fitted effectively into the pattern of his geological studies; while Herbert Spencer—who had already written his social and psychological

[1] Merz: *History of European Thought in the Nineteenth Century*, I, 119.

[2] The historical introduction to the third and later editions of Darwin's *Origin of Species* lists his precursors. On Wells, who seems to have been the first to advance the actual Darwinian hypothesis, see R. H. Shryock: "William Charles Wells", *Dict. Amer. Biog.*; and "The Strange Case of Wells' Theory of Natural Selection: Some Comments on the Dissemination of Scientific Ideas", in a *Festschrift* (M. F. Ashley Montagu, Ed.) soon to be published in honour of George Sarton.

works around the concept of evolution—naturally welcomed this apparent confirmation of the theory in the field of zoology. There ensued some opposition from conservative biologists; and there was also a reaction against the thesis of evolution on the part of many religious leaders. The latter in turn aroused popular opposition, on the grounds that the theory conflicted with Biblical tradition and that it was generally degrading to trace man's origin to animal forms.

The genealogical implications here were hardly flattering, and the fear of many clergymen that the theory of evolution would have far-reaching implications was well founded. While it had little direct significance for medical practice, thoughtful physicians realized that Darwin's work was destined in the long run to prove a stimulating influence in all the biological sciences.[1] One medical man, Huxley, led the ensuing battle for "Darwinism" in England; while another, Ernst Haeckel, performed a like service in Germany. No one who has read either of these men will forget the fervour and ability which they devoted to this purpose. Biology became, for the first time, a subject of absorbing interest to thousands of educated people who had no technical concern with it whatsoever.[2] For the first time, the sciences of life loomed even larger than the physical sciences on the intellectual horizon.

[1] An example of direct influence, however, may be seen in the application of the "struggle for existence" idea to a concept of disease which emphasized the role of the invaded human host in resisting infection. This counteracted the early emphasis on the role of invading organisms. See George Rosen: "Disease and Social Criticism", *Bull. of the Hist. of Med.*, X (June 1941), 10.

[2] The story is recalled in Leonard Huxley: *Life and Letters of Thomas Huxley*, I (N.Y., and Lon., 1909) pp. 188 ff.

VIII

MEDICINE, MATHEMATICS, AND THE SOCIAL SCIENCES

LESS DRAMATIC than the development of biology, but certainly no less significant, was the history of mathematics during the century between 1750 and 1850. The early iatromathematicians, it may be recalled, had been interested in astrology, number mysticism, and the like;[1] while the physiologists of the seventeenth century had in contrast applied simple arithmetic in a strictly rational manner. These later iatromathematicians had often failed because they could not secure necessary medical data, and also because they lacked mathematical techniques which would have been essential to the handling of these data even if they could have been found. Both desiderata were now in part supplied by the development of statistics and of the calculus of probabilities.

Systematic vital records were preserved in a number of European towns from the sixteenth century on. During the ensuing era these records were sometimes published. In London, for example, the first publication of "bills of mortality" seems to have been occasioned by the fear of plague epidemics. It is said that opulent residents, by consulting the rise and fall in the death rate, "knew when to leave town". After 1657 bills noting the causes of death were issued regularly, and thus made possible a pioneer statistical analysis by Graunt (1662). Weekly lists which noted age and sex groupings were issued after 1728.[2] With records of this sort available in certain cities, it was obvious that they could be consulted in efforts to study the course of particular diseases, as well as that of disease in general.

Of all epidemic diseases, that which inspired the greatest alarm at the time was smallpox. Figures on this were watched with peculiar con-

[1] David Eugene Smith: "Medicine and Mathematics in the Sixteenth Century", *Ann. of Med. Hist.*, I (1917), 125 ff.

[2] The early work of Graunt, and Sir William Petty's studies in "political arithmetic" are described in A. Wolf: *A History of Science, Technology, and Philosophy*, pp. 588 ff. See also John Brownlee: "The Health of London in the Eighteenth Century", Roy. Med. Soc. *Proceeds.*, XVIII, Pt. 2, pp. 73 ff.

cern. Then, between 1718 and 1722, Lady Montagu and others began to advocate inoculation; and it became a matter of vital interest to compare the relative risk involved in natural cases and in those due to the new process. One of the first to make use of a statistical comparison in the interest of preventive medicine was the American clergyman, Cotton Mather. He reported to the Royal Society, during the severe Boston epidemic of 1721, that more than one in six of all who took the disease in the natural fashion died; but that out of 300 inoculated, only about one in sixty died. Dr. Jurin, of the Royal Society, taking the more extensive figures for London (from 1667 to 1721), found that more than one out of six natural cases proved fatal, but only one in ninety-one of the inoculated cases. The value of inoculation was made more apparent by his added calculation that for all persons above the age of two years, more than one out of every nine would, in the natural course of events, eventually die of smallpox alone! [1]

Such calculations were crude and by no means complicated. Yet, for the first time, a mathematical procedure involving something more than ordinary measurement was employed here in the service of medicine. And technical refinements were gradually evolved which made such analyses of increasing significance. It will be recalled that Pascal and others had become interested—through the medium of games of chance —in calculating probable outcomes in terms of a great number of cases. Mathematicians in nearly all European nations became greatly interested in the resulting "calculus of probabilities"; and after about 1760 the relative risk involved in natural and artificial smallpox afforded an excellent problem for those who hoped to improve this calculus.[2] Daniel Bernouilli, for example, published at Paris (1766) his essay on a new analysis of smallpox mortality. He calculated the risk of death and the life expectancy at different ages, in both the natural and in the induced disease. D'Alembert, who was sceptical of the calculus, also wrote on its application to smallpox, calling attention to the fact that the individual's interest was by no means identical with the interest of the statistical average.[3]

The rather rapid introduction of vaccination after 1800 put an end to

[1] James Jurin: "A Letter . . . Containing a Comparison between the Danger of the Natural Small Pox and that given by Inoculation", *Phil. Trans.*, XXXII, 213 ff. (No. 374, 1722). See, in connection with Mather's observations, George Lyman Kittredge: *Cotton Mather's Scientific Communications to the Royal Society* (Worcester, Mass., 1916), pp. 3 ff. (Reprint from the *Proceeds.*, of the Amer. Antiq. Soc., for April 1916).

[2] See J. F. Montucla: *Histoire des Mathématiques* (3 ed., Paris, 1820), p. 426; and especially, I. Todhunter: *History of Probabilities* (Lon., 1865), pp. 57 ff., 351 ff., 464 ff.

[3] E. Netto: "Kombinatorik, Wahrscheinlichkeitsrechnung", u.s.w., in *Vorlesungen über Geschichte der Mathematik*, herausgegeben von Moritz Cantor, IV (Leipzig, 1908), 221 ff.

statistical studies of inoculation. But general interest in statistics had already acquired a considerable momentum some time before that date.[1] A German authority, Süssmilch, showed how they might be employed to study the health of an entire population, not only with relation to smallpox but to disease in general. And in 1749 the Swedish Academy of Science persuaded its Government to take the first modern national census, which was developed during the century to include vital as well as other statistics. At about the same time a national council on the public health was established in Stockholm. Sweden was, in this respect, far ahead of other nations. William Farr reported, as late as 1838, that it was still the only country which possessed an efficient registration of births and deaths.[2]

There were doubtless scholars in other European lands who realized the potential value of national statistics. Gregory King, following Graunt and Petty, had published a general work on English demography in 1696; but much was left in these pioneer studies to ingenious estimates. What was obviously needed was a more systematic collection of figures. Dr. Thomas Percival appealed for a national census in England about 1773, but was ignored by the authorities. Towards the end of the century individual demographers attempted statistical surveys of both local and national areas, with the hope of revealing hitherto unrecognized conditions of economic, social, and medical significance. In Great Britain alone three important works of this sort were published between 1791 and 1797, which anticipated in some ways the more systematic health surveys directed by Chadwick after 1840.[3]

Local statistics were taken for granted long before this in cities of any size. How long would it be before it occurred to someone that such records might throw light upon the treatment, as well as upon the natural history of disease? Among the first to be granted such insight was—of all persons—one William Cobbett, an English pamphleteer and politician who was possessed of remarkable shrewdness along certain lines. This man was present in Philadelphia in 1793, when Benjamin Rush

[1] On the general history of statistics, see V. John: *Geschichte der Statistik* (Stuttgart, 1884), pp. 98 ff., 274 ff.

[2] "Vital Statistics of Sweden", *British Medical Almanack* (Lon., 1838), p. 216. See also Edward Arosenius: "The History and Organization of Swedish Official Statistics", in John Koren (Ed.): *History of Statistics* (N.Y., 1918), pp. 537 ff.; Otto E. A. Hjelt: *Svenska och Finska Medicinal Verkets Historia, 1663–1812*, I (Helsingfors, 1891), 339 ff.

[3] These pioneer works were Sinclair's *Statistical View of Scotland* (1791); Patrick Calquhon's *Treatise on the Police of the Metropolis* (1795); and Sir Frederick Eden's *The State of the Poor*, etc. (1797). Some demographic data were also available in the U.S.A. by this time. That available for the period prior to the first national census (1790) is carefully described in Evarts B. Greene and Virginia D. Harrington: *American Population before the Federal Census of 1790* (N.Y., 1932).

announced his sure cure for yellow fever. The cure was hardly a novel one, consisting largely of bleeding and purging, and it was strongly opposed by some of the other physicians in the town. Cobbet also distrusted it; and as he disliked Rush and was a born controversialist, he proceeded—some time after the epidemic—to give the doctor the benefit of a layman's opinion. The famous cure, observed Cobbett kindly, was "one of those great discoveries which have contributed to the depopulation of the earth".

He then went on to inquire, where was the doctor's proof that his treatment was effective? Where were his records? Rush claimed that practically all his patients recovered when the treatment was promptly applied. But, like his colleagues of the time, he seldom kept complete figures on his cases. Even had he desired to do so, he was too busy to attempt it. Despite this fact, Cobbett held that Rush ought to have statistical records. Since none were available, the former went to the bills of mortality to prove that the doctor was killing his patients instead of curing them. Now, Rush had begun to apply his cure on September 12th, and on that date there were twenty-three deaths from yellow fever. The doctor treated as many cases as he could possibly see, adding more each day, and finally calling in non-medical helpers to go about applying the treatment to all who would accept it. It has been estimated that he himself visited "at least half the houses of the city".[1] Yet between September 12th and 15th the mortality rose from twenty-three to fifty-six per day. On the 24th it was ninety-six, and on October 11th no fewer than 119 died of the dread disease. There, claimed Cobbett, was mathematical evidence that Rush was killing his patients. The more his treatment was applied, the faster the poor people succumbed.[2] We might say, to-day, that there seemed a positive correlation between the increase in bleeding and the increase in mortality.

Cobbett's conclusions failed, of course, to make allowance for certain variables. He should have considered the work of other doctors, the lack of any treatment in many cases, and especially what would now be termed the normal curve of the epidemic. Yet there would seem to be some basis for his essential claim; that is, had Rush supplied a real cure, the death rate should not have risen so rapidly during just the period when this cure was being increasingly employed. Here, at any rate, was an appeal to statistical evidence against a particular therapeutic procedure—a rather unique appeal for the period. So unique was it,

[1] Nathan Goodman: *Benjamin Rush: Physician and Citizen* (Phila., 1934), p. 181.
[2] William Cobbett: *The Rush Light* (N.Y., Feb. 28, 1800), p. 49.

indeed, that it received small attention from either doctors or laymen. Cobbett was eventually convicted of slander, fined, and practically driven out of town. Yet he actually suggested the use of statistics in therapeutic research. What one man saw, in the heat of controversy, others would realize sooner or later in the course of calm investigation.

The abandonment of inoculation, meanwhile, did not in any way discourage the further development of the calculus of probabilities. Halley demonstrated that its application to bills of mortality afforded a basis for commercial life insurance.[1] Here and there, physicians like Thomas Percival and George Fordyce saw that the calculus might be employed in the study of other disease rates than those for smallpox. Pinel made some attempt to use it in his work on insanity.[2] Such instances, however, were rather sporadic, pending the time when mathematicians would improve this tool. Finally, in 1810, Laplace published his classic study, *Théorie Analytique des Probabilités*, in which he not only perfected the calculus, but called especial attention to the manner in which it could be employed in medical research.[3] This work seems to have aroused, for the first time, considerable general interest in the possible use of medical statistics.

"Men began to hear with surprise," declared the English astronomer, Herschell, "not unmingled with some vague hope of ultimate benefit, that not only births, deaths, and marriages, but . . . the comparative value of medical remedies, and different modes of treatment of disease . . . might come to be surveyed with that lynx-eyed scrutiny of a dispassionate analysis, which if not at once leading to the discovery of positive truth, would at least secure the detection and proscription of many mischievous and besetting fallacies." [4]

A demand for elementary and relatively popular expositions of the calculus followed the first editions of Laplace, and was supplied by the publication of Lacroix's work in French (Paris, 1822) and encyclopædic essays in English by Sir John Lubbock and others. Physicians as well as the laity stood in great need of such explanations of the possible value of statistical procedures. Indeed, as late as 1846 the great Belgian authority on probabilities, M. Quetelet, complained that medical data were generally "incomplete, incomparable, suspected, heaped up pell-

[1] See A. Wolf: *A History of Science*, etc., pp. 609 ff. Later observations on the commercial applications of the calculus will be found in Condorcet's *Esquisse D'un Tableau Historique*, etc.; and in A. de Morgan: *An Essay on Probabilities*, etc. (Lon., 1838), pp. xv ff.

[2] Cuvier: *Histoire des Progrès des Sciences Naturelles depuis 1789*, etc., I (Brussels, 1837), 154–157; Bartlett: *Philosophy of the Medical Sciences*, p. 303.

[3] *Théorie Analytique des Probabilités* (3 ed., Paris, 1920), pp. 420–4.

[4] *Edinb. Rev.*, XCII (1850), 12.

mell . . . and nearly always it is neglected to inquire whether the number of observations is sufficient to inspire confidence". But by that time, as Herschell observed in quoting this passage, there were already signs that a fundamental reform was under way.

The collection and improvement of statistics, and the application of the calculus thereto, were going on in many fields after 1820; and a glance may be taken at this general situation before describing further developments in the utilization of medical statistics in particular. The apparent success achieved with the calculus in studying the inoculation problem—that is, with its application to questions involving large numbers of men—suggested that social as well as physical phenomena might be investigated in quantitative terms. The use of the calculus in connection with life insurance pointed all the more clearly in the same direction. Was it not possible that, by virtue of such quantitative method, even social phenomena could at last be approached in an objective, scientific fashion, rather than in a purely philosophical manner?

Condorcet and other enthusiastic *philosophes* were convinced that the objective procedures of physical science might well be taken to heart by those interested in social phenomena. The success of physics, which had stimulated the medical sciences a century before, now began to exert a like influence upon the social sciences. The chief accomplishment of eighteenth-century thinkers in the latter field, to be sure, was not so much a positive science, as it was the rejection of superstition and theology. The social field had to be rescued from theologians like Bossuet, and from sentimentalists like Rousseau. The *philosophes* cultivated a critical attitude, which was expressed in its more aggressive form in the writings of Voltaire and of Thomas Paine (1737–1809).[1] In a word, they cleared the way for the beginnings of real social science—a service analogous to that performed by those who had earlier eliminated alchemy from chemistry, and astrology from medicine.

Once a relatively objective attitude had been established with relation to social phenomena, the problem of actual procedure in this field became a more pressing one. Since the use of quantitative methods had established a new physics and a new chemistry, it now seemed possible that their employment might also lead to a new social science—perhaps to a real "social physics". It was true that certain methods and devices associated with quantitative work in physics were not available to socio-

[1] F. H. Hankins: "Sociology", in Harry Elmer Barnes (Ed.): *The History and Prospects of the Social Sciences* (N.Y., 1925), p. 290. Another suggestive account of the general development of sociology, is R. M. MacIver: "Sociology", *Encyclop. of the Social Sciences*, XIV (N.Y., 1934), 232 ff. This, however, does not stress the history of method.

logists. One could rarely experiment with human groups—a difficulty analogous to that faced by medicine in dealing with human individuals —but one could at least observe. And statistics made possible relatively exact observation.

It was also true that there were no instrumental devices which could be used, like a microscope, to extend the range of observation in the social field. Yet were not statistics themselves an instrument of observation, revealing situations which no one had observed before? Statistics thus seemed destined to aid the social sciences, as both measurement and microscope had served the physical disciplines.

One other difficulty remained in the employment of statistics in social research. Each human being was an independent variable—again the same problem that medicine faced—and statistical results might not apply to any given individual. The answer was that statistics would allow for such variations, in that they would give results holding true for the majority, or "in the long run". The use of the calculus, in establishing insurance on a successful basis, had proved this to be true. It was conceivable that "social laws" could be discovered which, if not identical with the laws of physics, would at least be statistical laws holding true for net results.[1] And these laws could be studied and made to serve human needs.

Such views entered into the social optimism of Condorcet and his contemporaries. Similar ideas were woven into the philosophy of some of the perfectionists and reformers who followed, though in a rather vague and romantic manner. The more critically minded students of social phenomena, meanwhile, devoted themselves to the collection and analysis of statistics relating to trade, manufacturing, crime, disease, politics, and other social phenomena. Various nations followed the example of Sweden in conducting a periodical national census. The United States, anxious to obtain a statistical view of a shifting and rapidly increasing population, established a census in 1790. Great Britain did likewise in 1801. Laplace's work on the calculus and, a generation later, Quetelet's *Physique Sociale*, encouraged an increasing use of such data as became available in these general collections. National statistical associations—such as the American association founded at Boston in 1839—were organized to encourage the more extensive collection of data. Finally, an international statistical congress was organized at Brussels in 1853, and continued to hold sessions until

[1] Twentieth-century developments in physics later led, as Merz points out, to the view that even physical laws are really of this nature.

1876. The increasing range of subjects included in the purview of statisticians, from simple vital statistics to such specific subjects as particular crimes and diseases, was indicated by the expanding reports and discussions of these bodies.[1]

Contrary to the hopes of Quetelet's generation, a quantitative social physics was slow in emerging. It was difficult to secure exact data on such subjects as were susceptible to measurement, while other phenomena eluded figures altogether. Thus Bentham apparently failed in his effort to construct a "moral and political arithmetic". Sociologists began to face the same situation that the medical world had confronted during the preceding century—new data were accumulating and demanded synthesis, yet many problems eluded quantitative solution, and exact information was still lacking in many fields. There ensued a period—analogous to that in medicine between 1700 and 1800—during which interest in quantitative procedure seemed to lapse. Sociologists became more concerned with bringing philosophic order out of the data already accumulated than they were with the observation and measurement of further facts. Their syntheses assembled all the latest information, but only to fit it into a general scheme of things, held together by speculation where facts were wanting.

The contrast between the sociology of 1830 and that of 1870 is illustrated in the contrast between the mathematical Quetelet and the philosophical Herbert Spencer. Quetelet's contemporary, Auguste Comte, also represented the tendency towards philosophical synthesis, but this process is realized more completely in the later work of Spencer. The latter worked out a grand synthesis of the new data in sociology, psychology, and related fields, and his achievement has been compared with that of such creators of theological systems as Aquinas and Calvin.[2] It might better be likened to the systems of such medical theorists as Brown and Broussais.

Like Comte before him, Spencer was theoretically impressed by mathematical principles. But he actually assembled most of his sociological data in inexact form, around the concept of evolution; somewhat as Broussais organized all pathological data around the concept of gastroenteritis. If the comparison seems far fetched, it would be well to recall that a similar tendency to ascribe all phenomena in a given sub-

[1] As indicated in the *Compte-Rendue général des Travaux du Congrès International de Statistique, à Bruxelles, 1853; Paris, 1855; Vienne, 1857*, etc. On the general significance of Quetelet's work, see Arne Fisher: *The Mathematical Theory of Probabilities*, I (transl. from the the Danish by Charlotte Dickson and Wm. Bonynge, N.Y., 1922), 178–81.

[2] Charles H. Cooley: "Reflections upon the Sociology of Herbert Spencer", *Amer. Jour. of Soc.*, XXVI (1920), 129 ff.

ject to some one underlying origin characterized other social sciences as well as sociology during the Victorian era. There appeared, for instance, a monistic ethics and a monistic historiography, just as there had been a monistic pathology half a century before. And as in the latter case, all this appeared before any factual basis justifying so ambitious a unification had been actually established.

Thus Bentham and the English utilitarians attempted to explain all ethical conduct in terms of a single, all-inclusive motivation; that is, as expressions of egoistic hedonism.[1] At about the same time, certain historians traced all historical developments to economic determinism; while others, of a more romantic temper, held that the ultimate clue to human history lay in *Heroes and Hero Worship* or in other idealistic influences. This stage of philosophical synthesis was naturally characterized in the social sciences, as it had been at an earlier period in medicine, by controversies between rival schools. A great gulf was fixed between the Marxists and the romantic historians; even as there had been between, let us say, the Brunonians and the homeopaths.[2] Such absolutely opposing schools cannot exist in a field in which quantitative, critical procedures have been established. For in the latter case opposing theories are held but tentatively, with full expectation that apparent contradictions will be resolved by further facts.

The quasi-speculative, controversial state of things continued in sociology clear down to the present century; indeed, vestiges of it persist at the present time. The outstanding sociologists of the period between 1880 and 1910 were still, as a rule, systematizers rather than observers. An example is afforded in Lester F. Ward (1841–1913), who was perhaps the best-known American sociologist of his generation. "Like Spencer," observes a contemporary sociologist, "Ward can be understood only in the light of his philosophical approach. He accepted fully and unreservedly the evolutionary viewpoint with its monistic . . . implications." [3]

The claim has been made that another sociologist of this generation, Vilfredo Pareto of Lausanne, finally applied mathematical methods to social phenomena, and so worked out a satisfactory systematization of the whole field. This is conceivable, but unlikely; since the first men to

[1] Leslie Stephen: *The English Utilitarians* (Lon., 1900), pp. 298, 299; Ernest Albee: *A History of English Utilitarians* (Lon., 1902), p. 102.

[2] This does not mean that Marx himself accepted a monistic economic determinism; on this point see Sidney Hook: *Towards the Understanding of Karl Marx* (N.Y., 1933), pp. 150 ff.

[3] F. H. Hankins: "Sociology", in Harry Elmer Barnes (Ed.), *The History and Prospects of the Social Sciences* (N.Y., 1925), p. 303. See Lester F. Ward: *Dynamic Sociology*, I (N.Y., 1902), 450 ff.

employ systematic measurements in any subject rarely attempt to create—indeed rarely live to see—a satisfactory systematization of that discipline. It is doubtful if Pareto's observations on the "residues" or inner drives of human action were really of a quantitative character. The claims for Pareto remind one, rather, of those made for Boerhaave about 1750, or for Broussais about 1830—that these leaders had finally created a sort of definite medical science.[1]

There can be no question, however, that during the first quarter of the twentieth century sociologists displayed more interest in exact, detailed observation, and a corresponding indifference to further philosophizing for the time being. They were doubtless influenced in this by the labours of the economists and of the anthropologists, who seem to have found it somewhat easier to be objective and mathematical in their procedure.[2] Perhaps, too, psychology set something of an example. This subject had been frankly viewed as a branch of philosophy, despite the attempts of various philosophers to approach their own consciousness in an empirical manner. Quantitative observations began to be made in psychology about the middle of the nineteenth century. Out of the work of such leaders as Fechner and Wundt there developed a real "psychophysics", which was quite consciously objective and mathematical in spirit. Psychophysics dealt, to be sure, with individuals; but something of its viewpoint carried over into social psychology as that subject was developed during the present century.[3] And the connection between the latter and sociology was so close as to make them, at times, almost indistinguishable.

Prominent in the renewed demand for an objective, quantitative sociology was the American authority, F. H. Giddings, of Columbia University. Observation, experiment, and measurements, he insisted, must be employed whenever possible. And as quantitative social analysis can only be made by the use of statistics, the latter should be made the standard procedure of the social sciences.[4] Giddings exercised a

[1] Sweeping claims for the unique character of Pareto's work are expressed in Bernard De Voto: "Sentiment and the Social Order", *Harper's Magazine*, Oct. 1933, pp. 569 ff. Cf. with such a work as De La Sarthe: *Histoire de la Revolution Médicale du XIX^e Siècle* (Paris, 1854), in which similarly sweeping claims are made for Broussais, as the Newton of medicine.

[2] The speculative element was not entirely lacking in anthropology, however. See, e.g., the criticism of "archaic civilization" theories in Alex. Goldenweiser: "Diffusionism and the American School of Historical Anthropology", *Amer. Jour. of Sociology*, XXXI (1925) 27.

[3] Gardner Murphy: *An Historical Introduction to Modern Psychology* (N.Y., 1929), pp. 298 ff.

[4] F. H. Giddings: *The Scientific Study of Human Society* (Chapel Hill, N.C., 1924), pp. 189 ff.

stimulating influence on many younger American sociologists, who proceeded to develop "a quantitative methodology in connection with studies of specific social phenomena".[1]

This renewed interest in measurement coincided with a growing concern about scientific method in general. The sociologists who called for quantitative analysis quite naturally reacted against the survivals of theoretical systematizing which persisted in their field. Here, again, one is reminded of an earlier stage in the development of the medical sciences—of that period when physicians were so conscious of methodological problems (1750–1850), and when they came finally to repudiate theoretical synthesis and to demand objectivity and measurement.

As the final modernization of the medical sciences has yet to be discussed, this parallel cannot be traced here in any detail, nor should it in any case be pushed too far. No two sciences are likely to have an exactly identical development. Yet it is suggestive to compare the protests of physicians against the medical systematists of a century ago, and those criticisms which contemporary sociologists direct against survivals of theoretical systematizing in *their* field.[2] It is also interesting to find that another phase of the final advent of a modern science—the protest against mutually exclusive hypotheses—characterizes sociology to-day, as it did the medicine of a century ago. Just as physicians once demanded a scientific solution of the *impasse* between humoral pathology and solidism, or between rational medicine and the empirical, so to-day critical sociologists seek to resolve the apparent incompatibility of theories concerning biological determinism on the one hand, or "cultural" determinism on the other.

So, too, sociologists find it necessary to seek middle ground between the older theoretical views (rationalism) and that extreme reaction thereto, the demand that sociology content itself with being a purely descriptive (empirical) science.[3] "Students of sociological theory",

[1] An excellent example is Malcolm M. Willey: *The Country Newspaper* (Chapel Hill, N.C., 1926). Various works on statistical social science have appeared since 1910, even as works on statistical medicine began to come out after 1840. Examples of the former type are A. L. Bowley: *The Measurement of Social Phenomena* (Lon., 1915); and P. S. Florence: *The Statistical Method in Economics and Political Science* (N.Y., and Lon., 1929). Subsequently, political scientists began applying quantitative analysis to topics usually thought beyond the pale of mathematics. Note, e.g., Stuart A. Rice: *Quantitative Methods in Politics* (N.Y., 1928).

[2] Cf. Josiah Bartlett: *Philosophy of Medical Science* (Phila., 1844), *passim*, with articles by American sociologists concerning methods; e.g., Albion W. Small: "Some Contributions to the History of Sociology", *Amer. Jour. of Sociology*, XXVIII (1923), 385 ff.; M. J. Herskovits and Malcolm M. Willey: "The Cultural Approach to Sociology", ibid., XXIX (1924), 188.

[3] See the discussion of this situation in Charles A. Ellwood: "Recent Developments in

writes a contemporary American sociologist, "are prone to fall into two contrasting types of error; either they accept speculative explanations of social phenomena with credulity, or they dismiss all theorizing as unscientific escapes from the hard reality of laborious research." [1] If the word "medical" were substituted here for "social" or "sociological", the whole passage might stand unchanged in any good work on medical method composed over 100 years ago.

Apparently every science, whether we begin with dynamics in the sixteenth century or end with sociology in the twentieth, goes through certain phases which might in a loose sense be termed stages of development. (Or, to put it in another way, each science follows what is, figuratively speaking, a normal curve of development. The actual course is not quite as simple as this, but approximates it.) The first stage reveals a minimum of observation and a maximum of theoretical synthesis; the second, an early attempt at objectivity and measurement, characterized by pioneer enthusiasms; the third reveals a partial reversion to speculative synthesis (with its inevitable "schools" and controversies) and a partial lapse of quantitative procedures—due to difficulties encountered in carrying out the quantitative programme; the fourth level witnesses a revival of the quantitative procedure, this time upon a firmer factual basis and with a technique so improved as to make possible a final victory for modern methodology.[2]

Some will have it, no doubt, that such a demarcation of stages in the evolution of the sciences is itself a speculative historical generalization. Be this as it may, there is certainly some evidence suggesting the view here presented, and in no case is this more true than in medicine. The brief consideration of the social sciences given above has been offered merely by way of comparison. It is now high time to return to the main theme; that is, to the final emergence of medicine on what may be termed the fourth level of development. For the many influences which have been noted were converging, between 1825 and 1850, to bring about momentous changes in the medical sciences.

Sociology", in E. C. Hayes (Ed.): *Recent Developments in the Social Sciences* (Phila., 1927), pp. 12 ff.

[1] F. Stuart Chapin, in his introduction to Pitirim Sorokin: *Contemporary Sociological Theories* (N.Y., 1928).

[2] This is not to say, of course, that quantitative methods are the only ones retained in modern methodology. There remain problems in medicine, as well as in the social sciences, which elude the application of mathematical methods. Whether there are inherent limitations here remains a matter of opinion. Cf., for instance, the essays by H. E. Jensen and Charles A. Ellwood in the latter's *Method in Sociology* (Durham, N.C., 1933), and those in Edgar A. Singer's *Mind as Behaviour* (Columbus, Ohio, 1924).

IX

THE EMERGENCE OF MODERN MEDICINE 1800–1850

THE EMERGENCE of medicine from the confusion of the eighteenth century into the relatively clear and critical atmosphere of modern science was the achievement of no single time or place. Yet, so far as one can put his historical finger on the process, it can best be pointed out in Paris during the half-century between 1800 and 1850. This seems the more remarkable when it is recalled that the period was ushered in by a certain Revolution, during which all learned societies were for a time forbidden and when a radical leader assured the nation that it had no need for learned men.

Lavoisier, whose execution elicited that remark, observed during some of his earlier work that none of the great names in chemistry had been French. He could hardly mention his own; yet his was indeed to symbolize the rapid rise of Paris thereafter as a centre for the physical sciences. Why was it chiefly after 1760 or 1770 that such interest developed in those fields in the French capital? There seems to be no easy answer, as the city had long been the metropolis of one of the most wealthy and progressive of European States. Whatever the explanation, the fact remains that a transformation occurred. The Paris of 1770–1800 became—so far as any one town could be—the world's scientific capital. In no city could there be found more brilliant and intensive research in mathematics, physics, chemistry, and biology. The Revolutionary period itself witnessed continued progress—Lavoisier was not really executed as a scientist but rather as an aristocrat. Something has been said of the manner in which even social phenomena were investigated. Progress in all the sciences acquired a momentum which has persisted into our own time.

All the sciences were advancing, one should add, except medicine. Physicians apparently were not yet in step with the brilliant physical scientists of the day. Where were the medical equals of Laplace, D'Alembert, and Lavoisier? As a matter of fact, the relative backward-

ness of French medicine was made painfully apparent by the very progress of physics and chemistry. It became fashionable, in the last decades of the eighteenth century, to ridicule physic openly. "Everyone repeated the reasonings of Montaigne, the jests of Molière, or the whims of Rousseau." There is no telling just what effect this ridicule had upon Parisian physicians, but it did not necessarily discourage them. It may have exercised just the opposite influence; it seems, for instance, to have led Cabanis to make his survey of the moot question of "medical certainty", and perhaps served as a challenge to the whole "French school" which was to follow.

Cabanis was a leader of a group of French thinkers known as the *Idéologues*, who accepted the philosophic empiricism of Locke, carried this in some cases to the extreme of materialism, and were especially interested in its scientific implications. Their view that all concepts originate in sensations—that the mind is at first a blank page—naturally lent itself to an emphasis upon the careful observation of phenomena and on the avoidance of speculative hypotheses. In a word, philosophic empiricism encouraged empirical scientific methods in France as it had in England.

During the 1790's a number of able physicians, notably Cabanis and Pinel, became interested in applying the principles of *Idéologie* to medical studies. According to Pinel, this really meant the use in medicine of the methods already commonly employed "in all other branches of natural history. . . ." Just as concepts could be analyzed into original sensations in psychology, so could clinical data be analyzed by tracing them back to their sources in organs. The changes in the latter should be further analyzed in order to see the entire pathologic picture.[1] None of this called for theories or speculations about ultimate pathologic processes. Thus the *Idéologues*, by an apparently philosophic approach, arrived at much the same interest in a localized pathology as had the earlier Morgagni through his clinical-pathologic investigations. This was the starting point of Xavier Bichat, who had been influenced by Pinel before undertaking the major research to be noted below.

Once the success of physical science and empirical enthusiasms had stirred Parisian medical leaders to new activity, they found themselves encouraged by unusual opportunities. Personal contact with brilliant chemists and mathematicians was no small thing in itself. Perhaps it

[1] George Rosen: "The Philosophy of Ideology and the Emergence of Modern Medicine in France", *Bull. of the Hist. of Med.*, XX (July 1946), 329 ff. The quotation above about Montaigne and Rousseau is from P. J. G. Cabanis: *Du Degré de Certitude de la Médecine* (Paris, 1798), 118, 119.

was of even greater advantage to live and have one's being in the intellectual climate which these men had produced. No *Naturphilosophie* could flourish here; nor was Paris so handicapped by moral objections to medical research as were the English-speaking lands.

In addition to such advantages, the city came to offer remarkable professional opportunities. By 1830 it possessed no fewer than thirty hospitals housing some 20,000 patients, and giving instruction to the 5,000 medical students resident in the city. The Hôtel Dieu alone possessed 1,000 beds.[1] Municipal growth during the eighteenth century necessitated a great extension of hospital facilities, and the gradual development of clinical teaching towards the end of that period made these facilities increasingly significant for both education and research. The *Idéologues* exerted a direct influence in improving hospitals and schools, in order to bring them into line with their ideas about medical science. Last but not least, it became Napoleon's policy to encourage physicians and their labours, in contrast to the earlier republican distrust of scientific leadership. These factors account for the fact that, after about 1795, a group of young medical men appeared who were more critical and more objective in their work than any of their local predecessors.

These younger men were closely connected with the hospitals, and so availed themselves of unsurpassed opportunities for systematic observation. Thus Pinel employed the hospitals for his studies in mental disease—his famous order to remove the shackles from insane patients is usually viewed as an epoch-making event in this field—while Bichat used the hospital necropsies for his equally important work in gross anatomy. In the hospitals, too, these physicians formed stimulating associations with each other which they carried over into the professional societies formed during the period. The *Société médicale d'émulation* was typical of the better organizations of this sort. Founded in part by Bichat, it had published twenty-nine volumes of papers by 1830.[2]

Xavier Bichat, who is often cited as the founder of modern medicine, was the product of this environment. He stood upon Morgagni's shoulders, but he breathed the intellectual atmosphere of the Paris of his day. This in no wise detracts from his youthful ability; it is only to say that he was born aright in time and place. His keen insight and amazing industry led to basic discoveries in the anatomy of tissues.

[1] J. Chalmers Da Costa: "The French School of Surgery in the Reign of Louis Philippe", *Ann. of Med. Hist.*, IV (1922), 77 ff.

[2] Marcel Fosseyeaux: *Paris Médicale En 1830* (Paris, 1930), pp. 97 ff.

Morgagni had thought that diseases were to be found in organs, but Bichat showed that they were actually located in the tissues of the same. There were diseases of the serous membranes which produced pain and fever, whether located in the pleura or in the peritoneum. This view represented another step towards a localized pathology, and away from the older theories relating to the humours or solids in general.[1]

Bichat's work, moreover, permitted a more promising departure from the nosography of symptoms than did the reaction of theorists like Brown and Rush. It has been pointed out that the latter, in protesting against de Sauvages's long list of diseases, had gone to the other extreme in recognizing only one. Bichat realized that neither an endless list on the one hand, nor a theory about the unity of disease on the other, could afford a basis for the medical sciences. He and his colleagues must follow rather in Morgagni's path, and study both symptoms *and* their related lesions. Only by correlating bedside observations and subsequent pathological findings could physicians arrive at distinctions between different disorders. Only by improving clinical observations and, at the same time, probing deeper into pathological anatomy, could medical men sketch out the rational nosography so necessary to further progress in all the medical sciences. That the young Frenchmen of 1800 went farther in this direction than Morgagni, was due to their consistent pursuit of these two ideals—to Bichat's more searching investigations in gross pathology, and to the improvement made by his colleagues in the technique of clinical observation.

Bichat stood truly on the threshold of a new era in the most vital of all sciences. He did not actually enter into it, presumably because his early death prevented him from using the modern microscope.[2] His great work, the *Anatomie Générale*, was published in 1801, during the last year of his life. An actual martyr to science, since his death was due to infection at the dissecting-table, the young Frenchman's fate serves as a reminder of the personal risks involved in medical research.

Among the ablest of Bichat's successors was the brusque Broussais, who in his first pathological studies seemed to follow in the master's paths. He emphasized patient, objective research, and recognized the importance of the localization of disease. As he became famous, how-

[1] Xavier Bichat: *Pathological Anatomy*, transl. by J. Togno (Phila., 1827), pp. 36 ff. See the discussion in Faber: *Nosography*, pp. 4–13; and also Paul Diepgen: "Virchow u. die Romantik", *Deutsches Med. Wochens.*, 1932, No. 32.

[2] He may therefore be viewed as the last leader of the old school of gross anatomy, rather than as a pioneer of the new histology; see George W. Corner: *Anatomy* (*Clio Medica* Ser., N.Y., 1930), p. 41.

ever, he became dogmatic in manner and pontifical in claims. He visibly reverted to the very type of systematist he had most condemned. Accused of this, he retorted that his "physiological medicine" was not a system but simply a method; only to give himself away in the next breath by adding that it *never* led to false results! As if to confirm the indictment of the critics, he proceeded to elaborate a monistic pathology in terms of "simple or complicated gastroenteritis".[1] He was now on all fours with the Brunonians and their ilk, but is significant here in that he lived on—almost the last great systematist in good standing—to do personal battle with the oncoming representatives of the new regime.

Stimulated by the same environment as Bichat, and trained in the tradition he had helped to establish, a number of great clinicians appeared in Paris during the next generation. When Broussais reverted to speculation, he had little influence on the others save to arouse their opposition. Although practitioners, they had their professional headquarters in the hospitals, and there carried on the systematic co-ordination of clinical and pathological studies which became the outstanding characteristic of the French school. Intensely interested in extending both the range of their observations (in terms of the number of cases) and in making each individual examination as thorough and exact as possible, their contributions to clinical methods were soon as outstanding as had been Bichat's findings in pathological anatomy. Seven years after the publication of the *Anatomie Générale*, appeared Corvisart's translation of Auenbrugger's work on percussion (1808), and in 1819 came Laënnec's *De l'auscultation mediate.* The invention and subsequent improvement of the stethoscope was of such obvious value in diagnosis, particularly of diseases of the chest, that comment seems superfluous.[2] Thereafter physicians would not simply observe their patients—they would *examine* them.

With such improved clinical methods it was possible to correlate diagnosis with Bichat's pathology and thus gradually to discard the

[1] F. J. V. Broussais: *Principles of Physiological Medicine*, etc. (transl. by Isaac Hays and R. E. Griffith, Phila., 1832); cf. the preface, p. iv, with p. 180. On his reversion, see Bartlett: *Philosophy of Medical Science*, pp. 207–9; his relationship to Brown and Rasori is discussed by a contemporary, J. H. Peisse: *Sketches of the Character and Writing of Eminent Living Surgeons and Physicians* (transl. by Bartlett, Boston, 1831).

[2] Henri Saintignon: *Laennec: Sa Vie et Son Œuvre* (Paris, 1904), pp. 88 ff. The story of how Laënnec happened to observe children listening to vibrations along a wooden beam, and thus hit upon the idea of his *baton*, is a human and dramatic one; it is told in G. B. Webb: *René Théophile Hyacinthe Laennec* (N.Y., 1928), pp. 92 ff. Able biographical sketches of the other French leaders of the period will be found in the *éloges* published in the *Mem. de L'Acad. Roy. de Méd.*, e.g., those of Corvisart, Pinel, and Berthollet in I (Paris, 1820), 107 ff.

nosologies based merely on symptoms. For the same reason, the monistic pathology of the speculative leaders was repudiated. It became possible to picture the pathological changes that occurred in the living subject. No longer was this a matter of theory or mere guesswork. The contrast between the "doctrines" of Broussais concerning the diseases of the chest, and the actual observations of Laënnec on the same subject, involves the whole contrast between speculative and objective procedures. Yet these men were contemporaries and colleagues.

Into the French school there came another young physician who shared Laënnec's general viewpoint. Pierre Louis had been sadly disillusioned by tragic experiences during an epidemic of diphtheria in Russia, and returned to his native France in an unhappy state of mind. Unlike Cabanis, he found no "degree of certitude" in medicine. The old systems were as useless in practice as they were absurd in doctrine; one must start all over again, go back to the inductive beginnings of things. This, he found, was just what the Parisian clinicians were doing. Noting among them an old friend, Chomel, he attached himself to his hospital service.

Here Louis evolved a unique procedure. For seven lean years he did nothing but study ward cases and post-mortems; he declined all private practice, and for three years did not write a line. He himself later described what it meant thus to devote seven of his best years to routine observation.[1] The rewards, however, were great, for he removed himself from the distractions of practice and was able to amass a relatively large number of data. That he appreciated the significance of such professional arrangements is evident from his advice to his American pupil, James Jackson, Jr., whom he urged to devote the first working years entirely to research in hospital service.[2] Louis's example in this respect was followed to some extent by other medical leaders, but it was to be some time before the first "full-time chairs" were established in medical schools.

While Louis followed the traditions of his colleagues in contributing to pathology, notably in connection with tuberculosis and typhoid, his early human concern for cures directed his attention for a time to therapeutics. Here the most pressing need was a critical examination of the confused and radical remedies which had been inherited from the old schools. The very number of cases at Louis's disposal, in making this examination, may have suggested to him the method he soon adopted;

[1] P.-Ch.-A. Louis: *Examen de L'Examen de M. Broussais*, etc. (Paris, 1834), pp. 3–5.

[2] See his interesting letter to James Jackson, Sr., in this connection; quoted in Sir Wm. Osler: *An Alabama Student and Other Essays* (N.Y., 1908), p. 201.

that is, to list these cases in numerical form, and to check one group receiving a certain treatment against another group denied it. Would not such statistical procedure provide a new method of clinical observation, even as had auscultation and percussion? Some tentative attempts so to use statistics had already been made, but these had been inexact or for other reasons undependable. Louis, it may be added, was familiar with the work of Laplace and with the latter's views concerning the potential value of medical statistics; and it may have been this influence also which led to the employment of the famous "numerical method."

In 1825 appeared his work on phthisis, which involved a study of 167 clinical cases and autopsies. Apprehensive of what was to come, Broussais had already attacked both Louis and Laënnec, raising those criticisms which were later to be heard again and again against the French school. There was, first, the old claim that they neglected practice while pursuing science. These pathologists, declared Broussais, were obsessed with their anatomical studies. They should give more heed to actual life processes, to what he termed "physiological medicine". Pathologists, he added, had no interest in their patients as such, no concern in curing them. Laënnec was more interested in *performing* autopsies than he was in *preventing* them.[1]

When Louis referred to Broussais, truthfully enough, as a physician who defended his conservative opinions with unnecessary heat, the latter began to talk about "the maladies which are the creations of M. L——".[2] The older man was fighting a losing battle, despite the fact that certain of his criticisms were sound. He apparently thought he was simply opposing another "school"; whereas in reality he had set himself, Canute-like, against the rising tides of modern science. He continued to storm in lectures to indifferent classes, who turned with relief to members of the new group who followed him upon the platform. Here in this dramatic classroom setting was fought out what was, in a sense, the final struggle between medieval and modern medicine.[3]

The full exposition of Louis's numerical method was afforded in his great therapeutic study, *Recherches sur Les Effets de la Saignée*, pub-

[1] *Examen des Doctrines Médicales*, II (Paris, 1821), 717.

[2] Ibid., p. 726. By a sort of poetic justice, the downfall of Broussais's own system was occasioned in part by statistical checks. He claimed that he lost in hospital practice only one patient in thirty, while other practitioners lost one in five. M. Brasquet thereupon inspected the records, and found that Broussais actually lost one in thirteen—considerably more than his colleagues! See Albert H. Buck: *The Dawn of Modern Medicine* (New Haven, 1920), pp. 211, 212.

[3] Some of the students were alive to the significance of the professional drama; see, e.g., Henry I. Bowditch, *Brief Memoirs of Louis*, etc. (Boston, 1872). Note also Léon Meunier: *Histoire de la Médecine* (Paris, 1911), pp. 412–15.

lished in 1835. In it he ridiculed the incomplete and misleading nature of the data hitherto employed to justify this and other therapeutic procedures. The analysis of his predecessors' claims was truly devastating in its clarity and in its conclusiveness. Physicians, he observed, witnessed a few fatal cases where no blood-letting was employed, and thereupon jumped to the conclusion that this process would have saved them. Other practitioners noted a few cases where death *followed* a resort to blood-letting, and denounced the practice as the whole cause of death. In neither instance did they employ any check or test of their sweeping conclusions. "*Quels faits!*" exclaimed Louis, "*quelle logic!*" We must, instead, know how often venesection aided and how often it impeded recovery. We must have such numbers and such "controls" as will eliminate individual factors, and establish conclusions with mathematical certainty.

He proceeded to give records of numerous cases in connection with pneumonia, demonstrating that bleeding in this disease was of a very uncertain value. He called the attention of the critics of this "numerical method" to the fact that it was really the only possible one that could be employed in this connection. He reminded them that they themselves approved a remedy largely because, in their own experience, "most" of their cases had seemed to benefit thereby. Only *their* experience, added Louis, was incomplete, and *their* numerical estimates were of the vaguest character. In the difference here between exactitude and vagueness, declared the great clinician, lay all the difference between truth and error.[1]

Here, for the first time, was a clear and emphatic realization of the value of medical statistics. Taken in connection with contemporary recognition of the need of vital statistics in public hygiene, it afforded indubitable evidence that the mathematical spirit—the great germinat-

[1] *De la Saignée*, p. 85. This work impresses me, in some ways, as the most significant study ever made in medical method. See especially the sections following p. 88. It is not as complete as such later works as Claude Bernard's *Experimental Medicine* (transl. by H. C. Greene, N.Y., 1927; first ed., 1865) or as some of the German writings; but in view of its historical setting it would seem to have primary significance. It is true that most medical historians, other than a few French and Americans, see in Louis simply an important clinician who exerted unusual influence in the United States; see, e.g., Castiglioni: *History of Medicine*, p. 69. Baas had rather unfavourable opinions. Osler, despite the tribute in his essay on "Louis' Influence on American Medicine", saw him (in his *Evolution of Modern Medicine*, New Haven, 1922, p. 204) only as a representative of the French clinical school. My comment on these estimates is that they do not emphasize the significance of the introduction of mathematical procedures into medical science. Marshall Hall, however, whose opinion merits respect, thought Louis the greatest pathologist of all time, and declared that "Monsieur Louis is the Bacon of Medicine"; quoted in Albert H. Buck: *The Dawn of Modern Medicine*, p. 204. And Singer observes that "The rise of medical statistics into a vocation places the crown on Medicine as a science," *Short History of Medicine*, p. 339.

ing influence in all modern sciences—was at last establishing itself in medicine. It is to be noted, too, that the introduction of quantitative procedures in medicine led to immediate results, even as it had in other sciences. For statistical checks tended to demolish the heroic treatment of the old systems, even as Bichat and others had demolished their theoretical pathology. The whole speculative edifice began to crumble.

There was a certain danger here; namely, that the older remedies would be discarded before anything had been found to replace them. This might prove unfortunate from a psychological point of view, if from no other. Some hope of constructive substitutes was found in the work of French chemists. Following the quantitative and analytical methods of Berzelius, they discovered a number of valuable drugs during the first quarter of the century. M. Séguin announced, in 1804, that he had isolated a new crystalline substance (morphine) from opium. Twelve years later, Sertuerner of Hanover described its properties, gave it the present name, and made it well known. The drug was finally introduced into general practice by Kynd, an Irish physician, who employed it with a hypodermic needle in 1844.[1]

Of equal importance with the discovery of morphine was the isolation of other active principles from old vegetable medicaments. In 1820 Pelletier and Carenton of Paris secured strychnine from nux vomica; and two years later succeeded in isolating quinine from cinchona bark.[2] Other useful alkaloids were discovered about 1820; notably emetine, a derivative of ipecac, which later proved to be an actual specific against amœbic dysentery.

The discovery of quinine was a major achievement in therapeutics, since it was a much more effective remedy for malaria than was crude cinchona—a fact sometimes forgotten by those who use the two terms interchangeably. The use of quinine sulphate in large doses was gradually introduced, for example, in the Southern States of the American Union, and made possible for the first time some control of the serious malaria problem in that region.[3]

The old quarrel between the advocates of vegetable and mineral remedies had no meaning in this critical age. While vegetable drugs proved most promising, mineral materials were also investigated to

[1] V. Bally: "Observations sur Les Effets Thérapeutiques de la Morphine ou Narcéine", *Mém. de L'Acad. Roy. de Méd.*, I (Paris, 1828), 101 ff.; Léon Meunier, *Histoire de la Médecine* (Paris, 1911), p. 498.

[2] MM. Pelletier et Carenton; "Recherches Chimiques sur les Quinquinas", *Ann. de Chim. et de Phys.*, 2 ser., XV (Paris, 1822), 289 ff., Léon Meunier: op. cit., pp. 498 ff.

[3] *So. Med. Rep'ts.*, II (New Orleans, 1850), 347, 402, 450 ff.; *So. Med. and Surg. Jour.*, n.s., XVI (Augusta, Ga., 1860).

good purpose. Special studies were made of the various emetics and purgatives, such as epsom and glauber salts, and these were made available as substitutes for the much-abused calomel. In 1836, Wallace employed potassium iodide in the treatment of syphilis. Ricord was impressed, and the use of potassium iodide in his Paris clinic resulted in its rapid introduction in the treatment of the tertiary stages. The French physiologist Magendie (1783–1855), who was then carrying on Haller's tradition in animal experimentation, also did much to introduce new drugs into medical practice.[1]

It might be assumed that the discovery of new drugs would compensate for the rejection of older remedies. Unfortunately the former were of a relatively specific nature, and their use was limited to a few particular purposes. They could not provide a substitute for the old bleeding and sweating procedures, which had supposedly been good for every conceivable condition. Hence, despite the contribution of valuable remedies, the chief therapeutic significance of the French school seemed to be its destructive criticism of the old systems.

The manner in which critical methods undermined the old order is well illustrated by the history of homeopathy. This system was established in Germany during the last days of the *Naturphilosophie*, and was characterized, like the others, by a monistic pathology and therapeutics. All diseases save two were viewed as forms of *psora* or "the itch", and a single scheme of treatment was recommended for all cases.[2] It is now often forgotten that homeopathy was originally as much a part of regular medicine as any other system, and that contemporary studies recognized it as such.[3] Hahnemann was as "regular" a medical philosopher as Brown, Rush or Rasori, but unfortunately he was born just too late. The views of Brown could be neither proved nor disproved by the rationalistic controversies of 1800, and so they remained in more or less

[1] Magendie was an unusual figure in the French school, where most of his colleagues were clinicians and pathologists. He carried Haller's animal experimentation further, did pioneer work on the functions of the spinal nerves, explained the vomiting mechanism, and demonstrated the cause of osmosis in blood vessels as well as in lymphatics. He was not apt at generalization, however, and his work was carried much further by his pupil Claude Bernard. See J. M. D. Olmsted: *François Magendie* (N.Y., 1943), 93 ff. for the famous Bell-Magendie controversy re the spinal nerves.

[2] Samuel Hahnemann: *Organon of Homeopathic Medicine* (first Amer. ed., Allentown, Pa., 1836, from fifth German ed.), p. 122.

[3] J. J. Reuss: *Die Medizinschen u. Heilmethoden der Neuesten Zeit*, u.s.w. (1831), pp. 269 ff.; Auber: *Traité de Philosophie Médicale* (1839), p. 534; P. V. Renouard: *Lettres Philosophiques*, etc. (1857), pp. 84 ff., describes its rejection in Paris. Geo. B. Wood, of the Univ. of Penna., noted in 1837 that it was a new system, but that it had not "laid hold" of the profession in the United States save in a few cases (*Medical Essays*, Phila., 1859, p. 132). Later it was more successful.

professional good standing; but Hahnemann's works appeared just in time to be subjected to exact and critical analyses.

The French clinicians seemed willing to give homeopathy a trial. Andral, in particular, checked the system with care, trying it out in his wards in Paris with negligible results, and reporting to the Academy of Medicine that he had systematically experimented with the *materia medica* without finding any of the results Hahnemann reported. Somewhat later Joerg at Leipzig, and Holmes in Boston, examined Hahnemann's citations of authorities and condemned them as misleading or unreliable.[1] It was as a result of such apparently inductive checks on the new school that it was eventually forced out of regular medicine. This did not mean its extinction, for various factors encouraged its survival. Indeed, homeopathy eventually responded to the more critical spirit of the day, and there is reason to believe that in the long run it may have been of service to orthodox medicine. The homeopathic attack on heroic treatments was certainly of service to society.[2]

The fact remains that this expulsion, this transfer from the status of a system to that of a sect, affords one of the best criteria for dating the final advent of modern medicine. When a monistic pathology and a related therapeutics were no longer tolerated in regular medicine, that medicine had come of scientific age. Since that day the same social and psychological factors that encouraged the eighteenth-century systems have continued to support essentially similar modern sects, each with its one cause and one cure—hygeists, chiropractors, Christian Scientists, and the like—but a more critical science no longer affords them recognition.[3]

To return to the story of the French school, it must not be assumed that their newer methods met with no opposition within regular medicine. Objections were raised to the introduction of numerical procedures, as has happened in all scientific fields at one time or another. Statistics, it was pointed out, eliminated individual differences, and one could not do this in medicine. Individuals varied too greatly. There was real danger that the conception of *l'homme moyen* in clinical statistics would lead to the use of routine treatments ignoring individual dif-

[1] O. W. Holmes: *Medical Essays* (3 ed., Boston, 1888), pp. 49 ff.

[2] See again Stephan Zweig: *Die Heilung durch den Geist*, pp. 7 ff.; R. H. Shryock: "Public Relations of the Medical Profession", etc., *Ann. of Med. Hist.*, n.s., II, 312. The continuation or revival of regular surgery in homeopathy may have had much to do with bringing it in line again with regular medicine; see Harvey Cushing: *Consecratio Medici* (Boston, 1928), p. 149; also Robert D. Rudolf: "A Few Thoughts on the Vis Medicatrix Naturæ", *Contributions to Medical and Biological Research Dedicated to Sir William Osler* (N.Y., 1919), pp. 874 ff.

[3] See Chap. XIV.

ferences.[1] Louis replied that he did not eliminate, but rather allowed for such differences; that it was one of the beauties of statistical method that it enabled physicians to overcome some of these variables which had long confused both art and science.

Other critics pointed out, truthfully enough, that the very employment of statistics assumed a sound nosography. It was absurd to list cases of tuberculosis unless there was such a thing, and unless one could recognize it when he saw it. Here the value of the clinical-pathological studies of the French leaders became apparent. They *were* sure of their nosography and their diagnoses to a degree that no other group had been before them.[2]

Broussais and the German school, to be sure, accused the French clinicians of inventing disease entities, of seeing "ontological abstractions" where only symptom-complexes (syndromes) actually appeared. There was no such thing, held these critics, as tuberculosis *per se*, which could enter into the body after the manner of the demons of folklore. But the eventual rise of a new ætiology based on bacteriology finally confirmed the soundness of the early French conclusions; since it showed that the causes of such a disease as tuberculosis—if not the malady itself—were indeed definite realities.[3] It proved that there was, in a case of tuberculosis, *something there* that acted as if it were an entity. This might invade almost any part of the body. And it was quite specific, quite distinct from other diseases. The first clinical statistics therefore possessed real value, for entities can be counted.

It must be admitted that Louis's numerical method was vulnerable to certain technical criticisms. His work involved only the use of simple arithmetic. Since he did not employ the calculus of probabilities, he made no allowance for "probable error", and may have viewed as significant small statistical differences which were really meaningless. No less an authority than Bertillon later declared that for these reasons the work of Louis hurt the reputation of medical statistics in general opinion.[4] Yet there would seem to be no question that the French physician was the first to make systematic use of clinical statistics, and to em-

[1] Baas repeated this criticism many years later, *History of Medicine*, p. 897. See also Quetelet: *Sur l'Homme et la Developpement de ses Facultés*, II (Brussels, 1836), 281; Auber: *Traité de Philosophie Médicale*, etc. (Paris, 1839), pp. 87–92.

[2] Morgagni is said to have realized the need for statistical procedures, but the lack of a sound nosography prevented his employing them; Jules Gavarret: *Principes Généraux de Statistique Médicale*, etc. (Paris, 1840), p. 49.

[3] Knud Faber: *Nosography*, 2 ed., pp. 28 ff., 42 ff., 65 ff., especially p. 100.

[4] *Congrès International de Statistique* (Paris, 1855. *Extrait de la Gazette Hebdomadaire de Médecine et de Chirurgie*), p. 3.

phasize their significance in medical methodology. The technical weaknesses in his work were gradually pointed out and corrected by others.

It has been observed that great interest was taken at the time, in Paris and elsewhere, in the application of the calculus of probabilities to social and medical data. In the very year that Louis published his work on bleeding, an important debate on such uses of the calculus was held in the French Academy of Sciences, and in 1837 a similar discussion took place in the Academy of Medicine in Paris.[1] Three years later Jules Gavarret published an able study of the principles of medical statistics, in which he showed the shortcomings of the simple numerical method in not making allowance for the limit of possible error. He pointed out that observation and simple enumeration must be interpreted in terms of the calculus, a refinement of importance to practical conclusions.[2]

Recognition of the values of mathematical procedures now came rapidly. Humboldt reported statistical observations on yellow fever to the Prussian Academy, and Dupuytren performed a like service for the French Academy. Numerical studies of the causes and remedies for various diseases—for cholera and scurvy, as well as for typhoid and tuberculosis—began to appear. During the forties there followed dramatic demonstrations of the role played by doctors in spreading puerperal fever. These demonstrations were based on statistical evidence first suggested by Holmes in Boston, and later independently collected in a systematic manner by Semmelweis in Vienna.[3]

While some workers thus practised statistical procedure, others continued to preach it with increasing vigour. Bouillaud declared in 1836 that statistical method was bringing about a revolution in medicine. A "conjectural art" was being replaced by an "exact science".[4] J. F. Double observed in 1842 that all critics now admitted the great value of statistics in therapeutic studies, despite the disadvantages involved.[5] A contemporary English authority, impressed by Louis's work, announced that "through medical statistics lies the most secure path into the philosophy of medicine".[6] In like manner, W. P. Alison of the

[1] Gavarret, op. cit., preface, pp. ix–xiii.

[2] Ibid., pp. 143 ff. This generalization remains true, of course, to the present day. For illustrations of modern uses of the calculus in public health work, see G. C. Whipple: *Vital Statistics: An Introduction to the Science of Demography* (N.Y., 1923), pp. 432 ff.

[3] Note the interesting tables in *Die Ätiologie, der Begriff, u. die Prophylaxis des Kindbettfiebers* (Wien, 1861).

[4] J. Bouillaud: *Essai sur la Philosophie Médicale*, etc. (Paris, 1836), pp. 96, 97.

[5] In a French edition of J. P. Frank, cited in P. V. Renouard: *History of Medicine* (transl. by C. G. Comegys), pp. 569, 570.

[6] Henry Holland: "On Medical Evidence", *Medical Notes and Reflections* (2 ed., Lon., 1840), p. 6.

University of Edinburgh assured British scientists (1855) that many of the most important questions in medicine could be investigated by the use of statistics "and in no other way".[1] Meanwhile, further abstract studies in the mathematics of medicine testified to a growing interest in the whole subject.[2]

The same period that witnessed the development of both clinical and vital statistics also saw the advent of other quantitative methods in medicine. The fact that these procedures were sometimes extremely simple ones in no wise detracts from their significance. Early attempts to use the thermometer in practice were never entirely forgotten. Late in the eighteenth century, James Currie (1756–1805) had employed a practical clinical thermometer; and during the second quarter of the nineteenth century the French clinicians began to recommend it to their students. This interest was taken up by a great group of clinicians at Vienna (who followed the French school in their major activities), and there the work of Wunderlich marked the final establishment of clinical thermometry in important medical centres.[3] Meanwhile Louis, like Floyer and Haller before him, used a watch with a second hand, and the history of pulse-counts after 1840 is similar to that of medical thermometry. In both cases, of course, another generation had to pass before these procedures became routine in general practice.

The trend towards measurement did not stop with obvious phenomena like the pulse and temperature. Andral, who used the thermometer, followed examples set by chemistry when he began to weigh fibrin and corpuscles, and even the albumin in the blood serum. Marey developed various graphic representations, later to be employed in cardiographs and the like. Vierordt worked out a method for making "blood counts" as early as 1852, and soon after this developed a device for registering the pulse beat. Hypodermic needles were first used during the forties, and within a few years were marked with a graduated scale.[4] It soon appeared that relatively exact measurement would be applied to any device or technique developed in medical work. Most useful in general practice was the invention, by S. S. K. von Basch in 1887, of an instrument for measuring the blood pressure.

[1] "Notes on the Application of Statistics to Questions in Medical Science", etc., Brit. Asso. for the Advancement of Sci. *Rep't.*, for 1855, pp. 155 ff.

[2] E.g., J. Ch. M. Boudin: *Traité de géographie et de statistique médicales*, etc. (Paris, 1857). Bertillon viewed as the most significant of all works in the field, beside the writings of Quetelet and Gavarret, Ach. Guillard's *Eléments de Statistique Humaine.*

[3] Haeser: *Geschichte der Medizin* (3 ed., Jena, 1881), p. 912.

[4] There is an interesting collection of such early needles in the museum of the College of Physicians in Philadelphia.

There was a close analogy between such means of measurement and the concomitant improvement of instruments and methods of observation. Thus auscultation involved greater exactitude in observation, even though no actual measurement was employed. Conversely, collections of statistics brought to view new facts or situations, much as might an instrumental device. Despite this analogy, the French school which did so much to develop statistical medicine did relatively little to invent or exploit new instruments.[1] They even seem to have neglected the microscope.

It remained for the Germans of the fifties to invent the ophthalmoscope and the otoscope, in the order named. Other scopes for the exploration of every accessible organ followed in due time. Most unexpected and remarkable was the discovery of X-rays by the physicist Wilhelm Röntgen, in 1895, and the rapid application of these to the observation of hitherto hidden body parts. Used in connection with photography, the revelations of these rays seemed little short of the miraculous. With such means of observation and with the quantitative recording of data, the medical sciences at last emerged on the plane of modern methodology. From this level, new intellectual and social horizons were revealed.

[1] With the important exception of the stethoscope, and the fact that some of the early work on the achromatic microscope was done in Paris. Perhaps the pioneer French work in photography should also be listed, because of its potential significance to medicine.

X

THE INFLUENCE OF FRENCH MEDICINE IN EUROPE AND AMERICA

THERE IS no obvious explanation as to why German physicians and biologists exploited the microscope, while the French tended at times to neglect it.[1] Some of the Germans who achieved much in microscopy had themselves been trained in Paris, and that city continued to be a centre for the manufacture of microscopes during this period.[2] Scientific history may have followed the course of military history. Napoleon is said to have taught other nations how to fight, only to find his methods finally equalled or improved upon in the field. So, perhaps, French medical leaders taught those of other lands how to conduct research, only to find their methods finally equalled or improved upon in hospital and laboratory. Despite the loss of the cultural unity of the Middle Ages, it was as impossible as it was undesirable to restrict scientific advance to one land. Indeed, it is to the credit of the French leaders that they welcomed the opportunity to exert an influence beyond their national borders. This influence was to prove a potent one throughout the western world.

Among the first to be inspired by French medical research were the Americans. Their more ambitious men, still feeling the need of European training, gravitated to the most progressive training institutions. Their presence in large numbers had long been an excellent test of the quality of such centres. Leyden, Edinburgh, and London had been recognized in turn, before the work of Bichat and Louis brought Americans rather suddenly to Paris. Another generation would see them in Berlin, Munich, and Vienna, but meanwhile a brilliant group carried French principles back to "the States" and made them dominant there

[1] The pathological atlases of Jean Cruveilhier, published as late as 1842, showed no use of the microscope; Garrison: *History of Medicine*, 4th ed., p. 445. There is, of course, the view that Germans were "naturally" more inclined to carry on such painstaking, tedious work as was involved in microscopy.

[2] Although a chief manufacturer there, patronized by German scientists, was the German George Oberhäuser, who was originally trained in Würzburg; see Justus Thiersch: "Zur Geschichte des Mikroskopes", *Münchener Med. Wochens.*, LXXX (nr. 42, 1933), 1671.

within a decade. In such centres as Boston, New York, and Philadelphia, the old idols crumbled more readily than they had in Paris. The repudiation of Rush, the true systematist, was noted in the opening chapter. Progress was also made in Montreal, although the French influence does not seem to have been so direct in Canada during this period.[1]

The men who returned from Paris possessed real enthusiasm for critical investigation. There is no better illustration than the case of Dr. John Bassett, who was to be immortalized as Osler's "Alabama student". Those who secured hospital connections in the larger cities continued there the type of clinical-pathological research they had learned under Louis and his colleagues. William Wood Gerhard investigated epidemic fevers in the Pennsylvania Hospital and in the Philadelphia Hospital (Blockley), and was able finally to differentiate typhus and typhoid in terms of distinctive lesions (1837). Several other Americans —Pennock, Alfred Stillé, and George B. Shattuck—made similar observations shortly thereafter, which were duly recognized in Paris and in other European centres.[2] It is true that American pathology did not, as a whole, establish itself on the same level as that of the schools of Paris, London, and Vienna; but these individual contributions were of great value. There was no better text of pathology in English during this period than that of Samuel D. Gross (1805–1884), who was professor of surgery in the Jefferson Medical College of Philadelphia.[3]

American students in Paris were interested in other matters than fundamental clinical and pathological investigations. They were particularly impressed by the surgeons and obstetricians who practised in the hospitals during and after Napoleon's day. Experience in the imperial armies produced so brilliant a surgeon as Larrey, and with him were associated Dupuytren, Lisfranc, and others. Volpeau's obstetrics made an impression on American practice; and Ricord—originally of Baltimore—taught his former compatriots that the venereal diseases were worthy of the most searching investigation.[4]

[1] Much has been written of the influence of the French school in the United States. The essays of Bowditch and Osler, noted above, are among the best. See also Packard: *History of Medicine in the United States*, 2 ed., II, 1031, 1041. Canadian history is traced in John J. Heagerty: *Four Centuries of Medical History in Canada* (Toronto, 1928); and in Maude E. Abbott: *History of Medicine in the Province of Quebec* (Montreal, 1931).

[2] Packard: *History of Medicine in the United States*, 2 ed., II, 1070 ff.; E. B. Krumbhaar: "The History of Pathology at the Philadelphia General Hospital", *Med. Life* (April 1933), pp. 162 ff.

[3] Gross, *Elements of Pathologic Anatomy* (Boston, 1839). See also Long: *History of Pathology*, p. 166; Garrison, op. cit., p. 599.

[4] I. de Fourmestraux: *Histoire de la Chirurgie Française, 1790–1920* (Paris, 1934), pp.

Two factors were making for progress in French surgery at this time. One was the opportunity afforded military surgery by the Napoleonic Wars. More fundamental was the whole development of a localized pathology. So long as disease had been thought of as a vague state of the body fluids or as a general condition of tension, there was no incentive to operate save as a last resort in obvious emergencies. One cannot, after all, operate on the blood. But once diseases were traced to lesions in specific organs, the most immediate way to deal with them was to remove the diseased parts whenever possible.[1] The growing scepticism about internal medicine made surgery seem all the more desirable, for the simple reason that there seemed to be no alternative.

Similar trends were to be observed in British surgery. John Hunter had done much to improve the technique and prestige of surgery prior to 1800, and his students like Abernethy in Britain and Physick in America carried on in his tradition. But their work continued to be largely of an emergency type—the relief or repair of aneurysms, strictures, abscesses, and the like. Gradually, as the interest in localized pathology increased, it reflected itself in more essential procedures. These might be dated, approximately, from the publication of Sir Benjamin Brodie's works on the diseases of the joints (1818) and on the urinary organs (1832).[2]

Both French and British surgeons were thus engaged in the improvement of their art as an essential part of medical treatment. Instead of simply repairing or relieving obvious emergencies, they sought to remove the local seats of relatively obscure diseases. The contrast involved can be observed in the case of appendicitis. In 1800, or even in 1850, acute pain in the lower right side was likely to be treated by purging, and "as a last resort" by morphine.[3] Here the hands of the physician was still guided by ancient humoral conceptions, which naturally did not suggest surgical interference. The local pathology of appendicitis was described early in the century, however, and clear cases of

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58 ff.; J. Chalmers Da Costa: "The French School of Surgery in the Reign of Louis Philippe", *Ann. of Med. Hist.*, IV (1922), 77 ff.

[1] H. E. Sigerist: "Surgery at the Time of the Introduction of Antisepsis", *Jour. Missouri State Med. Assoc.*, May 1935, pp. 169 ff. Harvey Cushing pointed out an interesting corollary to this; namely, that the revival of a humoral pathology and therapeutics (as found in the bacteriological laboratory) later detracted from an interest in surgery; *Consecratio Medici* (Boston, 1928), pp. 156 f.

[2] John S. Billings: *The History and Literature of Surgery*, pp. 85 ff. (Separate of a section in F. S. Dennis: *A System of Surgery*, Phila., 1895).

[3] An actual case of this sort is described in R. H. Shryock (Ed.): "Letters of Richard D. Arnold", *Bull. of the Johns Hopkins Hosp.*, XLII (1928), 156 ff.

rupture were subsequently found at autopsy. To prevent this development, Henry Hancock of London first removed an appendix in 1848.[1] A generation thereafter the operation was gradually adopted as a regular procedure.

The surgeon was here taking the place, it will be noted, of the general practitioner; and the former's work was becoming a vital part of therapeutics in the broadest sense of the term. Under these circumstances an increasing interest was naturally displayed in improving all the techniques and procedures of his art. This interest had appeared before 1800, but grew in an accelerated manner after 1840. The discoveries that followed may be viewed as the effects rather than—as is usually assumed—the cause of the modern emphasis upon surgery.[2]

This relationship is well illustrated in the history of anæsthetics, in which Americans played so prominent a part. Sir Humphry Davy had called attention about 1800 to the possible use of gases to prevent pain during operations. But the prevailing interest at that time was still in internal medicine, and so the Bristol Pneumatic Institute devoted itself to general therapeutics and failed to follow up the suggestion. A Mr. Hickman, who practised surgery in London, is said to have avoided pain by the use of "certain gases" in 1828, but nothing came of the matter. Three years later chloroform was independently discovered by Liebig, by the French chemist Soubeiran, and by a New York physician, Samuel Guthrie. The latter called it to the attention of Professor Silliman, who asked Dr. Eli Ives of the Yale Medical School to check its possible medical values. The latter reported that it seemed useful in the treatment of asthma; but apparently there was not yet sufficient interest in promoting surgery to lead him to investigate the possibilities in that field.[3]

During the decade which followed there came the demand for improving surgical procedures. One of the basic difficulties in most operations was that of the patient's suffering, since this encouraged haste on the part of the surgeon. Any means of avoiding pain would obviously aid the latter by enabling him to proceed deliberately and without distractions. It must also be remembered that even hardened surgeons had human qualities, and those who worked during the 1840's

[1] Garrison: *History of Medicine*, 4 ed., p. 504.

[2] I use the term "cause" here, as elsewhere, in the usual sense of priority; without entering into the philosophical question of the relation of the concept of "cause" to the phenomena of "concomitant variations". There are, however, historians who do not believe that the concept can be employed in any essential sense. See Carl Becker: *Progress and Power* (Stanford Univ. Press, 1936).

[3] Victor Robinson: *Pathfinders in Medicine* (2 ed., N.Y., 1929), pp. 326 ff.

were doubtless influenced in some degree by the rising humanitarianism of the times. It may not have been pure coincidence that the "discovery" of anæsthesia was concomitant with such other ameliorative developments as the movements to forbid flogging, or to provide a more merciful treatment of the insane. Hickman's work, for example, is said to have been prompted by a sympathy for those whose agonies he had observed at the operating-table.[1] In a word, surgeons had both technical and moral reasons for seeking a victory over pain.

These circumstances, combined with the availability of such chemicals as ether and chloroform, may explain the sporadic experiments with anæsthesia made by a few Europeans between 1825 and 1845. Humanitarianism and technical needs were particularly significant, however, in making for the rapid adoption of anæsthetic procedures once these were shown to be effective. The immediate stimulus to the first demonstrations was not a rational search, but rather the fact that itinerant lecturers introduced ether inhalations as a popular amusement. In the course of one of these "ether parties" Dr. Crawford W. Long of Georgia accidentally noted the effects of the gas in preventing pain. He thereupon successfully used it as an anæsthetic during several operations in 1842; but, fearful of public censure for using a dangerous procedure, made no reports.[2]

In 1844 Horace Wells, of Hartford, impressed by a popular demonstration of the effects of nitrous oxide, began using that gas in dental work; but a fatal case caused him to withdraw from practice. It remained for another dentist to whom Wells had transmitted his ideas, W. T. G. Morton, to return to the use of ether. Morton persuaded Dr. J. C. Warren to permit a famous demonstration in the Massachusetts General Hospital in 1846, which definitely introduced the use of ether into surgical practice in both America and Europe.[3]

The fact that both Wells and Morton were dentists provides a clue as to why anæsthesia was first successfully demonstrated in the United States rather than in Europe. American dentists, as will be noted below, were making more rapid advances than were those abroad; and dentists

[1] An interesting account of early nineteenth-century American surgery is given in E. H. Pool and F. J. McGowan: *Surgery at the New York Hospital One Hundred Years Ago* (N.Y., 1930), pp. 63 ff. The surgery of this period was not always superior to the best in the later medieval period; see Jas. J. Walsh: *Medieval Medicine* (Lon., 1920), pp. 88 ff.

[2] Frances Long Taylor: *Crawford W. Long* (N.Y., 1928), pp. 70 ff.

[3] Packard, op. cit., II, 1075 ff. Schuman of New York has just published (1946) a work by Victor Robinson on this subject—*Victory Over Pain: a History of Anesthesia.* See also the last issue of vol. I of the *Jour. of Med. Hist.* (Fall, 1946), which is devoted to the history of anæsthesia.

were probably under greater pressure than surgeons to find some means for preventing pain. The latter usually served a patient only once, and then in a case of dire necessity. But dentists saw their patients recurrently, and must persuade them to accept treatments that were not absolutely necessary. If painless work could be assured, patients would accept further treatments, and professional opportunities would expand to the benefit of the public as well as of the dentists. It was a new technique of soldering gold teeth, involving the extraction of the old fangs, which led Morton to seek a painless method of performing this operation.

Once the inhalation of ether was accepted as a desirable means for producing anæsthesia, there was naturally an interest in experimenting with other gases for the same purpose. In 1847 Sir James Simpson of Edinburgh introduced the use of chloroform into surgical procedure. Since this gas was easier to employ than ether, it soon became the more popular of the two, and remained so until almost 1900.

It was obvious that anæsthesia was a major contribution to human welfare, and also of great technical advantage to surgeons. The latter were now free to try procedures which had hitherto been beyond them. There was some danger here in the pre-antiseptic decades, since surgical interference was now made easier, and so increased the risks from infection. But in the long run the new freedom did much to revolutionize surgery.

This was made more apparent when, in 1884, Sigmund Freud and Carl Koller of Vienna introduced the use of cocaine as a local anæsthetic.[1] There were certain types of work in which the employment of general anæsthesia remained dangerous or otherwise undesirable. Ophthalmologists, in particular, were afraid of it. Local anæsthesia was just what was needed here. Kaller came to America, and American surgeons were especially active in following his lead. They searched for other drugs or procedures which might possess greater advantages. Halsted developed conduction anæsthesia through nerve-blocking; while Corning (1885) and Matas (1899) used cocaine by spinal route. Crile and Cushing put the use of conduction anæsthesia into general practice after 1900; by which time the whole procedure had been made relatively technical and exact.[2]

American surgery was notable not only for the introduction of

[1] Freud: "Ueber Coca", *Centralbl. f. d. Gesamte Therapie*, II (1844). Here was another valuable drug, the knowledge of which, like that of cinchona, went back to American Indian lore.

[2] Garrison: *History of Medicine*, 4 ed., pp. 723 f.

anæsthetics, but also for the development of difficult operations rarely attempted in European practice. This was particularly true in gynæcology. Local conditions, in the form of the "peculiar institution" of slavery, were a factor in making possible Marion Sims's epoch-making operation for vesico-vaginal fistula (1849). Sims first experimented upon slaves, and it is interesting to find him remarking that in one case he had to purchase his patient in order to operate upon her. Vaginal fistula was, in those days of rather crude obstetrics, a relatively common and distressing condition, and Sims's success in correcting it was a great boon to many. The Alabama surgeon later demonstrated in Paris and other European centres, and finally located in New York City. Here, despite the indifference of professional leaders, he did much to establish modern gynæcology.[1] The work of Sims and of other American surgeons so impressed French authorities that when Samuel D. Gross visited the Paris hospitals in 1868, the surgeon Chassaignac told him that "America at the moment wields the surgical sceptre of the world".[2]

An interesting illustration of this surgical pre-eminence is afforded by the history of dentistry. The first serious efforts to develop dental surgery were made largely in Paris and in London during the eighteenth century; and it was fortunate that men trained in those cities came into American towns to practise during the Revolutionary period.[3] The work of such French army surgeons as Joseph Lemaire, who remained in Philadelphia in 1784 to teach and practise dentistry, set a new standard for Americans.

The development of dental education in the United States through the usual stages is well illustrated in the case of two leaders of the early profession. John Greenwood, who was George Washington's dentist, was largely self-trained. Under him as preceptor, Horace H. Hayden (1768–1844) served his apprenticeship, just as regular medical students served under physicians at the time. Hayden decided that a general medical course was really necessary if dental surgery were to receive the attention it deserved. He therefore attended lectures, and was granted an honorary M.D. at the Jefferson Medical College. He lectured on dentistry in the medical school of the University of Maryland in 1825, at which time there was probably no university in Europe which granted recognition to this specialty. Joining with other men who felt

[1] J. Marion Sims: *The Story of My Life* (N.Y., 1886), pp. 226 ff., 307 ff. Note also Robert Olshausen: *Ueber Marion Sims*, u.s.w. (Berlin, 1897).
[2] *Autobiography of Samuel D. Gross, M.D.* (Phila., 1887), p. 319.
[3] Karl Sudhoff: *Geschichte der Zahnheilkunde*, zweite aufl. (Leipzig, 1926), pp. 189 ff.

that dental surgery was worthy of professional rank, he helped to organize the Baltimore College of Dental Surgery in 1840. This granted the degree of D.D.S., and was the first institution of its kind in the world.[1]

That year was a fateful one in the history of dentistry, for at the same time that the Baltimore College was founded, groups in that city joined with others in New York and in Philadelphia to incorporate the American Society of Dental Surgeons. The *American Journal of Dental Science*, a pioneer publication, was then in its first year, and local associations of dentists were in process of formation in other cities.

The improvement of training facilities and of professional organization was naturally related to progress in dental technique. Better dental forceps were made by various physicians during the last decades of the eighteenth century. Porcelain teeth, obviously an improvement over animal or substitute human teeth, were first made in Paris about 1774, and were introduced into the United States by A. A. Plantou, who came from that city to Philadelphia in 1817. Shortly thereafter Charles Peale, the artist, also made molars of this material.[2] In 1825 Samuel Stockton began to manufacture false teeth in Philadelphia, and his firm eventually made as many as half a million in one year. Under him was trained Samuel S. White, who in 1844 began to manufacture—in a garret at the corner of Seventh and Race Streets—the most life-like false teeth yet made. From his business there eventually evolved the S. S. White Dental Manufacturing Company.[3]

Various materials were used as a base in which to insert false teeth, but gold became most popular after Gardette, another French army surgeon, introduced it in Philadelphia in 1787. Meanwhile Edward Hudson, an Irish political refugee living in the same city, advocated the saving of teeth as an alternative to extraction. During the first quarter of the nineteenth century American dentists followed him in a study of methods of filling, and by 1825 were commonly using gold for that purpose. This was the period when American dentists improved upon the work of Europeans, largely because the latter seem to have ignored the filling methods which meant so much in the preservation of natural teeth.

It has been suggested that the relatively rapid progress of American dentistry was due to the higher living standards already enjoyed by the "common man" in America. He was able to afford gold fillings, and

[1] I am following here the account in [James E. Dexter]: *A History of Dental and Oral Science in America* (Phila., 1876), pp. 10 ff.

[2] The porcelain teeth stage of American dentistry is described in Josiah F. Flagg: *The Family Dentist* (Boston, 1822). For the next decade, see Horace Kimball: "On the Present State of Dental Surgery as Practised in the United States", *Boston Med. and Surg. Jour.*, XI (1834), 39 ff.

[3] Dexter, op. cit., p. 26.

therefore encouraged dentists working with this material. Be this as it may, American dentists enjoyed other advantages. Since no professional discrimination was ever displayed towards surgeons in this country, the dentists rapidly attained professional and social recognition. In due time they also secured recognition abroad, and were in great demand in the chief Continental cities. The migration of these men—which reversed the usual direction of the transit of culture—was well illustrated in the interesting career of Dr. Thomas W. Evans of Philadelphia, who became the dentist of Emperor Napoleon III.

Another medical product of American conditions was Daniel Drake's unique work, *The Diseases of the Mississippi Valley* (1850–1854). To assemble the material for this work—one of the few great monuments of geographic pathology—Drake travelled over the entire Valley, investigating its climate, topography, anthropology, natural history, and diseases. "Nothing quite like it had appeared since Hippocrates wrote on Airs, Waters, and Places." [1] Herein one saw diseases as a part of natural history, against the geographic background of a new continent. Drake, like Walt Whitman, devoted himself to the American scene.

While Drake was travelling over the Middle West, that region was the site as well as the subject of important scientific investigations. Far out on its edge a young Army surgeon, William Beaumont of the Mackinac post, found it impossible to close a gunshot wound in the stomach of one, Alexis St. Martin. When St. Martin regained his health, despite a permanent fistula, Beaumont had sufficient imagination to realize the possibilities of the unusual case. He proceeded to use his patient's stomach as a laboratory for the study of gastric digestion, injecting and withdrawing materials through the fistula as occasion demanded. The living laboratory did not appreciate these experiments, and ran away a number of times; but Beaumont—who did not have much other apparatus—could not afford to lose him. St. Martin was finally found again, and the doctor continued his experiments. In this way he was able to demonstrate, in 1833, that the gastric juice contained hydrochloric acid, and that gastric digestion was essentially a chemical process. His work was well received abroad; and its significance, in connection with the contemporary work of Wöhler and others in physiological chemistry, was quite clear.[2]

[1] F. H. Garrison: "Contribution of the West to American Medicine". Lecture given before the Northwestern University Medical School, Nov. 8, 1928 (reprint, p. 168).

[2] Packard, op. cit., II, 1058 ff.; J. S. Myer: *Life and Letters of Dr. William Beaumont* (St. Louis, 1912), pp. 180 ff.

Of the studies made in this country, perhaps the most valuable were the official contributions of the Medical Department of the United States Army. Careful records were kept by this Department throughout the Civil War, and this made possible the publication (1870–1888) of the seven impressive volumes of the *Medical and Surgical History of the War of the Rebellion.* This struggle—unfortunately here given a partisan name—was one of the most sanguinary of modern times, and its medical records were very extensive. Virchow wrote of them:

> Whoever takes up and reads the extensive publications of the American medical staff will be constantly astonished at the wealth of experience therein found. The greatest exactness in detail, careful statistics even in the smallest matters, and a scholarly statement embracing all sides of medical experience are here united, in order to preserve and transmit . . . the knowledge purchased at so vast an expense.[1]

Much of the credit for initiating and completing the *Medical and Surgical History* was due to Surgeon-General Joseph K. Barnes, and to his successor, C. H. Crane. Barnes began another project which was to prove of even greater value. At the close of the War he found his department in possession of unused hospital funds. Seeking a proper place for spending the money, he decided to build up the department's small library. Eighty thousand dollars was devoted to the purpose, and Congress was later persuaded to make an annual appropriation. Most fortunate of all was Barnes's selection of John Shaw Billings, then a young army surgeon, as the man to develop the collection. Billings proved a versatile genius, who eventually attained first rank as a librarian, as a sanitarian, and as an authority on medical education and hospitals. He remained with the Surgeon-General's Library for thirty years, and purchased rare books from all over Europe at what now seem absurdly small prices.[2]

Some 80,000 volumes were accumulated by 1876, and a tentative catalogue was prepared. At about this period Americans were developing a superior library technique, and Billings was keenly alive to the service which improved catalogues could perform for medical readers. It is true that Haller, Sinclair, and others had made a good beginning during

[1] Quoted in Packard: *History of Medicine in the United States*, 2 ed., I, 650.

[2] Packard, op. cit., I, 850. See also F. H. Garrison: "Billings, A Maker of American Medicine", *Lectures on the History of Medicine: A Series . . . at the Mayo Foundation*, etc. (Phila., 1933), p. 192; and the same author's *John Shaw Billings, A Memoir* (N.Y. and Lon., 1915).

the eighteenth century in the study of medical bibliography. Yet as late as 1879, when the literature had increased enormously in all fields, European authorities had available in printed form only bibliographies for special subjects or authors. In that year Billings had sufficient material at hand to begin publishing the *Index Medicus*, a monthly index of new medical books and articles appearing throughout the world.[1] During the following year there appeared the first volume of the *Index Catalogue* of the library, which has since then run through many large volumes in four series.[2]

This catalogue was arranged with great care under both subject and author headings, and included select lists of periodical articles as well as of books and pamphlets. As the Surgeon-General's Library eventually became "a great national institution, probably the greatest and most useful medical library in the world", the *Index Medicus* and the *Index Catalogue* became the most valuable bibliographical publications in the medical sciences. In few other fields is the enormous volume of current literature, as well as the vast accumulation of the past, so well organized and so well under control.[3]

Despite such contributions to medicine in general, and such instances of originality, there was still much provincialism and mediocrity in American medicine during the half-century between 1840 and 1890. The truth is that Americans, as a result of the current exploitation of vast natural resources, had become a commercially-minded people, and were inclined to encourage only such science as was of obvious utility. The popular ideals were those of business success and of efficiency. This meant that in medicine there was respect for fashionable practitioners, but little more than toleration for physicians who dabbled in apparently useless clinical or pathological studies. Neither governments nor universities encouraged such "pure science"; and even those men who wished to do research were inclined to neglect it under the necessity of seeking prestige as well as income in practice. Under these circumstances only sporadic attention was given to original work. Prior to about 1890 no American city became a genuine centre of medical research, and very few American physicians can be said to have had a real research career.[4]

[1] Now continued in the *Quarterly Cumulative Index Medicus of the Jour. of the A.M.A.*

[2] Packard, op. cit., I, 650.

[3] They have, in consequence, great potential value to general historians as well as to medicine; R. H. Shryock: "The Value of Medical Sources to Social Historians", *Amer. Hist. Rev.*, XLI, No. 2 (April 1936).

[4] R. H. Shryock: "Factors Affecting Medical Research in the United States, 1800–1900", *Bull.* of the Soc. of Med. Hist. of Chicago, V (July 1943), 1 ff.; and *American Medical Re-*

This is another way of saying that American medicine remained colonial in nature until the end of the century. Side by side with the interest in things French, there continued an old dependence on the British. The common language was a potent influence here, since for every American doctor who observed French procedures in Paris there were countless others who read English works at home. A tendency to translate French works could not entirely change the situation.[1] Medical leaders were conscious of this, and the growing nationalism of the times moved them to no uncertain protest. When Sidney Smith inquired in the *Edinburgh Review*: "What does the world yet owe to American physicians or surgeons?" the *Philadelphia Journal of the Medical and Physical Sciences* accepted the implied challenge (1820) and printed the question thereafter on its title-page.[2] There was considerable talk about diseases peculiar to America, the opportunity for a distinctly American medicine, and the consequent need for medical as well as for political independence.

More than patriotic sentiment was involved. Medical men in the United States were all too inclined to "edit" English books, adding a few comments to the text and their own names to the title-page. In this way they masqueraded as original contributors to science. The practice elicited a spirited rebuke from Oliver Wendell Holmes, who declared in an able survey of medical literature in 1848 that most American writing consisted simply of "putting British portraits of disease in American frames".[3]

Perhaps the fact that the younger leaders of this generation had been trained in Paris, rather than in London, made them the more opposed to dependence on English books and journals. Like the literary men, they deplored the lack of an international copyright. This situation discouraged genuine American publications, since it was cheaper for American publishers to "pirate" British works than to pay American authors. The younger leaders in "the States" felt that this was a positive as well as a negative misfortune, since they were inclined to view English medicine as too conservative. They felt that it had failed to keep pace with the more critical trends in French theory and practice.

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search: Past and Present, chaps. 1–3 (to be published shortly for the Com't. on Med. and the Changing Order of the N.Y. Acad. of Med.).

[1] Gross: *Autobiography*, pp. 40 ff.

[2] Since 1827 the title has been the *American Journal of the Medical Sciences*. See John M. Armstrong: "The First American Medical Journals", Lecture on the Mayo Foundation, 1930 (reprint, p. 367). See also J. Eckman: "Anglo-American Hostility in American Medical Literature of the Nineteenth Century", *Bull. of the History of Medicine*, IX (1941), 31.

[3] Amer. Med. Asso. *Trans.*, I (1848), 283–8.

Bartlett declared, during the forties, that speculation was a peculiarly British failing; while Holmes was also quite critical of "those hard-dosing islanders". He felt that the blessings of therapeutic nihilism had not yet penetrated into the nooks and crannies of John Bull's practice.

There may have been some truth in this. The very fact that Englishmen felt less need of foreign training was likely to make them less responsive to French influence than were Americans. Yet British and Irish clinicians were actually moving in the same direction as the French, during the second quarter of the century. The story relates especially to the work done in a single London institution. The medical school in Guy's Hospital was established early in the century by Sir Astley Cooper and others, who made it independent of the parent institution at St. Thomas's. "The great men of Guy's" gradually established a fine tradition in anatomy, pathology, and surgery. While this tradition represented the development of standards already set by such men as the Hunters and Matthew Baillie, it was also in part a product of French influence. Thus the curator of the hospital museum, Thomas Hodgkin (1798–1866), was the first pathologist in England to follow Bichat's lead in investigating morbid changes in the tissues.[1]

Richard Bright (1789–1858) began his work at Guy's in 1820. By this time the post-mortem had become more nearly routine here than had been the case heretofore in British schools, and opportunities for systematic research in pathology were beginning to approach those in Paris. (Some 250 post-mortem examinations were performed annually at Guy's during the fifties.) Becoming interested in dropsy, Bright was able to differentiate this symptom-complex—hitherto thought of as a disease in itself—into "Bright's disease" (1827), acute peritonitis, tuberculosis of the peritoneum, and other specific conditions defined in pathological terms. His colleague, Thomas Addison, made similar studies in anæmia after 1837, differentiating pernicious anæmia and "Addison's disease". And while such progress was being made in London, a group of clinicians in Dublin—Corrigan, Stokes, Cheyne, and others—was doing equally brilliant work in pathology, notably with relation to the circulatory system.[2]

The importance of these studies is the more apparent, when it is repeated that they were at last making possible a rational nosography. This was usually achieved by a process of *analysis*, by breaking down the old symptom-complexes into various pathological elements defined

[1] Long: *History of Pathology*, pp. 156 ff.

[2] Long, op. cit., p. 163; James J. Walsh: *Makers of Modern Medicine* (N.Y., 1907), pp. 165 ff.

in terms of characteristic lesions. After Laënnec and Louis there was less discussion of general "inflammation of the chest" or "peri-pneumonia", and clearer recognition of such distinct conditions as pneumonia, pulmonary tuberculosis, and pleurisy. After Gerhard and Stillé, "fevers" were no longer classified simply as "intermittent", "malignant", "ship fever", and the like, since there was now a definite distinction between malaria, typhus, and typhoid. After Bright, there was less talk of "dropsies", and more understanding of the widely differing diseases in which œdema may appear.

Many other illustrations could be given. It is interesting to recall, for example, that John Hunter held the view that there was but one venereal disease. Had he not proved it by the inoculation of his own person? Ricord of Paris, however, clearly demonstrated the distinction between gonorrhœa and syphilis; incidentally showing that Hunter was probably so unfortunate as to have inoculated himself simultaneously with both diseases.

Occasionally an improved classification resulted from a *synthesis* of symptoms. A striking instance is afforded in the case of the disease just mentioned, syphilis. After Ricord had broken down Hunter's conception of a single venereal infection into two distinct disorders, Samuel Wilks of Guy's Hospital (1824–1911) proceeded to build up the clinical picture of one of these by showing that a wide range of hitherto unconnected lesions was really syphilitic in origin. The subsequent tracing of this insidious enemy of the race through the many body parts which it attacks partook of the nature of a medical detective story. There were at first few pathological clues, but the plot thickened as one after another was discovered. Only in the present century has the enormity of the villain been fully realized.[1]

The progress of pathology continued to be of major significance to the medical sciences in general. For one thing, it directed attention more and more to specific body parts—first to entire organs (Morgagni) and then to tissues (Bichat). Classical interest in the sick man as a whole was more and more lost sight of, as physicians became concerned with increasingly small anatomical parts. Indeed, pathologists came to think almost entirely of diseases as such, and of the sick *man* hardly at all. This was desirable up to a certain point for all the reasons suggested above, but it was eventually carried too far. For the clinician of 1850 tuberculosis was the same in all bodies.

Another consequence of the discoveries in pathological anatomy was

[1] Edward Vedder: *Syphilis and Public Health* (Phila., 1918), pp. 17–25.

a growing emphasis upon what would once have been termed solidism. Here again the classical emphasis upon the humoral pathology of Galen was more and more abandoned. To be sure, the distinction was no longer a matter of mere controversy; there was now cumulative evidence that disease was often located in the so-called solid parts. Yet there was a tendency to forget entirely the possibilities of pathological conditions in the body fluids.

Strangely enough, the last attempt to maintain a systematic humoral pathology was made by the very man who developed most completely the clinical-pathological studies characteristic of the years under discussion. This was Carl Rokitansky of Vienna (1804–1878). It will be recalled that two able physicians, Van Swieten and De Haen—both trained in Holland—had established the "Old Vienna School" about the middle of the eighteenth century. Hospitals and clinics were established. The *Allgemeines Krankenhaus* was founded in 1784, and came in 1795 under the direction of the great sanitarian, Johann Peter Frank.[1] Some 14,000 patients were passing annually through the institution at this time, and earlier limitations on pathological studies were largely removed. Here, obviously, were potential opportunities of an unusual order. Vetter, the first prosector, engaged in promising research, but became discouraged by professional opposition, and he and Frank left the hospital within a few years. During the first quarter of the nineteenth century, while the clinicians of Paris and London were building up an international reputation, little was accomplished in Vienna.

Then, in 1832, Rokitansky was appointed prosector in the general hospital. Educated at Prague and Vienna, he had been deeply influenced by Andral and others of the French school; and he plunged with enthusiasm into studies similar to those being carried out in Paris. Necropsies began to be performed on an unprecedented scale, eventually averaging 1,500 to 1,800 per year; and during the next thirty-five years Rokitansky actually wrote some 30,000 protocols.[2] If anything, he wrote too many, for the point may have been reached where haste led to diminishing returns in actual findings. Meanwhile another Bohemian, Josef Skoda (1805–1881), organized the hospital's clinical studies in an equally systematic manner. He also was inspired by the French clinicians, and pursued similar methods and investigations.

Indeed, under these two leaders the tendencies and interests of the

[1] Descriptions of the old Vienna school will be found in Max Neuburger: *Das Alte Medizinische Wien in Zeitgenössischen Schilderungen* (Wien, 1921).

[2] Long, op. cit., p. 172.

French school were carried to their logical extreme.[1] Never had there been so thorough a study of the fundamental branches of clinical medicine and pathological anatomy; nor so complete a disregard of therapy and the interests of the patient. It was rumoured that the whole *materia medica*, with Skoda, was nothing but cherry brandy or something of that sort. The general practitioners of the city were outraged; but French influence on the one hand, and the success of homeopathy on the other, were inclining critical men to abstain from drugs and to depend more and more upon "nature". Wunderlich, who had first made the work of Rokitansky and Skoda well known, tried to mediate in this quarrel between research and practice, by developing a sort of physiological *Heilkunde*.[2]

This did not prevent the tentative therapeutic nihilism of the French school from being made final. "Pure science" reigned supreme. No wonder that Rokitansky rounded out the pictures and definitions of disease which the pathologists of London, Paris, Dublin, and Philadelphia had begun to sketch in. After his day the names of many diseases suggested definite anatomical pictures rather than the old symptom-complexes.[3] No wonder, also, that Rokitansky and Skoda made Vienna the world's chief centre for research in pathological anatomy—a position maintained, to a considerable degree, into the present century.[4]

Yet, in the midst of all this research and in an atmosphere of such objectivity, Rokitansky elaborated a humoral pathology which was in essence almost as speculative as that of Galen or the scholastics. It is difficult to account for this apparent reversion in the last person in whom one would have expected it; but it should be observed that Rokitansky made no effort to build a whole "medical system" upon his theory, and that he was willing to abandon it when the evidence seemed to refute it. Without making any effort to explain a theory which today seems well-nigh inexplicable, suffice it to say that Rokitansky referred all diseases to an ultimate origin in some abnormal state of the blood. This view had at least some merit, in that it aroused interest in the possible values of blood chemistry, a field to which Andral had already given some attention.

[1] C. A. Wunderlich: *Wien u. Paris*, as quoted in Neuburger: *Die Wiener Medizinische Schüle im Vormärz* (Wien, 1921), pp. 223 ff.

[2] Georg Honigmann: *Geschichtliche Entwicklung der Medizin* (München, 1925).

[3] Carl Rokitansky: *A Manual of Pathological Anatomy* (transl. by W. E. Swaine, Phila., 1855). The section on the liver (II, 97 ff.) well illustrates the character of Rokitansky's work, and also suggests Louis's influence.

[4] Wilde: *Austria, Its Literary, Scientific and Medical Institutions* (Dublin, 1843), as quoted in Neuburger: *Die Wiener Medizinische Schüle im Vormärz*, pp. 24 ff.

Had such a view been advocated only half a century before, and had it been backed by a reputation like that of the Viennese clinician, the medical world would undoubtedly have witnessed the development of another complete "system". There would have been disciples and adoration on the one hand, critics and condemnation on the other. Of the writing of books there would have been no end. But it was far otherwise in 1850. A young German pathologist, Rudolf Virchow, examined Rokitansky's thesis, found it speculative and unsound, denounced it as "a monstrous anachronism" and—it simply disappeared. The author himself promptly abandoned it. There was in fact no other choice, unless he wished to take both himself and his theory without the pale of the regular profession.[1]

[1] The influence of the French school can be traced, in varying degrees, in the other countries of western Europe. In Italy it can be followed, for example, in the career of Giovanni Semmola of Naples (1793–1865), who was a pupil of Magendie, and like him was devoted to animal experimentation; Castiglioni: *History of Medicine*, p. 712. French influence in Spain and in Spanish America is described in Altamira: *Historia de España y de la Civilización Española* (Barcelona, 1914), pp. 353 ff.

Indigenous medical science was not well established in Russia until about the middle of the nineteenth century. During the early part of that century there were no strong research centres in the country; and although foreign physicians were imported, French influence does not seem to have been outstanding. See, for example, Franz Dörbeck, "Die Anfänge der Medizin in Russland und deren weitere Entwicklung", *Archiv für Geschichte der Medizin*, II (1909), 417 f.

XI

MODERN MEDICINE IN GERMANY

1830–1880

GERMANY, ENGROSSED in the philosophy of nature, was at first less stirred by the scientific trends of the new century than were her western neighbours. The surge of patriotic emotion which followed defeat at the hands of Napoleon brought with it a reaction against French thought as well as against French military power. In the place of positivistic and sceptical analysis, there welled up an idealistic synthesis which appealed to the romantic mood of the German people. Let the *philosophes* doubt and deny if they would; German thinkers would create constructive philosophies that would at once bring all of art and all of science within the compass of an idealistic *Weltanschauung*. The effect of such romanticism upon science in general, and on medicine in particular, has already been suggested. For nearly a generation German physicians remained philosophically aloof from the achievements of the French school.

Yet when the response to positivism did come, east of the Rhine, it developed with a suddenness and a sweep unlike anything which had occurred before. It was as if the weight of metaphysical speculation, pressed too far, had suddenly released a long-pent reaction against it.

There had been premonitions, to be sure, that such a reaction might occur. The consciousness that French science was more advanced than that in the Fatherland was not an altogether pleasing one. Herder deplored, in 1801, that modern chemistry was of French origin, and that French terms must be employed in this subject even in Germany.[1] There were always individuals who desired to preserve a real science of nature, as distinct from the prevailing philosophy of nature. Notable

[1] "Wäre Lavoisiers' System der Chemie bei uns enstanden, so hätten Wir ihm namen gegeben, jetzt müssten wir fremde Worte nachsprechen und nachmodeln"; quoted by Robert Stein: "Naturwissenschaftliches bei Lessing u. Herder", in *Historische Studien u. Skizzen zu Natur—und Heilwissenschaft: Festgabe Georg Sticker*, u.s.w. (Berlin, 1930), p. 116.

among these were some of the anatomists in the medical faculties. Their subject was of a substantial sort, and was calculated to hold their thoughts down to earth, the while some of their colleagues soared on the wings of speculation. So Rudolphi, of Berlin, remained for a time apart from the philosophers and insisted upon the prime importance of anatomy to medical science.

The influence of such individual sceptics was gradually reinforced, during the twenties and thirties, by that of students returning to Germany after training in Paris. The scientific supremacy of the French metropolis eventually attracted many German medical students, even as it did Americans and other foreigners. Not only did students cross the Rhine, but physicians seeking post-graduate opportunities came in increasing numbers to attend the lectures and visit the hospitals of Paris. A considerable literature developed in Germany, just as it had in England and in the United States, in an effort to explain "*das unendliche Labyrinth des medizinischen Paris*".

German students and physicians were amazed at the contrast between the work of French clinicians and surgeons and the armchair theorizing of their own land. Never, they reported, had there been anything like the new medicine of Paris. They were especially impressed with the systematic character of hospital studies, with the manner in which Louis and others were checking the results in numerical terms, and with the way in which such procedures were eliminating vestiges of the speculative systems. They found that sweeping monistic claims were no longer tolerated. Did a certain M. Jadelot, at the foundling's hospital, assert that he could diagnose all conditions simply by the appearance of the face? Very well, then, the absurd simplicity of this scheme proved the man a charlatan. And if France would no longer tolerate such system-makers, why should the Fatherland? Should Germans remain content with a philosophy of nature, while their western neighbours developed real science? Such questions seeped into Germany with every medical student and traveller who returned from Paris during the second quarter of the century.[1]

Among the Germans trained in Paris in this period were such leaders as Liebig and Alexander von Humboldt. Other outstanding scientists

[1] Examples of this interesting German literature on Parisian medicine will be found in J. H. Kopp: *Aerztliche Bemerkungen veranlasst durch eine Reise in Deutschland u. Frankreich*, u.s.w. (Frankfurt am Main, 1825); G. F. Schwalbe: *Journal meiner Reise nach Paris im Sommer 1831* (edited by Ernst Schwalbe, Rostock, 1910); S. J. Otterburg: *Das Medizinische Paris: Ein Beitrag zur Geschichte der Medizin und ein Wegweiser für Deutsche Ärzte* (Carlsruhe, 1841). The reference to Jadelot above is from Schwalbe, p. 89. For interest in Louis' numerical method, see Otterburg, pp. 60–62.

were influenced indirectly by Paris, through their education in Vienna. Such men, returning to Prussia, Bavaria, or the Rhineland, inaugurated a new scientific era in the years following 1825. Conditions were most discouraging at first, it is true. Helmholtz afterwards recalled that his medical education "fell within a period of the development of medicine when among thinking and conscientious minds [in Germany] there reigned perfect despair". The old theories were by this time demolished, and the example of the physical sciences pointed the way to rebuilding, but the task seemed almost beyond them! [1]

Fortunately there were factors that aided the Germans in this difficult period. There was, first, their university system, which proved a peculiarly effective means to the organization and direction of systematic research. Nothing quite like it existed in the other Western nations.[2] Nearly all the more significant research in France was concentrated within Paris. In England medical work tended to centre in the London hospital schools, where it was more or less divorced from the related sciences. Few, if any, American universities were yet worthy of the name. Scandinavian institutions were analogous to the German, and continued to flourish as scientific centres throughout the nineteenth century; but they were relatively few in number, and suffered from linguistic handicaps in a period when the modern languages had come into general usage in scientific circles. In Spain it was chiefly the University of Madrid which assumed importance as a medical centre—especially towards the end of the century under the leadership of Cajal.

The eighteenth century had witnessed an increase in both the number and the activity of German universities. Scientific faculties were well established, and the correlation of their varied interests easily provided for. By 1800 there existed some twenty or more institutions offering excellent opportunities for scientific research, once the spirit of a positivistic science could be introduced. This opportunity was seized upon by the younger men returning from Paris and Vienna, who gradually filled the chairs vacated by older men of philosophical tendencies. The former, with all the enthusiasm of proselytes, immediately displayed a tendency to improve upon their foreign masters. At the same time something of the nationalistic feeling aroused in the preceding generation carried over into this era and inspired them with a

[1] Quoted in Merz: *History of European Thought in the Nineteenth Century*, II, 388.

[2] By "German" here are meant all universities in which instruction was given primarily in the German language. The Italian universities were similar in some ways; but were relatively less productive in this era, perhaps because of economic and political difficulties in Italian life.

continued enthusiasm. They were working for Germany as well as for science.[1]

Fully conscious of the importance of research, as distinct from arm-chair theorizing, they eagerly strove to improve the conditions of objective investigation and of teaching. Not only did they leave the study and go into the workroom, but they so improved the character and facilities of the latter that they created (1825) the first genuine laboratories in the modern sense of the term—excepting only the older ones devoted to anatomy. The value of laboratories to systematic research in chemistry and medicine was soon demonstrated. It is difficult to realize, on the other hand, that there were no teaching and research laboratories in England, even in chemistry, prior to 1844.[2]

It is clear enough, then, that German medical men were first influenced by foreign training, and that they later benefited by organizing university laboratories as research centres. It is not so certain just what impression, if any, the older *Naturphilosophie* left upon the sciences as they developed after 1830. The younger men were usually educated in the speculative school before going abroad, and traces of earlier views must at times have survived into their mature years. It has been suggested that when Germans borrowed the exact methods of the French they were especially inclined to apply them to subjects of human concern, because the philosophical and ethical tradition of Kant and Hegel had maintained a high and serious interest in all that pertained directly to man.[3] The view has also been taken that certain definite achievements in German medicine can be traced to the influence of philosophical doctrines. Perhaps Virchow's conception of the essential unity of disease patterns, in terms of a cellular pathology, represented an empirical fulfilment of philosophical views about the unity of all phenomena.[4]

It is quite likely that some of the general ideas elaborated by philosophers proved, in the hands of laboratory men, to have definite value. Has it not been the historic function of speculation to suggest and keep alive ideas, until such time as they could be tested by experience? An illustration is afforded in the case of the ætiology of infections. Disease

[1] Richard Graf Du Moulin Eckart: *Geschichte der deutschen Universitäten* (Stuttgart 1929), pp. 351 ff.; for the more recent period, see also Theodor Billroth: *The Medical Sciences in the German Universities* (transl. by Wm. H. Welch, N.Y., 1924).

[2] William H. Welch: "The Evolution of Modern Scientific Laboratories", *Papers and Addresses*, III (Baltimore, 1920), 203 ff.; Tilden: *Famous Chemists*, p. 192.

[3] Merz, op. cit., I, 216.

[4] Walter Pagel: "Virchow u. die Grundlagen der Medizin des XIX Jahrhunderts", *Jenaer Med. histor. Beitr.*, XIV (1931), 44 ff.; Diepgen: "Virchow u. die Romantik", *Deutsche Med. Wochens.*, no. 32 (1932).

had sometimes been viewed by the philosophers of nature as a sort of living, parasitic entity, inseparably associated with man. This idea, exaggerated as it was, might lead somewhere when subjected to real investigation. The transition here, from theory to fact, was actually accomplished in the career of one man. Johann Schönlein was taught as a youth the theory of parasitic disease entities. Gradually freeing himself from the speculative point of view, he nevertheless was able to show the element of truth it had contained when he demonstrated, in 1839, that a human skin disease (favus) was actually caused by a living parasite.[1]

The transition from speculative thinking to science is even better illustrated in the career of Johannes Müller (1801–1858). Although he began as a student of the *Naturphilosophie* at Bonn, he was eventually responsible, more than any other one man, for the introduction of the modern scientific spirit into Germany. An early interest in anatomy and physiology was combined with an enthusiasm for Schelling and Goethe, as a result of which he was convinced that physiology and philosophy must be made one. It was, he felt, the function of physiology to study living phenomena as a whole, in terms of a grand concept of life which unified and gave meaning to the data of experience. This concept was the creation, not of ordinary understanding, but of philosophical thought operating "*durch ein Organ höherer Art, und dieses subigiert jeden Stoff, wo ihn der empirische Mensch nur häuft*".[2] All this was pure *Naturphilosophie*, and as foreign to the later work of Müller as it was to the contemporary attitude of Laênnec or Louis.

After completing his work at Bonn, Müller continued his training at Berlin, where he came under the influence of the critical Rudolphi. He became familiar also with the work of Magendie, who—unlike most of the French school—devoted himself to physiological investigations. At this stage of his career Müller naturally found Magendie too mechanical and sceptical, and blind to the values of generalization. Magendie was, nevertheless, performing a great service in emphasizing the importance of experimental physiology and functional pathology; and it is hardly possible that such brilliant studies as his work on the functions of spinal nerves (1822) failed to impress the young German physiologist.

[1] Honigmann: *Geschichtliche Entwicklung der Medizin*, pp. 67–70.

[2] Johannes Müller: *Physiologie des Gesichtssinnes* (1826), p. 19, quoted in Martin Müller: *Über die Philosophischen Anschauungen des Naturforschers Johannes Müller* (Leipzig, 1927) p. 8. This work traces carefully the whole development of Müller's philosophical and methodological point of view. On his general career, see W. Haberling: *Johannes Müller* (Leipzig, 1924).

Müller returned to Bonn in 1824, and taught there until 1833, when he succeeded Rudolphi at Berlin. He read Berzelius, and the exact, quantitative procedures of the Swedish chemist so impressed him that he later credited Berzelius with freeing him from the toils of Schelling and the speculative school.[1] Müller now proceeded with zeal, employing both physical and chemical laboratory procedure, to the investigation of human physiology. Following Haller and Magendie in their interest in nerve activity, he confirmed (1831) the views which the latter and the English physiologist, Bell, held regarding the spinal nerves; that is, that the anterior roots of these nerves are motor, the posterior sensory in function. Later, he demonstrated that each sense organ is related to its own peculiar sensation and to no other. The discovery of this "law of specific nerve-energies" illustrated his ability to generalize; a power which was lacking in Magendie's excessively empirical procedure, and which may conceivably be credited to the German scientist's earlier philosophical training.

Müller did not limit his research in physiology to the study of nerves. He experimented, for instance, on the vocal cords. Nor did he limit himself to physiology as a whole. Turning to anatomy, he was one of the first to seize upon the use of the new, achromatic microscope, and so gave an impetus to those histological studies which were to prove a distinguishing feature of German medicine. He worked out, by 1830, the finer anatomy of both the glandular and cartilaginous tissues; and in 1838 published a pioneer work on the histology of tumours. He thus superseded Bichat, and prepared the way for the cellular pathology of his own pupils, Virchow and Schwann.[2]

Meanwhile, Müller published (1834–1840) his comprehensive and original *Handbuch* on human physiology. This incorporated all the advances made in the field since Haller, such as the work on nerve functions just noted, the new light on respiration resulting from the chemistry of Lavoisier and others, Magendie's discoveries in osmosis, the early findings in the physiology of digestion, and so on. He showed that physiological research was reaching farther into the recesses of body functions than it had when the more obvious features of a few great systems were the only objects of investigation. "More especially, however, was the new handbook distinguished by its method", observes a contemporary historian, who adds: "So far as Germany was concerned, it denoted a turning away from natural philosophy and to-

[1] Martin Müller, op. cit., p. 16.

[2] An impressive list of his outstanding achievements is given in Garrison: *History of Medicine*, 5 ed., pp. 451, 452.

wards observation and experiment".[1] For by this time Müller was definitely committed to a rational empiricism, to the employment of both chemical and physical investigations of living phenomena, and to the use of only such theories and generalizations as were checked carefully against the data of experience.

It is true that Müller continued to believe there was an essential difference between physical and chemical phenomena on the one hand, and biological phenomena on the other. The purposeful nature of life seemed to necessitate the assumption of some innate principle peculiar to it. This "vitalism" may be interpreted as a vestige of Müller's earlier point of view, since the trend of positive science was already setting, by 1840, in the other direction. Most biologists were coming to feel that vitalism was useless, even dangerous to the objective experimenter. Magendie and other French authorities held such a view; and R. H. Lotze proclaimed it in Germany, in contributing to Wagner's *Handbuch* on physiology in 1843.

This reaction away from speculative idealism quite naturally tended, thereafter, towards scientific scepticism or actual materialism. To understand the whole sweep of German thought that was involved, with particular reference to medicine and biology, one should begin by reading Schelling and end with Ernst Haeckel. Perhaps Müller may be represented as poised between the two extremes. For his vitalism was in no sense mystical, but was based rather, as he saw it, on empirical grounds. And he was quite willing that mechanistic explanations should be pushed, so far as they seemed to fit the facts.[2] Much the same thing may be said of Humboldt and of Liebig. Hence they worked just as the mechanists worked. From this time on, one may view the differences between vitalists and mechanists as relating largely to philosophical interpretations, rather than to programmes or methods of research.[3] Perhaps both attitudes will be resolved, ultimately, in a new synthesis; as in the view that life can be conceived only in terms of purpose, but that purpose itself can be best defined in an objective and even mathematical manner.[4]

Müller was a most versatile and productive investigator. Although

[1] H. E. Sigerist: *The Great Doctors: A Biographical History of Medicine* (N.Y., 1933), p. 309.

[2] Martin Müller: *Über die Philosophischen Anschauungen des Naturforschers Johannes Müller*, pp. 49, 50.

[3] Diepgen: "Vitalismus und Medizin im Wandel der Zeiten", *Klin. Wochens.*, X (1931), 1433 ff.

[4] This is the position of the American philosopher, Edgar A. Singer, Jr.; see his *Mind as Behaviour* (Columbus, Ohio, 1924), pp. 53 ff. The general history of vitalism is given by one of its modern advocates, Hans Driesch, in his *Geschichte des Vitalismus* (1922).

credited with no outstanding discoveries, he became "one of the great all-round medical naturalists". He was equally prominent in comparative anatomy, psychology, physiology, and pathology; and it has been estimated that his literary output finally amounted to over 15,000 printed pages. So outstanding were his own students that through them may be traced the main currents in modern German medicine.[1] The histologists, Schwann, Henle, and Virchow were all trained under Müller; so also were the physiologists Du Bois-Reymond, Helmholtz, and Brücke. And the major achievements of German medicine during the next generation were to be made in histology and physiology.

Observations on the cellular structure of plants had been made as early as the seventeenth century, but the real study of living cells had to await the invention of the achromatic microscope. Shortly after this became available (1831), the Englishman Robert Brown discovered the plant-cell nucleus; and its significance was emphasized by Matthias Schleiden of Hamburg. Meanwhile Theodore Schwann, Müller's prosector at Berlin, followed his master in using the microscope, and was rewarded by the discovery of nucleated cells in animal tissues.[2]

As a result of the findings of Schleiden and Schwann there ensued, during the thirties and forties, an intensive study of cell structure. This involved a differentiation of cell wall, nucleus, chromosomes, and protoplasm; and led to the view that the cell was both the structural and physiological unit of all living organisms. Outstanding was the work of Jacob Henle (1809–1885), who followed in Bichat's footsteps in investigating tissue structure. He first described (1836) the epithelia of the skin and intestines, and showed that this tissue makes up the lining membrane of all free surfaces of the body and also of the inner lining of its cavities. He went farther than Bichat, since he used the microscope systematically, and viewed the tissue cells in their functional and developmental, as well as in their structural relationships. Henle and his contemporaries later benefited by the development of improved techniques in the staining, hardening, and cutting of tissues. His *Allgemeine Anatomie* (1841) was the first text on microscopic histology, and in a sense prepared the way for all later work in that field.[3]

The significance of the microscopic approach was soon apparent. Henle's prosector, Kölliker, immediately carried the study of cell struc-

[1] Garrison, op. cit., pp. 451, 452; Sigerist, *The Great Doctors*, pp. 307–10.

[2] An interesting brief account of Schleiden and Schwann is given in Victor Robinson: *Pathfinders of Medicine* (2 ed., N.Y., 1929), pp. 383 ff.

[3] George W. Corner: *Anatomy*, pp. 42 ff.; Garrison, op. cit., pp. 457, 458. A concise account of the development of histological technique is given in E. R. Long: *History of Pathology*, pp. 211 ff.

ture over into embryology. And it remained only to apply this view to pathology in order to revolutionize that science.

This was primarily the achievement of Rudolf Virchow (1812–1902), who succeeded Johannes Müller as the dominating figure in German medicine. Trained under the latter, Virchow was devoted to an objective methodology from the start. He was only twenty-five when his criticism demolished Rokitansky's speculative doctrine of "crasis"; and a year later (1847) he established his famous *Archiv* for pathological anatomy, physiology, and clinical medicine. Like his great French contemporary, Claude Bernard, he was highly conscious of the importance of combining critical observation and experiment with an equally cautious and critical employment of "working" hypotheses. The views of these two leaders finally formulated, at least for the present era, the standards of medical methodology which had been disputed throughout the centuries.[1]

As a young man, Virchow was much interested in the humanitarian aspects of medical practice; and hence was more concerned than some French and Austrian clinicians had been with the search for treatments and cures. If these were ever to be found, the study of diseased structures must be supplemented by the study of diseased functions. In the last analysis, disease was a phenomenon of life, and must be studied in relation to life-processes. Virchow therefore undertook researches in pathological physiology which he hoped might prove of therapeutic value.

After taking his degree in 1843 he became prosector at the Charité in Berlin three years later, and there began investigating the diseases of the blood and its vessels. Following Müller and Henle, he worked systematically with the microscope and demonstrated the nature of leukæmia—a fatal disease characterized by a great increase in the number of leucocytes (white blood corpuscles). He also threw light on the perplexing problems of thrombosis and embolism, through a series of studies which involved observation in the wards, animal experimentation, and the use of clinical statistics.

Prior to this there had been no adequate understanding of the origin of blood-clots in the vessels, and of their interference with the normal mechanism of the circulatory system. Clotting of blood within living

[1] See especially chaps. 1 and 2 ("Observation and Experience" and "The A Priori Idea and Doubt in Experimental Reasoning") in Claude Bernard: *An Introduction to the Study of Experimental Medicine* (transl. by H. C. Greene, N.Y., 1927). Cf. J. M. D. Olmsted: "The Contemplative Works of Claude Bernard", *Bull. of the Inst. of the Hist. of Med.* of Johns Hopkins Univ., III (1935), 335 ff.

vessels had been observed long before, and John Hunter had noted its common association with inflammation. The French pathologist Cruveilhier even held that all inflammation began with some sort of blood coagulation. If this could not be found in the veins, he blamed it upon a hypothetical capillary condition. Virchow concluded that inflammation of the veins was neither the cause nor result of coagulation within their walls; but that coagulation resulted rather from slowing of the current in various bodily conditions.

While this view had later to be modified, it led the German pathologist to a further study of thrombi, as they were formed in various parts of the vascular system. He found that a central softening of the clots was often followed by a detachment of fragments that might be carried in the blood-stream to remote vessels. Fatal results ensued, in case large masses became impacted in the main arteries of the lungs. To such impacted fragments, whatever their size, he gave the name of "emboli"; and the process of obstruction, whether by thrombi or other means, has since been known as "embolism".

Virchow was also able to show that the formation of abscesses in the lungs or other parts (pyæmia) was due to this transfer of pyogenic thrombi from one inflamed region to another point by the blood-stream. It is interesting to observe, in passing, that Virchow was unable to find objective proof of small emboli in the lung abscesses; but demonstrated his thesis by resort to clinical statistics relating to an outbreak of puerperal fever. "The regular association of abscesses in the lungs with softening thrombi in the veins around the uterus, and regular absence in just as severe cases where the inflammation was limited to the uterine lymphatics, furnished overwhelming proof that pyæmia, a mystery to physicians from the day of its recognition, was a simple consequence of the transfer of solid particles from one inflamed region to another point. . . ." [1] Here, again, Louis's "numerical method" proved itself invaluable as a means of observation, where ordinary instruments (including the microscope) had failed.

There remained to be solved the old problem of the nature of inflammation in general. In examining tissue under the microscope, Virchow noted the apparent changes in component cells. He therefore ascribed the essence of inflammation to such conditions as are now known as "cloudy swelling" and "intra-cellular œdema".[2] This view, like many others held by Virchow, was soon proved incorrect, and his whole work in this field was limited by his ignorance of the infectious

[1] E. R. Long: *History of Pathology*, p. 196. [2] Ibid., p. 198.

character of the processes with which he was dealing. Nevertheless, his interest in the inflammation of cells led eventually into his whole programme for the study of cellular pathology.

At this stage of his researches Virchow became involved in the disturbances of the Revolution of 1848, lost his post at the Charité, and accepted instead a chair at Würzburg. Here, during the next seven years, he so devoted himself to researches in cellular pathology that it is almost forgotten, at times, that he ever was a physiologist. Since Schwann and others had found that the cell was the unit of normal animal life, it seemed to follow that it must be the centre of abnormal life as well. And what was abnormal life but disease itself? Abnormal stimuli of some sort—bacteriology would later describe some of these —must affect the cells, producing therein lesions, paralysis, or other disturbances. It was therefore sheer ontology, declared Virchow (just as Broussais had once claimed), to speak of tuberculosis or other diseases as entities in themselves. All that Virchow could find, in a case of tuberculosis, was abnormal cell life, or the death of cells, within the tissues.

To prove this view, he demonstrated more or less typical changes in cell structure or function in various diseases. He also demonstrated that all cell growth originated in preceding cells, and thus threw light on the nature of tumours of all types. His general findings and point of view have remained basic to pathological studies to this day. Pathology, which had once consisted of speculations about humours and solids in general, and had then moved on to the organs and later to tissues, at last seemed to come to a final basis on the ultimate components of organic structure.[1]

The man who had demonstrated this basis was recalled to Berlin in 1856, made the director of the first independent pathological institute, and became, as a present writer puts it, "the pope of German medicine". In the midst of such dignity and achievement, it was naturally overlooked that one of Virchow's first purposes, the actual cure of disease, had not been realized to any degree in the course of his researches. This objective tended to be lost sight of, as he became more and more absorbed in pathological and anthropological studies.

This did not mean that the entire trend in German medicine was in this direction. Research in physiology and in therapeutics was carried on with notable success by Virchow's contemporaries. A great day

[1] Rudolf Virchow: *Cellular Pathology*, etc. (transl. by Frank Chance, N.Y., 1860), pp. 27 ff.; Wm. H. Welch: "Rudolph Virchow, Pathologist", *Papers and Addresses*, III, 365 ff.

dawned when, in 1847, Brücke, Ludwig, Helmhotz, and Du Bois-Reymond came together in Berlin and determined to "constitute physiology on a chemicophysical foundation, and give it equal scientific rank with physics. . . ." [1] Through their efforts a modern school of iatrochemists arose in Germany, just as a new school of iatromathematicians had arisen in France. Purkinje's work on the physiology of the senses, that of the brothers Weber on the muscles and acoustics, and the findings of Johannes Müller, had already shown that German physiologists could rival the achievements of Magendie, and of the English physiologists Bell and Marshall Hall. The Berlin group approached the field with rare enthusiasm, and with a full appreciation of chemicophysical procedures.

There were still peculiar difficulties to be overcome in making physiology a physical science. An example is afforded in the experiments of Rosenbach (1851–1907), who found himself more or less baffled by psychic variables in his efforts to study with ergostats the classic problem of the working power of the heart. Ludwig later admitted that the whole task of physiological investigation "turned out to be much more difficult than we had anticipated"; but there was to be no long lag or turning back, as there had been in the eighteenth century.

Hermann von Helmholtz was ideally fitted to pursue research in a physiology intended to attain "equal scientific rank with physics". He was really a physicist who happened to receive medical training—in order to support himself as an army surgeon—and who thereafter combined an interest in the two fields, to their mutual advantage. His first outstanding work demonstrated the law of the conservation of energy (1847), confirming the views which had already been advanced by the physician Robert Mayer in the course of the latter's famous voyage to Java (1840). Securing the chair of physiology and pathology at Königsberg, Helmholtz promptly gave up medical practice and devoted himself to the application of physical studies to living organisms. Especially notable were his measurements of the velocity of nervous conduction and of reflex action. Out of such studies developed the new psychophysics, with which the names of Fechner and Wundt were so closely associated.

It was natural enough that Helmholtz was deeply interested, not only in experimentation, but also in improving methods of observation and in measuring results. Such interests could be taken for granted among physicists; but it was still unusual to find them combined in the work

[1] Carl Ludwig, quoted in John Fulton: *Physiology*, p. 87.

of any one medical scientist. Thus Laënnec developed an instrument of observation, but was not particularly concerned with the measurement of his data. Louis was enthusiastic about measurements, but developed no observational instruments as such. Neither of them employed any systematic experimentation. Even Müller depended largely on passive observation, although in much of his work this was no doubt the only method to employ. Helmholtz, in contrast, represented the complete scientist, in that he persistently experimented, constantly sought for improved instruments and techniques, and always sought to measure his findings.[1]

An excellent illustration is afforded in his remarkable studies of the sense organs, to which he turned after completing his work on nerve function. In 1851 came his invention of the ophthalmoscope, and a year later that of the ophthalmometer. These two devices made possible, for the first time, the observation of the interior of a living organ and the measurement of its intricate mechanism. A science of ophthalmology was now made possible, and a stimulus was afforded to the search for other instruments which would permit a direct observation of various organs. Thus in 1854 a London singing teacher, Manuel Garcia, produced the first laryngoscope, and this instrument was developed for clinical use by Johann Czermak and other physicians.

Helmholtz followed his studies of the eye with similar investigations of the ear, carried out chiefly during the sixties. But after 1871, having been appointed to the chair of physics at Berlin, he devoted himself largely to pure physics. In this science both he and his American student, Willard Gibbs of Yale, made outstanding contributions to thermodynamics and electrodynamics. Others carried on in the now well-established work of physical investigations in physiology. Thus Emil Du Bois-Reymond (1818–1896) practically established electrophysiology, in which field little had been achieved since the early discoveries of Galvani and Volta.

The chemical approach to physiological studies was pursued with equal vigour between 1840 and 1870. While German physiologists were active in this aspect of their subject, the outstanding leader was Claude Bernard (1813–1878), the student and successor of Magendie in Paris. This was the period when Liebig and Wöhler were so successfully developing organic chemistry in general, and when the time was ripe for its application to specific studies on the living organism. Bernard was primarily a physiologist, and pursued either physical or

[1] See J. G. M'Kendrick: *Helmholtz* (New York, 1899), pp. 25 ff.

chemical investigations as the occasion demanded, but his best-known work was usually of a chemical character.[1]

This was the case with his famous analysis of digestive processes, in which he employed the same systematic animal experimentation as had his master Magendie. He began with a study of the gastric juice, then proceeded to an investigation of the saliva and of the pancreatic fluid. Interested in the fate of carbohydrates in food, he was able to show that sugar—in the form of glycogen—was stored in the muscles and liver, and that certain amounts were always present in the blood. This prepared the way for later studies of diabetes, which in turn resulted in major therapeutic achievements. In 1849 Bernard discovered that the puncture of a spot in the medulla oblongata resulted in overloading the blood with sugar; that is, it produced an artificial diabetes. This was followed by an investigation of the influence exerted by the nervous system upon other organs, with especial relation to the function of the vasomotor nerves.[2]

Bernard's investigation of blood conditions likewise led him into a study of various poisons and their methods of operation. Here again he employed extensive animal experimentation. He found, in general, that poisons acted upon particular parts of an organism rather than—as had hitherto been assumed—upon the body as a whole. Carbon monoxide, for instance, acted simply upon the hæmoglobin of the red blood cells. More important, perhaps, was the discovery that remedies also acted in the same manner. Once the locale in which a drug operated was known, it could be given with more discretion. Working along this line, he made pharmacology for the first time an experimental and a relatively exact science. Finally, the operation of drugs upon diseased tissues could be studied in just the same way as their influence upon healthy parts. Hence experimental pharmacology became closely associated with the relatively new field of experimental pathology.[3]

Here, at last, was a demonstration which had long been needed in rational medicine; that is, that the problems of therapeutics could be successfully approached from a rational or theoretical angle. Bernard's studies in pharmacology were far superior to the older trial-and-error investigations, since the former proceeded systematically and followed

[1] See Charles-V. Daremberg: *Histoire des Sciences Médicales*, I (Paris, 1870), 488 f.

[2] Bernard: *Leçons sur Les Propriétés Physiologiques et Les Altérations Pathologiques des Liquides De L'Organisme*, II (Paris, 1859), 337 ff., and other sections of the same work.

[3] Sigerist: *The Great Doctors*, p. 320. On the general career of Bernard, see Sir Michael Foster: *Claude Bernard* (Lon., 1899).

a clue to its logical termination. They revealed just when a drug was operating, and how.

It is significant that the development of physiological research, of both a physical and chemical character, tended to correct the over-emphasis upon purely clinical and anatomical studies. Primarily important as the latter had been in modernizing the spirit of the medical sciences, they had led to an over-emphasis upon the pathology of the solid parts, rather than on that of the body in its entirety. While the study of physiology led at times to similar results, as in the work on poisons mentioned above, it tended in general to call attention to the operation of the complete organism.

The progress of physiology also afforded some hope of escape from the therapeutic nihilism of French and Austrian clinicians. Ultimately medicine had to return to its prime function of aiding the healing art. The appearance of an experimental pharmacology, arising out of research in physiology, pointed in this direction. German pathologists reacted against the apparent indifference of their colleagues in Paris and Vienna, and rallied to the slogan: "*Wir wollen heilen, und nicht klassifizieren*". After all, patients were human beings as well as "subjects"; and even pure science should, in the long run, serve their interests.

XII

MEDICINE AND THE PUBLIC HEALTH MOVEMENT

1800–1880

RUDOLF VIRCHOW was one of the German authorities who felt that medicine should cure as well as classify. Absorbed as he was in research, he still found time to be much concerned with the state of society in general, and with the social significance of medicine in particular. When a serious outbreak of typhoid occurred in the mill districts of Silesia, in 1848, Virchow was appointed by the Prussian Government as head of a special commission of investigation. Here he found those tragic conditions which, years later, were to be so effectively portrayed in Gerhardt Hauptmann's drama, *Die Weber.* Virchow, with all his skill, had no remedy for typhoid; but he saw at once that the disease was in some way caused by the insanitary surroundings amid which the weavers lived. Had these conditions been corrected in time, the disease would never have appeared. Even if some treatment had been available, science could have done far more in this case by preventing disease than by curing it.[1] The young pathologist returned to Berlin convinced that the humane trend in medicine must express itself, not only in a revived interest in therapeutics, but also in a concern for prophylaxis on a public scale.

The idea, of course, was not essentially new. The preventive ideal had been clearly presented in the first volume of Frank's *Medicinische Polizey* (1779), and the later eighteenth century had witnessed some of the most striking achievements in the realization of this ideal. English physicians, following a programme of education and prevention, and working in co-operation with lay philanthropists, had apparently reduced the infant death rate of their nation nearly fifty per cent during the thirty years between 1770 and 1800.[2]

Yet both medical and lay interest in public-health work seemed to

[1] Rudolf Virchow: "Mittheilungen über die in Oberschlesien herrschende Typhus-Epidemie", *Arch. f. path. Anat.* u.s.w. (Berlin, 1848).

[2] See Chap. V.

lag somewhat in the early decades of the nineteenth century. Somewhat encouraging was the adoption of vaccination in all Western nations; but even this remained at first a matter of private practice, and only a minority of people were directly benefited. Meanwhile, disease problems were becoming more serious among the working classes. The advent of industrialism, first in Great Britain, then in France and the United States, and finally in Germany and other Continental nations, brought an obvious change in the social order. The rapid growth of manufacturing towns either found responsible authorities indifferent to proper planning, or caught them unawares and powerless.

No adequate adjustments were made, in most cases, for the increasing complexity of such problems as housing, ventilation, street-cleaning, water supply, drainage, and sewage disposal. The early efforts of civil engineers to solve these questions sometimes only made matters worse; as when the improvement of pipes, steam pumps, and water-closets led to the contamination of London sewers and the Thames water supply after 1785.[1] English-speaking cities, in which there was little realization of the need for medical police,[2] were notorious for the growth of evil slums. Insanitary surroundings were combined, in such neighbourhoods, with dire poverty and with an apparent increase in drunkenness and crime.[3]

A contemporary description of living conditions in an English slum may make the situation more real than would lengthy descriptions of a general nature. In 1849, the London *Times* published the following appeal for aid, the details of which were later corroborated by that paper:

> Sur,—May we beg and beseach your proteckshion and power. We are Sur, as it may be, livin in a Wilderness, so far as the rest of London knows anything of us, or as the rich and great people care about. We live in muck and filth. We aint got no priviz, no dust bins, no drains, no water-splies, and no drain or suer in the hole

[1] M. C. Buer: *Health, Wealth, and Population in the Early Days of the Industrial Revolution*, p. 108; G. W. Fuller: "Water Works", in "Historic Review of the Development of Sanitary Engineering in the United States", Amer. Soc. of Civil Engineers, *Trans.*, XCII (1928), 1211.

[2] See Chap. V.

[3] Similar reports to this effect were made from widely separated lands: for instance, from Great Britain, Prussia, and the United States. For the British story, see J. Wade: *Police and Crime in the Metropolis* (Lon., 1829), pp. 207 ff.; Sir Benj. Richardson: *The Health of Nations*, etc., I (Lon., 1887), 110 ff. Increased drinking in Germany is described in A. Fischer: *Geschichte des Deutschen Gesundheitswesens*, II, 578 ff. The rather amazing story of drunkenness and crime in American cities is well summed up in A. C. Cole: *The Irrepressible Conflict* (N.Y., 1934), pp. 154, 160, etc.

place. The Suer Company, in Greek St., Soho Square, all great, rich powerfool men take no notice watsomdever of our complaints. The Stenche of a Gulley-hole is disgustin. We all of us suffer, and numbers are ill, and if the Cholera comes Lord help us.

Some gentelmans comed yesterday, and we thought they was comishioners from the Suer Company, but they was complaining of the noosance and stenche our lanes and corts was to them in New Oxforde Streete. They was much surprized to see the sellar in No. 12, Carrier St., in our lane, where a child was dyin from fever, and would not believe that Sixty persons sleep in it every night. This here seller you couldent swing a cat in, and the rent is five shillings a week; but theare are greate many sich deare sellers. Sur, we hope you will let us have our cumplaints put into your hinfluenshall paper, and make these landlords of our houses and these comishioners (the friends we spose of the landlords) make our houses decent for Christians to live in.

Praeye Sir com and see us, for we are livin like piggs, and it aint faire we shoulde be so ill treted. . . .[1]

The conditions noted in this humble but graphic protest were by no means exceptional in the slums of the day. It was inevitable that the vicious circle of poverty, drunkenness, and crime should be completed by disease. British history again affords the most striking illustrations, though similar accounts could be given of the United States and of Continental nations. After about 1815 the British infant death rate again began to rise, and the crude death rate rose with it. So far as locality was concerned, the chief factor in these increases was obviously the high mortality of cities. It had long been realized, for instance, that disease was more serious in London than in rural areas. For a considerable period the population of the metropolis was maintained only by fresh immigration from the more healthy country districts. As the early decades of the nineteenth century passed, the mortality in the northern manufacturing towns was also rising.[2] The surroundings of urban populations in general were evidently dangerous to health and happiness. "It is proven beyond doubt", observed the best-known English statistician of the period, "that if the population be the same in other

[1] London *Times*, July 5, 1849, quoted in D. D. Jones: *Edwin Chadwick and the Early Public Health Movement in England* (Iowa City, Iowa, 1931), p. 117. It has been suggested that this statement may have been composed as reform propaganda. If so, it is consistent with much other evidence.

[2] This was noted before 1800; see Sinclair: *Code of Health and Longevity*, pp. 111 ff.

respects, an increase in density implies an increase in mortality; and that the ratio of increase . . . is as certain roots of density." [1]

The increase in mortality did not, in itself, tell the whole story. Where there were many deaths, there was still more serious sickness, and behind the latter an incalculable amount of "sub-clinical" illness—all of which was related to poverty and crime and to the general quality of the population. British military authorities noted, before 1850, the physical inferiority of men recruited from urban districts. That this phenomenon was not peculiar to England was indicated by the submission of essentially similar reports by Prussian officials.[2] Much as we may ridicule it to-day, the Victorian tradition that cities were dangerous places was essentially a valid one.

The infant death rate was not the only factor making for increases in urban mortality. Various "fevers" seemed to be taking an increasing toll of the adult population, especially among the working classes in British industrial towns. Typhus and typhoid, as yet undifferentiated, apparently became more serious towards the end of the eighteenth century. Special "fever wards" were opened in the Chester infirmary in 1784; and most British cities found it necessary thereafter to provide wards or hospitals for fever patients. Over 14,000 cases of typhus were admitted to the London Fever Hospital as late as the decade 1861–1870.[3] This disease had been responsible for those devastating outbreaks in ships, jails, and other congested living quarters, that so terrified people during the preceding century. Occasionally it spread through whole towns in the fashion that alarmed the Prussian authorities in 1848, and which suddenly focused Virchow's attention on the whole problem of public hygiene.

On the other hand, the fevers usually increased slowly outside of institutions, or flared up only in the poorer sections of towns, and so did not at first attract much attention from the ruling classes. Pulmonary tuberculosis, which was by this time succeeding smallpox as the great plague of the age, was even less likely than were other fevers to arouse sudden fear. It produced no alarming superficial lesions, like leprosy, nor did it strike with the fatal suddenness of the "Black Death". This "consumption" spread insidiously, killed slowly, and rarely drove

[1] Wm. Farr, in the *Prov. Med. and Surg. Jour.*, VII, 441 (1844). See also his similar statement in the *Sixteenth Ann. Rep't. of the Reg. Gen.* (Lon., 1855), pp. 144–53. The exact formula may be expressed: $m' : m :: \sqrt[6]{d'} : \sqrt[6]{d}$. Urban health has so improved recently that it no longer holds; see Sir Arthur Newsholme: *Vital Statistics* (Lon., 1923), pp. 280 ff.

[2] *Report on . . . the Sanitary Condition of the Labouring Population* (Lon., 1842), p. 186; Fischer: *Geschichte des Deutschen Gesundheitswesens*, II, 311.

[3] Sir Arthur Newsholme: *Evolution of Preventive Medicine* (Baltimore, 1927), p. 132.

people to take public action against it. What was evidently needed in order to revive the public health movement was a salutary epidemic of the first magnitude.[1]

Yellow fever fulfilled most of these specifications, when it ravaged American seaport cities in a series of epidemics during and after the year 1793. The disease appeared in a mysterious manner, spread rapidly, produced alarming symptoms, and was highly fatal. Townspeople were literally terror-stricken in its presence. During the famous Philadelphia outbreak of 1793, which was in many ways the worst calamity of the kind ever suffered by an American city, even the departments of the Federal Government deserted the city. Physicians, as was to be expected, differed on the question of origins; but many ascribed yellow fever to local filth or marsh miasmas, rather than to importation and contagion. There was, in consequence, a clamour for sanitary improvements—for cleaning and paving streets, and for improving water supplies. Philadelphia was actually frightened into building an effective water system. This was the first in the United States, and was certainly one of the first in the whole English-speaking world which supplied water pumped by steam at public expense.

Other American cities followed French and Italian examples [2] in attempting drainage operations. The most remarkable instance of this was in the case of Savannah, where the "wet culture" of rice in or near the city was gradually eliminated between the years 1817 and 1823. In so doing, this small Southern seaport achieved a unique record, for it apparently reduced the death rate caused by "autumnal diseases" from the amazing average of 91 per thousand, to about 28·5.[3] There was one disconcerting thing about early sanitary improvements in American cities, however, and this was that they did not prevent the occasional return of yellow fever.

With this problem still pending, as it were, American officials suddenly found themselves facing a new and even more serious question of disease control, which loomed up on the European horizon. Asiatic cholera had been endemic in India for centuries, was pandemic in Asia between 1816 and 1830, and spread over Russia during this last year.

[1] See M. P. Ravenel: "Endemic Diseases Versus Acute Epidemics", *Amer. Jour. of Pub. Health*, X (1920), 761 ff.

[2] See Hallé et Nysten: "Histoire de l'Hygiène publique", *Dictionnaire des Sciences Médicales*, XXIII (Paris, 1818), 549.

[3] These figures are for three-year averages, and are of course by no means exact. As indicative of trends, however, they are probably significant. See Thomas Gamble, Jr.: "History of the Municipal Government of Savannah, 1790–1901", (incorporated in the Mayor's *Annual Report* for 1900), pp.144 ff.; also W. Duncan: *Tabulated Mortality Record of the City of Savannah* (Sav., 1870), p. 36.

Just why it first set out on its travels is still unknown; but once started, the disease followed an itinerary laid down by trade routes. It entered Germany from Russia, and took ship at Hamburg for the British Isles in 1831. Thence it accompanied emigrants to Quebec and to New York in the following year. Meanwhile it spread to France and to other Continental nations.

The mystery of its origin, its frequent termination in sudden death, and its ominous, irresistible advance across continents and over seas made cholera the most feared of all diseases. Not without reason was it known as "the scourge of nations". Wherever one turned, it was the same story. When it appeared in Paris, for instance, 7,000 deaths occurred within eighteen days! Public halls were filled with the bodies of the victims, there were fears of poisoning, riots occurred, and over 100,000 persons were said to have fled the city.[1] The world over, people turned desperately to physicians for some means of treating the disease. Would medical science be equal to the emergency?

Now it so happened that this was the very period when French nihilism dominated the best-informed medical circles—when critical physicians doubted the ability of medical art to do much against cholera or any other serious disease.[2] Little could be expected, therefore, from what would now be termed "the great research centres". Busy practitioners, of course, tried anything which tradition or their own ingenuity suggested. Everywhere they sought for some cure or palliative procedure, but could find no ground for general agreement. Opium was employed in Glasgow and condemned in London; approved in Augusta, Georgia, and spurned in Savannah in the same State. French authorities recommended a dozen drugs which they admitted were not specifics, but which they hoped might be better than nothing.[3] One American practitioner, after trying sixteen different medicines and a half-dozen special procedures in addition, observed philosophically that "it has been well impressed on my mind that there is *no one* remedy for cholera".[4]

If there was no hope for cure, the need for prophylaxis was all the more pressing. Hence the very failure of therapeutics, the very nihilism of the dominant French school, may have promoted the search for a

[1] Garrison: *History of Medicine*, 4th ed., p. 655, quoted Heine's famous account.
[2] See Chap. VIII.
[3] Noirot: *Exposé Des Travaux des Conseils D'Hygiène Publique . . . Du Departement de la Côte-D'or, 1849–1859* (Dijon, 1861), p. 107.
[4] C. H. Stone: "Report on Epidemic Cholera in the Vicinity of Natchez", *Southern Medical Reports*, I (New Orleans, 1850), 364.

means of prevention.[1] In the case of choléra, the slow approach of the disease afforded ample time to prepare against it. England had been warned by an outbreak in Bengal as early as 1819. As the disease approached Europe in 1830, the upper classes began to ponder old warnings against the slums, and feared that if an epidemic started therein it might spread to the better parts of towns.[2] In June 1831 a royal proclamation was issued in London warning the country against the approach of the disease, and a temporary national board of health was appointed with powers of advice and investigation.[3] This marked a step forward in health administration, since here for the first time the British Government created a body analogous to the medical councils of Spain and of the German States. The British national board ordered town authorities to appoint local boards, which were also to investigate, keep records, and to advise Government officials. Parallel administrative developments in France resulted in the creation of similar public health councils in the various *départements* of that country.[4]

While physicians were not always appointed on such boards, they were usually consulted on the proper measures to be taken. If medical men could not provide a cure, perhaps they could tell lay administrators something about the causes of cholera and how it could be excluded from a community. The reply was none too clear. In some way the disease was associated with insanitary surroundings; but in some way, also, it invaded a country after the manner of a contagion. Consequently medical advisers recommended the old quarantine-and-isolation procedures, but stressed also the need for sanitary improvements.

This simultaneous advocacy of sanitation and of quarantines, of clean-ups and of isolation, revealed a confusion as to the ætiology and transmission of cholera that was typical of the times, and similar to that which had already appeared in the case of yellow fever. Was this new plague due to filth and noxious airs, or to contagion? Both views had their advocates. If the disease were not contagious, was it carried primarily by airs, or by water, or by some other means? In cholera, as in yellow fever, certain phenomena suggested contagion, while others seemed to deny it.[5] Looking back, it is easy to find an explanation in

[1] This was pointed out about the middle of the century by a German medical authority, N. Friedrich of Heidelberg; see Fischer, op. cit., II, 323.

[2] Friedrich Engels: *The Condition of the Labouring Classes in England* (1844), p. 41. Engels's work is quite consistent with Chadwick's *Report* of 1842.

[3] C. Creighton: *History of Epidemics in Britain*, II (Cambridge, Eng., 1894), 794.

[4] Indeed, some organization of this sort was provided in all progressive nations. In Sweden, for example, the regular town health boards (set up after 1800) were looked to for regulations against the epidemics.

[5] The debate re cholera is summarized in Sir Arthur Newsholme: *The Story of Modern*

water transmission in the one case, and in insect carriers in the other; but lacking such answers, no problems were more difficult of solution.

The medical advice given to local authorities was, despite its ambiguity, applied with some vigour. This was especially true in the case of the larger cities. Thus the Manchester Town Council, fearful of their slums, conducted in 1831 one of the earliest general investigations of living conditions ever made in an English-speaking city. Some 7,000 houses were examined, and such terrible conditions were revealed that there was no time to clean up the Augean stables before the cholera arrived.[1] Glasgow, at the same time, appointed a health board which within a year raised £8,000 and provided over 200 beds for the care of cholera patients. This board also enforced isolation procedures, closed the theatres, and solemnly advised against evening sermons.[2]

When the cholera finally broke out in the British Isles in July 1831, it attacked chiefly the poor and destitute classes. It was most severe in Scotland and in Ireland, where the slum conditions were relatively worse than in England. The death rates were not as high as in the old plague pandemics, but were bad enough in such towns as Glasgow, Edinburgh, Dublin, and Limerick. Glasgow, with a population of 202,000, had 6,200 cases and 3,000 deaths—228 deaths were reported in one week of August. The approximate mean death rates were, for England, 1·57 per 1,000; for Ireland, 2·57; and for Scotland, almost 4.[3]

The experience of Continental countries and of the American nations was similar on the whole; the disease moved like a contagious invasion, but struck with greatest force among the poor. Fresh air, cleanliness, pure food and water—in some way these conditions seemed to protect a community. Quarantines and isolation, on the other hand, appeared a failure. The "scourge of nations" passed with seeming ease over border quarantines established between Russia and Prussia, and between the different German States; and English and American port quarantines proved equally ineffective. Even the closing of the pulpits in devoted Glasgow proved of no avail.

The connection between bad living conditions and cholera was too obvious and too dramatic to be overlooked. After 1831 there was a sudden increase of interest throughout Europe and America in the

Preventive Medicine (Baltimore, 1929), pp. 83 ff. An interesting picture of the even greater uncertainty re yellow fever will be found in the sixty pages of debate reported in the *Proceedings* of the Third National Sanitary Convention in the U.S.A. (N.Y., 1859), pp. 23–83.

[1] Engels: *Conditions of the Labouring Classes in England*, p. 41.

[2] Creighton: *History of Epidemics*, etc., II, 808.

[3] Ibid., II, 815.

whole problem of public hygiene. Fear now combined with humanitarianism to demand investigations, clean-ups, and general sanitary reform, as these things had never been demanded before. Whenever enthusiasm waned, further invasions of cholera, supplemented by occasional outbreaks of yellow fever, typhoid, typhus, and smallpox, terrified authorities into renewed activity. In these circumstances is to be found the genesis of the modern public-health movement.

More than this, the need for sanitary reform emphasized the need for social reform in general. Those who protested against economic and social injustice frequently saw in the disease situation the most striking occasion of their protests. Hence the liberal movements that developed in all Western nations between 1820 and 1848, in opposition to the reactionary governments of that period, were closely associated with the movement for sanitary reform. It was no accident, for instance, that such a medical leader as Virchow sympathized with the Revolution of 1848; or that, on the other hand, Friedrich Engels prepared his indictment of English society largely in terms of unnecessary disease and death.[1]

The English sanitarians who became prominent in this period were not usually in direct association with such radical reformers as Place, Owen, and the Chartists. But like the latter, the sanitarians were brought face to face with the basic problems of poverty. There was, in consequence, much latent socialism in the views of such leaders as Chadwick, Simon, and Southwood Smith. Simon admitted that he could not solve the problem of poverty, but sounded the economic plea for social control when he observed:

> If given wages will not purchase such food and lodgment as were necessary for health, the rate payers, who sooner or later have to doctor, or perhaps bury the dead labourer, when starvation or filth diseases have laid him low, are in effect paying the too late arrears of wages which must have hindered the suffering and sorrows.[2]

Dr. Southwood Smith, in some ways a more remarkable leader than any of his colleagues, demanded in even stronger language recognition of the people's right to decent living and health. In 1847, indignant at continued indifference in Parliament, he penned an "Address to the Working Classes . . ." which still rings with something of the tone of Shelley's "Song to the Men of England". After reviewing the tragedy of unnecessary suffering and death among the masses, he continued:

[1] *Die Lage der Arbeitenden Klassen in England* (1844). I have used above the English translation.

[2] *English Sanitary Institutions*, p. 254.

> For every one of the lives of these 15,000 persons who have thus perished during the last quarter, and who might have been saved . . . those are responsible whose proper office it is to interfere and endeavour to stay the calamity—who have the power to save, but who will not use it. But their apathy is an additional reason why you should rouse yourselves. . . . Let a voice come from your streets, lanes, alleys . . . that shall startle the ear of the public and command the attention of the legislature.[1]

Inspired by such feeling, Chadwick, Smith, and their associates led a great social reform which was in some ways the most essential of all the contemporary ameliorative movements. Although political and economic historians have tended to view the sanitary programme as incidental to industrial reform, this view would seem to involve a somewhat distorted perspective. Men did not demand sanitary reform simply in order to improve factories—rather did they urge factory legislation as part of a far wider, comprehensive programme of health reform. "Not even the Poor Law or the Factory Question," declares a modern English historian, "go so deep into the national life." [2]

The first stage in the public health movement was one of investigation on a grand scale. Governmental authorities must learn the worst about living conditions and disease among the masses, in order to inaugurate improvements. While the rank and file of physicians were rarely much concerned with public hygiene, the lead in investigations was taken by individual medical men who combined humane sentiments with a scientific interest in disease prevention. Virchow's report on Silesia was paralleled by more sweeping inquiries made in Great Britain and in the United States between 1835 and 1850. In the former nation, Drs. Arnot, Kay, and Southwood Smith submitted striking reports on the causes of fevers in London. These were followed in 1842 by a comprehensive survey of national health as a whole. This took the form of a report to the Poor Law Board from all the poor-law physicians in England, on the disease conditions in their respective "unions".[3]

In the United States the National Institute at Washington tried for several years to secure a similar national survey. Failing in this, the In-

[1] Quoted in B. L. Hutchins: *The Public Health Agitation, 1833–1848* (Lon., 1909), pp. 109, 110.

[2] Hutchins, op. cit., p. 58. The Poor Law Act of 1834, of course, had itself possessed public health significance. See Sir Allen Powell: "The Relation of Public Assistance to Public Health", *Amer. Jour. Pub. Health*, XXI (1931), 1315 ff.

[3] *Report on an Inquiry into the Sanitary Condition of the Labouring Population of Great Britain* (Lon., 1842). The English narrative history can best be followed in Sir John Simon: *English Sanitary Institutions* (Lon., 1897), pp. 183 ff.

stitute persuaded the newly formed American Medical Association to examine conditions in the larger American cities. The resulting reports indicated clearly that just such poverty and disease as obtained in London, Manchester, and Glasgow also disgraced Boston, New York, and Philadelphia, and that the death rates were even higher in the American cities.[1]

In some cases medical investigators co-operated effectively with lay reformers. Thus the reports of the English poor-law doctors were directed and interpreted by the great lay sanitarian, Edwin Chadwick. In like manner a public health survey of Massachusetts made in 1850—in some ways the most complete study of the period—was inaugurated in part by the Massachusetts Medical Society, but was carried out by the layman, Lemuel Shattuck.[2]

Such lay sanitarians as Chadwick and Shattuck viewed their work in a manner quite as scientific as that of the physicians, although their approach was usually a mathematical rather than a medical one. Thus Chadwick, influenced by Bentham and his humane "utilitarianism", became interested in health conditions through a study of benefit societies among English working-men.[3] In the course of this work he noted the difficulties experienced in securing mortality data necessary to voluntary insurance projects. This led him into a study of vital statistics, which in turn drew his attention to the general field of public hygiene.[4] Shattuck, a school teacher, became interested in vital statistics in the course of writing a history of the town of Concord. He later made statistical and sanitary studies of Boston, Lawrence, and Lynn. He was active in the American Statistical Association, and had the support of that body, as well as of the State medical society, in securing legislative authorization for his health survey of 1850.[5]

In some ways the statistical approach to public hygiene seemed more effective than any other then available. Louis and other physicians had shown that clinical statistics were invaluable in therapeutics; it now appeared that vital statistics would perform a similar service for studies

[1] Amer. Med. Assn. *Trans.*, I (1848), 305–10.

[2] *Report on the Sanitary Condition of Massachusetts* (Boston, 1850).

[3] The social approach to the whole public health problem, 1800–1840, is well described for England by George Newman: *Health and Social Evolution* (Lon., 1931), pp. 98 ff. Fischer presents the parallel story in Germany; but there is no similar study for the United States.

[4] See his article, "Life Assurances—Diminution of Sickness and Mortality", *Westm. Rev.*, IX (1828), 384 ff.

[5] I have enjoyed reading Shattuck's MS. autobiography, covering his earlier years, through the courtesy of his granddaughter, Miss Clara B. Dobson, of Pasadena, Cal. His various papers are collected in the library of the New England Genealogical Society. For a brief appreciation see M. E. M. Walker: *Pioneers of Public Health* (Edinb., 1930), pp. 59 ff.

in public hygiene. Indeed, too great a distinction between lay and medical sanitarians should not be made in this connection, for physicians often led in adopting statistical procedures in this field as well as in clinical research.

Thus Villermé, inspired by the same general interest in statistics as influenced Louis, had made a series of studies in 1828, showing the relationship between neighbourhoods and disease in different parts of Paris and in the provinces. He was able to prove that morbidity and mortality rates were, in a sense, a function of the living conditions of a given class.[1] These investigations made by a medical authority afforded an example to the layman, Chadwick, when he made his statistical analysis of the English poor-law union reports in 1842. Chadwick found an undeniable relationship between class and mortality in English cities, similar to that already noted in Paris. It appeared that in Liverpool the average age of "gentry" at death was thirty-five years, while that of labourers was only fifteen. "The difference in the ages of these several classes", observed the English sanitarian, "presents to my mind a tolerably exact scale of the difference in their abodes."[2] Here social statistics were affording a service to ætiology just as real—if not quite so specific—as would bacteriology at a later day. Filth and overcrowding were proved to act, in some manner not yet made clear, as the indirect causes of fevers.[3] This view had been held by earlier observers as a result of individual observations and shrewd inference, but it could be finally proven only in mathematical terms.[4]

So impressed was Chadwick with the value of vital statistics, that he became much concerned with securing systematic and reliable data for the nation as a whole. Others shared this interest, especially after the first cholera epidemic precipitated a demand for definite information on the spread of the disease. The office of the Registrar-General of Births, Deaths, and Marriages was established in consequence in 1836.

Outstanding in the development of this office was the work of William Farr. Originally trained as a physician in London, Farr also studied in Paris, where he became interested in statistical work relating

[1] L. R. Villermé: "Mémoire sur la Mortalité en France, dans la Classe Aisée et dans la Classe Indigente", *Mémoires de L'Acad. Roy. de Méd.*, I (Paris, 1828), 51 ff. See also his *Tableau de L'Etat Physique et Moral des Ouvriers Employés dans les Manufactures de Coton, de Laine et de Soie* (Paris, 1840).

[2] *Report* of 1842, pp. 154 ff., 168.

[3] This is what an eighteenth-century physician would have termed the "ultimate", as distinct from a "proximate" cause.

[4] Just as, in therapeutics, many observers had decried blood-letting as a result of individual observation; but their view could only be proven by resort to clinical statistics. See Chap. IX.

to public hygiene. In a word, he also came under the influence of the enthusiasm for quantitative procedures then so common in the French capital. Returning to London, he attempted to give lectures on "hygiology" but with no success, and had to content himself with private practice and continued research in demography. In 1838 Farr published an article "On Prognosis" which well illustrated the rapidly increasing use of statistics in relation to public health problems. He was much interested in studying mortality and morbidity rates, not only for smallpox, but for all important diseases, and in calculating probable risks in terms of the different age groups, sexes, and seasons.[1]

Two years later Farr, partly through Chadwick's influence, was appointed compiler of abstracts in the Registrar General's office, and was regularly consulted by the census authorities from 1841 on. Resigning at once from private practice, he was soon known as an authority on vital statistics; and was later largely responsible for the gradual improvement in accuracy and extension in the range of British census reports.[2] These reports eventually revealed to some extent the seriousness of poverty and disease in British manufacturing towns, and thus supplemented the official investigations of slum conditions undertaken in 1842 and thereafter.[3]

Farr came to realize that regular statistical reports were essential to any systematic public health control. He was by no means unique in this respect. The same general point of view, indeed, was dawning on the medical men and sanitarians of every progressive land. Reference has already been made to the valuable studies in vital statistics made in France and in Sweden. In the United States the national census gave no figures on the causes of death, but the newly formed American Statistical Society worked vigorously for State legislation along that line. Massachusetts provided for the registration and reporting of vital statistics in 1843. The American Medical Association, immediately after its organization in 1847, appointed sub-committees to press the issue in every State, and eight responded between 1850 and 1860. Un-

[1] *Brit. Med. Almanack* (Lon., 1838), pp. 199 ff.

[2] See M. C. Buer: *Health, Wealth, and Population in the Early Days of the Industrial Revolution* (Lon., 1926), pp. 15 ff. Interesting observations on English vital statistics prior to Farr's time are found in T. H. Marshall, "The Population Problem during the Industrial Revolution", *The Econ. Jour.* (Econ. Hist. Ser., No. 4). Jan. 1929, pp. 429 ff. The standard work on Farr is N. A. Humphries: "Biographical Sketch of William Farr", in the former's edition of Farr's *Vital Statistics* (Lon., 1885), p. xiii. The article on Farr in the *Dict. of Nat. Biog.* calls attention to the character of his Paris training.

[3] Typical of the medical opinion of these reports, was the comment of an editor of the *Brit. and Foreign Medico-Chir. Rev.* (V, 1850, 216–22): ". . . these ranks of impressive figures . . . present to us a true picture of the present condition of our country and nation".

fortunately the extreme individualism, and in many cases the plain ignorance of democratic assemblies, prevented favourable action in the less progressive States. When a registration bill was introduced in the Georgia legislature in 1849, for instance, the members "fairly hooted at the idea", and opposed it as just another "trick of the doctors".[1]

More rapid progress was made in the chief European nations. Official data were collected in Prussia from 1817 on, and in 1843 J. G. Hoffmann published a general report on the same. More complete vital statistics were issued in 1860 and thereafter, by a newly founded national statistical bureau in Berlin.[2] The first reports from the British Registrar-General's office showing causes of death were compiled by Dr. Greenhouse in 1856, and were issued by a national health board established a few years prior to that time.[3] Statistics showing the supposed causes of death were also issued in Italy during the sixties.[4] But most reliable for that country, as for the United States, were the statistical statements of the larger cities and the studies based thereon by individual physicians.

As statistical evidence against slums accumulated in all Western nations, a demand for further research into the causes of epidemic diseases ensued; and with this appeared an enthusiasm for administrative reform along such lines as medicine could already lay down. The problems of public health received increasing attention at the meetings of medical societies, and increasing space in their publications.[5] Because this interest was not always sustained, and also because of a natural desire for specialization, separate public health associations were founded between 1840 and 1860. Such organizations proved to have an additional advantage, in that they brought physicians and laymen together to their mutual advantage. A group composed of doctors, lay sanitarians, and public officials was more likely to secure action in a community than was a purely professional society.

Typical of the former groups were the "health of towns" organizations in England, the "sanitary associations" of the larger American cities, and the provincial health associations in Germany. These bodies

[1] R. H. Shryock (Ed.): *The Arnold Letters* (Durham, N.C., 1929), pp. 36–9. (*Papers of the Trinity College Hist. Soc.*, XVIII–XIX.)

[2] Fischer: *Geschichte des Deutschen Gesundheitswesens*, II, 421, 422. The census bureau had been established at Berlin in 1805.

[3] Simon: *English Sanitary Institutions*, p. 274.

[4] Gabriel Millot: *De L'Hygiène Publique at de la Chirurgie en Italie* (Paris, 1876), pp. 34, 35.

[5] Thus the American Medical Association appointed (1847) a standing committee on public hygiene; the German Association of Natural Scientists and Physicians set up a section on medical reform (1865) and one on public health (1867), etc.

held meetings, published proceedings, reports, or journals, and at one and the same time promoted scientific discussions and actual sanitary improvements.

The best work of the unofficial organizations between 1840 and 1870 was naturally achieved along local lines. First of all, they set out to arouse public opinion. In meetings and publications they sought to popularize the findings of various investigations, and to arouse the educated classes to a realization of the danger of unnecessary disease. Something of the romantic spirit which pervaded other social movements of the time was infused into projects for sanitary reform. There was no lack of "horrible examples" calculated to arouse fear or indignation, and there is no question that those who cited them were often inspired by moral enthusiasm of a high order. A lay leader of the New York Sanitary Association referred to their programme as "a crusade against a gigantic and growing evil".[1] That some public interest was aroused is suggested by the appearance of articles in the daily Press, describing the meetings of public health organizations and discussing the problems with which they were concerned.[2]

On the other hand, the predominance of medical men lent an objective and realistic tone to the discussions of the sanitary associations, in contrast to the romantic enthusiasms of contemporary social reformers. The American temperance drive, for example, had also related largely to health reform in the early days, when Dr. Benjamin Rush was its chief protagonist; but when leadership was assumed by the clergy it acquired an ecstatic character in sharp contrast to that of the major public health movement. The latter, although it represented by far the more comprehensive programme, was never likely to arouse such popular support as could the campaign against "the demon rum".[3] Sanitarians, it may be observed parenthetically, were quite conscious of the physical evils of intemperance, but do not seem to have been so concerned with the problem as are some modern public health authorities.[4] Perhaps the former, with so many things to worry about, felt that the liquor question was already taken care of by temperance enthusiasts.

[1] *Proceeds.* of the Third Nat. San. Conv. (N.Y., 1859), p. 229.

[2] See, e.g., the *Phila. North American*, May 13, 1857; the *Baltimore Republican*, April 29, 1858; the editorials in the *N.Y. Herald*, Aug. 27–30, 1859, etc.

[3] See John A. Krout: *The Origins of Prohibition* (N.Y., 1925), Chaps. IV and V. Note also the source quotations in Chap. I of D. L. Colvin, *Prohibition in the United States* (N.Y., 1926).

[4] Cf. Haven Emerson: "Prohibition and Public Health", the *Survey*, XLI (N.Y., 1926), 298 ff.

Certain voluntary groups were concerned with direct action, as well as with attempts to arouse public opinion. Numerous societies were founded after the manner of other welfare agencies, to construct model baths or to improve the housing of "the industrial classes". Especially good work along this line was accomplished in London during the forties and fifties.[1] But the most usual procedure was to encourage local authorities to set up boards of health and to construct water and sewerage systems. Permanent boards of health, which in a few cases had been established even before 1800, became the rule in large towns by the middle of the century; and where they were willing to co-operate with local public health and medical societies they sometimes secured results that were most encouraging. Contemporary improvements in sanitary engineering, as in the methods of constructing pipe-lines and in estimating the volume of flow, were of great value in this connection.[2]

Sanitary improvements were not limited to the metropolitan areas. An example of actual progress in a small city is to be found in the history of Macclesfield, England. Here the death rate stood at 42 per 1,000 in 1847, and the average age at death was twenty-four years—as compared with an average of thirty-four in the surrounding countryside. In 1848 the town inaugurated sweeping improvements in its street cleaning, drainage, sewage disposal, and water supply. Ten years later the mortality rate had declined to twenty-six, and the average life expectancy had risen in a corresponding manner. Local leaders were convinced that this improvement was due entirely to sanitary reform.[3] Similar claims were made for Liverpool and for other English cities.

This does not mean that the majority of towns were thoroughly cleaned out before 1870. Cities continued to grow rapidly in all industrial countries while the sanitary reform movement was getting under way, and new problems were sometimes created faster than old ones could be solved. New York in 1840, for example, was still a city with a relatively homogeneous population of about 313,000; twenty years later it had grown to over 800,000,[4] and faced a problem that was long the despair of both English and American sanitarians—the Irish slum.

[1] Southwood Smith: *Results of Sanitary Improvement Illustrated by the Operation of the Metropolitan Societies for Improving the Dwellings of the Industrial Classes* (Lon., 1854), p. 10 ff.

[2] Contemporary improvements in the methods of constructing pipes, estimating water flow, etc., are described in H. P. Eddy: "Sewage and Drainage of Towns", Amer. Soc. of Civil Engineers, *Trans.*, XCII (1928), 1227.

[3] John May: "Sanitary Measures in a Provincial Town, and Their Results", Nat. Asso. for the Promotion of the Social Sciences, *Trans.*, I (Lon., 1858), 403 ff.

[4] F. J. Guetter and A. E. McKinley: *Statistical Tables Relating to the Economic Growth of the United States* (Phila., 1924), p. 7.

While New York did at least build its Croton Dam, many towns made little or no attempt to provide adequate water and sewerage systems. Such large places as Milwaukee and Providence had no public water systems as late as the sixties.[1]

Nor should it be assumed that urban health administration assumed modern form in this period. The health boards in most cities continued to serve only in an advisory capacity; and the actual enforcement of health ordinances was still loosely distributed among quarantine officers, street-cleaning and sewerage officials, and more or less ignorant inspectors. Political corruption added to the confusion in American cities. In New York City, where the aldermen constituted the board of health, Mayor Wood refused to call them together when cholera threatened early in the sixties—on the ground that he considered them more dangerous to the city than the disease itself! "The most conspicuous figure in the list of sanitary officials was the head of the Street-cleaning Department, who had an annual appropriation of nearly one million dollars to spend upon his political followers. The sanitary inspection of the city was one of his duties and for this work he appointed scores of 'Health Wardens', who were generally saloon-keepers." The qualifications of these men may be suggested by the fact that when one was requested to define the term "hygiene" he answered: "The vapour which rises from stagnant water." Another, when asked what he did when called to a case of contagious disease, replied: "I go to the house and call the people to the street, where I give my orders, which are, to burn sulphur; I never go into the house."[2]

The sanitary regime in London was probably more honest, but was just as confused in its administrative arrangements.[3] Such conditions eventually proved intolerable in metropolitan centres; and in consequence the health administration in London was reorganized during the fifties, and that of New York in the next decade. Various agencies were co-ordinated, legal powers were increased, and physicians were appointed to the permanent service of the boards. This was significant, in that it made for the prompt application of such new knowledge about disease control as medicine might at any time supply. John Simon in London, and Stephen Smith in New York City, were the first appointees, and were therefore among the earliest full-time "health officers" in the modern sense. Interested in positive sanitation rather than simply

[1] A. C. Cole: *The Irrepressible Conflict*, p. 181.
[2] Stephen Smith: "The History of Public Health, 1871–1921", in M. P. Ravenel (Ed.): *A Half Century of Public Health* (N.Y., 1921), p. 7.
[3] Simon gives an interesting account in his *English Sanitary Institutions*, pp. 250 f.

in negative quarantine, and in the prevention of disease rather than in routine medical charity, these men held a far wider view of their functions than had the old quarantine officers, poor-law doctors, and city physicians.[1]

It is fair to say, then, that at least a beginning was achieved by 1870 in practical sanitary reform and in the development of permanent urban health administration. This was also true of the development of health organization on a provincial and national scale. If Great Britain, Germany, and the United States are again taken as examples of the more progressive nations, certain common trends are discernible in the evolution of their general health institutions. Following the investigations of the forties, various medical and health societies attempted to organize on a national scale, and advocated national health offices. Most rapid progress in this direction was at first made in Great Britain, where excellent opportunities were afforded by the existence of a relatively small area and a centralized governmental authority. Southwood Smith was able to organize an effective "Health of Towns Association" for the whole country in 1844; and four years later Chadwick and others persuaded Parliament to establish a national board of health in London.

At the very start, the board was given some administrative as well as advisory functions. This proved its undoing, for despite some good work it soon ran foul of the typical Anglo-Saxon fear of governmental regimentation. It was one thing to persuade Parliament to limit the scandalous overcrowding of cemeteries; but quite another to force parsimonious local authorities to clean houses and institute special medical relief.[2] When the board "conferred" special sanitary powers on nearly 200 places that did not desire them, local opposition was added to that of general conservatism in the capital; and the board was abolished in 1854.[3] In its place there was eventually provided a merely advisory office under the Local Government Board.

[1] Simon, op. cit., describes the reorganization in London. The detailed history of the city is given in Henry L. Jephson: *The Sanitary Evolution of London* (Lon., 1907). For the New York story, see Stephen Smith: op. cit., and C. F. Bolduan: "Over a Century of Health Administration in New York City", N.Y.C. Dept. of Health *Mon. Ser.*, No. 13 (1916), pp. 3 ff. The outstanding history of municipal health in the U.S.A. is W. T. Howard: *Public Health Administration and the Natural History of Disease in Baltimore, Maryland, 1898–1920* (Washington, 1924). Further references to New York and other American cities are given in R. H. Shryock: "Origins of the American Public Health Movement", *Anns. of Med. Hist.*, n.s., I, 658, n. 24; 660, n. 38.

[2] *Rep't. by the Gen. Bd. of Health on the Measures for the Execution of the . . . Public Health Act, up to July, 1849* (Lon., 1849), pp. 20 ff.

[3] The opposition to the Board is described in J. L. and Barbara Hammond: *Lord Shaftesbury* (Lon., 1923), p. 167; and in Maurice Marston: *Sir Edwin Chadwick* (Lon., 1925), pp. 62 ff.

Similar defeat apparently attended the first efforts at national organization in Germany. Virchow had recommended a national health council for Prussia, and the liberal political leaders of the forties viewed medical and sanitary reform as an important part of their programme. The famous national congress which met at Frankfurt-am-Main in 1849 discussed the need for a national medical and sanitary code and for a German pharmacopœia. Unfortunately the failure of the Revolution and subsequent conservative reaction defeated these ends for the time being. Renewed efforts to secure a central health office under the North German Confederation, after 1866, were in turn checked by the advent of the Franco-Prussian War.[1]

In the United States the existence of the federal system and of the large area it covered seemed to indicate a need for State rather than for national health administration. Little was accomplished until almost 1870, despite Shattuck's appeal in 1850 for a State board in Massachusetts. More promising was the co-operation of local medical and sanitary societies with the city officials of the eastern seaports, in a series of national "sanitary conventions" held between 1857 and 1860.

It so happened that an international *Conférence Sanitaire* convened in Paris in 1851 and 1852, as a result of the general fear of cholera and of the growing uncertainty about quarantines. It occurred to Dr. Wilson Jewell, of the Philadelphia health board, that the situation of the American States—each with its own quarantine regulations and health laws—was analogous to that of the European nations, and that a sanitary conference of State authorities might well be held on this side of the Atlantic. He finally persuaded the Philadelphia board to issue a call for such a meeting in that city in 1857. Here it was at once decided, in line with the trend of the time, to consider the problems of sanitation as well as of quarantine. This trend was further emphasized when the second meeting, at Baltimore, became known as "the Great American Congress for Hygiene Reform". Throughout the debates of these meetings increasing reliance upon sanitation and increasing distrust of quarantines was evident. Reports were submitted on tenement houses, urban food supplies, drugs, dispensaries, and the disposal of refuse, and recommendations were made to city authorities on these matters.

Jewell viewed the conventions as "a permanent institution; an association destined to revolutionize the public mind and will, on the great

[1] Fischer: *Geschichte des Deutschen Gesundheitswesens*, II, 299, 306; Nelly Böhne-Schwelm: "Nationale u. sozialpolitische Regungen auf den Versammlungen deutscher Naturforscher u. Ärzte bis zum Revolutionsjahr 1848", *Sudhofs Archiv*, XXVII (1934), 87 ff.

question of Sanitary Legislation".[1] The Boston meeting of 1860 took steps looking towards permanent organization; and the convention seemed about to resolve itself into the first American public health association, when the Civil War put an end to such aspirations.

These setbacks, while serious, proved temporary in nature. It happened that all three nations here considered engaged in major wars between 1858 and 1870, and the suffering occasioned by unnecessary disease among both troops and civilians revived a desire for some sort of national health control. Progress was now most rapid in Germany, where, after 1870, the new Reichstag and Chancellor Bismarck were favourably disposed towards the State control of health as well as of other social conditions. In 1876 the Reichstag established in Berlin a national health office for the entire Empire.[2]

The first director, Dr. Struck, envisaged his chief functions as the enforcement of a national vaccination system, the publication of unified vital statistics, and the preparation for the Reichstag of a national sanitary and health code embodying the principles already practised by the more progressive cities.[3] In this respect his position was analogous to that of Simon, who was by this time directing the British health office under the Local Government Board in London; but in one important point Struck's opportunities were greater than those of his English colleague. The latter had to operate under governmental authorities whose social and economic views were still determined by the *laissez-faire* doctrines of the "Manchester School". They therefore tended to resist every step towards governmental social control, even of disease, until this was forced upon them by necessity.

The German national health office, on the contrary, found itself working under a Chancellor who believed it necessary for the State to control social and economic conditions, in the interest of both the poorer classes and of society as a whole. Bismarck, it will be recalled, opposed the rising tide of Marxian socialism with a social programme of his own. He reflected, in this respect, a growing opinion among such German economists as Schmoller and Gustav Wagner, that the still prevailing doctrines of the conservative English school were inapplicable to modern society. To German scholars and political leaders the opportunity to develop a comprehensive public health programme afforded an important argument in justifying the new national union.

It was no accident, therefore, that more rapid provision was made in

[1] Third Nat. Quar. and San. Convention, *Proceeds. and Debates* (N.Y., 1859), p. 6.
[2] Fischer, op. cit., II, 306, 307.
[3] *Das Reichsgesundheitsamt: 1876–1926;* a *Festschrift* issued by that Office, Berlin, 1926.

German than in British law for certain phases of health reform. Sanitary engineering in German cities owed something to British example and to British engineers; but, on the other hand, few such slum conditions were permitted to develop in Central Europe as were common in the United States and in the British Isles. This may have been due, in part, to the traditional willingness of German peoples to submit to governmental discipline; in part to the fact that industrial development came late in Germany—notably after 1870—and the Fatherland was able to profit by earlier British experience. Nor did Germany and other Continental nations have to deal, as did the English-speaking countries, with a sudden influx of poor and ignorant Irish peasantry into the industrial towns. The clean and orderly character of the poorer districts in large German cities impressed the visitor from English-speaking lands as a remarkable phenomenon.

The most notable aspect of German public health administration after 1870 was the enactment of a social reform code primarily directed towards caring for the sick poor. Such "social medicine" was a part of public health in the broader sense of the term. Sanitary reform and the control of practice were not enough, if—when sickness came—the poor could not pay for service. Charity care conveyed a stigma; while "State medicine" (poor-law doctors) was casual and inferior. As industrialism expanded and labour organizations appeared, workers and Socialist leaders demanded better medical care. Workmen's benefit societies began to adopt the insurance principle in providing sickness allowances.

Noting these demands and the availability of insurance procedures, the German Government decided to adopt legislation that would provide systematic medical care for all who needed it. As early as 1883 the well-known national sickness insurance law provided that, in case of illness, working people should continue to receive their wages for some six months, and should also be provided with free medical attention. As the funds for this system were provided jointly by employers and employees, it involved little of the old stigma of poor relief. A year later an accident-insurance measure of a generally similar character was adopted. In 1889 an old-age pension and invalidity law provided protection against long illness, as well as financial support for those over seventy years of age. Thus the German Empire set the example, for better or for worse, in expanding public health protection beyond the prevention of disease and into the field of non-charitable medical care of the poor. After 1884 such medical services were established on fundamentally the same grounds as were the public schools. It was in

part this German example, as well as the press of circumstances, which inspired the spread of the social insurance programme into other European nations.[1]

Meanwhile, despite the development of excellent health codes in British cities and the gradual introduction of improved engineering, there was some discouragement among British sanitarians. The continued difficulty of dealing with some of the worst slums in the Western world, and the apparent failure of the first attempt to secure a national health organization, may have accounted for this. As late as 1869 Sir Henry Acland, then president of the British Medical Association, regretted the ineffectiveness of British health administration, and declared that "Preventive and Public Medicine is more fully developed in several continental countries and, of late, in a noble manner in the United States".[2]

The turn of the tide in British sanitary affairs really came after 1870. It was then that, under Simon's able and tactful direction, the powers of the central health office in London were gradually enlarged in keeping with the spirit of the times. Somewhat hampered by the existence of a separate poor-law medical authority, and by the existence of special health functions in still other branches of the Government, the Local Government Board office gradually secured the passage of a superior national health code between 1870 and 1900. Simon's programme and that of British municipal health officers became so progressive, indeed, that British health administration attained international primacy before the end of the century.[3] By 1892 the dean of the medical faculty of Paris was ready to declare in London: "You [British] have taken the first rank in the art of formulating laws for the protection of health".[4] Similar praise emanated from Continental and American sources.

The situation in the United States after 1860 was actually more difficult than that in either Germany or Great Britain. Facing slum conditions similar to those in England, the American authorities had to deal with the difficulties inherent in the federal system and also with the consequences of a tragic civil war. These consequences were most serious in the South, where appalling military losses were followed by a disruption of the whole social and economic order. On the old planta-

[1] This theme will be resumed in Chap. XV.

[2] "The General Relations of Medicine in Modern Times", in W. Stokes et al., *Medicine in Modern Times* (Lon., 1869), p. 30.

[3] The administrative history and the parallel expansion of medical activities are described in Simon, op. cit., and brought down to 1918 in Sir Malcolm Morris: *The Story of English Public Health* (Lon., 1919).

[4] Seventh International Congress of Hygiene, *Trans.* I (Lon., 1892), 13.

tions, masters had possessed an obvious economic interest in the health of slaves; and they had sometimes provided the latter with crude sanitary regulations and free medical attendance. All such protection was lost under freedom. Many Negroes herded into the towns, where, living amid poverty and squalor, they suffered much from tuberculosis and syphilis, and their death rate rose rapidly above that of the neighbouring whites.[1] Nor was the health of the white population improved by four years of exhausting war. Southern physicians found most of their energy consumed in the attempt to regain lost ground.

Revival came relatively early in the victorious North. The reorganization of the New York City health service actually took place during the last years of the War. A few years later Massachusetts finally adopted Shattuck's original recommendation, and set up the first State board of health (1869). Nineteen other States followed this example during the next decade. These State boards, like the contemporary Local Government Board health office in London, possessed at the outset only the usual powers of investigation and advice. The limited scope of their first activities may be illustrated by the fact that the total annual appropriation, made for the North Carolina board during its first year (1876), amounted to only $100.[2] The American boards gradually expanded their functions with the development of preventive medicine during the decades that followed.[3]

The first effort to establish a national health council in Washington suffered a fate very similar to that of Chadwick's early board in London. A group of physicians and sanitarians, several of whom had been active in the United States Sanitary Commission during the Civil War, met in New York City in 1872 to form the American Public Health Association. This body revived the interests of the earlier sanitary conventions, and proceeded at once to promote both social and scientific research.[4] Its leaders, including such able men as Dr. Stephen Smith of New York, Dr. H. I. Bowditch of Massachusetts, and Dr. John S. Billings of the Army Medical service, were interested in establishing a national health board which would work for general sanitary

[1] See R. H. Shryock: "The Origins of the American Public Health Movement," *Ann. of Med. Hist.*, n.s., I, p. 661, notes 44, 45.

[2] Jane I. Lotspeich: *The Development of State Control of Public Health in North Carolina*, p. 21 (Unpub. master's thesis, Duke Univ., Durham, N.C., 1928).

[3] The best history of an American state board is that of Massachusetts, G. C. Whipple's *State Sanitation* (Cambridge, Mass., 1917). References to works on other States are noted in R. H. Shryock: "Origins of the Public Health Movement in the U.S.", op. cit., p. 664, n. 72.

[4] See M. P. Ravenel: "The American Public Health Association: Past, Present, Future", in Ravenel (Ed.): *A Half Century of Public Health* (N.Y., 1921), pp. 13 ff.

reform. Their efforts in this direction, unfortunately, came into conflict with the interests of the already existing United States Marine Hospital service, which was primarily interested in quarantine control.

Behind this administrative divergence there lay the issue between the old doctrine of quarantine and the current creed of sanitation. The latter was largely dominant in European circles by this time; but the American situation was peculiar in that the Southern States still suffered from yellow fever, and some of their medical men clung to the older faith in quarantines as the best protection against this tropical plague. Southern leaders taking this view preferred a national quarantine authority to a board which might interfere in local or State sanitary administration.

Out of the conflict of scientific opinions and administrative interests which ensued there emerged in 1879 a national health board empowered to investigate and advise, and also to enforce national quarantine regulations—a function in which the sanitarians who composed the board had little interest. Although excellent things were accomplished, as in Billings's work on vital statistics, he and his colleagues possessed little political influence, and were not able to overcome an opposition located especially in Louisiana. Congress declined to continue the board, after it had served for four years; and it remained for the Marine Hospital Service of the Treasury Department gradually to assume the functions of a national health department.[1]

While the United States, Britain, Germany, and other nations were developing their national health systems, there was also some progress in international organization. It has been noted that an international sanitary conference was held at Paris as early as 1851. There were similar meetings in subsequent years, and these broadened their interests with the trend of the times. At a conference in 1874 it was proposed to set up a permanent international council to check epidemics, since this phase of health control necessarily transcended national action. Finally, in 1907, the Rome Congress provided for the establishment of the "International Office of Public Health" in Paris. This office has since then collected and reported regularly on epidemic diseases and national regulations throughout the world.[2]

[1] An excellent account of this story is Robert D. Leigh's *Federal Health Administration in the United States* (N.Y., 1927), pp. 464 ff. The contemporary development of public health administration in Canada is described in John J. Heagerty: *Four Centuries of Medical History in Canada*, I, 337 ff.

[2] See the *Office International d'Hygiène Publique, Bulletin Mensuel;* e.g., XXVII, No. 2 (Paris, 1935). The Council of this Office was composed of the representatives of fifty-two governments. It supervised the application of an International Sanitary Con-

After 1920 the international control of health was broadened by the establishment of the health functions of the League of Nations. Article 23 of the Covenant of the League provided that the members "will endeavour to take steps in matters of international concern for the prevention and control of disease". In accordance with this, the League set up a permanent Health Committee to direct the work of a Health Section in the League Secretariat. The international health administration so created was quite active in providing for international co-operation along many lines.

Most obvious was the work in reporting on epidemic diseases. An eastern bureau was maintained at Singapore, in order to collect and broadcast information on the situation in the East and Far East, where infectious diseases are more prevalent. Its bulletins were promptly received by many health services and by ships at sea. In addition to this, the League's health secretariat provided for studies of diseases which seemed an especial menace to the nations at large, and encouraged co-operation between different health services in dealing with them. An International Leprosy Centre was maintained in Rio de Janeiro. Courses on malaria were offered each year in one of the European cities and at Singapore. Permanent Commissions were maintained to study and report on such matters as maternal welfare, child hygiene, tuberculosis, venereal disease, opium, and biological standards. Expert administrative and technical advice was afforded to nations desiring it, and progressive as well as relatively backward countries availed themselves of this aid.

The Health Section, in its continuous effort to encourage international co-operation, even arranged collective study tours for public health specialists, and provided scholarships for study abroad. The Secretariat also called special congresses for the consideration of any specific problem that demanded general attention. An interesting illustration of this was the European Conference of 1931 on rural hygiene, which met to consider the lag in hygienic improvement in rural areas. The Conference formulated guiding principles for the organization of medical attendance and health service in such regions.[1]

Paralleling the development of the League administration, there con-

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vention which was adopted in 1926; and acted in an advisory capacity to the Health Section of the League Secretariat.

[1] Much of the epidemiological service can be followed in the *Bulletin Mensuel* of the International Health Office noted above. The administrative arrangements are conveniently summarized in the League's handbook, *Essential Facts about the League of Nations* (4th ed., Geneva, 1935), pp. 175 ff.

tinued to be a wide range of co-operation through the medium of international congresses in the various medical specialties. Such meetings were held as early as the middle of the last century, and became increasingly numerous in the later decades. Prior to 1939 practically every professional group had its international gatherings. These varied from such great meetings as the congress on physiology which met at Moscow in 1935, to the relatively small meeting on the history of medicine held at Madrid in the same year. They all provided in a semi-official manner for scientific co-operation of an important character; and this, in the long run, was of significance to public health throughout the world.

Under the impact of World War II, much of the international health machinery ceased to function. Five years of total war left many nations facing the most dire health problems, without any effective international organization to assist in solution. One of the first acts of the United Nations Organization, therefore, was to call an International Health Conference in order to set up a new international health agency. This Conference opened its sessions in New York City on June 19, 1946. Delegates from all fifty-one members of the United Nations were present, and sixteen other countries sent observers. This greatest of international health congresses was presided over by Surgeon-General Thomas Parran of the United States Public Health Service.

A statement of objectives was prepared, and the Conference then adopted a constitution for a World Health Organization. Over the opposition of a few delegates, the Conference voted to accord the new agency administrative powers over such matters as sanitary and quarantine requirements, a standardized nomenclature for causes of death, and even the control of certain aspects of the drug trade. The older international bodies, whose powers had been only advisory, were to be absorbed. The public health field will thus become one of the first areas for experimentation in actual international control, providing that the constitution is ratified by twenty-six or more of the member-nations. Upon its adoption by the Conference on July 22, Dr. Parran presented the constitution to the nations as a "Magna Charta" for world health and as "a powerful instrument for peace".[1]

A further account of either national or international administrative history is hardly pertinent here, save in so far as it expressed new developments in medicine, or to the extent that it represented an important activity of the medical profession. This might be a good point,

[1] See issues of the *N.Y. Times*, June 19 to July 23, 1946.

therefore, at which to sum up the general relation of medical science and the medical profession to the public health movement. Looking back over the developments which have just been described, it is apparent that most of the leaders of both official and unofficial health organizations were physicians. Despite the indifference of most practitioners to public hygiene,[1] the medical profession did nevertheless supply this leadership in a great social movement.

It is easily forgotten, too, that not only individual physicians but medical societies as a whole were usually responsible for the establishment of Government health agencies. This seems to have been especially true in Germany and in the United States. In the latter, for instance, it was almost invariably the State medical societies that demanded the creation of State health departments. So true was this that it did not seem incongruous when, in the cases of Alabama and North Carolina, the societies were themselves constituted as boards of health in their respective territories.

A recognition of outstanding services on the part of the medical profession in no way detracts from the splendid achievements of lay sanitarians like Chadwick and Shattuck. The work of such engineers as the elder Lindley and Julius Adams was as essential as that of any of the contemporary physicians, in laying the sanitary basis necessary for the development of modern cities. Statisticians, engineers, and social philosophers all made their contributions to the public health movement. For one generation, about the middle of the century, it seemed, indeed, as though mathematicians might do more for public hygiene than could the medical men themselves. Statisticians revealed problems and engineers found solutions, at a time when the best medicine tended towards nihilism and seemed to have little to offer. Yet socially-minded physicians continued to insist that public hygiene must ultimately remain a medical science, and protested against the occasional tendency to ignore medical leadership.

An interesting illustration of such protest is afforded in the history of the first national health board in Great Britain. Chadwick was suspected of having little faith in medical science, and this suspicion seemed to be confirmed when no physicians were appointed to the board when it was first constituted in 1848. The editor of the *Lancet* promptly declared that this oversight was stupid and an insult to the

[1] The editor of *Ranking's Abstracts*, an important British medical journal, complained as early as 1856 that it was impossible to arouse the interest of most of the profession in public hygiene, ibid., XXIV, 216.

medical profession.[1] To make matters worse, a medical man *was* appointed to a board set up in 1850 to supervise a new Interments Act. Dr. Henry Rumsey, an authority on State medicine, inquired with some acidity:

> Who would have thought that in the last decade of advancing civilization . . . the whimsical experiment should have been actually tried of appointing three non-medical authorities, two lords and a barrister, to preserve the health of the living, and then after a year or so of doubtful success, calling in a physician to bury the dead.[2]

Dr. Southwood Smith was later appointed to the health board, but this did not entirely satisfy medical critics. Rumsey insisted that Chadwick, as secretary, was prejudiced against the medical profession, and that his whole programme was of too mathematical and mechanical a nature. Nothing, insisted the physician, could take the place of medical science and personal medical service.[3]

That such friction was not more common was doubtless due to the fact that physicians who entered public health work were naturally those who had some taste for statistics and even for engineering. At the same time, they shared Rumsey's conviction that the problems of public health were ultimately medical in character and must be solved by men who possessed medical as well as statistical or administrative training. They felt that "sanitary science" could do excellent work in controlling illness through the removal of nuisances—in the broadest sense of the term—but since it did not penetrate to the immediate causes or carriers of disease, certain problems would always elude it.

Such views were finally vindicated in a dramatic manner, after 1870, by a veritable revolution in the knowledge of disease causation. The development of medical bacteriology rapidly shifted the emphasis in public health work from what Rumsey called the "mathematical and mechanical" to the more essential biological problems. It was important to realize that cleanliness was in some way a protection against disease; but it was still more important to know just what caused a specific disease like cholera, or how it was carried, and therefore to know just what specific measures could be taken to prevent or cure it. Bacteriologists were able to get closer to grips with some of man's ancient enemies than the older sanitarians could ever have done.

The result was that the health programme entered a new phase after

[1] The *Lancet* (Lon.), Sept. 30, 1848, p. 383; Dec. 16, 1848, p. 673.
[2] Rumsey: *Essays on State Medicine* (Lon., 1856), preface.
[3] Ibid., pp. 127, 131, 282.

1870; so impressive a phase that it was soon viewed as the very beginning of things in public hygiene. There ensued a tendency to give too much credit to leaders in medical research; whereas up to 1870 they received too little. It is no discredit to the bacteriologists to note that, after all, the way was prepared for them by a generation of "sanitary reform"—that such serious plagues as smallpox, typhus, and malaria were already coming under partial control before the advent of the bacteriological revolution. Shattuck and Southwood Smith, as well as Pasteur and Koch, have their place among the immortals.

XIII

PUBLIC CONFIDENCE LOST

IMPORTANT AS were the early achievements in sanitary reform, they did not inspire the public with any great confidence in medical science. The very phrase "sanitary reform" suggested engineering improvements, rather than medication or other aspects of medical practice as it was then understood. This was unfortunate, since physicians stood in need, at the time, of all the credit due them for the public health programme.

A cynical distrust of the medical profession was common enough in all modern centuries, but it seemed to grow beyond ordinary bounds during the early Victorian decades. At any rate, it became more articulate and emphatic in this period. Various explanations suggest themselves. Rising standards in the general level of public education may have made laymen more critical of their professional advisers. There is some evidence that the clergy suffered in this connection, and it is quite possible that in the long run physicians did also. Expanding publishing facilities made it easier for the critics of any profession to attain a hearing, and so to create some feeling against it. It was no accident that newspapers, directly or indirectly, did something to undermine confidence in the medical profession.[1]

Then there was the increasing contrast between practical success in physical science, and the continued failure of medicine to contribute to human welfare on anything like the same scale. Everyone appreciated, by 1850, the significance of the steam engine and of the electric telegraph. The former alone, in its many applications, was revolutionizing conditions of life throughout the Western world. Except for Jenner's immortal discovery, what did medicine have to offer which was comparable to these?

No reassuring answer could be made for many years. The very progress physicians were making between 1830 and 1850 made it the more difficult for them to offer the public much encouragement. The first

[1] Instances will be cited below.

generation of critical clinicians had so much traditional trash to clear up, and such difficult foundations to lay, that it was never able to build a therapeutics that could impress the laity. This was inevitable. Someone had to distinguish between typhoid and typhus before anyone could look for the cause of either. But until causes and even cures were found, the public was conscious of little progress. Someone, again, had to show the futility of bleeding, before attention could be centred upon a search for more effective remedies. But until the latter were discovered, it appeared only as if that little which the patient had, had been taken away. Therapeutic nihilism was really a sign of progress in 1850, but it discouraged the generation that witnessed its appearance. Hence the paradox, that the most hopeful period in the history of medicine was the one in which the public looked to medicine with the least hope.

When critical physicians refused to promise all things, when learned doctors made the shocking announcement that medicines were useless, many patients turned to quacks for cures. Quackery was age-old, but it grew apace in the eighteenth and early nineteenth centuries with the advent of a new form—the patent medicines. Their appearance was obviously associated with the birth of patent law, and with the development of newspapers and other advertising media. Once these were available, the proprietary remedies were ready for rapid conquests, as they inherited an effective testimonial technique from still earlier quackeries.[1] Since the sale of advertised nostrums was increasing at the very time that regular physicians tended to decrease their dosing, the point was probably reached where far more physic was taken without prescription than with it.

One of the most striking and most common forms of quack advertising in the United States was that of abortifacient drugs; a fact which seems hardly consistent with our traditions of Victorian propriety.[2] Such advertising presumably represented a popular phase of the early birth-control movement. The common form of the "ads" was something of this sort:

> ☞ Dr. ——'s Female Pills, one dollar a box, with full directions. Married ladies should not use them. Sent by mail.

Similar announcements and other quack appeals filled not only the daily and weekly papers, but the "family newspapers" and even the religious

[1] No less than sixty pages of testimonials were included in the *Strange Cures* of Valentine Greatrakes, a famous English quack of the seventeenth century (Lon., 1723, pp. 39–99; first ed., 1666).

[2] It also seems to have been overlooked by those who are shocked by the modern advertising of contraceptives as something new.

Press.[1] Some American observers believed their countrymen to be peculiarly addicted to panaceas, and ascribed the national "wonder-gaping" complex to the newness of the country. They were convinced that charlatans from all over the world were rushing to the Yankee happy hunting grounds.[2]

It is true that Americans were easily "taken in" at this period. The career of the showman Barnum even suggests that they enjoyed it; but does anyone seriously believe that tuberculosis victims relished being fooled by consumption cures? European quackery, moreover, was on all fours with that on the other side of the Atlantic. It is true, for instance, that New York City firms were already spending, during the forties, over $100,000 annually in advertising secret nostrums.[3] But it is also true that there were living in London at the same time no fewer than five men who had made at least £1,000,000 each out of patent medicines. The famous Morrison of the "London College of Health" was said to realize £80,000 annually from the sale of his purgative pills.[4] One type of nostrum alone—the vicious "soothing syrup"—was reputed to sell in such quantities in England as to cause the death of some 15,000 children each year.[5]

Things were little better on the Continent. In France, laws of 1803 and 1805 forbade the advertisement or sale of patent remedies, but these laws could not be enforced.[6] The German States made strenuous efforts, between 1840 and 1860, to suppress quackery through legislation and police power—at a time when little if anything was being attempted in English-speaking lands. Strict laws on the subject were passed in Baden (1845), in Prussia (1851), and in Bavaria (1861). The public, unfortunately, distrusted the motives of physicians in supporting such statutes, and was inclined to view any quacks who were suppressed as martyrs to the cause of human welfare. Virchow and other professional leaders then urged that the attempt to control quacks be abandoned, rather than that they should be canonized in this manner. A law of the North German Union finally, in response to such views, removed the barriers against quackery and unlicensed practitioners. A

[1] *Med. and Surg. Reporter*, n.s., I (Phila., 1858), 127.

[2] *New Orleans Med. Jour.*, I (1844), 78.

[3] Paul F. Eve: *The Present Position of the Medical Profession in Society* (Augusta, Ga., 1849), pp. 13, 17.

[4] Eve, op. cit., p. 17; R. H. Shryock (Ed.): "Selections from the Letters of Richard D. Arnold", *Johns Hopkins Hosp. Bull.*, XLII (1928), 164.

[5] G. King: "Sale of Quack Medicines", *Provincial Med. and Surg. Jour.*, VIII (1844), 596; R. R. Rentoul: "The Obligations of Medical Practitioners and Chemists to Each Other and to the Public", *Med. Press and Circular*, n.s., LII (Lon., 1892), 364.

[6] G. Bourgeau: *Les Erreurs et les Dangers de la grande Presse en Matière Médicale* (Paris, 1916), pp. 37–9.

veritable deluge of quackery thereupon descended upon a devoted Fatherland.[1] Credulity was, after all, not peculiar to Americans.

It was not only to quacks and charlatans that people turned in their search for effective treatments. Prior to 1800, such medical system-makers as Brown, Rush, and Rasori had found some single key to all the problems of pathology and therapeutics, and the public had been much impressed thereby. But in due time the system-makers were no longer tolerated in regular medicine,[2] and patients who sought their aid must find it without the pale of the profession. Hence one medical sect after another arose alongside of orthodox science, and each attracted thousands of hopeful followers. No wonder that patients fled from old-fashioned practitioners, who still resorted to the bleedings and purgings that the leaders of their own profession denounced. Small wonder, too, that the sick, sensing the nihilism of hospital clinicians, turned to homeopaths who provided milder remedies and a hope of cure.

The actual principles of the different sects varied widely, although nearly all displayed the typical features of the older medical systems—a monistic pathology and a monistic therapeutics.[3] The latter was most emphasized, since therein lay the secret of popular appeal. There was one simple method of treatment which everyone could understand, and by which everyone could be cured. The Thomsonians, for example, found that the one great secret of health lay in the use of vegetable drugs; and they therefore revived the ancient controversy concerning the relative merits of vegetable and chemical remedies.

This American sect exploited the rising tide of protest against excessive purging with calomel.[4] Rush had promoted that practice, and another American systematizer, Dr. John E. Cooke, had carried it to amazing extremes in the South and West.[5] As much as an ounce of calomel was prescribed at a time and at frequent intervals, and a man sometimes harboured a pound of this mercurial at his doctor's orders.[6]

[1] H. Magnus, *Die Kurierfreiheit, und das Recht auf den eignen Körper: Ein geschichtlicher Beitrag zum Kampf gegen das Kurpfuschertum* (Breslau, 1905), pp. 1–4; Th. L. W. Von Bischoff: *Der Einfluss der Norddeutschen Gewerbgesetzes auf die Medizin* (Munich, 1871), *passim.*

[2] See Chap. IX.

[3] See Chap. II.

[4] See Paul F. Eve: *The Present Position of the Medical Profession in Society* (Augusta, Ga., 1849), p. 17. The Thomsonian system is explained in the *Narrative of the Life and Medical Discoveries of S. A. Thomson, Written by Himself* (Boston, 1822).

[5] Cooke, now generally forgotten, published his treatise on pathology and therapeutics in 1828, and was the most elaborate of all American systematizers. See Elisha Bartlett: *The Philosophy of Medical Science* (Phila., 1844), pp. 233 f.

[6] T. D. Mitchell: "Calomel Considered as a Poison", *New Orleans Med. Jour.* I (1844) 30 ff.

Patients began to protest, and sometimes expressed themselves in no uncertain terms. The following example, put into verse after the eighteenth-century manner, appeared in an important Virginia newspaper in 1825:[1]

Calomel: An Old Song, Altered and Enlarged.

Physicians of the highest rank
To pay their bills we need a Bank;
Not talents bright, nor art, nor skill,
Preserve us safe from *Calomel.*

Howe'er their patients do complain
Of head, or heart, or nerve, or vein,
Of fever, thirst, or temper fell,
The Medicine still, is *Calomel.*

Since Calomel's become their boast
How many patients have they lost,
How many thousands they make ill,
Of poison, with their *Calomel.*

The bard then related the details of a patient's last illness, due entirely to mercury poisoning, and concluded with the peroration:

Physicians, hear a friendly voice
Receive my counsel, take advice,
Be not offended, tho' I tell,
The dire effects of *Calomel.*

And when I do resign my breath,
Pray let me die a natural death,
And bid you all a long farewell,
Without one dose of *Calomel.*

Vegetable concoctions, on the other hand, were declared by the Thomsonians to be safe and dependable. Their "botanic system" flourished until after the American Civil War, when it gradually declined as an organized cult. The fact that various nostrums are still recommended to the public on the ground that they contain "only vegetable compounds" may be ascribed to a lingering Thomsonian influence.

Another interesting cult was that of hydropathy, with its bent for bathing—inside and out. Physicians had long realized that hydrotherapy was of real value, but it remained for a Silesian peasant, Priessnitz, to discover that it was good for what ailed you! "Water cures" were rapidly established, first in Germany, and then during the forties in other European countries and in the United States. People who failed to secure satisfaction from regular practitioners went to live in these institutions in order to "take the cure". Here they enjoyed an

[1] *Richmond Enquirer*, Mar. 5, 1825. This selection, in which the writer explains that he only opposes the "abuse" of the drug, was called to my attention by Dr. J. C. Robert, of Ohio State University.

atmosphere which was in part that of a sanitorium, in part that of the older "springs" which had been genteel vacation centres.

It was inevitable that some of the medical or health sects, arising as they did in a romantic era, should be invested with the halo of humanitarian reform. This was the distinction of a small but articulate hygiene cult which flourished in the United States between 1830 and 1860. Its founder, Sylvester Graham, was a sincere reformer who believed that if the doctors could find no way to save mankind, it was high time the laity did so. Inspired by the temperance movement, Graham inquired why this should be limited to drink? After all, the most thirsty could not live by liquor alone. Were not men made gross, even diseased, by intemperance in food as well?[1] Vegetarianism and the use of whole-grain cereals were recommended, and the latter programme is still recalled when one refers to "Graham bread" or "Graham crackers". Once aroused, the reformer naturally discovered that the problem was more serious than at first supposed. The American people were apparently headed straight for physical destruction, and physicians were doing little to stop it. Men not only suffered from bad habits in eating and drinking, but from many other violations of Nature's "physiological laws". They rarely bathed, rarely opened a window, and in the case of city dwellers rarely took any exercise. Still more shocking were their sexual habits, which in the very heyday of prudery were filling the insane asylums with the victims of their own perversions.[2] Such was the picture which Graham drew of the customs of his countrymen.

Personal hygiene was an old story, but carrying it to the people with the fervour of a crusade was something relatively new. Soon all the organization and ritual typical of contemporary reform movements grew up around Graham's activities. Lectures were given, books and pamphlets were issued, and groups were established to work for the good of the cause. Apparently the most effective means of reaching the public was the health paper, a weekly or monthly which was analogous to the temperance magazine. Considerable literature of this sort appeared; some eighty health journals were published in the United States alone between 1830 and 1890. Most of these were ephemeral in character, except for a few published by regular physicians or sectarian

[1] One of Graham's associates estimated that although alcoholism killed 50,000 Americans annually, "folly in dress" accounted for 80,000, and "downright gluttony" destroyed a round 100,000! *Boston Moral Reformer*, I (1835), 198.

[2] See ibid., I. 64; also the *Boston Health Journal and Advocate* for 1840, *passim*. Such alarming exaggeration was based to some extent upon earlier exaggeration in regular medical literature. See especially M. P. Tissot: *Œuvres*, III (Paris, 1809), 248, 291 ff.; and Thomas Beddoes: *Hygeia*, I (Bristol, 1802), 34 ff.

societies, but their cumulative influence may have been of some significance.

Certain it is that their publication coincided with an improvement in the hygienic habits of the American people. While much of this can be explained in terms of better living standards and "modern improvements", a change in attitudes was also involved.[1] When engineering progress introduced running water into many homes, for example, no great interest was at first displayed in using the bathing facilities so provided.[2] Instead, a deep-rooted fear of winter bathing led to the passage of those regulations against it which seem so amusing to modern readers. It was against such conservatism that the health journals directed their campaign. Gradually the now much-maligned "Saturday night bath" was generally adopted—a distinct triumph at the time for the forces of sweetness and light. Anyone who doubts this might well contemplate the following statement, solemnly printed in a reform journal of 1835. The writer, termed by the editor "a young man of great promise", declared that he had been "in the habit during the past winter of taking a warm bath *every three weeks*". He then inquired anxiously: "Is this too often to follow the year round?"[3]

Women displayed a marked interest in "health reform", perhaps because they suffered to a peculiar degree from the unhygienic customs of the day. The romantic cult of frailty in women was unfortunately associated, probably as both cause and effect, with chlorosis and "consumption"; and the "genteel female" was considered the more fascinating if she fainted at small provocation, or went upon sundry occasions "into a decline". Romantic exaggeration in dress also led to tight lacing and prevented normal exercise. No wonder that the Grahamites established "Ladies' Physiological Reform Societies" to work for the emancipation of women from chronic disease.[4]

Grahamism shared public attention with numbers of other quasi-medical cults, some of which were surprisingly popular in their day. Mesmerism and phrenology loomed large on the social, if not on the

[1] The newer attitudes were well expressed by Harriet Beecher Stowe: "Sermon on Your Health", *Atlantic Monthly*, XVII (Boston, 1865), 85 ff.

[2] I recall one respectable old gentleman who, having had a bathtub in his home for some forty years, quite frankly admitted that he had never used it.

[3] *Boston Moral Reformer*, I (1835), 184. Italics my own.

[4] Interesting pictures of this sort of thing are given in C. J. Furness (Ed.): *The Genteel Female* (N.Y., 1931), *passim*. Further comment on the health cults will be found in R. H. Shryock: "Sylvester Graham and the Popular Health Movement," *Miss. Valley Hist. Rev.*, XVIII (Lincoln, Neb., 1931), 172 ff. The best biography of Graham is Mrs. George Genzmer's article in the *Dict. of Amer. Biog.* His own defence against medical criticism is given in "Mr. Graham's Reply to Dr. Bell," *Boston Med. and Surg. Jour.*, XI (1834), 39 ff.

scientific horizon. The former was seriously employed by surgeons, prior to the discovery of anæsthetics, and is to-day considered by some to have been a forerunner of modern psychiatry. Meanwhile it indirectly inspired the most successful of all popular health cults, the so-called "Christian Science".

The story of how one woman, Mary Baker Eddy, built up the most remarkable system of religious healing of modern times cannot be entered into here. Something was said of it in a preceding chapter. It is important to recall, however, that she originally thought of herself as being in competition with the other health cults of her day. One of her first advertisements for patients contained the statement: "No medicine, electricity, physiology or hygiene [is] required for unparalleled success in the most difficult cases".[1] Religion was apparently thought of here as a means to healing, rather than as an end in itself. The significance of the Christian Science movement can be best appreciated if all the circumstances of its origin—amid the popular distrust of regular medicine and the support of rival cults—are recalled.

Widely as they varied among themselves, all the medical sects and health cults had one thing in common. This was their disdain for regular practice. Homœopaths, Thomsonians, osteopaths,[2] hydropaths, Grahamites, and Christian Scientists all agreed that regular medicine was futile and even dangerous. In meeting this onslaught, regular physicians were handicapped by two circumstances. There was, first, the increasing therapeutic scepticism of their own leaders. It was difficult to claim much for the orthodox pharmacopœia. One could, to be sure, point out the superior tradition and training of the regular profession. But here a second difficulty intervened. How superior was this training, after all?

Unfortunately, the standards in medical education tended to fall during the first half of the nineteenth century. This was true in an absolute sense in the United States, and at least relatively in Great Britain. In the former the rapid expansion of population over a large area brought with it a mushroom growth of private "medical colleges".[3] Their owners' interest in fees, or perhaps the limitations of their staffs, led these schools to shorten courses and otherwise to cheapen their degree;

[1] E. S. Bates and J. V. Dittemore: *Mary Baker Eddy: The Truth and the Tradition* (N.Y., 1932), p. 124.

[2] Osteopathy, which appeared in the United States after 1875, affords another excellent example of a medical system developing in a day when systems were no longer tolerated in regular medicine. Its monistic pathology and therapeutics related primarily to the spine and the spinal nerves. See E. R. Booth: *History of Osteopathy and Twentieth-Century Medical Practice* (Cincinnati, 1905), pp. 48 ff.

[3] See Chap. III.

and by 1850 it was easy for a man of no particular training to attend lectures for one winter and to emerge a full-fledged doctor. Graduates of the older schools protested in vain. This situation was most marked in the remote regions of the South and West, where the population was expanding rapidly, and where the restraining influence of the older medical centres like Boston and Philadelphia was least likely to be felt.[1]

A somewhat analogous trend in Great Britain was marked by a continued dependence upon partially-trained apothecaries. In England the orthodox training authorities did not supply the number of practitioners required by a growing population. The Apothecaries Act of 1815, which finally authorized that group to practise, unfortunately permitted them to charge only for their drugs. They were therefore placed in a position in which physic naturally was ordered to "make a long bill rather than a quick cure".[2] In view of this and of their educational limitations, it is doubtful if the apothecaries represented any higher grade of service than did the graduates of "inferior schools wrongly located" in the States. The British situation was further confused by the competition of other types of practitioners of a more or less regular character. Fellows, licentiates, and extra-licentiates of the London College of Physicians, graduates of non-English universities, surgeons, barbers, apothecaries, and even chemists—who encroached upon apothecaries much as the latter did upon the licentiates—all pressed their claims upon a bewildered people. According to one good authority, anyone could assume the title of "doctor" or "surgeon" in England and practise with impunity.[3]

It was quite natural that, under these circumstances, the public should become as suspicious of certain types of physicians as it was of their physic. In some cases criticism was aimed especially at medical

[1] This led to an interesting controversy between physicians with "Boston notions", and those who taught in "inferior schools wrongly located". The use of the latter phrase involved Oliver Wendell Holmes in a long quarrel with Southern editors, which was not helped by the general sectional feeling of the period. See Amer. Med. Asso. *Trans.*, I, 283–8. See also the discussion of medical education in H. B. Schafer: *The American Medical Profession, 1783–1850* (N.Y., 1936), pp. 55 ff.

[2] There is a picture of the apothecary, at his worst, in Crabbe's "Village"; see his *Works*, II (Lon., 1840), 85. An interesting analysis of such service is given in an article from *Chambers Journal* printed in the *Eclectic Mag.*, XVIII (1849), 520 ff.

[3] Simon: *English Sanitary Institutions*, p. 269; Ernest M. Little, *History of the British Medical Association, 1832–1932* (Lon., 1932), pp. 5 ff. Confusion does not seem to have been so great on the Continent. There was still a partly-trained class of practitioners in France early in the nineteenth century, but in general the French profession was well trained after 1830, and its social status was relatively high. There was some complaint, early in the century, that it was easy to enter the German medical profession, and certain German schools had a poor reputation in Britain. See the *Edinb. Rev.*, LXXXI (1845), 254; F. Rasse: *Von der Stellung der Aerzte im Staate* (Leipzig, 1823), *Vorwort*.

schools and their students. In Philadelphia, the chief training centre in the United States, violent attacks were made by the Press on the latter group. One editor claimed in 1858 that the young "medics" were crude, coarse, and ignorant; and he added that they went forth after an inadequate training, to contribute to the "poisoning and surgical butchery [which] have already been committed in thousands of instances". Similar accounts of medical students in London were expressed in the British literature of the period.[1]

In other cases lay criticism was levied at the profession as a whole. Open attacks sometimes appeared in the Press. A New York State paper announced that doctors tried to keep their patients ill.[2] In New York City a citizen died after treatment by a specialist, whereupon *Harper's Weekly* carried long and agonizing accounts, including drawings illustrating the case.[3] One of the large dailies waxed so indignant that it declared the entire medical guild a "stupendous humbug".[4] Charges were made from time to time, not only that standards in medical education were low, but that quackery was nearly as common within the profession as without. Were not the everlasting quarrels and controversies between doctors good evidence of this? "It has become fashionable", wrote an American medical editor in 1857, "to speak of the Medical Profession as a body of jealous quarrelsome men, whose chief delight is the annoyance and ridicule of each other." [5] Most serious was the continued charge that every art and science was making progress except medicine, the most vital of them all.[6]

While some criticisms were overdrawn, there was no question that the medical profession needed reform from within. The attacks on training standards were largely justified. A demand for medical reform developed among physicians in both the United States and Great Britain during the forties, and was carried through with considerable success in the latter country during the next decade.[7] The Medical Act of 1858 provided a national "medical register" as a means of providing a clear distinction between regular and irregular practitioners. A central coun-

[1] *Phila. City Item*, Nov. 6, 1858: Little, op. cit., pp. 5 ff., cites *Punch* (II, 1842) and *The Pickwick Papers* in this connection.

[2] The *Elmira Daily Gazette*, May 20, 1879.

[3] Issue for Feb. 5, 1859.

[4] See the *Med. and Surg. Reporter*, n.s., I (Phila., 1859), 356.

[5] *Cincinnati Med. Observer*, II, 129.

[6] See the *N.Y. Jour. Med.*, V (1845), 267–9; *Med. and Surg. Reporter*, n.s., I (Phila., 1858), 187; M. Paine: *A Defence of the Medical Profession in the United States* (N.Y., 1846), pp. 11, 12.

[7] The English movement is well described in "Medical Reform", *Edinb. Rev.*, LXXXI (1845), 237 ff.; and also in "Medical Practice throughout the United Kingdom", *Dublin Rev.*, XVIII (1845), 174 ff.

cil was provided to administer the same, and all candidates for a licence were required to pass a common type of examination. Such laws and such organization proved effective in raising the standards of both medical training and practice in Great Britain.

On the other hand, the early efforts of the American Medical Association to promote reforms in training and licensing proved largely fruitless. The existence of many separate State licensing authorities, some of them in backward rural regions, made the American problem far more difficult than the British. Matters became worse rather than better in the States. The earlier American licensing laws, passed between 1760 and 1830, were creditable enough. With the rapid development thereafter of medical sects, health cults, and quackery on the one hand, and of the decline of educational standards in regular medicine on the other, the scene changed. One by one the States began to abandon restrictions against irregular practice. During the forties Alabama legalized Thomsonian practice, Connecticut withdrew exclusive control of licensing from the State medical society, and New York abandoned all restrictions save those against "gross ignorance" and malpractice. By 1845, ten States had repealed their licensing restrictions, and in only three was there any pretence at enforcing such medical legislation as existed.[1]

This trend resulted, in part, from a conscious repeal propaganda which proved more potent than medical counter-propaganda. One editor, beset by arguments from both sides, proposed that the public act as an umpire between the regulars and irregulars and "decide after a careful perusal of the undertakers' bills".[2] It is probably significant that the attack on regular medicine coincided with a widespread democratic movement in American society—with what historians are pleased to call "the rise of the common man". The masses seemed incapable of appreciating superior training in any profession, and to the degree that they came into political power they were likely to lower training standards. It is also to be remembered that most Americans had become impatient of governmental restraints. If it were deemed desirable to throw all business and the professions open to unrestricted competition, why not medicine among the rest? If all religious sects were to be tolerated, why not all medical sects as well? The State senator who introduced a bill repealing licensing restrictions in New York State (1844)

[1] C. B. Coventry: "History of Medical Legislation in the State of New York", *N.Y. Jour. of Med.*, IV (1845), 160.

[2] See the *U.S. Democratic Rev.*, whole ser., XXXV (1855), 270; *North Amer. Rev.*, XXXIII (1831), 370.

declared that: "A people accustomed to govern themselves, and boasting of their intelligence, are impatient of restraint. They want no protection but freedom of inquiry and freedom of action." [1] Thus did the democratic American of the day declare his inalienable rights to life, liberty, and quackery!

To make matters worse in English-speaking countries, certain old quarrels between physicians and laity took on new life in the nineteenth century. While the suspicion that medical men were an irreligious lot tended to decline,[2] along with the general decline of theological enthusiasm, there remained a feeling that doctors delved into forbidden things. Were not their dissections of the human body—the temple of the human spirit—an offence to both God and man? Serious rioting was precipitated in New York City, late in the eighteenth century, when a mob detected evidences of dissections.[3] Subsequent scandals concerning the "sack-'em-up men" and the murders in Edinburgh have been discussed in an earlier chapter. [4] Itwas not until well after the Civil War that most American States legalized dissection, and the supply of subjects for anatomical laboratories continued to be inadequate until near the end of the century.

As public feeling against dissection declined in Britain and in the United States, its place was taken by a similar feeling against vivisection. Experimentation upon living animals became increasingly common during the eighteenth and nineteenth centuries, and by 1840 had been established as an essential procedure in physiology. Popular aversion was natural enough under the circumstances; and, as is often the case, there was just enough justification for this feeling to render it a serious threat to research programmes. Some of the work of so great a physiologist as Magendie has been indicted as "without aim and needlessly cruel"; [5] and even in later years routine work has been done on small animals without benefit of anæsthesia. Exaggerating this element of truth in their protest, a group of well-meaning but sentimental enthusiasts organized anti-vivisection societies in the latter part of the nineteenth century, and threatened to block all progress in physiology, bacteriology, and the related sciences.

[1] Coventry, op. cit., p. 160.

[2] When clergymen accused physicians of being irreligious, the latter replied that the clergy meddled more in medicine than *they* did in theology—the ministers were more inclined to quackery than the doctors were to heresy.

[3] See the description of this "doctors' riot" (1788) in J. J. Walsh: *History of Medicine in New York*, II (N.Y., 1919), 382 ff.

[4] The whole subject is traced in an interesting history, J. M. Bell: *The Sack-'Em-Up Men* (Edinb., 1928).

[5] Garrison: *History of Medicine*, 4 ed., p. 465.

When medical men insisted that a certain amount of suffering in animals was more than justified by the saving of human lives, the anti-vivisectionists denied that lives were actually saved in this manner. Antitoxins and vaccines were not only denounced as unholy, but as useless and dangerous. Most persistent, for instance, was the old fear of vaccination, and protest against it was carried clear into the present century.[1] Efforts were made to persuade legislatures to forbid vivisection and to prohibit compulsory vaccination. The situation became quite serious between 1870 and 1900, when it proved impossible to require vaccination in English-speaking lands, and when it was even necessary for English research men to go to the Continent in some cases to carry on their experiments.

Long before this the medical profession had become quite conscious of its difficulties with the public. As early as 1840 American physicians were convinced that things were going from bad to worse. "The Present Position of the Medical Profession" became a common subject in medical literature.[2] After some years, essays appeared under such pessimistic captions as: "To What Cause Are We to Attribute the Diminished Respectability of the Medical Profession in the Estimation of the American Public?" [3] Grounds for pessimism were found, not only in the development of sectarianism and quackery, but also in the mere lack of expressions of public esteem. Politicians were constantly praised by governments and societies, but physicians were rarely so encouraged. All army officers were well treated, except the medical officers. Philanthropists gave much to churches, and but little to hospitals and medical schools. There was much truth in all of these complaints. The State of Georgia, for instance, possessed no less than 1,000 churches in 1850, but could boast of but four or five hospitals—such as they were.[4]

More serious was the fact that the public suspected the medical profession as a selfish and mercenary one. Every effort of physicians to suppress quackery was viewed in this light. Many persons could not conceive that the interests of the profession and of the public might be identical in this matter. Some of the finest men in the profession felt hurt by this suspicion, and they naturally concluded that their personal

[1] A brief history of this controversy is B. J. Stern: *Should We Be Vaccinated?*, etc. (N.Y., 1927). For a sample of anti-vaccination literature, see J. M. Peebles: *Vaccination a Curse*, etc. (Battle Creek, Mich.), *c.* 1910.

[2] See Paul Eve's pamphlet of this title (1849).

[3] See article with this title in the *Med. and Surg. Reporter*, n.s., I (Phila., 1858), 141. For other references to this type of literature see the author's "Public Relations of the Medical Profession", etc., *Ann. Med. Hist.*, n.s., II (1930), 308 ff.

[4] Paul Eve, op. cit., p. 20.

risks and sacrifices—often undertaken on behalf of the poorest classes—were generally unappreciated.[1]

Almost every practitioner was reminded of this by the proverbial reluctance with which patients paid their bills. No complaint was so chronic in the profession as this one relating to unpaid fees. That it was no idle one is obvious enough. At best, the doctor's bill was the last to be settled. Even very successful practitioners found themselves embarrassed at times. Dr. Richard Arnold, one of the first two secretaries of the American Medical Association, wrote in 1839: "The longer I practice the more I feel determined to go hereafter for the 'quid pro quo'! It runs me almost crazy to think that with hundreds upon hundreds due me professionally I find the greatest difficulty in raising a simple fifty dollars." [2] The physician with only an average practice was likely to find himself in even more difficult circumstances.

Few studies of medical income were made during the earlier part of the nineteenth century; but there is evidence that the younger men often had a hard time of it. Their training investment was relatively small, but there were very few opportunities for salaried positions at the start. Hence they had to take their chance, in competition with older men, at collecting bills from reluctant patients.[3] It was assumed by this time that a fee basis was the only proper one for medical practice. Despite condemnation by professional leaders, however, some practitioners were glad to contract with families to provide medical care for as little as $10 per year.[4]

Hard times continued, for many practitioners, throughout the Victorian era. A study made near the end of the century revealed that seventy-five out of a group of 100 American medical graduates turned within five years to non-medical work—chiefly in order to secure a better income. Nor was financial difficulty peculiar to America. One half of the physicians of Berlin received, in 1895, an annual income of less than 3,000 marks; and that of their colleagues in Vienna was even less than that amount.[5]

[1] See W. W. Keen: "Debt of the Public to the Medical Profession", *Phila. Med. Jour.*, III (1899), 939–6.

[2] R. H. Shryock (Ed.): *The Arnold Letters*, p. 164.

[3] Medical fees have varied with purchasing power, and even their real value is no necessary criterion for actual income, since the inclination or ability to pay varied with time and place. See George Rosen, *Fees and Fee Bills: Some Economic Aspects of Medical Practice in Nineteenth-Century America*, Supplements to the *Bull. of the Hist. of Med.*, No. 6 (Baltimore, 1946).

[4] *Med. and Surg. Reporter*, n.s., I (Phila., 1858), 142. For similar practices in France, see Letourneur: *Rapports Du Médecin avec La Société en Général* (Paris, 1821), p. 23 (thèse).

[5] Rosenfeld: "Ueber die Lage des arztlichen Standes", *Med. Bl.*, XVIII (Wien, 1895), 5–8.

There was, of course, another side to the public relations of the profession. Some degree of popular confidence in regular medicine persisted through the most sceptical decades. Many good people remained devoted to their general practitioners. The "family doctor" of Victorian days is even looked back to as one who enjoyed ideal relations with his patients.[1] The truth is that in this very period, when the technical side of medicine was distrusted, the general procedures in medical practice were well adapted to the needs of society. Through the eighteenth century and the greater part of the nineteenth, practice remained largely an individual matter. It is generally fair to say that most physicians served in accordance with their patients' needs, and charged in proportion to their capacity to pay. This involved much "free work", especially in the United States, where few States developed any real system of medical relief;[2] and it also necessitated a "sliding scale" of charges. There were rarely any reports that charity was refused on the one hand, or that excessive fees were demanded on the other.

The chief frictions in practice developed between rival doctors or between regulars and irregulars, rather than between practitioners and patients. Incursions into a rival's practice, whether by personal criticism, by advertising, or by the abuse of emergency and consultation calls, resulted in much bitterness. The natural inclination of leading physicians was to condemn such practices, not only in the interest of their own income but for the sake of the general *esprit de corps*. There was evidently need for a code of professional etiquette and of organizations to enforce it, if the profession was to maintain the dignity and prestige it desired.

This need led to the establishment of the many medical societies which were set up between 1790 and 1850. The societies adopted codes of "professional ethics", which in English-speaking countries were usually based upon the one published by Thomas Percival of Manchester in 1803. This code stated clearly the rules of professional etiquette, and at the same time reviewed the broader obligations of physicians to society. The one theme related largely to the doctor's income and prestige; the other to his traditional idealism.[3] The two interests conflicted at times, as when etiquette forbade criticism of the work of

[1] J. B. McMaster: *History of the People of the United States*, I (N.Y., 1883), 17 ff., gives a very favourable account of the public esteem of physicians about 1800. It is doubtful if this esteem was so general fifty years later.

[2] See R. H. Shryock: "Medical Practice in the Old South," *South Atlantic Quarterly*, XXIX (Durham, N.C., 1930), 176.

[3] See Chauncey D. Leake: *Percival's Medical Ethics* (Baltimore, 1927), especially the "Introductory Essay".

an inferior or careless colleague, while the welfare of possible patients seemed to demand it. Such conflicts became more serious as a more complicated society developed in later decades.

The professional quarrels of Percival's day naturally led him to emphasize points of etiquette, while the genuinely ethical problems were not stressed. This resulted eventually in some ridicule of "professional ethics" as "trade union rules"; but whatever one terms them, the codes exercised wide influence and were of real service. Even the public benefited, in the long run, by the effort to inhibit demoralizing rivalries and jealousies within the profession.

The consequent gain in professional prestige did something to maintain the layman's respect for his family doctor, at the very time that he tended to question the value of medical science. The resulting compromise in attitudes was illustrated by the report that most laymen thought little of medical work as a whole, but esteemed their own particular practitioners most highly.[1]

There was probably no lack of such esteem in the daily conversation of our grandparents. But since trustful patients were not inclined to rush into print, their views can be found only in their private correspondence. Thus one finds Miss Virginia Randolph, granddaughter of Thomas Jefferson, writing her fiancé in 1822 as follows:

> We have had a rather sick family lately . . . and I am on Dr. Watkins' list. He has bled me twice, and prescribed a course of calomel, which I am getting safely through . . . and I expect soon to be perfectly restored. You must not expect that my health is worse than it frequently is in summer, but Dr. Watkins wishes to make a permanent cure at once of all my complaints . . . I have no doubt but that his advice is most judicious. . . .[2]

The modern cynic will note that it was the "course of calomel" rather than the illness which Miss Randolph was "getting safely through". It is also interesting to observe that Dr. Watkins was not content with simply curing all her "complaints", but intended to do so "at once"!

The naïve faith displayed by some patients even amused physicians themselves. In writing of such confiding souls, a British practitioner observed that there were still those in England

> . . . who believe in the orthodox practitioners, who have faith in the Royal College of Physicians and Surgeons . . . who call things by

[1] This is pointed out in an article in the *North Amer. Rev.*, XXXII (1831), 368.
[2] To Nicholas Trist, Monticello, Va., July 26, 1822, Trist MSS. (Lib. of Congress).

> old names . . . respect fervently leeches, blisters, black draughts, blue pills . . . send for their doctors just as soon as they are ill, do just exactly as they are bid, inquire simply whether the liver is affected or the head, and are satisfied with what they term his pronouncings. . . . They respect most highly someone who knows their constitution, and who knew their father's. . . . They believe in bile and bilious attacks; and, giving themselves up to the man who brought them into the world, or sent their fathers out, they are bothered with no doubts, and are content to be killed or cured in the approved and orthodox old way.[1]

Throughout this period, despite public scepticism, society continued to observe certain formal distinctions which were creditable to the profession.[2] Hospitals were almost always staffed by regular physicians or by homœopaths. Medical missionaries were usually drawn from the regular profession. Public honours were accorded to individual physicians; witness the thirty-six British doctors who were knighted between 1850 and 1883, and the sixteen who were granted baronetcies during the same period.[3]

There were, too, some open expressions of public confidence in medicine, which appeared by way of counter-propaganda to the campaign of disapproval. "As an intellectual and thus a very useful class of the community," observed the editor of a Boston religious journal in 1831, "the medical men are entitled to high respect and esteem—they are invaluable, generous, likeable, and disinterested. They are . . . eminently entitled to the confidence of their fellow men." [4] While the emphasis here smacks of admonition and suggests the reproval of philistines, it at least indicates the feeling that attacks on the medical profession had gone too far.

It is a nice question now, even as it was in the last century, whether these attacks did or did not go too far for the public good. Certainly vast damage was done in all countries by the unintelligent resort to quacks and sectarians, damage to health as well as to pocket-books. The estimated annual loss of 15,000 children in England, through the use of vicious "soothing syrups", well illustrates the tragic significance

[1] J. R. Reynold: *On the Relation of Practical Medicine to Philosophic Method and Popular Opinion* (Address before the North London Med. Soc., Lon., 1858), p. 19. Note the disdain for constitution doctrines typical of the middle of the century.

[2] I am following here and in the ensuing paragraphs my article entitled "Public Relations of the Medical Profession", etc., *Ann. of Med. Hist.*, n.s., II (1930), 324.

[3] "Baronetcies Conferred on Physicians", etc. (A "return" from the Home Office to Parliament, Aug. 14, 1883).

[4] The *Christian Examiner*, quoted in the *Boston Med. and Surg. Jour.*, IV (1831), 10.

of patent medicines. On the other hand, it is hard to avoid the conclusion that regular physicians were themselves responsible for much harm in these years. It is quite possible that their excessive use of mercury was the cause, in the United States, of almost as much promiscuous poisoning as was ever credited to secret nostrums.

The public protest against this and other heroic procedures would seem, then, to have been well grounded. It quite naturally found expression in the encouragement of the various medical sects, all of which condemned bleeding, purging, and the use of strong drugs in general. It is also a plausible conclusion, though one can reach it only on *a priori* grounds, that the necessity of competing with the sects inclined the regular profession to moderate its own procedures.[1] If so, society owes something to the sects, particularly to the homœopaths who led in the attack on the old *materia medica*. The latter group, it is interesting to note in passing, was unique in keeping pace with advances in regular medicine, and in consequence it is usually recognized to-day as part of the regular profession.[2]

Society also owed something to the sects for contributions along specific lines. The Grahamite and water-cure cults were associated with a broader movement for the improvement of personal hygiene, which certainly promoted healthy living in both Europe and America. This involved such diverse phases as the summer vacation movement, the development of physical training, and the cultivation of sports.[3] The personal habits of the American youth of 1910, in such matters as bathing, diet, exercise, and the use of fresh air, were quite different from those of his parents' generation—to say nothing of his grandparents. Physicians played their part in this transformation, especially in encouraging physical training;[4] but most of the improvement in personal hygiene resulted from better living standards, and from the cultivation of lay interests first aroused by the reformers of the forties.

It is possible that the protests of anti-vivisectionists inclined experimenters to be more humane than they might otherwise have been. Much can also be claimed for Christian Science, as having offered men-

[1] See, in this connection, W. J. Mayo: "The Medical Profession and the Public", *Jour. Amer. Med. Asso.*, LXXVI (1921), 923.

[2] Ibid.

[3] The advent of American interest in sports, summer camps, vacations, etc., is described in A. C. Cole: *The Irrepressible Conflict*, pp. 187 ff.; and the subsequent expansion of such interest is discussed in A. M. Schlesinger: *The Rise of the City* (N.Y., 1933), pp. 309 ff. Since *c.* 1900, British and American sports have been gradually adopted all over the world.

[4] The history of modern physical training, especially the development of the German and Swedish systems, and their introduction into the United States, is carefully traced in F. E. Leonard: *The History of Physical Education* (Phila., 1927).

tal healing at the very time when regular medicine most neglected it. The whole trend in the latter towards a localized pathology of physical lesions, while essential to scientific progress, naturally inhibited for some time any interest in mental healing. The very success of Mrs. Eddy in stepping into the consequent gap in therapeutics may be interpreted as a measure of contemporary professional indifference to this important field.

Yet it eventually proved impossible for Christian Science to reconcile itself with, or to contribute to, medicine as a whole.[1] Here was perhaps the chief reason for condemning this and most of the other sects. So long as regular medicine was relatively ineffective, there was some excuse for the existence of its rivals. But once the regular healing art began to improve, this was no longer the case. The further regular medicine advanced, and the greater the consequent gap between it and the sects, the more the latter became a menace to the general welfare. This situation became most serious when an era of remarkable progress in both preventive medicine and therapeutics set in towards the end of the century.

[1] It is an interesting fact that one sect which developed out of the Christian Science movement did actually reconcile itself to the use of regular physicians—on metaphysical grounds! See A. K. Swihart: *Since Mrs. Eddy* (N.Y., 1931), pp. 290 ff.

XIV

THE TRIUMPHS OF MODERN MEDICINE

1870–1900

THE HISTORY of bacteriology is too often treated as though it were a drama opening with a prologue in the seventeenth century, followed by a long curtain and a first act dated about 1870. Such a presentation suggests over-simplicity to "scientific" historians, who have the same respect for actual facts that one finds among workers in scientific medicine. It affords an example of what the former term the "episodical" interpretation of history. As a matter of fact, examination of the sources shows that history is usually a matter of evolution rather than of episodes, and the history of bacteriology would seem to be no exception to this rule.[1]

The possible significance of micro-organisms was never entirely forgotten after 1670, though, like other brilliant conceptions of that period, it was one that could not be demonstrated or made fruitful during the confusion which ensued. As is well known, Plenciz of Vienna carried on the work begun by Leeuwenhoek, and worked out (1762) a well-rounded "germ theory" of disease causation.[2] Meanwhile, the view that large animals either caused or carried communicable diseases seems to have remained a fairly common one. Dogs and cats were sometimes ordered to be destroyed during plague epidemics, and it is just too bad that rats did not suffer as well. Nor was the possible danger from insects entirely overlooked. Rush of Philadelphia informed his students in 1790 of the possibility that fevers were carried by insects from the marshes, and he added that "some facts seem to prove this view".[3]

Such suggestions are often dismissed as partaking of a speculative character. This they did indeed, but it is probable that persistence of such speculation kept alive an interest which finally led to results. It is of some significance, too, that observation in this field was never en-

[1] See in this connection George Sarton: *The History of Science and the New Humanism* (N.Y., 1931), p. 36.

[2] Max Rubner: "Die Geschichte der Hygiene", in his *Handbuch der Hygiene*, I (Leipzig, 1911), 37.

[3] "Notes on Lectures on Theory and Practice" (1790), MSS. p. 68. Yet he never seems to have thought of employing this hypothesis in connection with yellow fever.

tirely abandoned, and became more active with the growing enthusiasm for induction after 1750. Plenciz examined the minute animals found in decomposing organic materials. Linnæus listed the "infusoria" in his classifications; and the Dane, O. F. Müller, described them carefully (using plates) in his pioneer work on "animalcula infusoria". Buffon and Cuvier were both interested in this field.[1]

Most essential to the revival of interest in parasitology and bacteriology, however, were the basic trends in pathology during the early nineteenth century. Studies in ætiology were necessarily confused and inconclusive, so long as diseases were not clearly differentiated. One could not find the causative factors in typhoid, so long as it was confused with other infections like typhus and malaria which we now know were associated with entirely distinct pathogenic organisms. But as pathologists identified an increasing number of specific entities or "clinical pictures", they could then seek intelligently for specific causative factors in each case.

Here is to be observed a long and logical sequence in the history of medical research. This began with the work of the Italian Renaissance physicians, who, in providing an anatomical basis for modern medicine, made possible all that was to follow. For the study of normal anatomy led to that of morbid anatomy, and the latter in turn to the identification of diseases and the search for their causes just mentioned. Note that there was involved here more than three centuries of "pure science", little of which was of any value for practice, since not until causes were demonstrated could there be a rational search for prevention and cure. This sequence affords one of the most impressive illustrations in the history of science, of the ultimate social value of science pursued *at the time* largely for its own sake.

Pathologists, in proceeding from the identification of diseases to a study of their causes, did not limit themselves to the search for a living contagion. The older pathological systems had always assumed that there was some approximate (external) cause of disease, and had often identified this vaguely with "noxious miasms" in the surrounding air. As specific forms of illness were differentiated, physicians attempted to break down miasms in general into distinct atmospheric poisons related in each case to a given disease. Classifications of this sort were worked out on paper, as rational elaborations of fragmentary, empirical evidence. Although this approach to ætiology subsequently proved of

[1] Baas: *History of Medicine*, p. 599; Nordenskiöld: *The History of Biology* (transl. from the Swedish by L. B. Eyre, N.Y., 1928), pp. 426–8.

little value, it was significant in expressing a trend—the search for specific causes—which was to achieve revolutionary results when directed into other channels.

Meanwhile, between 1800 and 1840 the growing interest in the biology of micro-organisms invited work on this other tradition in ætiology—the theory of a *contagium vivum*. The logic here was the same as in the search for atmospheric poisons: there must be specific causes of specific diseases. The development of improved microscopes proved more helpful to those who suspected a living contagion than it did for those who focused their thought upon atmospheric conditions.

Early in the nineteenth century, articles appeared in which the "cause" of diseases was ascribed to particular organisms or "insects". ("Bugs" would be the current slang equivalent of the latter term.) In the United States, for example, one John Crawford of Baltimore expressed views along this line in a literary journal entitled the *Observer and Repertory* as early as 1807. With the advent of the achromatic microscope in the 1830's, however, there came Ehrenberg's great work on the infusoria, which immediately aroused a widespread interest in the ætiological significance of "animalculae" in general.[1] This was reflected in the United States in the ingenious essays of Nott, Drake, and J. K. Mitchell, in which an animalcular origin or transmission was claimed for cholera, yellow fever, and other communicable diseases.[2] These essays were by no means "merely speculative", just because they were not worked out in laboratories. They were based upon a consideration of all the data then available.

Pursuing very practical clues, meanwhile, Paget found the *trichina spiralis* in the human body in 1833, and Joseph Leidy later (1847) found it in pork. Virchow and others showed its ætiological significance and the method of transfer between infected animals and man. Bassi demonstrated that silkworm disease is due to micro-organisms; and J. L. Schönlein proved that the scalp infection known as favus was caused by a microscopic vegetable infection. Other leaders, such as Henle, Rokitansky, and Dujardin, became concerned with parasitology, which had an obvious relationship to bacteriology. As a result of considerable evi-

[1] On Christian G. Ehrenberg (1795–1876), see Eugen Holländer: *Anekdote aus der medizinischen Weltgeschichte* (Stuttgart, 1925), pp. 150–2; Nordenskiöld, op. cit., p. 427.

[2] See J. C. Nott: "Yellow Fever Contrasted with Billious Fever: probable insect or animalcular origin," etc., *New Orleans Medical Journal*, IV (1848), 563 ff. Interesting notes on the growing interest in animal transmission of disease, c. 1840, will be found in Henry Holland: "On the Hypothesis of Insect Life as a Cause of Disease," *Medical Notes and Reflections* (Lon., 1840), pp. 567 ff. I am also indebted above to a MS. by Phyllis Allen: *Etiological Theory in America Prior to the Civil War* (1946).

dence as well as of intelligent reasoning, both Bassi and Henle gave clear statements of animalcular factors in the causation of communicable disease. The latter declared that the material of contagion is not only organic in nature, but is made up of individual living things which stand in a parasitical relationship to the diseased body.[1]

At about the same time, Cagniard de la Tour and Schwann independently discovered that micro-organisms were involved in the ordinary processes of fermentation. As it had long been suspected that fermentation was closely related to the process of infection, the likelihood that the latter was also due to micro-organisms became all the clearer.

Interest was likewise displayed, during the forties, in the use of viruses to produce artificial immunization. Hence most of the basic ideas, and even some of the procedures shortly to be utilized in the rapid development of bacteriology, parasitology, and immunology between 1865 and 1880, had already been marked out by the middle of the century. It is true that such writings as Henle's remained of necessity partly theoretical in character, and that this prevented their receiving such attention as one would—from our present viewpoint—think they deserved.[2] What was needed was a definite and final demonstration of the role of micro-organisms, as causative factors in the contagious diseases of men and animals. But how could such a demonstration be made?

When Henle was writing his essay on contagion, in 1840, so many suggestions about a *contagium vivum* were coming in as to necessitate some check on reckless theorizing. He therefore laid down several principles to be observed before a disease should be ascribed to a given organism. The supposed cause must always be associated with the particular disease, and not with others; and again, the parasites must be isolated, and then proved to be capable of producing the disease once more.[3] It remained for the next generation, aided by the same microscopes as were employed in histology, to realize in practice the value of these procedures.

The demonstration in several cases that animal and human diseases were due to minute parasites naturally stimulated research along this

[1] *Pathologische Untersuchungen* (Berlin, 1840), p. 15. Cf. Duclaux: *Pasteur: The History of a Mind* (trans. by E. F. Smith and Florence Hedger, Phila., 1920), p. 229. On Bassi, see Castiglioni: *History of Medicine*, pp. 692–3; Ugo Faucci, "In memoria di Agostino Bassi (1773–1856)", *Riv. d. Stor. delle Sci. Med. e Nat.*, 4 S., XXVII (Florence, 1936); and the same author's "Contributo alla storia della doctrina parassitaria delle infezioni", ibid., 4 S., XXV (1934), 27 ff.

[2] H. E. Sigerist: *Einführung in die Medizin* (Leipzig, 1931), pp. 236–8. On the early attempts at immunization, see Chap. IV.

[3] Jacob Henle: *Pathologische Untersuchungen*, pp. 15 ff.

line.[1] Further instances of the same sort were soon found, as when Griesinger showed the cause of Egyptian chlorosis (hookworm), and Küchenmeister discovered the tapeworm (1852). At this point the search for other pathogenic organisms was assisted from an unexpected quarter. La Tour's theory that fermentation in beer was due to the growth of living yeast cells aroused a controversy that lasted for some years. Liebig and others opposed this view, holding that the so-called cells were simply by-products of a chemical process. The presence of minute organisms, chiefly bacteria, was soon demonstrated in other kinds of fermentation; and while many authorities viewed this as substantiating La Tour's work, others again insisted that the bacteria were only by-products of the fermentation itself. If so, their presence indicated a spontaneous generation of living organisms. But was this possible?

It had long since been proved that there was no such thing as the spontaneous generation of large animals. Van Helmont's supposed recipe for producing rats—"cook up a pot containing corn with a dirty shirt"—would hardly have been taken seriously by an educated person in this period.[2] The origin of the mysterious, swarming microscopic life that always appeared in putrefying materials was another matter. Various experimenters sought to show that if liquids were made sterile by boiling, no organisms appeared and no fermentation ensued. These tests were sometimes successful, sometimes not. The battle over spontaneous generation continued to be waged until 1858. Then Louis Pasteur of Paris, who became interested in fermentation as a chemical problem, repeated the experiments on sterilization in such a manner as to put an end to this ancient controversy, so far as visible organisms were concerned.[3]

If bacteria and other small organisms were not the products of fermentation, then it seemed clear that they must be the cause. The repercussions of this conclusion were immediate and memorable. The analogy between fermentation and putrefaction on the one hand, and of infection on the other, was too obvious to be overlooked. The pus of

[1] I am following here the excellent brief account in E. R. Long: *A History of Pathology*, pp. 235 ff.

[2] Newsholme quotes the recipe in *Modern Preventive Medicine*, p. 25.

[3] See his contributions to the *Ann. de Chemie. et de Phys.* 3 ser., LII (1858), 404; and to the *Comptes rendus de l'Acad. des Sci.*, XLVII (1858), 224. Ferdinand Cohn, of Breslau, later accounted for the failure of boiling to sterilize some mixtures, by showing that certain bacteria pass through a spore stage, during which they are able to resist remarkably high temperatures. It is not certain, to this day, that filterable viruses may not be spontaneously generated. On this point see Joseph Needham: *A History of Embryology* (Cambridge, Eng., 1934), p. 189.

wound infections, for instance, had long been compared to fermentation products. It was apparent, too, that if small bacterial parasites were responsible for wound infections, they were also likely to produce infections of a less obvious nature. It appeared that biologists were at last on the threshold of a new era in the study of disease causation. Two centuries of observation and speculation about a *contagium vivum* were about to be vindicated.

Wound infections had always been the chief cause of the mortality following operations of any sort. Surgeons ever dreaded the development of erysipelas or of "hospital gangrene", and viewed the appearance of "laudable pus" as unavoidable.[1] The English surgeon John Bell stated, in 1812, that abdominal wounds were so dangerous that it was only necessary to record the rare cases of recovery. Compound fractures were so commonly fatal that amputation was usually resorted to, but the latter procedure itself represented only a choice of evils. Despite the use of ligatures and other improvements that went back to the time of Paré, military surgery was at best a ghastly business. Of the 13,000 amputations performed by French army surgeons during the Franco-Prussian War, no fewer than 10,000 proved fatal![2]

The introduction of anæsthetics during the forties was not quite the perfect boon to mankind that has generally been supposed. General anæsthetics prevented much suffering, but their use made more operations possible, and thereby indirectly increased the mortality from infections. Here and there individual surgeons were greatly concerned about the whole situation; and some attempted unsuccessfully to lessen mortality by cleanliness and by the employment of special washes for wounds. The work of Holmes and Semmelweis in preventing puerperal infection would seem to have pointed the way; but as this was based simply upon empirical observation in their own field, it does not seem to have greatly impressed surgeons in general. Pasteur's demonstration that bacteria were responsible for fermentations now gave the clue that was needed to reform surgical procedure all along the line.

Joseph Lister, who had been seeking for three years to reduce the mortality in the surgical wards of the Glasgow Infirmary, seized upon this clue in 1864 and put it to a systematic test.[3] If micro-organisms caused wound fermentation (suppuration) they must be excluded at all

[1] The memoirs of the Philadelphia surgeon, W. W. Keen, who served with the Union armies during the Civil War, are especially interesting in this connection.

[2] Newsholme: *Modern Preventive Medicine*, p. 30.

[3] Pasteur's work was called to his attention by the chemist, Thomas Anderson; J. D. Comrie: *History of Scottish Medicine*, II (1932), 635 ff.

costs. Not only the hands and instruments of the surgeon must be clean, but the very air surrounding the patient must be purified. Searching for a powerful antiseptic, Lister followed the sanitarians in adopting carbolic acid, and provided a continuous spray of this over the wound during operation. It was immediately found that this prevented infection and permitted healing "by first intention" in the majority of cases.[1] Experience later showed the air-spray to be unnecessary; but this in no way changed the principles involved, and the general idea of air sterilization has actually been revived in recent years. The work of the British surgeon eventually made a great impression on the entire Western world. Antiseptic and aseptic procedures were gradually introduced into general surgical practice, although, strangely enough, it required about two decades to bring about their complete acceptance in the United States. Surgery was at last rendered relatively safe as well as relatively painless. This development, in combination with the continued use of anæsthetics, made possible many types of operations hitherto unthinkable. An ancient but very limited art was metamorphosed, within a generation, into a potent and impressive science.

The development of antiseptic surgery was, then, one of the first triumphs of modern medicine. But medical bacteriology would not stop with this. Wound suppuration represented only a special class of infections; what of the many contagious diseases? Chemists, biologists, and medical men worked with increasing enthusiasm, especially in France and in Germany, to search out the bacteria which might be responsible for these illnesses. Davaine, who had actually observed anthrax bacilli as early as 1850, returned to a study of them with renewed enthusiasm after reading Pasteur's papers in 1863. He was soon able to show that the inoculation of healthy sheep with blood containing these rod-like creatures produced anthrax, while blood free from them had no such effect.[2] He did not, however, succeed in isolating the bacilli in pure culture.

During the late sixties and early seventies a number of investigators discovered micro-organisms usually associated with wound infections. Obermeier observed long spiral bodies (spirochætes) in the blood of patients ill with relapsing fever (1873); and Losch ascribed tropical dysentery to an amœba (1875). Cohn worked out the fundamental biology of bacteria by 1870. But complete proof of the pathogenic rôle

[1] Sir Wm. W. Cheyne: *Lister and His Achievement* (Lon., 1925), pp. 42 ff.
[2] Long: *History of Pathology*, p. 236.

of these organisms was still lacking. Not until 1876 did Robert Koch, who had studied under Henle at Göttingen, demonstrate beyond doubt that specific bacteria caused a specific animal disease. Working again on the anthrax bacillus—which made a good "laboratory animal" on account of its size—he was able to (1) obtain pure cultures, (2) produce the disease by inoculating these into a healthy animal, and (3) recover the same organisms from the blood of the now infected animal. Almost simultaneously Pasteur reached the same results.[1]

Henle's principles, henceforth known in an elaborated form as "Koch's postulates", were at last satisfied. The moment is perhaps worthy of dramatic emphasis. "First of all searchers, of all men that ever lived, ahead of the prophet Pasteur who blazed the trail for him, Koch had really made sure that one certain kind of microbe causes one definite kind of disease, that miserably small bacilli may be the assassins of formidable animals." [2]

Koch's success was due in part to the skill and originality which he and his colleagues displayed in developing a technique for bacteriological research. Pasteur and others, working in the sixties, had simply observed bacteria as best they could without any other aid than that of the microscope. In 1871 Carl Weigert was able to stain certain organisms with a dye taken from cochineal. Four years later the aniline dyes, developed by Ehrlich, became available; and Weigert found that one of these, methyl violet, stained bacteria with fine clarity. Koch now employed such stains, fixing films of bacteria by heat on a glass "coverslip" prior to the application of the dye. The latter's greatest technical contribution, however, was his utilization of solid culture media, in the place of the broths hitherto employed. It was almost impossible to separate different bacterial strains in these latter mixtures; but the addition of gelatine and subsequent cooling in solid form found different strains fixed at different points in the mass, whence they could be removed and studied in isolated cultures. As it was most important to know that only one sort of bacteria was involved in a given experiment, this seemingly simple device really provided the conditions essential to all further progress in bacteriology. The most rapid development of

[1] Koch reported his work in *Beiträge zur Biologie der Pflanzen*, II (1877), 277; Pasteur in *Comptes rendus de l'Acad. des Sci.*, LXXXIV (1877), 900. On Pasteur's work, see also René Vallery-Radot: *The Life of Pasteur*, transl. by Mrs. R. L. Devonshire (N.Y., 1926), pp. 75 ff.

[2] Paul de Kruif: *Microbe Hunters* (N.Y., 1926), p. 116. Priority is sometimes claimed for Pasteur; see, e.g., F. P. Gorham: "The History of Bacteriology", etc., in M. P. Ravenel (Ed.): *A Half Century of Public Health*, p. 69.

the science dates from 1882, when the new technique was made generally known.

Bacteriology and the related sciences were also aided, at this point, by the establishment of laboratories and of special institutes. Increasing public attention to the field led, sooner or later, to increasing public support. The Imperial Health Office in Berlin was provided with its own laboratory soon after its establishment. Max von Pettenkofer, who was professor of dietetic chemistry at the University of Munich, founded in connection therewith (1879) the first general institute devoted to research in all phases of public hygiene. The Pasteur Institute was founded in Paris in 1888; and this example was soon followed in large cities throughout the world. The first American laboratories of hygiene, for example, were established at the University of Pennsylvania and in connection with the New York City health department in 1892.[1]

Some of the men who directed the bacteriological laboratories were physicians who, like Koch, were attracted to them from private practice. Others were army medical officers or public health men, trained originally in "sanitary science", who found new fields more promising. Pioneers of this type were largely self-trained, as was George M. Sternberg, Surgeon-General of the United States Army, whose interest in bacteriology was to prove valuable to medical science.[2] As the fame of certain laboratories grew, young medical men entered them to assist in research and to receive the training which could not yet be had in regular medical schools.

The most famous centre was Pasteur's Institute. But as the work of Cohnheim, Koch, Loeffler and others threatened to overshadow that of Pasteur, a conscious rivalry developed between Paris and Berlin which was partly individual and partly nationalistic in nature. Able students flocked to both centres from all over the Western world. Russians in Paris and Americans in Berlin were soon to make names for themselves. A young American physician, William H. Welch, after working with Cohnheim and Koch, brought home a splendid training and enthusiasm that did much to bring American medicine finally up to the standards of the best European schools.

[1] It may be interesting to historians in this country, to recall that the Pennsylvania school owed its origin in part to the efforts of a distinguished American historian, Henry C. Lea of Philadelphia; E. S. Bradley: *Henry Charles Lea* (Phila., 1931), p. 209. See C.-E. A. Winslow: *The Evolution and Significance of the Modern Public Health Campaign* (New Haven, 1923), pp. 34 ff., for the work of William H. Park in developing the New York City laboratory.

[2] Martha L. Sternberg: *George Miller Sternberg* (Chicago, 1920), pp. 69 ff.

The emergence of Russian medical research paralleled that of American, and was stimulated by much the same circumstances. Although a modern medical school had been provided with the establishment of the University of Moscow in 1755, little original work was done there during the ensuing century. About the middle of the nineteenth century, Nikolai I. Pirogov aided in founding the Military Medical Academy in St. Petersburg, and this soon became the best school in the country. Here Pirogov, who had studied in Germany, inaugurated original studies in military medicine. Thereafter, Russian physicians were attracted to France and Germany for further training. Virchow's influence led to much excellent work in pathologic anatomy in the Russian centres; while Pettenkofer stimulated a scientific approach to hygiene which the Zemstvo system strove to apply to the work of its rural government physicians and hospitals. Russian medical men were also attracted after 1870, as noted, to the French and German institutes which cultivated the new bacteriology, and Ilya Metchnikoff of Odessa and Paris became one of the leading figures in that field.

Notable was Russian achievement in physiology, which has been ascribed to the philosophical character of the Russian mind. Original inspiration came from such foreign teachers as Helmholtz, Ludwig, and Claude Bernard; but by 1875 Ivan M. Sechenov and others were pursuing independent physiologic research in the Russian schools. Best known of the native physiologists was Ivan P. Pavlov (1849–1936), who linked the old and the Soviet régimes, and whose work on the conditioned reflex was justly famous. The Oldenburg Institute of Experimental Medicine, which he directed after 1890, became a world centre for training and research in physiology.[1]

Most interesting was the appearance for the first time, especially at the Berlin centre, of the students of an Oriental nation. The best Japanese medicine had been based, since the seventeenth century, on Dutch sources known to the medical faculty at Nagasaki. This was desirable during the period when Dutch leadership was pre-eminent in Europe and America; but by 1870 the Japanese found that many of their Dutch works were translations from the German. This discovery coincided with the reopening of Japan to general European contacts, and led promptly to a desire to go directly to the best sources. Dr. Tomoyasu Sagara of the University of Tokyo is said to have consulted a Dutch scholar, one Professor Verbeck, who had been sent to Nagasaki

[1] W. Horsley Gantt, *History of Russian Medicine* (N.Y., 1937), 90 ff.; Henry E. Sigerist, *Medicine and Health in the Soviet Union* (N.Y., 1947), 17 ff. Sigerist emphasizes the philosophic bent of the Russian mind, as mentioned above.

by an American missionary organization, as to the fountainhead of medical learning. Verbeck recommended Germany, and to Germany the Japanese students proceeded to go. This was about 1870, and they consequently arrived in time to participate in some of the pioneer work in bacteriology and immunology. Within a generation such German-trained leaders as Kitasato and Shiga—as well as Noguchi in the United States—had established outstanding reputations in these fields.[1]

Progress in research benefited in this way from a growing international interest, as well as from technical advances and the establishment of laboratories. This was well, since in 1883 there came the first great test of the new bacteriology. Once more the "scourge of nations" moved out of India, invaded Egypt, and threatened the ports of southern Europe. Apparently another of the repeated epidemics, which had so terrorized the Continent during the preceding half-century, was about to descend upon it. Or was it at last possible to discover the actual cause of cholera and a means to prevent it?

In an attempt to answer this question both the Berlin and the Paris groups sent cholera commissions to Egypt, the one headed by Koch and Gaffky, the other by Émile Roux, one of the ablest of Pasteur's associates. Working amidst tropical heat and constant danger, the French Commission failed to find the cause of the disease. One of their own leaders, Thuillier, fell a victim to it, and thus became one of the first martyrs of science in this field. Koch and Gaffky, on the other hand, returned to Berlin and reported to the Imperial Health Office that they had found a "comma bacillus" which was always associated with cholera. They had not proved a causal relationship, and to do this Koch then made a famous trip to the old focus of the disease in India. Here he was not only able to isolate the deadly commas in pure culture and to prove that they caused the disease, but he also demonstrated the method of its transfer via soiled garments and contaminated water.[2] This made clear just what measures were necessary in order to protect a people against further cholera invasions.

Other discoveries of pathogenic bacteria were now made in rapid succession, for the bacteriologists were at last on the trail of the major infectious diseases. Koch, just before his Egyptian investigations, had made what was perhaps his greatest discovery—that of the tubercle bacillus, then responsible for about one seventh of the deaths from all

[1] Bun-ichi Hasama: "Geschichtliche Daten über die Ersteinführung des deutschen Medizinalwesens in Japan", *Sudhofs Archiv*, XXVI (Leipzig, 1933), 329; Herbert Fox: "Baron Shibasaburo Kitasato", *Ann. Med. Hist.*, n.s., VI (1934), 491 ff.

[2] Paul de Kruif, op. cit., pp. 140 ff.

known causes. This was the first instance in which final proof was afforded, in terms of the "postulates", of a bacterial cause of human disease.[1] Neisser had found the gonococcus in 1879; and Pasteur and Sternberg, working independently, had described the pneumococcus (then undifferentiated) in 1881; but in none of these cases was the ætiological role completely demonstrated. Edwin Klebs and Loeffler found the diphtheria bacillus in 1883; and during the next year Gaffky isolated the typhoid bacillus, already described by Eberth four years previously.[2] The tetanus bacillus was added to the growing list of suspected microbes by Nicolaier in 1884, and was isolated by the German-trained Japanese bacteriologist Kitasato. The latter added to his fame in 1894, when he discovered the cause of bubonic plague during an epidemic at Hong Kong, and thus exposed one of the greatest historic enemies of man. The cause of still another great plague, syphilis, was at last located by Schaudinn and Hoffmann in the form of the *Spirochæta pallida* (1905). It was hard to believe that these minute, transparent creatures could have been responsible for the social havoc wrought by this disease during the past 400 years!

Long before this the study of medical bacteriology had been paralleled by an equally fruitful investigation of other types of organisms which either caused or carried disease. Certain protozoa have been mentioned in this connection; for example, the amœba which causes tropical dysentery. Reference has also been made to early work on the more obvious fungi which are responsible for certain cutaneous infections, and to the identification of several insects and worms as causative factors in specific human diseases. A search for other pernicious parasites was continued through the seventies, and was rewarded at the end of that decade by a most important discovery. Laveran, a French physician on duty in Algiers, found in the blood of malaria patients the minute organisms he believed responsible for that illness.[3]

Some doubt was expressed about this, but Laveran's results were soon confirmed at Rome by Tomasi Crudeli and his associates.[4] In the Italian capital, close to the fever-ridden Campagna, other biologists followed this lead with great interest. Here Celli and Marchiafava, as well as Golgi of Pavia, demonstrated the distinct species responsible for the several forms of malaria, and the relation which the changes in their life-cycle bore to the several stages of the disease. Since a fairly effec-

[1] Reported in the *Berliner Klin. Wochens*, XIX (1882), 221.
[2] A useful list of this series of discoveries is given in Gorham, op. cit., pp. 71, 72.
[3] Edm. et Ét. Sergent et L. Parrot: *La Découverte de Laveran* (Paris, 1929), p. 5.
[4] Angelo Celli: *The History of Malaria in the Roman Campagna* (Lon., 1933), p. 168.

tive remedy was already available against malaria, what was most pressing in this case was to find some means of prevention. How could Laveran's parasite be kept from gaining access to the blood in the first place?

This remained in doubt for nearly a decade longer, and was finally solved by two British physicians, Patrick Manson and Ronald Ross, and by Grassi and others of the group in Rome. Manson, as early as 1879, had shown that the mosquito was responsible for the transfer of worms causing the tropical disease filariasis. The probable role of insects as disease carriers had been suspected on good empirical grounds since ancient times; and now that Manson had clearly demonstrated the truth of this in one case, he was inclined to suspect it in others. Living in London, he conveyed his "speculative" ideas to Ronald Ross of the Indian medical service; and the latter proceeded to work them out at a tropical post, despite his original ignorance of the subject and under no end of difficulties. Finally, in 1898, he was able to prove experimentally that the malaria of birds was transmitted by the anopheles mosquito. Later in the same year Grassi and others showed that human malaria is carried in the same manner.[1] This they did, as was necessary under the circumstances, by using human beings as laboratory animals. It appeared that the elimination of one species of mosquito would free a community of this ancient scourge.

This was demonstrated promptly by Grassi's work in the Roman Campagna. Even before this, Celli had prevented much of the cattle malaria there by protecting the animals from the sting of ticks. His programme was based on the findings of two American bacteriologists, Theobald Smith and F. L. Kilborne, who proved (1889) that "Texas fever" was spread among cattle by the bite of an infected tick. This discovery was of paramount importance, in that it preceded the work of Ross and Grassi, and revealed the complicated role of a secondary host necessary to the full life-cycle of an offending parasite.[2] It afforded an example, incidentally, of American contributions to European medicine; for Grassi, Celli, and other Italians who contributed to the final solution of the malaria problem were influenced by Theobald Smith's reports.[3]

Indeed, at this point some of the most striking achievements of medical entomology resulted from a sort of Italo-American entente. Smith impressed Grassi and Celli, and the latter in turn stimulated bacterio-

[1] Celli, op. cit., p. 169; Newsholme: *The Story of Modern Preventive Medicine*, pp. 146 ff.; L. O. Howard: "A Fifty Year Sketch History of Medical Entomology and Its Relation to Public Health," in Ravenel (Ed.): *A Half Century of Public Health*, pp. 417 ff.

[2] Howard, op. cit., pp. 415, 416.

[3] Celli, op. cit., p. 168.

logists and entomologists in the United States. Dr. William S. Thayer, of Johns Hopkins University, went to Italy to study in their laboratories and to observe field work in the Campagna. Upon returning to Baltimore he aroused the enthusiastic interest of a group of Hopkins men, Doctors Walter Reed, J. W. Lazear, and James Carroll, who were destined to make one of the next great advances in the field. The story of their final solution of medicine's greatest detective story—the origin of yellow-fever epidemics—is one of the best known of all the achievements of modern medicine.

Despite two centuries of observation and ingenious speculation, despite even the excellent work of Dr. Carlos Finlay of Havana in identifying the exact species of mosquito which carries yellow fever, no final proof of this transfer by insects had been afforded so late as 1899. In that year the American army in Cuba was threatened with an outbreak of the disease. A general clean-up—the old remedy of "sanitary science" based on the classical miasm theory—failed to give protection. Surgeon-General Sternberg sent a commission headed by the men named and by Aristide Agramonte, a Cuban physician, to make a special study of the whole yellow-fever question. Their subsequent investigation, involving the usual difficulties and risks, the use of human subjects, and the death of one of the leaders, Lazear, was dramatic in the extreme. By 1901 the Commission had completed a final case against the *stegomyia fasciata*—a criminal which had also passed under such aliases as *culex fasciatus* and *ædes calopus*.[1] Yellow fever, as well as malaria, could be eliminated by guarding against the mosquito.

There were other accomplishments in store for the medical entomologists. Notable were the ingenious and courageous studies of Colonel David Bruce, of the British Army, on the nagana disease of South African cattle, and on the sleeping sickness of Uganda. His discovery that nagana was caused by a protozoan carried by the tsetse-fly antedated Reed's work in Cuba. A few years later a similar cause and transfer of human sleeping sickness was demonstrated by Bruce, Castellani, and others, and the fly was found to serve as a secondary host as well as a carrier.[2]

Since that time insects have been proved responsible for the carriage of such important diseases as dengue, Rocky Mountain spotted fever, typhoid, typhus, and the bubonic plague. The Frenchman Yersin un-

[1] The extent to which Finlay rather than Reed is recognized as the discoverer of the yellow-fever carrier, would seem to be a matter of interpretation and emphasis. Recognition has been accorded to Finlay in the U.S.A. as well as in Latin America. See Benj. Lee: "A Tribute to Carlos Finlay", etc., Amer. Pub. Health Asso. *Rep'ts and Papers*, XXX (1905), 8 ff.

[2] Howard, op. cit., pp. 424–6.

earthed the essential fact that rats as well as fleas were involved in bubonic plague, and thus revealed a complicated causal chain which is said to have slain as many as 25,000,000 Europeans during the fourteenth century alone.

The indictment of the body-louse, as a carrier of typhus, was made by French and American biologists between 1909 and 1912, just in time to provide more scientific protection than would otherwise have been possible for millions of troops during the First World War. It is interesting to add that, from the historians' point of view, the louse is really the innocent victim in this case. For man was originally infected through rats and their parasites, and he in turn infected the louse. Worse than this, man sometimes recovers from typhus, while the infected lice always die. As Zinsser pointed out, we are a great menace to the health and happiness of these little animals.[1]

No one would question that medical entomologists share with bacteriologists the credit for the unprecedented achievements of modern preventive medicine. Indeed, their methods were so similar and the personnel involved was so often identical that the distinction between the two groups seems an academic one. The implications for public health work, however, were sometimes different. Entomological discoveries usually indicated that a disease was not literally contagious; while the finding of a direct bacteriological cause often implied transfer by contact. Thus the Reed Commission proved dramatically that yellow fever was *not* contagious, while Loeffler and Roux showed that diphtheria most certainly *was*.

Health authorities now had to exercise discrimination. In the case of cholera they must place less emphasis on quarantines and more on the sanitary control of water supplies. In the case of diphtheria, on the other hand, sanitation was useless, while isolation was all-important. On the whole, sufficient evidence of contagion accumulated to revive that declining doctrine. At the same time, the failure to find many dangerous organisms in the air suggested that sanitary reform had involved too much fear of miasms and odours as such. These changing viewpoints gradually expressed themselves in the activities of health departments after 1875. John Simon, for example, urged upon the British Ministry the fact that none of the earlier health legislation had made provision for adequate protection against contagion; and his office was able to secure a "Contagious Diseases Act" in 1878, and a "Notification Act" during the same year.

[1] *Rats, Lice, and History* (N.Y., 1935).

Quarantine regulations, meanwhile, were modified in the light of bacteriological discoveries. Where the period of incubation in a given disease was discovered, the number of days during which a suspected ship needed to be held was more exactly determined. Enthusiasm for fumigation was revived, in the belief that all sorts of bacteria could be destroyed in this manner. A period of "promiscuous disinfection and fumigation" ensued, which involved more than sailors and shipping. School children were liable, on any morning, to find their eyes smarting and the classroom air heavy with fumes of one sort or another. When the role of carriers was finally discovered, it was realized that the effectiveness of all this fumigation had depended on the destruction of mosquitoes, rats, and lice, rather than of bacteria as such.[1] Yet the idea of sterilizing the air within buildings may be revived, particularly as a protection against respiratory infections, if better methods can be found.

The revival of quarantines and fumigation did not mean that the sanitary reform programme was entirely abandoned. Rather was it continued under rational direction. The "sanitary science", which in early Victorian days had worked largely in the dark, now had a clear light turned on the exact problems to be solved. Once Koch had demonstrated the cause and means of transfer of cholera, and Gaffky had performed a similar service in the case of typhoid fever (1884), it was clear that it was not enough simply to supply a city with "clean" water. The most wicked bacteria could and did upon occasion lurk in the most translucent depths.

It is true that Dr. Snow of London, in the famous "Broad Street pump" investigation of 1854, had already shown cholera to be waterborne; but his work had failed to impress the world at large, perhaps just because it lacked any basis in rational medicine. Sanitarians, in their exaggerated reaction against the contagion theory, regarded Snow's view as a "peculiar doctrine";[2] and they were later convinced only by the specific evidence of the bacteriologists. Even then, so distinguished a sanitarian as Max von Pettenkofer of Munich (1818–1901) denied that evidence. In order to settle the matter he made his magnificent gesture—which by all odds ought to have killed him—of drinking a whole glass full of cholera bacilli without suffering any harm. It remained for the Hamburg cholera epidemic of 1892 to put an end to such scepticism.

In this epidemic the adjacent cities of Hamburg and Altona both used

[1] Hugh S. Cumming: "The United States Quarantine System During the Past Fifty Years", in Ravenel (Ed.): *A Half Century of Public Health*, p. 120.
[2] Newsholme: *Evolution of Preventive Medicine*, pp. 142 ff.

infected water supplies; yet the latter escaped the disease because it was already equipped with a filtering system which eliminated the cholera bacilli. Some of the best work of health departments in Britain, Germany, and the United States was devoted after this time to the building of such improved filtering systems. In the latter country it was the pioneer Massachusetts State board which took the initiative, and after making a study of methods employed in northern Europe, set up an experimental station at Lowell (1887). Here, under the able direction of William T. Sedgwick and in general co-operation with the Massachusetts Institute of Technology, outstanding research in water purification was conducted.[1] The first effective sand filter was constructed for Lowell in 1893. After 1900 most large cities constructed modern filters either of this type or of a mechanical nature.[2] Their advent put an end to most of the danger from cholera, typhoid, and other waterborne diseases. In due time, as bacteriologists discovered the role of food carriers as well as of water—notably in the case of milk—similar action was undertaken to protect communities from these additional sources of infection.[3]

There was still another important respect in which bacteriology profoundly influenced preventive medicine, and this was through the development of the related science of immunology. The essential principle involved here had already been established in inoculation against smallpox, and in vaccination with cowpox; namely, that inoculation with a relatively weak virus produced a mild case of the disease, and that this in turn protected the subject from further attacks even when the infection was a potent one. Efforts were made to achieve similar results by direct inoculation with such diseases as measles, the plague, and syphilis; and in the case of the first, definite claims of good results were made about 1845.

These experiments were by no means without danger. The serious

[1] William H. Welch viewed Sedgwick's work and that of the New York City laboratory as the two major American contributions to public hygiene; *Public Health Theory and Practice*, pp. 3, 4. See also C.-E. A. Winslow: *The Evolution and Significance of the Modern Public Health Campaign*, p. 34 ff.

[2] Filtering is an older process than is generally supposed. The first sand filters for London were constructed in 1829, but there was no exact knowledge of their relation to disease until after about 1890. See G. W. Fuller: "Water Works", Amer. Soc. of Civil Engineers, *Trans.*, XCII, 1218 ff.

[3] Charles E. North: "Milk and its Relation to Public Health", in Ravenel (Ed.): *Fifty Years of Public Health*, pp. 236 ff. Milk was noted as a disease carrier as early as 1881. For present knowledge of milk and other foods as carriers, see W. H. Park: "Contaminations of Milk from Microörganisms", etc., in his *Public Health and Hygiene* (Pila., 1928), pp. 371 ff., and his article, "Transmission of Pathogenic Bacteria to Man Through Meat, Fish and Shellfish", ibid., pp. 424 ff.

proposal to inoculate all the youth of France with syphilis, which actually went so far as a hearing before the Parisian Academy of Medicine, was certainly a somewhat perturbing one.[1] Yet, lacking an understanding of the persistent character of syphilitic infection, it may have seemed as logical to inoculate with the great pox as with the small.

The discovery and isolation of specific organisms causing a certain disease provided, for the first time, a rational basis on which to build a real knowledge of the immunizing process. Indeed, there are few better examples of the advantage of rational medicine than is afforded in the contrast between the slow progress made by immunology in the empirical stage, 1720–1880, and its rapid advance after some rational understanding was achieved. Logically, one might have expected the bacteriologists of 1880 to reason from the analogy of vaccination, and to experiment at once with attenuated strains of the first pathogenic organisms isolated. But it should be remembered that Jenner had no theory about what happened in his procedure, and that the very idea of attenuation was usually lacking in early inoculations. One only knew that a certain technique somehow produced mild reactions of a protective character.[2]

So it apparently was a happy accident, rather than close reasoning, which revealed to Pasteur new possibilities in immunology. Having isolated the organisms, he thought responsible for chicken cholera, he proceeded to prove this (1880) by injecting them into healthy fowls. The latter promptly died and the organisms were recovered from their bodies. This would ordinarily have been viewed as the end of the project; the cause of the disease had been found.

It happened, however, that some virulent cultures were put aside for several days in the course of the experiments. Then the birds, into which these were later injected, failed to die as expected. More remarkable still, the same birds refused to die even when inoculated with fresh cultures ordinarily lethal for their kind. Pasteur, quick to sense the significance of the seeming accident, realized that the inoculation with stale, attenuated cultures had in some way provided protection against further infection.[3] Perhaps the attenuated virus was able to arouse a protective mechanism in the bird, which in turn was powerful enough to ward off later attacks of a more serious character. The analogy with

[1] See Chap. IV.

[2] There is evidence that a few eighteenth-century investigators made conscious attempts to attenuate the virus used in smallpox inoculations; see Arnold C. Klebs: "The History of Infection," *Ann. Med. Hist.*, I (1917), 171.

[3] See his reports in *Comptes rendus de l'Acad. des Sci.*, XC (1880), 239 ff., and in the vols. immediately following.

smallpox inoculation was now apparent. Perhaps every pathogenic virus could be attenuated as soon as it was discovered, and then be employed to provide protection against the very disease for which it was responsible.

This seemed reminiscent of Hahnemann's doctrine that like cures like, but regardless of what tradition it substantiated, the subject obviously had amazing possibilities. Within a year, Pasteur produced an attenuated virus (vaccine) against anthrax. In the process of this work he found that one way to weaken or increase the virulency of a bacterial strain was to pass it through the bodies of appropriate animals. Jenner had noted this long before in the case of cowpox; but the observation does not seem to have made much impression on his contemporaries.[1] After 1880 the idea proved more fruitful, since it suggested that the newly discovered pathogenic organisms were by no means a constant factor in disease causation; they evidently had a natural history of their own, just as did larger plants and animals. The gain or loss of their virulency might well account for the manner in which epidemic diseases were known to have waxed and waned through the centuries. Sydenham, in his pioneer studies on specific diseases, had recognized these changes and ascribed them to what he termed "telluric influences". Pasteur was now able to suggest what these "influences" actually might be.

The French chemist's most brilliant success in immunization was his famous preparation of a vaccine against hydrophobia. Nothing better illustrates the futility of most early ætiology and therapeutics than the speculations of eighteenth-century physicians about the cause and cure of this particular disease. Benjamin Rush, for example, thought that one cause was "cold night air", another "the eating of beechnuts"; and he could think of no better remedies than cinchona bark and bleeding.[2] Pasteur was not able to discover the causative organism, but he traced the infection to the brain, and thus was able to obtain immunizing materials from that organ. The vaccine so prepared and injected after the bite of a mad animal seemed to prevent the disease in almost all cases. It was possible to secure such protection, even after infection, because of the slow progress which the virus makes from the point of infection to the brain. In most other diseases, having a shorter period of incubation, it was found that immunization must be provided before infection occurs.

[1] *An Inquiry into the Causes and Effects of the Variolae Vaccineae*, etc. (Lon., 1798), p. 52.

[2] *Medical Inquiries and Observations*, V (Phila., 1798), 213, 217.

The successful preparation of protective vaccines against chicken cholera, anthrax, and hydrophobia aroused an immediate interest in a rational explanation of the phenomena involved. What was the protective mechanism which the injection of vaccines seemed to set in motion? One of the first to suggest an answer, on the basis of actual observation, was Ilya Metchnikoff of Odessa. He had noted (1883) that amœboid cells in fluids could engulf solid particles; and upon coming to work with Pasteur in Paris, he found that the leucocytes of the blood surrounded and destroyed pathogenic bacteria in a similar manner. Further study revealed that the leucocytes were more effective as scavengers *after* a body had been immunized against a given disease than they were before. Evidently this process of "phagocytosis" must be the one by which a body defended itself against infection. But several investigators showed that the blood serum also destroyed bacteria, and that it too did this more effectively after immunization. Perhaps then it was primarily the serum which developed bactericidal properties.

The apparent issue here between cellular and humoral theories was resolved when Denys and Leclef discovered (1895) that both elements were involved. Immune or relatively immune serum seemed to affect bacteria in such a way as to make them more susceptible to the onslaughts of phagocytes. Paul Ehrlich then attempted to picture the process of immunization in terms of his complicated "side-chain" theory, which assumed the development of "antibodies" in immune serum. Further light was thrown on the protective mechanism when Pfeiffer observed that bacteria, placed in serum containing sufficient antibodies, tended to lose their motility, to clump together, and even to disintegrate. These agglutination phenomena were found by Widal to have diagnostic value in the case of typhoid fever; inasmuch as clumping indicated that antibodies were being formed in a patient's serum in reaction to infection with the typhoid bacilli.[1] If the bacilli did not clump when immersed in the patient's blood, that blood was free of antibodies and therefore of infection.

The plot thickened further when Jules Bordet and Octave Gengou discovered (1900–1902) that serum could be immunized against foreign red blood cells, as well as against bacteria, and that serum so modified would destroy such cells thereafter. They found another element in the serum involved in the process, which they termed "complement"; and this led in turn to the elaboration of a so-called "complement-fixation

[1] The principle has since been employed in diagnosing various infections.

reaction".[1] The whole matter became quite technical at this stage, and cannot be entered into here, other than to note that it was a modification of this complement-fixation reaction which August Wassermann developed (1906) as a valuable means for the diagnosis of syphilis.[2]

While Metchnikoff and Ehrlich were exploring the protective mechanism, other bacteriologists discovered practical possibilities in immune serum. Roux and Yersin of the Pasteur Institute found (1888) that the broth used in cultivating diphtheria bacilli itself possessed poisonous qualitities. Kitasato noted similar qualities in culture media which had once contained tetanus bacilli. Evidently some pathogenic bacteria produced a poison which saturated the surrounding media—a most sinister phenomenon. Emil Behring, of Berlin, proceeded to inject the poison or "toxin" of diphtheria into healthy animals and an immunization against the poison ensued, analogous to that which followed upon the injection of vaccines. In due time a normally lethal dose of the toxin could be injected into an immunized animal with impunity. More remarkable, if the latter's immune serum were now injected into a normal animal, this one in turn immediately acquired increased resistance to the toxins. This suggested that children might be immunized by injecting them with the immune sera of laboratory animals; but the idea was abandoned when it was found that the protection thus conveyed was likely to be of short duration.

Then it was that Behring decided to approach the matter from another angle. The chief contributions made by bacteriologists up to this time had related to preventive medicine. Was it not possible that cures might also be found? Behring thought it was, and injected immune animal serum (antitoxin) into a large number of Berlin babies already desperately ill with diphtheria. The results were encouraging, in some cases little short of miraculous. A cure had apparently been found for the most feared of all the children's diseases.[3]

It is true that not all the babies were saved. There was difficulty, too, in securing adequate supplies of serum from immune animals. Guinea-pigs make an ideal laboratory animal in some respects—they are so utterly stupid and uninteresting, for one thing, that one is not likely to waste much sympathy on them—but the quantity of blood they supply is naturally limited. Sheep proved more satisfactory in this respect.

[1] The history of these developments is well summarized in A. C. Abbott: *Principles of Bacteriology* (Phila., 1915), pp. 267 ff.—an account based upon German sources.

[2] Just how technical this work did become will be made apparent by examining the technique of the Wassermann test (since modified) as it was employed about 1918; see Edward Vedder: *Syphilis and Public Health* (Phila., 1918), pp. 265 ff.

[3] De Kruif: *The Microbe Hunters*, pp. 193 ff.

Roux, returning to the study of diphtheria antitoxin, eventually employed horses, and was thus able to secure an ample supply. Working in the children's hospital in Paris, Pasteur's brilliant associate was able to reduce the diphtheria mortality from 50 to about 25 per cent. Although the final proof of the value of the antitoxin was never provided—since no one had the heart to refuse so promising a remedy to a "control group"[1]—the cumulative evidence was certainly in favour of this remarkable procedure. The same was true of an antitoxin against tetanus, which was developed in much the same manner as that against diphtheria.

Such risks as obtained in the use of diphtheria antitoxin were later avoided, to a considerable degree, by the development of a successful immunizing process. This involved the injection of small amounts of toxin held in leash, as it were, by the presence of antitoxin. The process was analogous to the use of immunizing vaccines against typhoid fever and certain other diseases, save that the latter consisted of attenuated or killed cultures rather than of immune serum. Children who received the toxin-antitoxin treatment were usually protected against further danger from diptheria.

One of the complications which sometimes ensued when antitoxin did have to be employed was the distressing reaction known as "serum sickness". This was soon seen to be analogous to certain reactions already recorded in the laboratory. Several American observers, notably Simon Flexner and Theobald Smith, had found that an animal which received one dose of foreign serum sometimes succumbed to a second, even when the latter had no serious effect on a control animal. Rosenau and others published systematic observations on such phenomena in 1906, showing that after a lapse of about two weeks following the harmless injection of some foreign protein, a second injection was followed by severe reactions and often by death.

This "anaphylactic shock" seemed the exact opposite of the expected result in immunization procedures, where repeated injections usually *increased* resistance to a given foreign material. In anaphylactic cases all resistance seemed suddenly to disappear. Nevertheless, anaphylaxis was found to be a phenomenon of immunization, since it was apparently due to the exaggerated action of antibodies produced in the host's blood or tissues by the first injection. It was as if a boxer, in responding to a blow, over-reached himself, and lost his own balance in

[1] De Kruif emphasized the lack of this final demonstration (*The Microbe Hunters*, pp. 203 ff.), and also stresses it in dramatic fashion in Sinclair Lewis's *Arrowsmith*.

the process. It has been suggested that symptoms suffered by patients in various infectious diseases may be due, not to the direct action of invading bacteria, but rather to the exaggerated activity of the antibodies marshalled to destroy them.

Within a decade, between about 1905 and 1915, it was recognized that various baffling diseases were caused by similar reactions to foreign proteins.[1] Thus hay fever, and some forms of asthma, eczema, and migraine, were traced to a "hypersensitiveness" in the presence of a long list of proteins. These gain access to the body largely through air or food. Since the allergic symptoms caused by such proteins were found to be merely local when small quantities were injected beneath the skin, the particular item causing the trouble could often be identified by harmless skin reactions. When all contact with the offending proteins could be avoided, the disease was eliminated. Later studies brought the strange phenomena of drug hypersensitiveness into the same general field of allergic disease, along with serum sickness, anaphylaxis, and protein sensitivity; and while the resulting picture is still something of a puzzle, the parts are at least beginning to fit together.

A very different approach proved successful in the case of the most serious venereal disease. Paul Ehrlich, working in Berlin, became interested in applying certain of the aniline dyes to the staining of living cells. To his surprise, the dyes showed a highly selective affinity with certain tissues. When methylene blue was injected into the veins of a rabbit, for example, it coursed through the body and sought out and stained only the nerve endings. Now, a stain that affected a particular tissue and passed all others by might conceivably unite with special species of bacteria or parasites and leave all surrounding cells unharmed. This idea revived in Ehrlich's mind an ancient ambition; that is, to find some specific drug that would destroy the cause of a disease without injuring those who suffered from it.

Ehrlich at once set to work, taking a hint from Laveran, who had found that arsenic killed certain trypanosomes causing a disease of horses. The former began, in 1902, to inject these minute animals into mice, and the mice died in due time in every case. Then there followed seemingly endless experiments, first at Berlin and later at Frankfurt, as Ehrlich sought to find a drug that would save his rodents from the fatal typanosomes. Aided by German organic chemists and by his Japanese associates, Shiga and Hata, he finally worked through a series of organic

[1] The curious fact of hypersensitiveness had, of course, been known for an indefinite length of time; Increase Mather, for instance, commented upon it in the seventeenth century in his *Remarkable Providences*, etc. (see Lon. ed. of 1856, pp. 70 f.).

arsenic compounds based upon the innumerable modifications of a single drug. Every conceivable form was created in the laboratories and tried out upon the sick mice. Hundreds of compounds failed, but the work went on in that thorough manner for which the Japanese as well as the Germans deserve no little fame. It was an interesting demonstration that even the oldest of all scientific methods—trial and error—might be combined with a rational approach in the modern laboratory. Finally, in the case of the six hundred and sixth experiment in the series there was employed a compound which literally worked like magic. A single injection destroyed every trypanosome in a mouse, and brought the animal back to health almost instantly. It was the first case of recovery on record.[1]

The next step was to find what human disease could be cured by the potent salvarsan, as Ehrlich named his drug—or "606", as it came to be called, because of the number of the experiment in which it was first tried.[2] The apparent similarity between trypanosomes and the treponema pallidum, identified a few years before as the cause of syphilis, suggested that salvarsan be directed first against the latter organism. A single dose, shot into an infected rabbit, proved most effective. The complicated "606" coursed through the body, apparently sought out and killed every spirochæte, and left all the body cells unharmed. Superficial lesions healed within a day, and there were no unpleasant reactions.

Considerable evidence of this sort soon justified the first trials upon human beings, which were made in 1910 in the hospitals of Berlin. Here, most remarkable of all, the drug proved as effective in men, in the earlier stages of infection, as it had in experimental animals. Patients whose vocal chords were so involved by syphilitic lesions that their voices were all but gone were enabled to speak clearly within a few days. It is doubtful if a more striking demonstration of the power of "the healing art" was ever made. A true specific was at hand, the like of which had not been discovered since the days when quinine was first isolated from cinchona bark. Better yet, this specific was apparently an immediate cure for one of the most sinister of diseases.

Later, as usual, there was some disillusionment. There were cases, fortunately rare, in which sudden death ensued upon the use of salvarsan. Such reactions seemed analogous to anaphylactic shock in serum therapy. In later stages of syphilis the drug did not always prove as

[1] I follow here De Kruif's dramatic account in *The Microbe Hunters*, pp. 343 ff.
[2] The technical term now used in this country is the more descriptive "arsphenamine".

effective as did potassium iodide and mercury. Some questions were raised, also, as to how one could be absolutely sure when syphilis—which in later stages is both tenacious and obscure—was finally eliminated. The Wassermann test and its later modifications proved of considerable service here, and apparently reliable criteria were set up in this connection.[1] So, despite the occasional risk, salvarsan and neo-salvarsan continued to be used with such apparent success that the epic of "606" took on heroic proportions.

[1] Edward Vedder: *Syphilis and the Public Health*, pp. 207 ff.

XV

FURTHER PROGRESS AND SOME OF THE CONSEQUENCES

AT ABOUT the time that Ehrlich first demonstrated his potent drug, it became possible to take stock of the developments in medical bacteriology and the related sciences, and to envisage their effects upon medicine as a whole. Some of these were obvious enough; other influences set at work were less apparent but no less far-reaching in their consequences. Most fundamental, perhaps, was the clarification of the nature of disease. Many diseases could now be defined in terms of causes, as well as in terms of symptoms or resulting lesions.

It will be recalled that the concept of specificity, developed by Sydenham on a basis of symptoms, had become a confused one when so many symptoms were found that there was no rhyme or reason in the whole business. The idea was rendered intelligible again when Morgagni and the French school worked out new classifications by correlating clinical with pathological findings. All this long-continued effort found its final justification in the discovery of specific causes. All three categories could now be employed—the symptom, the clinical-pathological correlation, and the cause—but the greatest of these was cause. Tuberculosis was a disease in which not only certain symptoms were frequently present and certain lesions usually produced, but it was a disease caused by specific bacteria and caused *in no other way*.

For practical purposes, then, the French clinicians had been right in viewing infectious diseases as invading entities, and Broussais and the German pathologists had been mistaken in opposing them.[1] It is one of the little ironies of medical history that German bacteriologists did so much to establish the validity of the older French view, in the very presence of their colleagues in pathology who had so condemned that view as mere "ontology". It was hardly surprising that Koch met with little encouragement from Virchow. To be sure, most moderns would agree that Virchow was correct in a philosophical sense—there would seem

[1] There are illuminating discussions of this theme in Knud Faber's *Nosography, passim*; and in H. E. Sigerist: *Man and Medicine* (N.Y., 1932), pp. 118 ff.

to be no need to think of diseases as absolute realities in a metaphysical sense.[1] Yet the organisms causing some of them *are* entities; and to the extent that diseases came to be thought of in terms of these causes, the former themselves could be quite conveniently conceived as things in themselves.

The evidence of this was soon found in the routine procedure of health authorities, who interpreted a diphtheria epidemic as the presence in the community of a definite invader. For that matter, the development of the individual case was best viewed in the same light; for the concept of a disease as a specific entity, caused by a specific entity, proved to be more useful in prevention and cure than any other view. First, the knowledge of a specific cause made possible a far more exact diagnosis than had hitherto been possible. The clinical symptoms in a case of "sore throat" might be confusing, but if great numbers of diphtheria bacilli were found in association with certain symptoms, then it *was* diphtheria. Second, once the diagnosis was made, a knowledge of specific cause made possible a rational search for specific prophylaxis or cures. This search had been rewarded by the development of all the successful immunizing processes, and of the partly successful serum and chemotherapy described.

The prevailing view of disease was also modified by a partial return to constitution doctrines.[2] It has been noted that the classical humoral pathology had been replaced, during the eighteenth century, by a growing interest in the localization of disease. Morgagni had found disease in special organs; Bichat refined upon this by tracing lesions into the tissue structures within organs; and Virchow carried the process to what seemed its logical conclusion in the cells within the tissues. By this time the "sick man" as a whole had been pretty well lost to sight. Studies in bacteriology, serology, and immunology, however, eventually tended to correct such extreme localization. Bacterial infection of the blood (septicæmia) was found to be a humoral condition which involved the body as a whole. Then again, the protective mechanism involved in immunization could not be localized. Phagocytes, antibodies, and complement pertained to no one region, but coursed gaily throughout the whole body. Perhaps the classical conception of the individual's "constitution"—never abandoned in folk-medicine—

[1] Protests are still made against the danger of this view; e.g., Wm. A. White: *The Meaning of Disease* (Baltimore, 1926), p. 153.

[2] Paul Diepgen: "Die Lehre von der Konstitution in der Vitalischen Medizin", *Klin. Wochens.*, XII, nr. 1 (Jan. 1933); H. E. Sigerist: "Konstitutionslehre u. Personalismus", *Neue Blätter*, III (1930), 101 ff.; Antonio Ciocco: "The Historical Background of the Modern Study of Constitution", *Bull. of the Inst. of the Hist. of Med.*, IV (Baltimore, 1936), 26 ff.

needed reviving in modern form. Perhaps the man with well-developed immunities was, after all, really blessed with a "strong constitution".

Incidentally, immunology suggested that it was an over-simplification to view pathogenic bacteria as *the* cause of a related disease. Lack of resistance, as well as attacks by bacteria, was a factor in producing illness. Ultimately, then, the "cause" of a given infection could only be defined as a reaction between pathogenic organisms and the body cells.

Quite important in reviving interest in constitution doctrines was the concomitant development of several branches of medical science which can be only mentioned in this connection. One was the rapid advance of biological studies in heredity. Originally stimulated by the work of Darwin, and by the rediscovery of Mendel's findings, these studies provided evidence that an individual's resistance to disease was at least in some cases and to some degree a hereditary matter. A man might not only possess a good "constitution", but he very likely owed it to the fact that his father had enjoyed one before him. Such views led to a desire for "eugenic" reform—to a demand that society protect its heredity as well as its environment.

Continued interest in the Darwinian theory contributed to the same end. Sir Francis Galton, a cousin of Darwin, established the Eugenics Laboratory in London in 1904; and here Karl Pearson developed the use of biometric methods in the further study of inheritance factors in disease. He held that the improvement of the social environment, through medical advances, was actually an obstacle to the improvement of the heredity of any given group. Did not the conscious prevention or cure of disease lessen the struggle for existence, and therefore limit the operation of a "natural selection" of the fittest? And would not the resulting survival of the weak lower the whole vigour of the race, and so ultimately produce increasing sickness despite all the medical devices set against it? "Darwinism and medical progress", declared Pearson in 1912, "are opposed forces." [1] He did not advocate the abandonment of medical services for this reason, but rather that they be supplemented by some control of heredity. Sterilization or segregation of those unfit for parenthood because of either physical or mental defects were the most obvious procedures to be followed.

Physicians who felt that such reasoning distorted environmental factors, pointed out that it involved unproved assumptions. It was not necessarily true that uncontrolled disease "selected" for death only

[1] "Darwinism, Medical Progress and Eugenics", Univ. of Lon. *Eugenics Lab., Lecture Ser.* IX (1912), p. 27.

the weak and spared only the strong. The accident of exposure might lead to the infection of the strong; and those of them who survived this might be the weaker for it during the rest of their lives. Indeed, uncontrolled disease of a non-acute character, such as chronic malaria or hookworm disease, seemed one of the best means to the development of a sickly race rather than of a virile one.[1]

Whatever the ultimate value of a eugenics programme may prove to be, it was not adopted promptly at the beginning of the present century. There were too many obvious objections based on "the sacred rights of the individual" with regard to his own person. Physicians themselves were naturally more concerned with the treatment of disease than with eugenics, either from habit of mind or from such reasoning as was suggested above. Hence studies of heredity led medical men to a broader observation of the results of heredity, rather than to efforts to control it—excepting always the extreme cases. Interest developed in the general physical types which were inherited from parents, and in their possible relation to disease. This was a revival, of course, of old constitution doctrines. By the end of the century German clinicians were once again observing the whole make-up and personality of their patients. The whole process would doubtless have been more rapid, had it not been inhibited by the trend towards specialization in practice.[2]

A third factor in promoting interest in "the sick man" was the development of endocrinology. The basis for this science was laid, as had been that of bacteriology, during the middle of the nineteenth century.[3] Addison, an English pathologist, then identified a disease of the suprarenal capsules which was subsequently named after him. This gave one of the first hints that the injury of ductless glands might be followed by systemic disturbances. No immediate progress followed. Other diseases now known to be endocrine disorders had long been recognized and were most carefully described, but no remedies were found. The truth is that the work of clinicians was essential in this as in other fields, to the end that the "clinical pictures" involved should be recognized. But once this base was provided, the clinicians could do little

[1] This aspect of the problem is analyzed in Edgar Sydenstricker: *Health and Environment* (N.Y., 1933), pp. 190 ff. (Monographs on Recent Social Trends in the United States).

[2] John C. Hemmeter: "The German Clinics of Today", *Phila. Med. Jour.*, May 4, 1901; Lewellys F. Barker: "The Development of Internal Medicine During the Past One Hundred Years, 1830–1930" (Reprint from *Centennial Vol.*, Univ. of Maryland, 1930).

[3] H. B. Shumacker, Jr.: "The Early History of the Adrenal Glands", etc., *Bull. Inst. of the Hist. of Med.*, IV (Baltimore, 1936), 54 ff.

more. It remained for biochemists, employing animals in laboratory experiment, to build upon it a real therapeutics.

Claude Bernard, as might be expected, was one of the pioneers in this work. In the course of his famous investigations of digestion he discovered the part played by the pancreas and other organs in the so-called "glycogen function". Experiments on the thyroid gland were made in 1856 and occasionally thereafter. Towards the end of the century rapid advances in the technique and knowledge of biochemistry in general led to revolutionary discoveries concerning the whole nature of internal secretions. Outstanding in this research was the work of American and Canadian investigators. Several of the ductless glands—the pineal, pituitary, thyroid, adrenal, and others—were found to produce one or more "hormones" which exerted a marked influence on the operation or development of other portions of the body. These substances, several of which were isolated and even produced synthetically, appeared to act as catalysts, and were amazingly potent. Epinephrine (adrenalin), which was isolated by John J. Abel of Johns Hopkins, could be detected in a dilution of 1 : 300,000,000 (shade of Hahnemann and his high dilutions!). Thyroxine, a thyroid hormone isolated by Edward C. Kendall of the Mayo Foundation, was equally potent. It was estimated that the total amount used by the normal body in the course of a year was only about three and a half grains. "But this pinch of material spells all the difference between complete imbecility and normal health." [1]

Such startling findings attracted many workers to the field, and in the ensuing enthusiasm it was difficult to distinguish speculation from actual demonstrations. An intricate correlation of glandular functions was said to provide a system which controlled all bodily processes in an almost mystical manner. While proof for this seemed lacking except in special cases, such speculation doubtless provided a stimulus for continued research of a sound order.

It was definitely shown, in experiments conducted in 1889 and thereafter, that any disturbance of the ductless glands was likely to produce specific diseases. Experimental studies on diabetes, for example, revealed that it was directly caused by the injury or destruction of the islands of Langerhans in the pancreas. The disease therefore had a local origin, but the resulting conditions involved the general metabolism of the body. This made it increasingly evident that certain diseases

[1] Roy G. Hoskins: "The Internal Secretions", in Julius Stieglitz (Ed.): *Chemistry in Medicine* (N.Y., 1928), p. 194; and the essays by Abel and Kendell in the same collection.

must be studied in terms of the interrelations of various parts or systems.[1]

Even a partial understanding of endocrine functions was rewarded by discoveries of therapeutic significance. Conditions due to the failure of glandular secretion could be controlled, though not cured, by the administration of the missing products prepared from animals. Thyroid extract was used successfully in cases of myxœdema as early as 1891. The same principle was involved in one of the major medical discoveries of the present century, when the Canadian physicians Banting and Best isolated (1922) the secretion of the islands of Langerhans. This secretion ("insulin") was found to act in the human blood serum just as would that of the normal islands of Langerhans, and so permitted a normal metabolism and the removal of diabetic symptoms.[2]

The discovery of insulin illustrated the manner in which different specialists in medical research were able, especially after 1890, to converge upon the study of a particular problem. Pathologists, working in the tradition of Virchow, were able to show the particular tissues in the pancreas, which were diseased or destroyed in diabetic cases. Biochemists, working in the tradition of Claude Bernard, were able to analyze the action of hormones, ferments, and the complex interchange of glucose and glycogen, which were involved in both normal metabolism and in the pathological physiology of diabetes. The pathological findings enabled Banting to select the particular tissue from which to secure the missing secretion, and a knowledge of physiological chemistry then enabled him to study its exact influence upon the metabolic process. It was only unfortunate that bacteriologists, working in the tradition of Pasteur and Koch, were unable to discover any infection responsible for the original necrosis of the pancreatic tissues.

Still another factor in the increasing attention paid to general condition, as distinct from a localized pathology, was the increasing understanding of the physiology of nutrition. The role of citrus-fruit juice in the prevention of scurvy, finally recognized in the eighteenth century was found to be paralleled by that of a number of other food elements in preventing various pathological developments. While the use of fresh fruit against scurvy had been a purely empirical procedure—and

[1] See Garrison: *History of Medicine*, 4 ed., pp. 694 ff.; and the same author's contribution on the history of endocrine doctrine in L. V. Barker: *Endocrinology and Metabolism* (N.Y., 1922). The complexity of present endocrinology is indicated in such a study as T. H. Larsen: *Physicians' and Surgeons' Text-Book on Endocrinology*, etc. (Los Angeles, 1934), a work of over eight hundred pages.

[2] J. J. R. MacLeod: "Insulin to the Rescue of the Diabetic", in Stieglitz, op. cit., pp. 298 ff.

therefore resulted in no further progress over a considerable period—the study of various food elements during the later nineteenth century proceeded upon the rational basis of physiological chemistry.

Lunin proved, while working on experimental diets with animals in 1880, that a synthetic milk diet lacked some element or elements necessary to nutrition. Two years later Takaki eradicated beri-beri from the Japanese navy by the use of a carefully studied mixed diet. This led Eijkman and Grijns to work with deficiency diets on fowls, as a result of which they were able to produce beri-beri experimentally in 1897. This was most significant, since up to this time there was no definite way of controlling or measuring malnutrition conditions. Thereafter such conditions could be produced in animals at will, while other animals were used as "controls" just as they were in most research. Food values could now be defined and measured in terms of animal reactions. In this way a technique and procedure was worked out for nutrition studies, which was just as valuable for that field as Koch's postulates and technique were for bacteriology.

Considerable experiment with beri-beri-producing diets followed, and between 1897 and 1906 a preventive substance was found to exist in rice husks. Such success encouraged renewed studies of scurvy, and a search for other diseases which might be due to a lack of vital food elements. Casimir Funk introduced, about 1911, the term "vitamins" for the extremely active elements in food so essential to normal metabolism. It was well understood by this time that in addition to the diseases mentioned, rickets and probably pellagra were caused by a lack of one or more of these mysterious substances. Whole classes of people were found to be living on a beri-beri- or a pellagra-producing diet, the prevention of which diseases remained thereafter a social rather than a scientific problem.[1]

One of the most remarkable discoveries in this field was the work of a number of Americans, who found evidence that the hitherto fatal condition known as pernicious anæmia was probably due to a long-continued defective diet. At any rate, Whipple and Robins found that the

[1] This was probably the case with *cachexia africana*, a strange disease which caused much concern among slave-owners in the West Indies and Louisiana before the American Civil War. While usually identified to-day as hookworm disease, it probably also involved pellagra or beri-beri: see R. H. Shryock: "Medical Practice in the Old South", *South Atlantic Quarterly*, XXIX (Durham, N.C., 1930), 160 f.; and "Medical Sources and the Social Historian", *Amer. Hist. Rev.*, XLI (1936), 467. Modern research in nutrition is clearly reviewed by E. V. McCollum and Nina Simmonds: "The Story of the Discovery of the Vitamins", in Stieglitz, *Chemistry in Medicine*, pp. 112 ff.; in Edward B. Vedder: "The Needless Sacrifice to Beriberi", ibid., pp. 180 ff.; and in other essays in the same collection.

addition of beef liver to an ordinary diet hastened the regeneration of blood in animals suffering from artificially produced anæmia; and in 1926 Minot and Murphy announced a similar improvement in human cases when beef or calves' liver was added to the diet.[1]

Evidence has accumulated in recent years to show that resistance to infection—whatever its exact mechanism—is conditioned to some extent by diet. An adequate supply of foods containing vitamins seems essential, not only as a defence against definite malnutrition diseases, but also against those of a bacterial origin. Here again the old constitution doctrines, such as the common-sense observation that one who was "run down" was likely to be taken ill, received a rational explanation. Immunization, in the broadest sense of the term, may hereafter involve dietary precautions as well as the possibility of using vaccines and serums.

Both cures and prophylaxis now came to include the cultivation of personal hygiene, as a means to improving one's "general condition". The treatment of pulmonary tuberculosis, which disease had resisted all the onslaughts of both serum and chemotherapy, was finally developed in terms of the body as a whole. The thing to do was to build up the patient's general health, and thus to aid the serum mechanism which waged actual war against invading bacteria. Open-air treatment was practised in Scotland in the eighteenth century; but the first real sanitorium was established by Hermann Brehmer in the Waldenberg mountains in 1859. Dettweiler, Spengler, and others opened similar institutions in mountain and winter resorts after 1875. The transformation which resulted, the contrast in methods between the horseback riding which helped to kill poor John Keats, and the fresh air, rest, and diet of the modern sanitorium, are too well known to call for further comment. Quite notable was the development of the sanitorium movement in the United States, beginning in 1876 with the splendid work of Dr. Edward Trudeau at Saranac Lake.[2]

The encouraging results attained in medical practice, especially after 1900, were bound to affect the whole spirit of the medical profession. The revolution in surgery, the services of endocrinology and dietetics, the value of serum and chemotherapy, the accomplishments of hygienic procedures—these in themselves were sufficient to banish the nihilism

[1] Edward Vedder: *Medicine: Its Contribution to Civilization* (Baltimore, 1929), pp. 182, 183. For a detailed account see B. S. Cornell: *Pernicious Anæmia* (Durham, N.C., 1927).

[2] The story is modestly told in his own *Autobiography* (N.Y., 1916). The yet-unrealized possibilities in chemotherapy are described in E. R. Long: "The Battle Against Tuberculosis," in Stieglitz: *Chemistry in Medicine*, pp. 686 ff.

of 1850. The negative enthusiasm of that day, which found its chief service in disclosing the uselessness of traditional remedies, was now replaced by positive amelioration and cure. Most of the drugs and procedures abandoned by the first generation of critical physicians were given up for good; their places were now taken by a limited number of remedies of proven value. No longer could one say, with Oliver Wendell Holmes, that if most medicines were thrown into the sea it would be so much better for mankind and so much worse for the fishes. Quinine, morphine, insulin, salvarsan, and diphtheria antitoxin were too precious for that.

The discovery of these remedies meant that the old search for cures was at last rewarded. For the first time, the medical sciences were coming to the aid of the healing art in a rational and systematic manner. The gap between science and medical practice, which seemed so impassable in Harvey's day and remained almost as wide in 1830, was finally bridged in many places. The old hope that pure science would prove of practical service to the sick was justified. This was an epochal achievement in the history of medicine. Indeed, it was an event in the history of mankind.

Yet the revolution in preventive medicine was of even greater significance. Something has been said of the development of public hygiene during the nineteenth century, and of the way in which medical bacteriology modified public health administration. The control of contagion, as well as sanitary improvement, was more intelligently directed after about 1890. The development of serology and of nutrition studies during the present century brought further significant changes in the activities of health departments. Since research in bacteriology and immunology went on in their laboratories, these departments took over to some extent the function of diagnosing contagious diseases and also of providing biological products to the public.

Such work had its advent in 1893, so far as the United States was concerned, when Dr. Hermann Biggs of the New York City health department set up a laboratory in which he provided diagnosis of diphtheria.[1] Health departments began supplying typhoid vaccines and diphtheria toxin-antitoxin, when the importance of protecting the whole community seemed to necessitate governmental assistance. A precedent for all such service had really been established long before in provisions for free vaccination against smallpox, but the new work was

[1] C.-E. A. Winslow: *The Evolution and Significance of the Modern Public Health Campaign*, pp. 35 f.

on a wider scale. Laboratories which were in a position to manufacture vaccines later found themselves in a position to undertake such complicated diagnostic work as Wassermann tests in a more efficient manner than could private practitioners. Only the outbreak of the First World War prevented the British public health service from providing diagnostic facilities to the whole medical profession.[1]

In addition, health departments found themselves engaged in another programme of a most ambitious sort. Laboratory investigation, co-ordinated with continued statistical reports, eventually carried them into a study of disease conditions which neither sanitation, isolation, nor vaccines could entirely control.

There was, notably, the old problem of venereal disease. Since the work of Ricord and his contemporaries, the medical profession had appreciated the grave character of these infections, but the public remained generally ignorant in this respect down to the period of the First World War.[2] The relationship of venereal disease to prostitution made it difficult for health officers to deal with the problem along the lines well established for other contagious diseases.[3] Social taboos seemed to prohibit notification and isolation procedures. Yet something had to be done about the matter, and one government after another sought for some sort of social control. In Great Britain, for instance, the authorities were stimulated by an international medical congress that met at London in 1913, and urged governmental action. A royal commission was appointed, and submitted a report in 1916 that startled and alarmed both Parliament and public. Hardly reassuring was its conclusion that "the number of persons that have been infected with syphilis, acquired or congenital, cannot fall below 10 per cent of the whole population in the large cities, and the percentage infected with gonorrhœa must greatly exceed this proportion".[4]

The danger of a general spread of these diseases, as a result of troop mobilizations, necessitated an immediate response to this report. The Local Government Board urged all town authorities to supply facilities for diagnosis and treatment, and in 1917 a venereal disease act prohibited anyone but a regular physician from treating such cases.[5] It will

[1] Newsholme, *Public Health and Insurance* (Baltimore, 1920), p. 85. On the other hand, the manufacture of biological products has developed primarily in the hands of pharmaceutical companies in the United States and approaches the scale of "big business".

[2] Wm. Allen Pusey: *The History and Epidemiology of Syphilis* (Springfield, Ill., 1933), Chap. VIII, affords an authoritative brief history of the medical side of the story.

[3] Edward Vedder: *Syphilis and Public Health*, pp. 212 ff.

[4] *Report of the Royal Commission on Venereal Diseases* (Lon., 1916), conclusion.

[5] Sir Malcolm Morris: *Story of English Public Health*, p. 137.

be noted that emphasis was laid here on the welfare of individuals, and that physicians rather than "sanitary inspectors" were becoming responsible for the programme.

Meanwhile an unofficial National Council, composed of doctors, churchmen, and other citizens, carried on a publicity campaign, advising all people of the dangers of infection and of the methods of prevention and treatment. Taking over the propaganda procedures developed during the war, they issued advertisements, posters, and pamphlets, supplied lecturers, and tried in every way to overcome the ignorance and prudishness of the times. Army and navy authorities carried on much the same campaign among soldiers and sailors. Many will recall that similar action was undertaken in the United States when she entered the war.

The public response was moderately encouraging, partly because of the war emergency, and partly because of a realistic reaction against the Victorian prudishness which had hitherto seemed a barrier to public education in all matters pertaining to sex. This was significant, inasmuch as it was realized that much future public health work must be devoted to the education of the individual along personal lines. The idea was by no means entirely new. Eighteenth-century writers on hygiene had certainly sought to educate their readers. The sanitary associations of 1850 had striven earnestly to enlighten public opinion, but chiefly with relation to general conditions rather than in regard to individual care.[1] After 1900 greater emphasis was laid upon the individual's contribution to public health through the care of his own. Examples of this may be had not only in venereal disease work, but also in the recent educational campaigns against tuberculosis, cancer, and "the common cold".

So it happened that modern health administration first emphasized sanitary reform, superimposed upon this a renewed interest in contagion, added laboratory research and immunization services a generation later, and finally became involved in a programme of prevention through popular education.[2] No governmental department experienced a more interesting or significant evolution during the same period.

How significant the whole trend of preventive medicine really was,

[1] See Chap. XII.

[2] C. V. Chapin discusses the several stages in the evolution of preventive medicine and health administration in an interesting article: "The History of Preventive Medicine," *Jour. Amer. Med. Asso.*, Jan. 1921. See also Winslow: *Evolution and Significance of the Modern Public Health Campaign*, pp. 57 ff.; E. J. Stieglitz: *A Future for Preventive Medicine* (N.Y., 1945), 15 ff.

in terms of both official and unofficial medical service, soon became apparent. After about 1870 the mortality of one after another of the chief infectious diseases began to fall; and this process was accelerated in a remarkable manner during the first decades of the present century. Except in special cases where definite remedies were found, or where a change in virulency seemed probable,[1] the fall in mortality was primarily caused by a decline in the frequency of certain diseases. Smallpox, typhoid, tuberculosis, diphtheria, typhus, malaria, yellow fever, and other plagues were checked or even eliminated in the half-century between 1870 and 1920. This was indeed the most impressive record in all medical history. The very type of statistical study which had first revealed the need for public health administration now recorded its achievements in convincing manner.

Although there was occasionally a wide variation in the experience of one nation as compared with another—as in the case of tuberculosis —the trend in most diseases was roughly the same in all progressive lands. Hence the illustrations taken here from England and from the United States possess in most cases a general significance. In like manner, the record of New York City can be cited as an approximate indication of the experience of progressive metropolitan areas in western Europe and America.

In a few instances serious diseases were checked largely by old-fashioned methods. The history of typhus fever affords the best example. The English death rate from "fevers" fell from an average of 885 per 1,000,000 for the decade 1861–1870, to 482 in 1871–1880. It will be noted that this remarkable improvement began during the heyday of "sanitary reform", and before the major developments in bacteriology. The typhus rate for the last decade mentioned was fifty-seven. It had probably already fallen before it was differentiated officially from the other fevers, but during the next two decades its decline was nothing short of spectacular. In 1881–1890 it was fourteen, and for 1891–1900 it averaged only three.[2] This continued improvement occurred before any rational knowledge of either cause or carrier was attained. It is true that Nicolle and others later found a rational explanation (1909) of all the preventive work accomplished, in terms of transmission by body-lice. But three years before that time the annual health report of the London County Council included the laconic statement: "There were no deaths from typhus . . . during 1906".[3] Im-

[1] As in scarlet fever.

[2] Newsholme: *Modern Preventive Medicine*, p. 112; and *Evolution of Preventive Medicine*, p. 132.

[3] *Lon. County Council, Ann. Report of Health Officer*, p. 39.

provements in personal cleanliness and housing had made this possible. All honour should be accorded here to the "sanitary science" of our grandparents!

In contrast to this, certain infectious diseases—against which sanitation was largely helpless—were checked simply by the widespread adoption of immunization procedures. Smallpox and diphtheria afford the most striking examples, although tetanus and perhaps typhoid could be cited as well. Smallpox is perhaps the most interesting case of all, not only because it was so long a major plague, but also because it was the first disease to yield to artificial immunization. The mortality reached a maximum just before Jenner's discovery, and fell immediately thereafter in such localities as introduced it on a considerable scale. In Glasgow, for instance, the disease caused nearly 20 per cent of all deaths over a twenty-year period between 1780 and 1800; between 1801 and 1806 this fell to about 9 per cent, and then to 4 per cent for the period 1807–1812.[1] As smallpox is a disease showing cyclical tendencies, however, it may be that only long-range views of its mortality are significant. This was afforded by the statistics for England and Wales, reported after 1837, which showed a definite though irregular decline from that date down to the epidemic of 1872. In that year, the worst during the registration period, the percentage of the total deaths in London caused by the disease was only 4·5, which rate had been exceeded in nearly every year of the preceding century.[2] Thereafter the English curve dropped rapidly. The actual rate was still as high as 160 per 1,000,000 for 1865–1870; by 1891–1895 this had fallen to twenty, and as early as 1906 not one death from smallpox was reported in any of the five chief manufacturing cities.[3] The disease largely disappeared from England after that time.

The same trend was observable in most other progressive nations during the same era, when more or less compulsory vaccination was being gradually introduced. Certain striking illustrations of the value of vaccination encouraged public action. The Franco-Prussian War served almost as a controlled experiment in this connection, for it happened that the German army was vaccinated systematically and lost less than 300 men from smallpox, while the unprotected French army lost over 20,000 from the same cause. The immune German troops, more-

[1] Vedder: *Medicine: Its Contribution to Civilization*, p. 266.

[2] Newsholme: *The Elements of Vital Statistics* (Lon., 1923), p. 409. About 23,000 people died in England of the disease in 1872.

[3] *Ann. Report of the Health Com't., Lon. County Council*, 1906, p. 22. The complicated course of British vaccination legislation, with its continued recognition of "conscientious objection" to the practice, possibly accounts for the persistence of the disease into recent decades. See J. H. Harley Williams: *A Century of Public Health in Britain*, p. 248.

over, had smallpox behind as well as in front of them; for during the same years nearly 130,000 deaths occurred among unprotected civilians in Prussia alone.[1] Vaccination was made compulsory in Germany in 1874, and was enforced with military precision. The disease thereupon practically disappeared from that country.

It is significant that where smallpox continued to appear it was in nations like the United States, where vaccination remained in part a voluntary matter. In this country, which reported one-fifth of the world cases in 1923–1924, the widest variation in mortality obtained between States or territories having adequate vaccination laws and those lacking them. Thus Puerto Rico, which possessed effective legislation, reported but 137 cases in the decade 1913–1923; while the State of Washington, with only slightly larger population, reported over 24,000.[2] The large American cities, which developed health codes and administration similar to the British, experienced a similar decline of the disease. The record of New York City, for instance, was quite analogous to that of London. A graphic representation appears on page 263.[3]

Cumulative evidence of this sort, for many years and for many cities, States, and nations, indicated clearly that it was not a variable virulency or other factors that accounted for the elimination of the disease.[4] The most-feared of all eighteenth-century plagues was conquered primarily by man's conscious effort, working through a discovery made before 1800, but applied on a wide scale only in the progressive era following 1870.

Water-borne infections were checked by a combination of bacteriological diagnosis and improved sanitary engineering. Cholera made its last serious appearance in the United States in 1873, and in western Europe in 1895. Subsequent freedom from the disease was not to be explained in terms of a decreased virulency. This is shown by the mortality of 1892–1893 in Hamburg, where out of a total of 16,957 cases there were no fewer than 8,606 deaths.[5] The contrast between that city and Altona, already mentioned, clearly indicated that the infection was eliminated by the protection of water supplies.

Similar procedures were responsible for a parallel decline in typhoid

[1] Kitasato was able to report a similar experience in the case of the protected Japanese army during the Russo-Japanese War; Vedder: *Medicine*, p. 266.

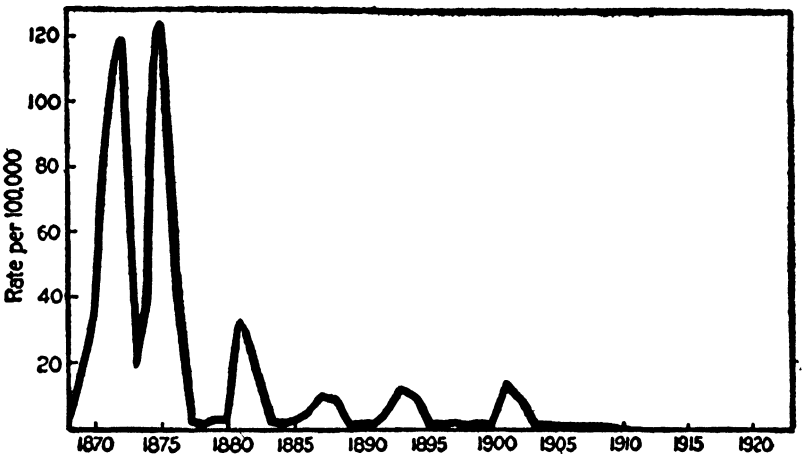
[2] Ibid., p. 267. Fortunately the disease in many parts of the United States seemed to be of a distinct type, so mild as to suggest a mutation of the original form; Newsholme: *Vital Statistics*, p. 412.

[3] From Harry H. Moore: *Public Health in the United States* (N.Y., 1923), p. 78. Reproduced by permission of the author and the publishers, Harper Brothers.

[4] For a discussion of other possible factors involved, see Newsholme: *Vital Statistics*, pp. 409 ff.

[5] Vedder: *Medicine*, p. 270.

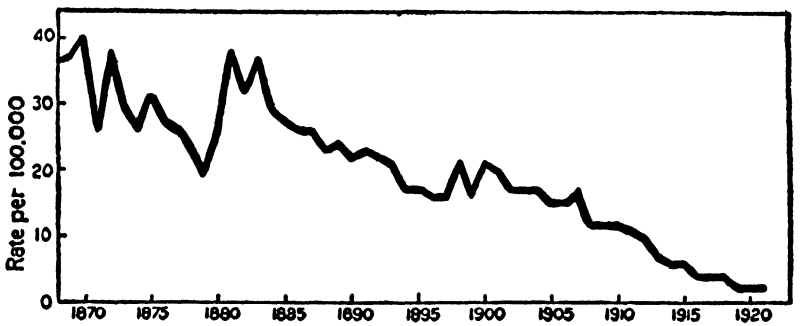
fever. As this was an endemic disease, it could not be checked so abruptly as was the exotic cholera; yet the progress of sanitary engineering reduced it in marked fashion after 1880. The average death rate from typhoid in England and Wales was 332 per 1,000,000 for the



Mortality Rate for Smallpox by Years, New York City

decade 1871–1880; during the next ten years it fell to 198. The correlation here with the protection of water supplies was quite clear. By 1901–1910 the rate was down to ninety-one, and for the years 1921–1925 it was only twenty-five.[1]

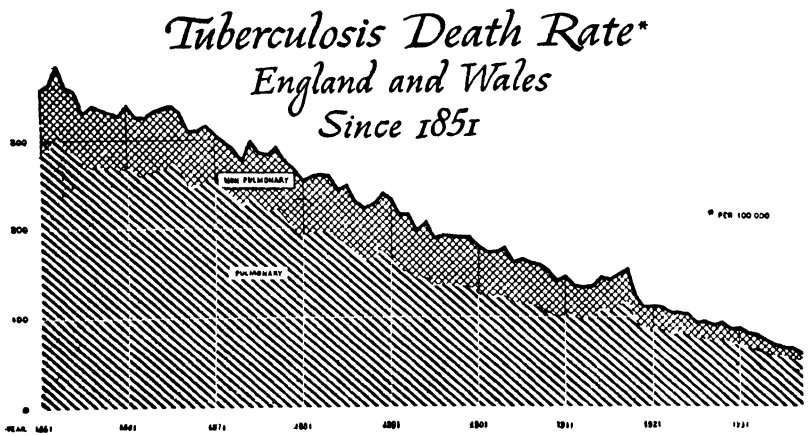
In the United States the less progressive areas lagged in the control of typhoid, but in the more advanced States and cities the record was again similar to the British. The mortality curve for New York City, which appears below,[2] roughly parallels that of the English cities:



Mortality Rate for Typhoid Fever by Years, New York City

[1] Newsholme: *The Evolution of Preventive Medicine*, p. 84.
[2] From Moore: *Public Health in the United States*, p. 84.

Less obvious in its origins, but almost as impressive as the story of typhoid, was the great decline in the mortality from tuberculosis. Although this apparently began early in the century in England, the actual rate was so high in the first decades that it may still be viewed there, as elsewhere, as the great plague of the Victorian era. It happens that pulmonary tuberculosis was usually well recognized as a clinical entity (phthisis, consumption) early in the nineteenth century, at a time when many other diseases were confused under such classifications as "fevers" and "dropsies". Hence in the chart on this page it is possible to represent graphically the decline in the death rate for all England and



Wales from 1851 on; and the same drop can be shown in the case of certain British or American cities for an even earlier period. The English curve [1] shows the long decline noted.

The remarkable chart on page 266 shows the average mortality from the same disease in New York, Philadelphia, and Boston, from 1812 down to 1945. It will be observed that the decline in "the white plague" in the United States did not set in until after 1880.[2]

There is no obvious explanation of the epic fall in the pulmonary tuberculosis curve, which has been roughly paralleled by that for other forms. Prevention has certainly played a major part, though improved hygienic treatment was probably also a factor. Many circumstances seem to have combined to decrease the frequency of the disease. If the history of the American cities was alone considered, it would appear as

[1] Chart prepared by G. J. Drolet for the New York Tuberculosis and Health Association and reproduced by permission.

[2] Chart prepared by the Prudential Insurance Company of America for the period 1812–1912; thereafter by the N.Y. Tub. and Health Assn. Reproduced by permission.

if improved living conditions, more exact diagnosis, and the increasing isolation of patients in sanitoriums were largely responsible. Their cumulative influence tended undoubtedly to decrease exposure to massive infection, and at the same time to increase resistance thereto.

The decline of the English curve before such changes were well under way is not so easily explained. It has suggested to some that a decreasing virulence in the bacilli concerned, or perhaps a development of racial immunity, will have to be taken into account. A British authority, however, after considering all possible factors, still explains the English record on the same grounds as those which apparently account for the American.[1] If this is correct, the evidence in both countries credits preventive medicine with another major contribution to human welfare.

Space will not permit of further accounts of the decline in specific infectious diseases. Many striking stories, such as the recent check of diphtheria and the vast improvement in the health of the tropics resulting from the control of malaria, yellow fever, and other insect-borne plagues, must therefore be omitted.[2] One especial phase of the subject, nevertheless, must be noted before the ultimate results in terms of total death rates are reviewed. This is the great gain in child health that has been so apparent for the last four decades.

The remarkable infant welfare movement which culminated about 1800[3] was followed by a period when infant mortality remained stationary or, as in the case of England, even increased at times. Improvement was again marked in most European countries in the last quarter of the nineteenth century, for both infants and for other children under five; but in Great Britain and Ireland it was only the latter class which experienced any drop in the death rate. So it happened that while the average infant death rate in France fell slightly from 226 in 1877–1886 to 218 in 1886–1895, the English rate actually increased during about the same periods from 167 to 176.[4]

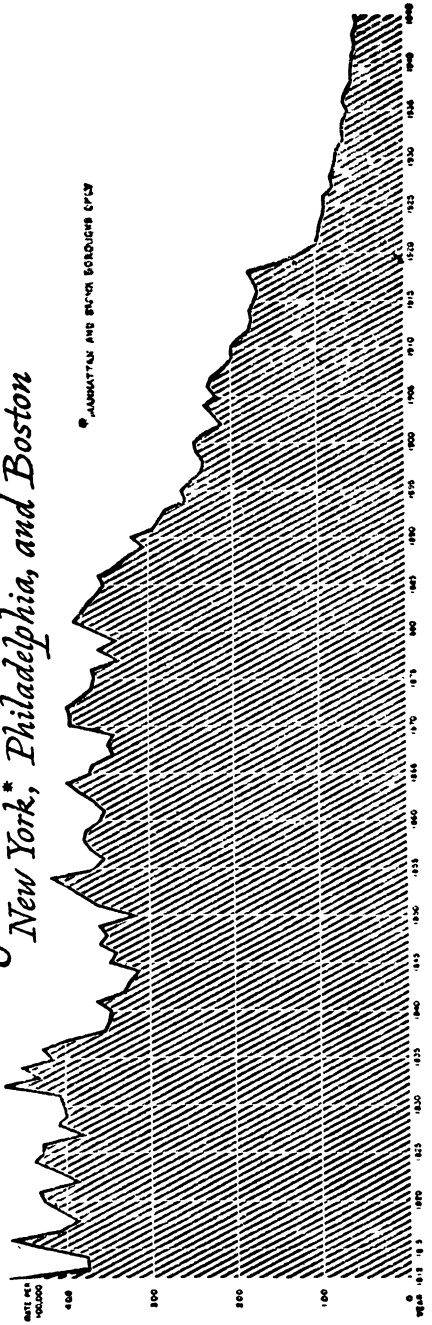
Many of the gains noted in the battle against infectious disease proved of especial benefit to children, since they were inevitably the first to suffer from this type of illness. They were not only the first to

[1] Newsholme: *Vital Statistics*, pp. 452 ff.

[2] The story of Gorgas and the Panama Canal Zone, for instance, has become the property of every American school child. See Edward B. Vedder: "The Development of Tropical Medicine", *Amer. Jour. of Trop. Med.*, XVI (Jan. 1936).

[3] See Chap. V. "Infant" is used here as indicating the period under one year; "child" includes those over one and under five years.

[4] The rates are per 1,000 inhabitants under one year. Newsholme gives the comparative figures (*Vital Statistics*, p. 350), quoting the *Annuaire Internationale de Statistique*, II (1917).


Pulmonary Tuberculosis Death Rate Since 1812
New York*, Philadelphia, and Boston
*MANHATTAN AND BRONX BOROUGHS ONLY
RATE PER 100,000
400
300
200
100
0
1812
1820
1830
1840
1850
1860
1870
1880
1890
1900
1910
1920
1930
1940
1950

be exposed, but in some cases possessed less natural immunity than did adults. Hence vaccination against smallpox, the control of diseases carried in water and milk, and immunization against diphtheria, were preeminently boons to children.[1] They also benefited from an apparently spontaneous decline in the virulence of scarlet fever. Equally encouraging was the appearance of a new infant welfare movement, directed towards improved diet, general hygiene, and prenatal care.

This movement expressed, in the medical field, that increasing concern about all phases of child life so noticeable in Western nations at the beginning of the present century. The exact cause of this is not easy to determine. Perhaps it was related to the decline in the birth rate noticeable in Western nations after 1870. Perhaps it was a result of the increasingly urban manner of living, which gave children free time and invited attention to what they did with it. At any rate, the movement expressed itself in art, manners, and education, as well as in medicine.

Juvenile literature in English was transformed during the half-century between 1850 and 1900, when writers ceased "improving" their young readers and sought only to entertain them. Maria Edgeworth, of unlamented memory, was succeeded by Mark Twain. "Little Rollo" made way for "The Treasure Seekers" and "The Wouldbegoods". Time was, even in American homes, when children "were seen and not heard"; after 1890 they were encouraged to express themselves, to say the least. Teachers in American schools once "taught the three R's"; after 1910 they were cautioned "not to teach subjects, but to teach children"! Both home and school became "child-centred" to a degree unknown before; and such organizations as the Y.M.C.A., Boy Scouts, and the juvenile courts looked after the time which youngsters spent outside of either home or school. It was natural that a generation so solicitous about children's freedom, and even their amusement, should also undertake the improvement of their health.

It was no longer necessary, thanks to the now-forgotten eighteenth-century movement, to relieve an infant of swaddling clothes. But there was still room for the improvement of his diet. Hergott at Nancy, and Variot at Paris, began during the nineties to establish welfare stations, where mothers could secure examination of babies and advice about their care. The New York City Department of Health was probably the first in the United States to become interested in similar work, and provided "well baby clinics" in 1908. The United States Children's

[1] On the percentage of children's diseases now curable by therapeutic procedures, see W. C. Davison: "Pediatrics—What Is It?" *Jour. Ped.*, III (1933), 64 ff.; and the same author's *The Compleat Pediatrician* (Durham, N.C., 1934).

Bureau, established four years later, encouraged the same programme among health departments and welfare agencies throughout the country.

In the more progressive communities a service was established in which, as soon as a birth was registered, the mother was visited by a district or public health nurse and urged to enroll at the nearest baby clinic. Here examinations and advice were given at weekly or other regular intervals. This service was often made free to the poor. While some parents capable of paying medical fees probably imposed on the clinics, the service seems to have reached primarily those for whom it was intended; that is, the poorer mothers who had hitherto received little or no regular assistance in the care of their babies. So valuable was the work considered by the American Public Health Association, that it now "sets as a standard that one-third of all babies born in a well-organized community should be registered at such clinics".[1]

One phase of infant welfare work of especial interest in the United States was the increasing attention paid to prenatal care. The maternal mortality rate remained relatively high in the United States, while it was reduced in the more progressive European lands. This was due in part to social factors which impeded public health work in all provincial areas. Rural isolation denied adequate obstetrical care to millions of women; and even where hospitals were available, the difficult cases often reached them when it was too late. Southern Negroes were attended largely by midwives of their own race, whose primitive folk-medicine was sometimes employed side by side with the best of modern hospital practice. These midwives are still the despair of both obstetricians and local health authorities.[2] It was also claimed that even regular American physicians received inadequate training in obstetrics, and that this was a factor in the relatively high maternal mortality rates in the United States.

To supplement such guidance as general practitioners provided, or to take care of those cases not seen by physicians at all, considerable pre-natal advice was given in some baby clinics. Associations or commissions were eventually organized in the larger cities to deal directly with all phases of the problem of maternal mortality. The Maternity Centre Association of New York City, to cite one of the best examples, provided advice and hospital service to a group of mothers between

[1] C.-E. A. Winslow: *The Road to Health*, p. 96.

[2] See Carolyn C. van Blarcom's article, "Rat Pie and Black Magic", in *Harpers Magazine* for Feb. 1930. An equally realistic and yet amusing account is given in R. A. Ross: "Granny Grandiosity", *South Med. and Surg.*, XCVI (Charlotte, N.C., Feb. 1934), No. 2.

1925 and 1929, and was able in this way to reduce the maternal death rate among them to less than 2 per 1,000, as compared with a rate of more than six for the country as a whole.[1] This showed that it was possible to bring American rates down as low as those of such progressive nations as Denmark and Holland.

The child welfare movement expressed itself in other ways than in direct medical service. Various old procedures for advising parents, or for helping children directly, took on new life. The concern for proper diet expressed itself in the improvement of school lunches, hitherto prepared—in the United States—by the janitors during their spare moments. In some cases the lunches were provided free. This practice had been inaugurated by Count Rumford at Munich, during the late eighteenth century, but it was now developed in a much more systematic manner.

More significant was the development of medical inspection in the public schools. This was inaugurated, in the United States, by the Boston school system in 1894; and the functions of the inspectors expanded along the lines of the public health work of the times. Meanwhile the rather formal and meaningless instruction in "physiology", which had held a place in the elementary curriculum for more than half a century was replaced or supplemented by instruction in personal and public hygiene.[2] There was less talk about "the effects of alcohol and tobacco", and more about the care of the teeth. Much remains to be done here, which has been overlooked or is not yet feasible. The schools might well be used, for example, to teach the dangers of quackery as well as the possible merits of the tooth-brush.[3]

Many welfare groups contributed indirectly to child health, even though this was not their sole function. Child labour legislation, first made effective in the United States when Massachusetts passed a stringent law in 1888, was of no little value. Day nurseries had some significance. The history of local, State, and federal child-welfare agencies, especially since the establishment of the Children's Bureau in 1912, was a complicated and important one which cannot be entered into here.[4] It was paralleled by a similar evolution in the chief European States.

It is interesting to note, with regard to both federal and State child-

[1] Winslow, *The Road to Health*, p. 96.

[2] See J. H. Beard: "The Need of a Revaluation of Health Education", *School and Society*, XXVII (N.Y., 1928), 703.

[3] R. H. Shryock: "Neglected Health Values in the Social Studies", *Educational Outlook*, V (Phila., 1931), 30, 31.

[4] The whole theme is well outlined in Philip Van Ingen: "The History of Child Welfare Work in the United States", in Ravenel (Ed.), *A Half Century of Public Health*, pp. 290 ff.

welfare departments, that much of their work has been directed towards the education of parents. Professional groups and lay publishers have also done much along this line. Health magazines, like school luncheons, can be traced to the eighteenth-century *Aufklärung*; but they became more effective during the new period of enlightenment that ensued after 1890. It is doubtful if any of the innumerable health periodicals that were published in the United States between 1830 and 1890 could compare with such current journals as *Parents' Magazine* or *Hygeia*. Both these devote considerable space to the physical and mental health of children, and are typical in this respect of the better sort of publicity now associated with the child-welfare movement.

Thus in many ways the conviction grew that children's health deserved more care, and that the high morbidity and mortality rates of the early years could be definitely lowered in this manner. The demand of the Enlightenment was echoed in the twentieth century. The massacre of the innocents must stop!

And stop it did, to a remarkable degree. Improvement in child health set in once more after 1900, and proceeded at a rate which few would have dared to hope for only a generation before. The English infant mortality rate, per 1,000 under one year of age, averaged 172 for the years 1896–1905; it dropped to 130 for 1906–1915. For nearly the same periods the French rate fell from 168 to 128. By way of comparison, it may be noted that the concomitant Norwegian decline was from ninety-two to seventy-two.[1] The favourable record of this and other Scandinavian nations reflected not only their progressive institutions but also their relatively rural character. Urban mortality in 1900, particularly for infants and children, was still much higher than was the rural rate. Only in recent years has the rapid improvement of child health in large cities at last brought their mortality down almost to that of country districts.

The improvement in infant health was usually most marked in the first months of life, and as this was always the period of greatest mortality, such improvement was bound to have a marked effect upon the crude death rate of whole populations. The same thing was true, to a lesser extent, for the health of all the years up to five. It is not surprising, then, that crude death rates, which were already declining in the later nineteenth century, fell sharply during the twentieth. There were naturally certain variations in different countries. Mortality remained

[1] Newsholme: *Vital Statistics*, p. 350.

relatively high in France, for example, and relatively low in the Scandinavian countries. Substantial decline had actually begun as early as 1810 in Sweden; but did not set in until about 1880 in England, and until 1895 in France. Yet the general trend was the same in all progressive nations after the latter year. This is indicated by the following figures for England and France:[1]

DEATH RATES IN ENGLAND AND WALES, AND IN FRANCE, PER 1,000 INHABITANTS

Years	1841–50	1851–60	1861–70	1871–80	1881–85	1886–90
England and Wales	22·4	22·3	21·3	21·3	19·4	18·9
France	23·3	23·9	23·6	24·3	22·2	22·0

Years	1891–95	1896–1900	1901–05	1906–10	1922	1925	1926
England and Wales	18·7	17·7	16·0	14·7	—	12·2	11·6
France	22·3	20·7	19·6	19·2	17·5	—	—

The mortality figures for individual cities tended, in the long run, to parallel the national rates. This can be readily observed despite the fact that, because of differences in age groups and other factors which affect the crude rate, there was considerable local variation. A chart showing the average death rate of four large American cities indicates a steady decline after about 1890.[2]

The improvement in urban health was so marked after 1900 that life in the cities at last became almost as safe as in rural areas. This was particularly true in the United States, where the usual advantages of rural conditions were offset by the relative superiority of urban health administration. In one case—that of New York City—the mortality was actually reported as below that of rural areas in the same State. While this was exceptional, it served as a pointed reminder that great cities could no longer be condemned as "the graves of mankind".[3]

The drop in crude death rates for both town and country was reflected in a parallel increase in the average life span. In the United States, for instance, the average life expectancy at birth in 1800 is estimated to have been thirty-three years. In 1850 it had increased to about forty-two. In 1900 it was approximately fifty; and by 1942 it had reached 63·7 for males. Thus, life expectancy at birth, which is a more significant index of well-being than are crude death rates, rose about

[1] Vedder: *Medicine*, p. 177. By permission of Williams and Wilkins Co.

[2] Chart prepared by the Prudential Insurance Co. of America, published in F. L. Hoffman: "American Mortality Progress During the Last Half Century", in M. P. Ravenel (Ed.): *A Half Century of Public Health*, p. 102. The cities are Boston, Philadelphia, New York, and New Orleans.

[3] It is also open to some difference of interpretation on statistical grounds, see Newsholme: *Vital Statistics*, pp. 276 f.

fifteen years during the very period when the full force of the new medicine was first felt in American society.[1]

The spectacular fall in the mortality of infants and children, to be sure, accounted for a large part of the decline in general death rates and the improvement in life expectancy. Yet some progress was made in the rates for all age groups up to about forty-five years, and in some cases even beyond that. After all, there were many instances in which medical progress was of service to adults as well as to children. When typhoid was checked, or when insulin was discovered, it was not simply the children who benefited.

In the long run nothing could be more impressive than such declines in death rates and the corresponding increase in the average life-span. There were many variations in time and place; but if the net result for all the Western World was longer life in the twentieth century than in any earlier epoch, something great and unprecedented had actually been accomplished. That this was due in considerable part to the development of the medical sciences can hardly be doubted to-day. Medical advance to be sure was inextricably entwined with general scientific progress on the one hand, and with general social progress on the other. The whole improvement in living standards, increased educational opportunities, and the conscious drive for the economic betterment of the working classes—as witnessed, for example, in the labour movement—were certainly basic to the decline in mortality. It is difficult to tell just how much of the drop in tuberculosis death rates was due to these circumstances, for example, and how much resulted from more effective medical treatment. Both medicine and social reform were certainly involved.

In other instances mortality decline can be traced primarily to medical procedures. Thus Winslow, in a study of the public health in New Haven during the half-century between 1875 and 1925, noted that the death rate there dropped from 18·2 to 12·5, and that over 90 per cent of this was due to a decline in the mortality from five specific diseases. In addition to pulmonary tuberculosis, these were diphtheria, typhoid, scarlet fever, and infant diarrhœa. The improvement relating to the last four was clearly due to definite medical procedures, plus such decrease in virulency as may have occurred in the case of scarlet fever.[2]

[1] See *Vital Statistics—Special Reports, Bur. of the Census*, vol. 16, no. 2 (July 8, 1942); Louis I. Dublin: "Trends in Longevity", *Annals Amer. Acad. Polit. and Social Sci.*, vol. 237 (Jan. 1945), 123 ff.

[2] The relative significance of medical and social factors in modern mortality trends is discussed in Edgar Sydenstricker: *Health and Environment*, pp. 184 ff.; and in C.-E. A. Winslow: Chap. VIII in Charles A. Beard (Ed.): *Whither Mankind* (N.Y., 1928).

There was no guesswork about it. And this leads to an observation of some sweep and significance; namely, that medicine entered during the present era that stage of development to which dynamics attained in the seventeenth century. Medicine at last became something more than an intellectual and professional tradition. It really worked!

XVI

PUBLIC CONFIDENCE REGAINED

STRIKING DISCOVERIES in therapeutics and preventive medicine, and the resulting decline in mortality, were bound to impress the public as no advances in anatomy or physiology could ever do. After all, what most men wanted from medicine was prevention or cure. It seemed for many years as if they sought these ends in vain; and the consequent decline of public confidence in medicine has already been described. It is quite likely that "the triumphs of modern medicine" came just in time to prevent a widespread revolt against orthodox medicine.

Bacteriological discoveries were especially calculated to arouse respect for the "wonders of science". There was something dramatic, to begin with, in the very idea of a living contagion—in the thought of doing battle against invading hosts. More than this, some of the first achievements of bacteriologists were of a decidedly spectacular character.

The best illustration is afforded by the work of Pasteur, whose genius for legitimate publicity was almost as great as his scientific skill. In some cases his influence was due to the force of fear. Hydrophobia was a relatively rare but particularly terrifying disease. Pasteur apparently conquered it, and for a short time was the only one who could perform this miracle. The news flashed round the world, and frantic individuals from many nations rushed to Paris as to a saviour. In other cases Pasteur's discoveries made an immediate impression on the most sceptical by virtue of their economic value. This was true of his work on beer, wine, and silk, and also of his studies on anthrax. Great industries were rescued from serious blights, and practical men to whom pure research meant nothing could not fail to respect a science which saved them millions.

On at least one occasion the French chemist staged a great public demonstration—at the request of an agricultural society—of the value of his anthrax vaccine. In the famous experiment at Melun, in 1881, every sheep not vaccinated by Pasteur succumbed to a deadly injection of anthrax bacilli, while not one of his vaccinated animals was even

harmed by the same treatment. Farmers, veterinarians, and newspaper reporters stood by in awe while the unprotected animals died before their very eyes.

> The world received this news and waited, confusedly believing that Pasteur was a kind of Messiah who was going to lift from men the burden of all suffering. France went wild and called him her greatest son and conferred on him the Grand Cordon of the Legion of Honour. Agricultural societies, horse doctors, poor farmers . . . all these sent telegrams begging him for thousands of doses of the life-saving vaccine.[1]

The Melun demonstration reminds one of that day when Otto von Guericke awed the burghers of Magdeburg with the wonders of his air-pump. It had required three centuries of travail, but medicine was at last able to impress the multitude even as physics had done before it.

The new surgery was likewise calculated to impress people. It, too, was spectacular in the nature of the case. The mystery of anæsthesia, the asceptic technique, the unheard-of achievements of great surgeons —all these amazed observers who could recall the suffering and the mortality common but a decade or two before.[2] It was notorious that men talked more about their successful operations than about any other medical procedure. Surgeons consequently achieved, at least in the United States, a popular recognition which was rarely bestowed upon their colleagues in physiology and anatomy.

Another development in medical practice which impressed the laity was the trend towards specialization. This had been retarded, during the first half of the nineteenth century, by an unfortunate tradition of quackery in certain fields. "Eye doctors" and "ear doctors" were then in particularly bad repute. The development of a localized pathology, however, invited attention to particular body parts or functions. Then, as knowledge increased, the point was inevitably reached where anyone wishing to excel in such studies necessarily devoted all his time to them.[3] In due time there evolved a new type of physician, the efficient, modern specialist, who was as different from the old-fashioned general

[1] Paul de Kruif: *The Microbe Hunters*, p. 164.

[2] This was impressed upon the public before 1890, as is shown in W. W. Keen's interesting article: "Recent Progress in Surgery", *Harpers Mag.*, LXXIX (N.Y., 1889), 703 ff.

[3] The basic factors leading to specialization are well analyzed in George Rosen: *The Specialization of Medicine* (N.Y., 1944), 14 ff.; its history in the United States in Bernhard Stern: *American Medical Practice* (N.Y., 1945), 45 ff. A brief account of the evolution of British specialization is given in Sir D'Arcy Power: *Medicine in the British Isles*, pp. 38 ff. (*Clio Medica* ser., N.Y., 1930).

practitioner as two members of the same profession well could be. While the latter held the friendly confidence of his patient, the specialist was relatively impersonal in his contacts. While the practitioner was simple in manner and limited in equipment, the specialist was relatively scientific in procedure and was surrounded by a technical equipment calculated to impress all but the boldest of the laity. Each type had its merits and its limitations. The family doctor inspired a feeling of personal confidence, the specialist a sense of awe.[1]

It would be a mistake, finally, to overlook the popular appreciation of collateral medical services. Although dentistry was originally handicapped by a tradition of charlatanism, it was completely transformed during the middle decades of the nineteenth century. The Americans continued to develop an indigenous technique superior to that in any other land. During the last decades of the century, in consequence, European students came to study in American dental schools. Here, as was observed in an earlier chapter, was probably the first instance in which the transit of culture between the two continents was reversed.

This did not mean that Europeans were entirely dependent upon the United States for dental education. The first English school dated from 1859, the first French from 1879. A promising development appeared when the universities of Berlin and Leipzig set up dental institutes in 1884. When the first international congress of dentistry convened in 1900 there were but five schools in France, eleven in England, and twelve in Russia. Germany by this time possessed twenty, and the United States no fewer than forty-six.[2] The German and English schools were probably the best in Europe; but such American colleges as those connected with the University of Pennsylvania and with North-western University remained pre-eminent.[3] The latter institution underwent a remarkable development in this period under the leadership of G. V. Black, who, although largely self-trained, displayed a native genius characteristic of the best work in the United States.

That progress in dentistry involved much more than convenience and appearance was made evident by the increasing significance attached to the so-called "focal infection" of teeth—the danger from which was pointed out by Benjamin Rush prior to 1800. Bacteriology afforded a rational explanation of this empirical observation; and so

[1] An interesting account of the best type of American family doctor is afforded in Wm. Allen Pusey: *A Doctor of the 1870's and 80's* (Springfield, Ill., 1932).

[2] Karl Sudhoff: *Geschichte der Zahnheilkunde*, zweite aufl., p. 200.

[3] German dentists trained both in the United States and in Germany assured me in 1933 that their university schools had finally become the equals of the best in America.

stimulated a further investigation which led to the indictment of the tonsils and the appendix as similarly dangerous foci. Patients were impressed by skilful dental operators, who at one and the same time ministered to their comfort, preserved appearances, and protected them against serious disease. It is worthy of remark that it was the dental profession which first encouraged patients to report periodically for examination.

Veterinary medicine benefited from bacteriological studies, just as did human medicine. Originally possessing the status of a trade, as had surgery and midwifery before it, veterinary medicine rose gradually in public esteem during the last century. In the place of the one-time "horse-doctor" there appeared a practitioner trained in much the same sciences as was the regular physician. The veterinarian's services were often of economic value, and so were likely to impress even ignorant observers.

In a somewhat analogous manner, the old trade of spectacle-making took on increasing prestige with technical advances in ophthalmology and lens-making. The trend of things in Philadelphia was typical of that in most great cities. As early as 1800 the optician John McAllister held an honoured place in the life of the city; and the scientific skill of such subsequent Philadelphia opticians as Joseph Zentmayer and his student Ivan Fox established the profession on a dignified plane after 1860.[1] It is a fair assumption that people appreciated the proper adjustment of essential visual aid through glasses.

The collateral service that exerted the most favourable influence on public feeling was probably the ancient profession of nursing. This maintained a certain dignity in Catholic lands through the labour of religious orders, and also in those Protestant countries in which respectable "sisters" continued to serve in the lay hospitals. Unfortunately, nursing in both Protestant and Catholic countries suffered by the employment of women helpers or servant-nurses, needed in connection with the expansion of the hospitals after 1750. Their work was put largely under the control of the male medical staffs, which does not seem to have improved the service. These women acquired a particularly unhappy reputation in England. The servant-nurses of the London hospitals were described in the *Times*, in 1857, as follows:

> Lectured by committees, preached at by chaplains, scowled on by treasurers and stewards, scolded by matrons, sworn at by surgeons,

[1] Zentmayer's contributions to optics received both American and international recognition; see W. C. Posey and S. H. Brown: *The Wills Hospital of Philadelphia*, etc. (Phila., 1931), pp. 260, 261.

bullied by dressers, grumbled at and abused by patients, insulted if old and ill-favoured . . . tempted and seduced if young and well-looking—they are what any woman might be under the circumstances.[1]

This account of London conditions was somewhat extreme, since there were other hospitals throughout Europe in which the nursing services were respectable, or even superior at the beginning of the nineteenth century.[2] But these were usually those served by religious orders: it was the improvement of secular nursing personnel that had not yet been solved in this increasingly secular age. To the extent that medical practice became more technical and effective, moreover, there was another potential problem to be met—that is, to define the part that nurses would take in such practice and to provide the professional training necessary in this connection.

The need of better nursing did not entirely escape the humanitarians of the Enlightenment. The medical significance of the art was recognized in the famous *Encyclopédie* in 1765, and a serious literature on the subject appeared in Germany later in that century. In 1781 Dr. Franz May introduced instruction for secular nurses at Mannheim. Nursing schools there and in other German cities provided hospital training, elementary lectures, and certificates. May realized that formal instruction and certificates must be supplemented by some means for attracting better personnel; but the German schools were unable to supply this because they could not improve the hospital conditions under which secular nurses served.[3]

During the first half of the nineteenth century, therefore, such success as was had in seeking better personnel was attained only by religious groups. These could continue the old appeal to personal devotion, whereas a secular tradition could be established only by granting a professional status which most institutions were still unwilling to accord.

[1] *London Times*, April 15, 1857, cited in M. Adelaide Nutting and L. L. Dock: *History of Nursing*, I (N.Y., 1907), 505. See ibid., pp. 501, 513, for an indictment of masculine supervision.

[2] The degradation of nursing *c.* 1800 has been exaggerated—apparently in order to laud the later Nightingale epic—by featuring the worst institutions. Cf.: John Howard, *The State of Prisons in England and Wales . . . and an Account of Some Foreign Prisons and Hospitals*, 4 ed. (London, 1792), 84, 96, 119, 142, etc.

[3] Franz May: *Unterricht für Krankenwater zum Gebrauche öffentlicher Vorlesungen*, 2 ed. (Mannheim, 1784), 5 ff.; J. G. Pfähler: *Unterricht für Personen welche Kranke Warten* (Riga, 1793). Nutting and Dock (*History of Nursing*, I, 536 f.) do not consider these early German attempts as real schools because of their supposed failure to improve nursing services; but cf. Alfons Fischer, *Geschichte des Deutschen Gesundheitswesens*, II (Berlin, 1933), 89, 403 ff.

The Protestant Churches in Germany had never entirely lost interest in nursing services; and in an effort to improve matters in that country, an order of Protestant deaconess nurses was established there in 1836. Deaconess houses were also built in certain American cities for the same purpose; and there are still hospitals in the United States—such as the Lankenau in Philadelphia—where the nursing is in charge of Protestant sisters. New Catholic nursing orders were founded in Ireland about the same time, and these came to the United States on a wave of Irish immigration after 1848. A few years later the Church of England established nursing orders which did good work and paved the way for further reform.[1]

A great transformation in nursing was to begin in the next decade, but the services of Catholic and Protestant sisters—and indeed the whole religious tradition—should not be lost to sight in the current admiration for Florence Nightingale. Admirable and far-reaching as her work was, moreover, it is to be remembered that the first steps towards training even secular nurses had been taken in German schools a half-century before her time. Miss Nightingale's major achievement was not in originating nursing schools or their objectives, but rather in securing such hospital conditions and professional recognition as would bring respectable lay-women into the guild.

It happened that in 1836 a German clergyman, Theodore Fliedner, turned his parish house at Kaiserwerth into an asylum for discharged women prisoners. He then set up the type of Church school mentioned, in which deaconesses were taught to provide decent nursing. Here was trained Florence Nightingale (1823–1910), an Englishwoman of rare ability and humanitarian enthusiasm. The story of her heroic and effective service during the Crimean War, when she organized a body of nurses to take charge of a barrack hospital at Scutari, has become one of the epics of modern medicine. Despite all manner of bureaucratic opposition, she was providing 10,000 men with clothing and other necessities within three months after her arrival.[2] So striking was Miss Nightingale's success that upon her return to England a school of nursing employing her methods was founded by public subscription at St. Thomas's Hospital. The plan spread to the continent and to America within the next decade.

[1] Sir D'Arcy Power: *Medicine in the British Isles* (N.Y., 1930), pp. 50, 51; Sarah A. Tooley: *A History of Nursing in the British Empire* (Lon., 1906), pp. 27, 56, 72 ff.

[2] Vivid descriptions will be found in Rosalind Nash: *A Short Life of Florence Nightingale* (N.Y., 1925), pp. 86 ff. This is based on the longer *Life* by Sir Edward Cook. See also Miss Nightingale's own *Notes on Nursing*, etc. (Lon., 1860).

The Nightingale school adapted nursing service to modern needs; first, because it trained secular nurses and overcame the medieval tradition that respectable nursing could be carried on only by religious orders. Second, it emphasized more than had most of the orders the need for a regular training in essential medical subjects.

It was commonly held until after 1860 that nursing ability was a "gift", and hence that nurses required little formal education. The analogy with the history of other professions is obvious enough—nurses were "born, not made". In overcoming this tradition, which had just enough truth in it to make it misleading, Miss Nightingale had to combat a second obstacle. It was difficult to persuade Victorian society to approve the training of women for any profession, let alone a new one. Yet, London first, and later the rest of the civilized world, accepted this innovation, and with it the provision for feminine supervision of the new service.[1] No doubt the private endowment of the St. Thomas's School, which made it independent of possible opposition, aided it in exerting this remarkable influence.

The success of the Nightingale school marked a triumph for feminism as well as for superior nursing. This was consistent with other trends of the time, making for at least a grudging professional recognition of women. The first phase of this movement had necessarily related to improved educational opportunities. In the United States, for example, a few secondary schools were provided for girls soon after 1820, and Oberlin College opened its doors to them in 1833.[2] By that time a conscious feminist movement was well under way, which naturally expressed itself in a demand for admission to the learned professions. Now it was shocking enough for women to study law or theology, but it was quite beyond the understanding of most Victorians that any respectable woman could desire to study medicine. (The medieval recognition of women as physicians had been long since forgotten.) Practitioners were usually shocked at the mere thought of a "doctress", though there were probably as many liberal exceptions in their ranks as in any other group. One American medical editor admitted the contributions of a French woman to his pages as early as 1831; and another argued, in 1859, for the general admission of women to the profession.[3]

[1] Lucy R. Seymer: *A General History of Nursing* (N.Y., 1933), pp. 99 ff.; Tooley: *A History of Nursing in the British Empire*, pp. 91 ff.

[2] See for this whole story, Thomas Woody: *A History of Women's Education in the United States* (2 vols., N.Y., 1929).

[3] *Boston Med. and Surg. Jour.*, IV (1831), 96; *Savannah Med. Jour.*, I (1859).

The first modern woman to take her degree in medicine was probably Elizabeth Blackwell, who graduated at the Geneva Medical School of Western New York in 1849. She began to practise in New York City the next year. Here the prejudice against her was such that she opened a private dispensary, which within a few years developed into a hospital and training school for women. Meanwhile, the Woman's Medical College was established in Philadelphia in 1850, where it has remained in continuous existence to this day.[1] About a generation later a medical school for women was founded in London, and the Continental universities also opened their doors to them.

Better known than any of the first feminine graduates was another American woman, whose work was of considerable medical significance. This was Dorothea Dix, whose career was more varied and in some ways more remarkable than that of Florence Nightingale. As a girl in Boston, Miss Dix was inspired by the social idealism of the Unitarians. She first devoted herself to teaching, but later found her true mission in caring for the neglected insane. She achieved unprecedented success in establishing asylums for these unfortunates in many American States, and was instrumental in founding or reforming similar institutions in such distant lands as Scotland, the Papal States, and Japan. She contributed in like manner to the improvement of jails and penitentiaries. Her positive ability, and the negative fact that her reforms threatened no vested interests, combined to secure her a unique reputation in her own country.

At the outbreak of the Civil War she promptly offered her services to the Union Government. After the manner of Miss Nightingale, she selected a group of nurses for the service of the military forces. The qualifications she demanded of her nurses were interesting. Candidates must be "plain-looking women", and there were to be "no bows, no curls or jewelry, and no hoopskirts". Miss Dix found inefficiency and bureaucracy in the army medical department, but was finally appointed by President Lincoln as the first Superintendent of United States Army Nurses. In this capacity she co-operated with the Sanitary Commission, and secured the aid of Elizabeth Blackwell in the hasty training of volunteer nurses at Bellevue Hospital in New York. Despite the inadequacy of this programme and the general confusion, Miss Dix finally built up in the Union base hospitals the first secular nursing service in America.[2]

[1] How this pioneer school was ostracized by the local county medical society is told in Kate C. Hurd-Mead: *Medical Women of America* (N.Y., 1933), pp. 26 ff. See also *Med. Life* for July 1928 ("The Woman's Number").

[2] Helen E. Marshall: *Dorothea Lynde Dix: A Forgotten Samaritan* (Chapel Hill, N.C.,

Regular training in nursing was established in the United States in 1873, when schools were established at the Bellevue Hospital, at the Massachusetts General Hospital, and in New Haven—the latter school later becoming a part of Yale University. Other schools were eventually founded in connection with nearly all hospitals of any size. The emphasis in nursing tended to shift from the religious to the scientific ideal, though the two were not necessarily incompatible. Superior training gradually attracted a better class of women, and in recent years this trend has been accelerated by a tendency to eliminate the vestiges of the old servant tradition and by the granting of a nursing degree.

All this related to the public opinion of medical service in general, since the nurse came into more continuous contact with the patient than did any other figure in the whole range of medical personnel. Good nursing was invaluable from a technical point of view. It might make all the difference in the outcome of the individual case, and patients sometimes realized this. Better nursing was an essential feature in the gradual improvement of hospitals, and this in turn modified the earlier popular attitude towards these institutions.

The expansion of hospitals during the first half of the nineteenth century has already been noted in connection with the development of clinical instruction and the advent of clinical statistics. The continued growth of population in all European nations, and particularly in the large cities, promoted a more rapid expansion of hospital facilities about the end of the century. The change in public attitude towards hospitals was even more responsible for this expansion than was the sheer increase in population. Once anæsthetics and antiseptic procedures were available, and the clinicians abandoned their earlier nihilism and became hopeful of cures, the whole spirit of hospitals changed. They became institutions in which patients went to get well, rather than simply to die. This meant that the middle and upper classes began to resort to them, as well as the urban poor who had once been their chief inhabitants. And this, in turn, meant that hospitals lost much of their early grimness. The most modern ones, in the United States, even took on something of the nature of good hotels.

These changes were, to be sure, of a relative character. The poor were still cared for in wards, and these could not be made as pleasant as the private rooms set apart for those who could pay. The personal attention accorded in better home nursing could hardly be reproduced in

1936). See also Howard W. Haggard: *The Doctor in History* (New Haven, 1934), pp. 361 ff.

the service of a large institution, unless private nurses were provided. The importance of the patient's morale was not always appreciated, and serious reports persisted of carelessness and neglect in large municipal and State institutions. While these circumstances have at times been overlooked by those who praise the modern hospital, there can be no question that their major claims are correct. The hospital of the present is a relatively humane and efficient institution, in comparison with the grim charity asylums of Victorian days.

In few countries was the evolution of the hospital so rapid and striking a process as it was in the United States. Many new hospitals were built in American cities throughout the nineteenth century, as the population increased with unusual rapidity. The older ones, such as the New York or the Pennsylvania, were well planned for their day; but an interest in more modern facilities was inaugurated when Billings made the plans for the new Johns Hopkins Hospital at Baltimore in 1876. Billings was interested in the so-called "pavilion plan", which can also be well observed to-day in the Peter Bent Brigham Hospital in Boston. In the last few decades this type was found impracticable in the congested areas of large cities, and the skyscraper form was adopted in much recent building. An impressive illustration is afforded by the present building of the New York Hospital.

The scientific efficiency of American hospitals, large and small, was due in no small part to the influence of the American College of Surgeons. This was founded in 1913, and at once set up very strict membership requirements. Surgeons desiring membership were required to submit 100 case histories of patients upon whom they had operated. It was then discovered that many could not do so, because their hospitals kept no detailed case records. This led to further inquiries, which revealed that many institutions did not yet possess laboratories or X-ray equipment. The College of Surgeons thereupon outlined a whole system of requirements relating to the staff, the records, and the diagnostic and therapeutic facilities available, which were to be met before a given hospital could be placed upon an approved list.

The publication of this list proved a most effective device for standardizing American hospital equipment and procedure, since institutions were anxious to be accredited in this manner. Within little more than a decade the influence of this private organization almost remade hundreds of institutions. Of 692 large hospitals examined in 1918, only about 13 per cent were approved. In 1932 nearly 1,600 were inspected, and of these no less than 93 per cent were found acceptable. This was

a truly impressive record. Similar standardization was secured in the hospitals of the more progressive European nations through systems of State control.[1]

The standardization of hospitals was quite naturally paralleled by a standardization of medical schools. Indeed, the two movements expressed a common trend towards professional improvement all along the line. Here again the most striking advance was to be observed in the United States, if for no other reason than that the earlier movement for "medical reform" had largely failed there. The era of bacteriological discovery coincided with the extension of such legal recognition to regular medicine as the States had hitherto most persistently withheld. The popular drive to remove licensing restrictions was brought to a definite halt about 1875; after that time one State after another either set up examining boards or designated certain superior schools whose diplomas entitled the holders to practice. Within the next two decades twenty-one States provided for State board examinations, and fourteen others restricted the right to practise to the graduates of accredited schools.[2]

Some of these accredited institutions were none too good, but a tendency to reform medical education was manifested at the same time. Such reform was partly undertaken in order to meet the new licensing requirements, but it also represented a voluntary move from within the profession. Real clinical teaching was introduced about 1867 by Jacob Da Costa at the Jefferson Medical College in Philadelphia. Several of the older university schools, notably Harvard under President Eliot and Pennsylvania under Provost William Pepper, took the lead in lengthening the training period and in expanding the curriculum during the next two decades. Notable, too, was the establishment of a new medical school at Johns Hopkins University in 1893. Here full-time chairs were established; and a brilliant group of physicians, led by William H. Welch and William Osler, developed scientific research after the German manner.[3]

[1] H. E. Sigerist: *American Medicine*, pp. 208 ff.; Franklin H. Martin: *Fifty Years of Medicine and Surgery* (Chicago, 1934), pp. 299 ff. A national survey recently submitted (Sept. 1946) to the American Hospital Assn., however, shows that these institutions are still inadequate for present needs. See, for present problems and politics, E. H. L. Corwin: The American Hospital (N.Y., 1946).

[2] The progress of this legislation can be followed in the reports of the U.S. Commissioner of Education. It is reviewed in A. M. Schlesinger: *The Rise of the City*.

[3] Harvey Cushing: *The Life of Sir William Osler*, I (Oxford, 1926), 311 ff.; Edith G. Reid: *The Great Physician: A Short Life of Sir William Osler* (N.Y., 1931), pp. 69 ff. A careful description of the developments at Johns Hopkins is given in Sigerist: *American Medicine*.

The somewhat moribund American Medical Association was reorganized in 1901, and its Council on Medical Education and Hospitals became very active in promoting better training standards throughout the country. Particularly interesting was the drive against inferior or unnecessary schools, stimulated by Abraham Flexner's famous report to the Carnegie Foundation for the Advancement of Teaching (1908), as a result of which nearly 100 were closed between that date and 1929. Even the number of medical students in the country actually declined, despite the increase in general population, from over 28,000 in 1904 to less than 20,000 in 1927. The prospect of an average improvement in quality was obvious.[1] In this way the American profession finally overcame old obstacles to efficient training, and duplicated the standards set in some European countries a generation before.[2]

This was indeed the period when American medicine finally came of age—when the old longing of Benjamin Rush for a native science coequal with the European was at last realized. No longer need American publications be satirized as "British portraits of disease in American frames". The better schools in the States, staffed largely by American-trained physicians, became the equals of the best European centres. In certain specialties the former became superior. And when this stage was reached the long period of colonial dependence in medical culture had come to an end.[3]

A most significant indication of confidence in medical science on both sides of the Atlantic was the provision of relatively large subsidies for the advancement of research. Hospitals had always been aided, in one way or another, on humanitarian grounds; but the direct support of actual research rarely antedated 1870. Both governments and private philanthropy began thereafter to devote funds to medical laboratories, as well as to churches, hospitals, and schools. It was the work of such leaders as Pasteur and Koch, it will be remembered, which encouraged this tendency, and they in turn benefited thereby. Some of the largest subsidies were secured in the United States, usually through the grants of private philanthropists after the traditional Anglo-Saxon manner.

The outstanding example was afforded by the founding of the Rockefeller Institute for Medical Research in 1901. At that time Frederick T. Gates advised John D. Rockefeller that " . . . medicine could hardly hope to become a science until it should be endowed, and

[1] Garrison: *History of Medicine*, 4 ed., p. 785.

[2] There is one exception to this. Some states will authorize certain sectarians to practice, without passing any examination save that set by their own profession.

[3] See D. R. Fox: "Civilization in Transit", *Amer. Hist. Rev.*, XXXII (1927), 753 ff.

qualified men be enabled to give themselves to uninterrupted study and investigation, on ample salary, entirely independent of practice".[1] Here was the ideal towards which Louis had striven more than seventy years before. Mr. Rockefeller responded by endowing the Institute of Medical Research in New York City, and William H. Welch, who had done so much to make Hopkins a centre of medical research, was made the first director.

During the next three decades the Institute was enlarged to include a hospital and departments of animal and plant pathology. One of Welch's students, Simon Flexner, became director in 1904; and he subsequently gathered around him a brilliant group of scientists interested in many phases of medicine, biology, chemistry, and physics. The reputation of such men as Carrel, Loeb, and Noguchi soon made the Institute internationally known.[2]

Other organizations devoted wholly or in part to medical research were set up in the United States in recent decades. Henry Phipps established in Philadelphia an institute for the study of tuberculosis. Still other foundations contributed large sums for medical projects or established institutes in some of the better-known schools. Thus the Carnegie Institution supported a laboratory of embryology in a Johns Hopkins building, and the Phipps Institute became a part of the University of Pennsylvania.

Official support for research also became available in the United States, though hardly to the degree that it did on the Continent. A number of city and State governments spent increasing amounts on their public health laboratories; and the national Government developed the important research carried on in the Washington laboratories of the United States Public Health Service. The history of this Service well illustrates the gradual realization, on the part of a legislative body, of the national significance of medical investigations. When the National Board of Health began the first research conducted by a federal agency, in 1879, most Congressmen considered its annual expenditure of $10,000 as an evidence of extravagance!

Ten years later the Marine Hospital Service set up a laboratory in New York City without any special authorization. Congress did not give this laboratory legal recognition until a decade after it had moved to Washington in 1891. In 1902 an advisory board including civilian experts was provided for the laboratory, and it is interesting to recall

[1] Quoted in Sigerist: *American Medicine*, p. 175.

[2] Sigerist, op. cit., pp. 275 ff. For the larger American story, see R. H. Shryock: *American Medical Research: Past and Present*, to be published shortly.

that such men as Welch, Simon Flexner, and Sedgwick became members of this body. In 1912 Congress recognized the increasingly important work of the laboratory by a special act, authorizing it to "study and investigate the diseases of man".[1] Recently it has been still further developed into a general institute for studies in public health, and a National Institute of Medical Research has also been established by the Service. There is no question that medical research is a well-recognized activity of the Public Health Service to-day, and that as such it commands a prestige that was largely lacking only a generation ago.

There were doubtless other indications of the increasing prestige of medicine and the medical profession besides the financial recognition described. Direct attacks on the profession in reputable newspapers and magazines were rare in recent decades. One of the last instances in this country was a striking series of cartoons, printed between 1908 and 1912, in the humorous weekly *Life*. These purported to reveal the horrors of vivisection, vaccination, and serum therapy, and probably represented a vestige of the old feeling that doctors delved in forbidden things. The cartoons were inspired by a single editor and were suddenly terminated.[2]

It is likely that a perusal of general literature would reveal an increasing regard for medicine. This would certainly be the case, if one compared current literature with that of two or three centuries ago. A comparison with that of 1850 is not so easily made, but even here the odds would seem to be in favour of the present period.[3] Sinclair Lewis's *Arrowsmith*, for example, contains unfavourable criticism of medical foundations, and yet displays an enthusiasm for medical research in itself that would be hard to match in Victorian literature. So, too, would it be difficult to find a Victorian parallel to *Yellow Jack*, the dramatization of the Walter Reed story which proved popular on the New York stage in 1934. And last but not least in their popular influence, no matter how distorted they may be, were the recent "movies" which provided favourable pictures of both the medical and the nursing professions. One of these, *The Country Doctor*, dramatized the story of Dr. Allan Dafoe and the Dionne quintuplets so effectively that it may have revived the prestige of the general practitioner in the United States and Canada. There were also such titles as *Men in White*, *The White Angel*,

[1] R. D. Leigh: *Federal Health Administration in the United States*, pp. 368 ff.
[2] Chauncey D. Leake: "Medical Caricature in the United States", *Bull. of the Soc. of Med. Hist. of Chicago*, IV (1928), 14.
[3] The literary evidence is considered in Garrison: *History of Medicine*, 4 ed., pp. 754 f.

and *The White Parade.* Indeed, for a time anything "in white" seemed good for box-office returns.[1]

As a matter of fact, it is unnecessary to labour the point that both medical science and physicians command general respect at the present time. Although anti-vivisectionists still protest, their protests are not generally heeded. Although rival sects continue to operate, and several are sanctioned by law in certain American States, it is clear that no large part of the educated public respects them as it does the regular profession. In the United States, where the sects were most numerous and influential, there were in 1932 some 32,000 sectarians in contrast with 142,000 regular physicians. About $125,000,000 was spent annually for the services of the irregulars. Among these, the 7,650 osteopaths were the most reputable group. Their schools now teach a certain part of regular medicine, and are to be criticized chiefly on the ground that they do not measure up to the requirements for "Grade A" schools. It is possible that osteopathy, like homeopathy before it, will be eventually absorbed into regular medicine.[2]

More numerous are the 16,000 chiropractors, whose low standards apparently have enabled them to take the place of osteopathy when the latter raised its requirements. Even chiropractors seem to have displayed some interest in improving their practice. At any rate, their numbers increased little, if at all, during the last few years. The 10,000 Christian Science and New Thought healers represented a very different group, since they practised only mental or faith healing. It has already been noted that they showed signs of weakening in their attitude towards regular medicine. Many Christian Scientists consulted physicians in certain emergencies; and one offshoot from the "Mother Church" came to employ them regularly, albeit upon metaphysical grounds.[3]

That quackery still flourishes on a grand scale in many lands, and that in the United States secret nostrums are now unctuously served by the radio as well as by the Press, is obvious enough.[4] This seems almost inevitable in the type of society in which we now live. It is claimed that some of the systems of health insurance or State medicine now maintained in Europe have reduced the temptation to resort to quack-

[1] Note also the pictures, such as *Pasteur* and *The Magic Bullet* (Ehrlich), which dramatized medical research.

[2] Louis S. Reed: *The Healing Cults*, etc. (Pub. No. 16, of the Com't. on the Costs of Med. Care, Chicago, 1932).

[3] A. K. Swihart: *Since Mrs. Eddy* (N.Y., 1931), pp. 282 ff.

[4] American quackery, except for the radio phase, is described in Harry H. Moore: *Public Health in the United States* (N.Y., 1923), pp. 167 ff.

ery, through popular education and extended services to the poor. Be that as it may, regular medicine has done something to change other conditions that once promoted sectarianism and quackery. It is probable that increasing recognition of physical therapy has lessened the appeal of chiropractors; and it may be that, as Freud pointed out, psychiatry will one day undermine other types of healing cults.[1]

After 1900 the American Medical Association and some of the European associations attacked quackery directly by investigating and reporting on patent remedies.[2] As a result of this and other factors the attitude of educated peoples really changed in a significant manner. The British Parliament once paid £5,000 for a secret remedy for stone, and a no less trusting American State legislature contributed $10,000 for a hydrophobia cure. If pessimists think there has been no improvement in this respect, let them imagine Congress in the act of buying the recipes for "Father John's Medicine" or for "Lydia Pinkham's" compound.

[1] Sigmund Freud: *A General Introduction to Psychoanalysis*, transl. by G. Stanley Hall (N.Y., 1920), p. 6. The history of psychiatry will be considered in the next chapter.

[2] This method of exposing quackery had been tried by individual physicians early in the last century. See Caleb Ticknor: *Exposition of Quackery and Imposition in Medicine* (Lon., 1839).

XVII

A DELAYED ADVANCE AGAINST MENTAL DISEASE

FREUD'S OBSERVATION that an adequate psychiatry would do much to eliminate quackery can hardly be gainsaid. The failure of medical science to deal with problems of mental disease left a gap in the advancing line of the medical sciences, into which poured all the forces of quackery and sectarianism. To close this gap and to drive out the invading cults was one of the most serious obligations of modern medicine. The advance was finally undertaken with considerable energy and courage during the latter half of the nineteenth century.

Not the least interesting feature of this advance was the apparent reversion, in certain respects, to more elementary scientific methods that had been tried and superseded in somatic medicine—a reversion apparently necessitated by the extraordinary difficulty of the disease terrain which had to be crossed. It was as if an army, possessing all the heavy equipment of modern warfare, had been forced to invade a wilderness permitting only of the advance of men on foot in the manner of preceding centuries.

It will be recalled that individual physicians had long insisted that the insane were ill and not wicked or "possessed", but that a demonological psychiatry remained dominant clear into the eighteenth century. Towards the end of that era Pinel and his contemporaries finally established the medical as opposed to the theological point of view, and Pinel described the more common types of mental disease on a basis of their general symptoms. In this respect his work may be compared with the much earlier labours of Sydenham in noting the specific character of some of the more common somatic conditions.

The most obvious achievement of Pinel's generation, however, was to encourage the humane treatment of mental cases. This was the "reform of the treatment of the insane", which naturally impressed the public at large. It really constituted a phase of the Enlightenment and the subsequent humanitarian movements, rather than an aspect of the

history of medicine proper. Like other phases of the Enlightenment, humane treatment had some significance for medicine, in that it involved the establishment of the "asylums" in which scientific studies of mental cases could be undertaken more systematically than ever before. This was an exact parallel to the value of the general hospitals for somatic medicine. The parallel applied not only to the collection of clinical material but also to the delay in its effective exploitation. Large general hospitals were built up after 1750, but were not well utilized for research until about 1820. In like manner, modern "asylums" for the insane were established from 1800 on, but were not effectively utilized for psychiatric research until around 1870 or even later. Although their superintendents were uniformly physicians before that date, they had little time or inclination for original investigation. In most cases they served as custodians, rather than as clinicians.

There were, of course, exceptions to this rule. Such a man as Kirkbride, of the Pennsylvania Hospital, was much more than a "keeper". Nor should one belittle the services of good custodians; they performed a humane service to society and at least prepared the way for scientific studies. The crusade for better treatment was not won in a single generation, and there was need of continued effort towards this end right through the century. Indeed, there is still dire need of it in some States of the American Union, if not in other lands. But it is important here to distinguish the scientific from the humanitarian phases of the story.

The history of psychiatry after Pinel's time displays a continuous effort to employ the concepts and methods of contemporary somatic medicine. These met with only partial success, and were thereupon complicated by the partial reversion to earlier methodologies already mentioned. Pinel's classification of mental disease was of a very general and approximate character, and obviously needed refinement. This was just the period when clinical-pathological correlations were taking the place of symptomatic classifications in ordinary medicine, and so were bringing order out of nosographical confusion. Such concepts as "typhoid" and "typhus" were replacing the endless "fevers".[1] If local lesions could now be found to distinguish the various "manias" in like manner, the advance of psychopathology would parallel that of the general science.

After the influence of the French school had stimulated clinical-pathological research in all Western nations there ensued a study of

[1] See Chaps. IV, VII.

psychopathology which led in some respects to quite encouraging results. The prevailing interest in local structure and function led naturally, first of all, to advances in neurology. Moritz Romberg, of Meiningen, published the first formal treatise on nervous diseases in 1840–1846, and this presented some precise clinical pictures where only scattered data had been heretofore available. Duchenne, who studied under Laënnec and Magendie, differentiated paralysis of different types, and identified them with distinct diseases in terms of spinal or other neural lesions. Thus in 1855 he identified "infantile paralysis" not only as a clinical picture already known, but as one due to lesions in the anterior horn of the spinal cord.

The outstanding neurologist during the last part of the century was Jean-Martin Charcot, who became physician to the Salpêtrière in 1862. Here he set up one of the chief neurological clinics of modern times and maintained French leadership in this field. He was primarily interested in clinical and pathological studies of such conditions as locomotor ataxia, the lesions in muscular atrophy, and so on. Studies of the various diseases of the cord and brain were continued by his students in Paris, by such English investigators as Sir William Gowers, and by S. Weir Mitchell and other American neurologists.

Mitchell was in charge of a Union base hospital in Philadelphia during the Civil War, and set aside a special ward for nervous patients. Here, with the surgeon W. W. Keen, he made valuable studies of nerve injuries. Later he developed procedures of considerable therapeutic value to nervous patients, such as his "rest cure", and the correction of eye-strain in the treatment of severe headache. Great advances were made by Americans in neural surgery after about 1890, notably by Keen, Spiller, and Frazier in Philadelphia, and by Harvey Cushing in Boston.[1] The latter, who was associated in turn with Johns Hopkins, Harvard, and Yale, stood "*facile princeps* in neurological surgery".[2]

In addition to original experimental work in physiology, Cushing did outstanding work on brain surgery. More than any other surgeon, he demonstrated the possibility of operative relief for intracranial conditions. A valuable technical contribution was his introduction of anæsthetic nerve-blocking in 1898. It may be added in this connection that the recent work of neural surgeons in sectioning nerves for the relief of great pain has proved a boon to many patients. It serves as a reminder that the relief of pain, as well as the cure of disease, is an end in itself in the practice of medicine.

[1] Garrison: *History of Medicine*, 4th ed., pp. 644 ff. [2] Ibid., p. 731.

The line between neurology and psychiatry, meanwhile, was not clearly drawn; in fact it was the natural feeling of the neurologists that it should not be so drawn. Clearly, research was on more solid ground if a somatic basis could be found for all mental symptoms. To the presence of physical lesions in certain cases was now added the discovery of physical causes. This was the work of bacteriologists, who, starting with the data of neurology, were able to show that mental symptoms resulted from several infectious diseases. To put it in eighteenth-century language, they proceeded from a knowledge of the "proximate" (pathological) to a knowledge of the "approximate" (bacteriological) causes. Much the most important instance here was the discovery that paresis (general paralysis of the insane) was a late-stage syphilitic infection of the brain.

The discovery was a most promising one, for, as in other cases of known physical origin, it opened the way not only to diagnosis but to prevention and possibly to cure. Early treatment with neo-salvarsan might prevent the later development of paresis. It was also demonstrated that heat therapy would clear up paretic symptoms in a remarkable manner. Patients who suffered a high fever showed marked improvement, and this suggested the actual treatment of paresis by inoculation with malaria or other fever-inducing diseases. Malarial parasites, having checked the vicious spirochætes, could be in turn controlled by quinine. Here was a therapeutic drama for a certainty—one disease set against another. Unfortunately for the dramatic instinct, it was soon found that fever-inducing "short-wave" machines developed in the electrical laboratory could achieve the same result, and in a manner somewhat more pleasant for the patient.

In the long run, then, the somatic approach to psychopathology brought tangible results. When the explanation of an infectious origin of paresis was added to a knowledge of mental abnormalities associated with other somatic conditions like senility, the total percentage of mental ills thus explained was a substantial one. It has been estimated that as high as 45 per cent of all mental cases reaching American institutions have a known somatic basis.[1] There is always the possibility, in addition, that further somatic correlations will be found for other types of mental illness. Clues along this line may be discovered, for example, in endocrinology, or in any other new field which develops in the future.

There remains the stubborn fact that down to the present time over

[1] Horatio M. Pollock, Director of Statistics in the N.Y. State Department of Mental Hygiene, cited in Iris W. Jones: "Man's Last Spectre", *Scribner's Mag.*, XCVIII (N.Y., 1935), 333.

50 per cent of the usually more serious cases—the so-called psychoses—have no known somatic pathology or cause. This is the other side of the story of successful search for physical bases—the persistent failure to discover origins for such subtle phenomena as "delusions" or "a sense of unreality". Throughout the last years of the nineteenth century the superintendents of American asylums toiled earnestly at autopsies seeking the local lesions which they rarely ever found. The experience was so discouraging that one American authority refers to this as the era of "brain mythology", and there was a tendency after 1900 to turn from a somatic to a psychic orientation.[1]

This orientation had not been entirely neglected in the period between 1825 and 1850, when neurology was first becoming a field of some promise. It was necessary, in the nature of the case, to continue the empirical treatment of mental diseases pending the much-desired discovery of rational (i.e., somatic) explanations. The actual treatments employed consisted at first of physical restraint and some hydrotherapy. As humanitarianism checked the use of physical restraint it was replaced by considerable chemical restraint; that is, by constant doses of bromides. In due time these were found just as objectionable, and there was a resort to such more satisfactory procedures as occupational therapy. But the important point here is that empirical treatment necessitated further attempts to classify the diseases concerned.

Pinel's nosography was a fairly useful one, but it was vague and approximate at best. Lacking any known local pathology such as was already clarifying somatic nosography, the psychiatrists had to continue their efforts at classification on a purely clinical basis. In a word, they had to operate on the methodological level occupied by general medicine during the preceding century—and with similar results. For in attempting to refine symptomatic classifications, psychiatry became involved in the very over-elaboration of symptom diseases which had confused the general nosographies of 1780.

While such work began with the French psychiatrists of 1800 and was elaborated by Esquirol in 1838, it was taken over by the Germans, who were close students of Parisian medicine in this period. During the second quarter of the century the subdivision of symptomatic classifications went so far as to list some 300 supposedly different men-

[1] William A. White: "Forty Years of Psychiatry" (N.Y., 1933), pp. 22 ff. (*Nervous and Mental Dis. Mon.*, Ser. No. 57). There are extensive studies of the evolution of psychiatric institutions and societies, notably Henry M. Hurd's *Institutional Care of the Insane in the United States and Canada* (1916). A suggestive brief account is afforded in Peter Bassol: "The Early History of Neurology and Psychiatry in the Middle West", *Bull. of the Soc. of Med. Hist. of Chicago*, III, No. 2 (Oct. 1923), 175 ff.

tal disorders.[1] Further confusion was occasioned by speculation about the ultimate nature of mental disease—an expression of the then popular *Naturphilosophie.* Heinroth wrote in 1818 of "sin" as a cause, and of "piety" as a necessary panacea. Even the ever-popular "polarity" was introduced into the mental field.[2] It is not strange that the majority of physicians were not greatly interested in a psychic orientation on this level.

There were, however, other levels more consistent with modern medicine, along which investigations of mental phenomena could be advanced. Indeed, it was in this era that physicians first undertook the objective, experimental study of what is now known as general psychology. Much of this story can be told in terms of three successive leaders at the University of Leipzig—E. H. Weber, Gustav Fechner, and Wilhelm Wundt. Weber began to teach anatomy and physiology there about 1820, and applied to studies on sensation the procedures already accepted in medical research. It was this acceptance of medical methodology, already beginning to involve quantitative as well as experimental procedures, which divorced the resulting psychology from the introspective type associated with general philosophy.[3]

Weber investigated muscular and cutaneous sensations, as well as vision and sound; and in the course of this developed his concepts of the "threshold", and of "the just noticeable differences" in all of these sensations. Obviously, he was working here in a border field between physics and physiology, which would eventually be known as psychophysics. Fechner and Wundt, who followed him in turn, continued the development of this science, emphasizing the general mechanics of nerve and muscle reactions. The intellectual atmosphere of the German universities during and after the days of Johannes Müller was conducive to this type of research, and it flourished accordingly. Wundt established an Institute for Experimental Psychology at Leipzig in 1878; and, following this lead, university departments of psychology were gradually set up quite separately from either physics or physiology laboratories. This was analogous to the separation of chemistry from medicine after 1800.

The process of professional differentiation was never made entirely complete. Research in psychophysics continued in the physiology laboratories of medical schools; while on the other hand psychologists

[1] Gardner Murphy: *Historical Introduction to Modern Psychology* (N.Y., 1932), p. 135.
[2] Emil Kraepelin: "Hundert Jahre Psychiatrie", *Zeits. für die ges. Neu. u. Psych.*, XXXVIII (Berlin, 1918), 163, 178 ff.; F. M. Harrison: "Psychiatry in Historical Retrospect", *Ann. Med. Hist.*, n.s., V (1935), pp. 90 ff.
[3] Murphy, op. cit., p. 84.

sometimes devoted themselves to clinical questions. Thus at the University of Pennsylvania, where J. McK. Cattell and G. S. Fullerton introduced studies along the line of Wundt's work, Witmer established in 1896 a "Psychological Clinic" for "problem children". Medical and psychological studies were combined, and a sort of psychiatric service was developed. Such "child guidance" clinics were eventually established in all large American cities, and have become necessary adjuncts to progressive school systems.[1] Wundt himself had not been primarily interested in child psychology, but his student G. Stanley Hall inaugurated a vigorous interest in the subject in the United States after 1900. J. Sully, an English psychologist who had followed German philosophical thought carefully,[2] organized a *British Association for Child Study* in 1895. Meanwhile, in Germany itself William Stern and others investigated child psychology in close association with educational research. The whole field was of potential interest to both pediatrics and to psychiatry.

While some of Wundt's students, notably Hall and Cattell in the United States, were inaugurating an interest in experimental psychology, one of the earlier German students had pursued a path which led back more directly into the medical field. This was Emil Kraepelin, of Heidelberg and Munich, who saw the possibility of extending Wundt's experimental methods to the investigation of psychopathology. Mental abnormalities could be studied experimentally and in quantitative terms. The milder types might even be produced experimentally, somewhat as a bacteriologist could artificially induce an infectious disease. The opportunities for further observations might be as valuable in one case as in the other. Kraepelin therefore observed his students' mental reactions under such disturbing influences as fatigue, hunger, and alcohol.[3]

From such studies, Kraepelin proceeded to an investigation of the more serious mental disturbances. Here he encountered the confusion in disease classification already described; and took up once more the effort to bring order out of this chaos. Still lacking pathological criteria, he attempted to skip over this historic means to identification

[1] See, e.g., F. L. Patry: "How Psychiatric Services Are Being Utilized by Schools of New York State", *N.Y. State Jour. Med.*, XXXV (1935), 1101 ff.; the development of child psychology in general is described in Murphy: *Historical Introduction to Modern Psychology*, pp. 279 ff.

[2] His work on *Pessimism* affords the best English critique of Schopenhauer and Hartmann, who were of some significance to later psychiatry because of the role of unconscious mind in their philosophy.

[3] Murphy: *Historical Introduction to Modern Psychology*, pp. 170 ff.

and sought for immediate ætiological distinctions. Whatever the value of Kraepelin's ideas about the causes of mental illness may have been, he was aided in his description of disease types by a relatively careful observation of large numbers of cases, and also by a new approach which proved immediately suggestive. This was the study of the patient's life-history—of the long range as well as the cross-section view of the patient's behaviour. It led to the identification of the manic-depressive type, in which the same individual was shown to go successively through these two states heretofore thought of as distinct disorders.

In addition to this type and to the obvious classes of the imbeciles, the feeble-minded, and the sex variants—which may be viewed as abnormal rather than as strictly pathological conditions—Kraepelin described more carefully the emerging concept of paranoia and the more generally recognized paresis. He also showed a certain similarity between several sorts of "deteriorating" psychoses which he classed together as dementia præcox—the common and serious dementia of youth. The resulting classification of the psychoses doubtless had its weaknesses. Indeed, one wonders whether even such improved categories were not based on concepts as vague as were the various "fevers" in the somatic nosographies of 1800. Yet Kraepelin's system, with its limited number of classes, was far more usable than were the confused lists preceding it; and it was adopted widely by medical superintendents responsible for the actual treatment of these conditions.[1]

During all the earlier years of medical psychiatry the more serious psychoses had naturally received the greatest attention. The same was true of the chief abnormalities, such as idiocy and imbecility. The more subtle abnormalities—the feeble-minded, and the sex variants—came into their own, so to speak, about the end of the century. Psychiatric and social interest in the feeble-minded was associated with the general child welfare movement and with the development of child psychology noted above. Sex variants became the object of increasing study when biologists, psychologists, and sociologists converged on the subject of sex in general, bringing to this ancient theme both the new techniques and new viewpoints which had lately developed in their respective fields. The more strictly medical approach was employed by Richard von Krafft-Ebing, who lectured in several German universities and

[1] For the entire classification system, see Emil Kraepelin: *Clinical Psychiatry*, transl. from second German ed. by Thomas Johnstone (N.Y., 1913), pp. 21, 204, etc.; William A. White: *Lectures in Psychiatry* (N.Y., 1928), p. 116, etc. (*Nerv. and Ment. Dis. Mon. Ser.*, No. 51).

whose *Psychopathia Sexualis* was published in 1886. This afforded a usable classification and careful description of the different types of variants. A general synthesis of both medical and psychological studies of sex was later provided in the writings of the English authority, Havelock Ellis.

The increasing study of sex was obviously to be interpreted in terms of general social trends as well as with relation to scientific developments. Folk attitudes towards the subject had often been realistic enough, even in puritan societies. Nevertheless, the medieval ascetic tradition had certainly affected the attitude of the educated classes; and the situation had been further complicated by the development of so-called "Victorian prudishness" in northern Europe and America.[1] During the latter part of the century conscious reactions to this set in on the ground that there was nothing evil in sex *per se*, and that in any case it was such a powerful force in human nature that it must be either recognized or reckoned with.

This reaction expressed itself in art as well as in science. Its advent may be conveniently dated in the United States, for example, by the publication of Walt Whitman's *Leaves of Grass*. Readers of that classic will recall that it expressed a naturalistic revolt against asceticism and prudery; a revolt which shocked most of his contemporaries "no end", but which shocks their descendants no longer. The difference here in the reaction of succeeding generations is a measure of the increasing acceptance of naturalism as a more healthy point of view. Soon the sex theme played an increasing or at least a more open part in the revived realism of contemporary literature. Then it naturally invaded the polite conversations of adults, who in turn demanded that even children should receive some sort of "sex education". By the time of the first World War, at least in the United States, a generation which was really still more afraid of sex than had been its puritan forebears had come to think of itself as quite daring and emancipated.

This intellectual and social atmosphere permitted, and was to some extent made possible by, a very striking development in psychiatry. Few medical leaders have received as much popular attention or exerted as much influence upon popular thinking as did Sigmund Freud of Vienna. After a regular medical training, Freud entered hospital ser-

[1] It is common to think of this as peculiar to English-speaking countries, but such differences as obtained between them and the northern Continental countries were certainly ones of degree rather than kind. Freud, for example, refers to "prudish America", but it is to be noted that there have been unfavourable reactions in Germany to his emphasis upon sex.

vice in Vienna; and it was during this early period in his career that he and Koller were associated in the discovery of local anæsthesia.[1] Becoming interested in neurology and psychiatry, he proceeded to Paris in order to study under Charcot. Freud found the master very busy, but introduced himself into the latter's immediate circle by offering to translate his works into German. (Charcot had lamented the fact that since the then recent Franco-Prussian War, there had been some loss of contact with German scientists.) In this way the young Austrian physician was able to observe closely the work of the outstanding neurologist of the day.[2]

Charcot was interested in hysteria, which he demonstrated was an important psychiatric phenomenon. More than this, he was able to check the symptoms by the use of hypnotism, which he considered a form of suggestion operating upon patients in an essentially hysterical condition. Hypnotism, it may be observed in passing, had been discredited after Mesmer's day, especially after it became associated with phrenology, and seemed to be tapering off into a device of cults and quackery. Interest in the possible medical values of the process, however, was revived by surgeons during the forties; and it was viewed as respectable at the time that Charcot employed it. Freud was stimulated by the work in French clinics, and became interested in the possible origins of the neuroses observed. He found, however, that Charcot was not concerned with this problem—did not wish to probe beneath the mental surface of these symptoms. That attitude, decided the Austrian, was due to the master's "base in pathological anatomy". This must be abandoned if mental phenomena were to be explored adequately. In other words, a psychic rather than a somatic orientation was again indicated.

Freud returned to Vienna and settled down to practice with neurotic patients in 1886. Like Mesmer before him in the same scene, he was frowned upon by conservative physicians. His therapeutic equipment was limited to electricity and hypnotism. The former he discarded as useless, but the latter seemed of some aid. Hearing of Bernheim's work at Nancy, where hypnotism of a rather forceful sort was employed, he went there for further study. Eventually, however, he found that "mesmerism" removed neurotic symptoms only temporarily, and was therefore no final solution for actual cases. At this point, Freud met Josef Breuer of Vienna, who had succeeded several years before in cur-

[1] Freud himself remarks that he suggested the experiments, but that the first actual demonstrations were made while he happened to be away from the hospital.

[2] Freud: *Autobiography*, transl. by James Strachey (N.Y., 1935), pp. 18 ff.

ing a girl of hysterical symptoms in an unusual manner. The cure occurred when the patient recalled, under hypnotism, certain repressed recollections of an unpleasant character which had apparently been responsible for the symptoms.

Here was a clue to the all-important problem of a possible mental ætiology of neuroses. Freud and Breuer followed it up in their practice for several years, and published jointly, in 1895, their work *Studien über Hysterie*. In this was presented the theory, now so generally known, that neuroses were caused by the suppression below the level of consciousness of unpleasant desires or experiences, from which vantage point they found an outlet in the symptoms observed. These could be cured by leading the patient to recall the original ideas—that is, by permitting them again what might be termed a normal outlet. Once this was achieved, the psychic tension was relieved somewhat as a somatic tension might disappear upon the lancing of a centre of infection. It was found, incidentally, that careful questioning of the patient was adequate for this purpose, and that the systematic use of hypnotism was not desirable. The authors were convinced that successful clinical experience confirmed the value of their thesis.[1]

Freud, after arduous investigations, now proceeded to an elaboration of the theory with which Breuer was not in sympathy. The former studied a wide range of mental phenomena, such as "slips of the tongue", proceeding from the relatively normal to the clearly pathological ones. Dreams, in particular, were found suggestive of the contents of the unconscious mind. This last was by no means a new conception, since, as usual, the philosophers—notably Schopenhauer and Hartmann—had written extensively on it before it became a matter for relatively objective study. Freud emerged, when he published his *Traumdeutung* in 1900, with a well-rounded, systematic theory as to the whole nature and treatment of neuroses. He did not feel that his system was applicable to the psychoses, except for certain types of depression.

It is hardly necessary here to review his theory in detail, since in approximate form it has become familiar to almost all educated peoples. The high points, proceeding from what has already been said, were that the suppressed ideas or desires were usually of a sexual nature; that the suppression was the work of a psychic mechanism known as the "censor"; that the suppression usually occurred in infancy or child-

[1] Freud: *Autobiography*, pp. 36 ff. See also his article "Psychoanalysis" in the *Encyc. Brit.*, 14 ed., XVIII (Lon., 1929), 674.

hood; that it was later revealed in an elaborate symbolism in dreams and, indirectly, in the neuroses themselves.[1]

The general treatment involved in psychoanalysis followed the "cathartic" procedure already worked out with Breuer, plus certain elaborations resulting from Freud's further experience. The analyst abandoned suggestion entirely, either under hypnosis or when the patient was awake. For this procedure took no account of the patient's own statements, which were so revealing of the nature and origin of his symptoms. Indeed, this was viewed as the basic mistake of earlier psychiatrists—that they considered the patient's "ramblings" to be meaningless, whereas they were really the most significant data to be had. Suggestion was, at best, a sort of salve upon the neurotic surface; after its application the symptoms always reappeared. Analysis, it was claimed, probed deeper into "depth psychology" and afforded a psychic surgery which removed the mental lesions.

Actual procedures went through several general steps, involving long and careful questioning, leading finally to a "free association" of the patient's thoughts. In the first step the "libido" or sex-urge originally responsible for the suppressed desires was brought above the surface of consciousness. For some strange reason, the libido then attached itself to the person of the analyst by a process of "transference". (This might be flattering enough when the patient was an attractive young woman, but was somewhat disconcerting in the cases of elderly women and other men.) The next step was to free the libido from the analyst. This accomplished, the patient's ego could face the libido, and, freed of all tensions, direct it into some healthy "sublimation".[2]

For several years Freud worked in apparent isolation and under considerable disapproval. By 1902 a small group had gathered around him in Vienna, and occasional interest was displayed in nearby Continental countries. In 1908 a first conference of psychoanalysts was held at Salzburg, and a journal was established. Then a group at Zurich, led by Carl Jung, proved sympathetic and established a centre for similar studies. They had become interested in the "stimulus-word" tests for "associated experience" suggested by Wundt, but went farther than the latter in attempting psychical explanations of the reaction data. In this

[1] Freud: *A General Introduction to Psychoanalysis*, transl. by G. Stanley Hall (N.Y., 1920), especially pp. 210 ff.

[2] Freud: *ibid.*, pp. 122, 177, 194, 379, 394, etc.; the same author's *On Dreams*, transl. by M. D. Eder (Lon., 1924), pp. 21, 88, etc.; William A. White: *Lectures on Psychiatry* (N.Y., 1928), pp. 122 ff.

way one group was led over, so to speak, from experimental psychology into psychoanalysis.[1]

In the study of the relation of the libido to personality types Jung evolved another concept to be rapidly absorbed in popular thought; that is, the distinction between "extraverts" and "introverts". This concept established a connection between psychoanalysis and Kraepelin's classification of the psychoses; since the extreme form of introversion was identical with dementia præcox—with the "shut-in" personality. The same thought was elaborated by Bleuler, of Zurich, who applied the term schizophrenia to the "split personality" observed in the same general type of disease.

Little interest was displayed in Latin countries in Freud's work, but the reaction in the English-speaking nations was more sympathetic. G. Stanley Hall at Clark University, Brill in New York, and Ernest Jones of London all became well known as supporters of Freudian procedures. Freud and Jung lectured at Clark in 1909, and this gave a considerable impetus to the movement despite Freud's remarks about "prudery" and the "lack of a scientific tradition" in the United States.

After returning to Europe, Freud and Jung diverged in certain respects, and such unity as the movement had possessed was broken by a development of distinct "schools". Jung felt that Freud over-emphasized the role of the libido; that a diffuse life-energy rather than sex alone was primarily responsible for the strivings suppressed in the unconscious. Symbolism in dreams was to be viewed not simply as a mask for the libido, but rather as the natural language of the very primitive thought processes involved. Jung's exposition of the libido became as fanciful to the Freudians as the work of the latter had appeared to orthodox psychology. To make the situation more confusing, another "school" appeared which centred about Alfred Adler of Vienna, whose work on the "inferiority" and "superiority complexes" and upon "compensation" became widely known. Adler stressed the organic origin of such complexes.[2] Here was to be noted a tendency to return to the traditional somatic explanations of mental disease, in contrast with Freud's more consistent dependence upon psychical interpretations.

Opposition to psychoanalysis among neurologists and psychiatrists, as well as among lay critics, was a natural concomitant to the growing popularity of the system. Freud himself ascribed unfavourable criticism to three factors: first, the fundamentally somatic orientation of the

[1] Freud: *A General Introduction to Psychoanalysis*, p. 86.

[2] Murphy: *Historical Introduction to Modern Psychology*, pp. 328 ff.; Garrison: *History of Medicine*, 4 ed., p. 703.

medical sciences; second, the disinclination of many minds to accept "the unconscious" as a basis for scientific conclusions; and, third, the common objection to what was viewed as an over-emphasis upon sex.[1] All these doubtless played their part. It is a fair assumption that the first factor was most potent with medical men, the last with the educated laity. Much of the Freudian discussion of sex, particularly that relating to the sexuality of small children and the effort to find sexual symbolism in the apparently meaningless data of dreams, was distinctly offensive to some readers. At best, it seemed far-fetched; at the worst, close to the obscene.[2] On the other hand, these very elements proved quite fascinating to others; so much so that psychoanalysis threatened to degenerate into a fashionable cult after the manner of mesmerism a century before.

In concluding a discussion of so controversial a subject as psychoanalysis, it may be well to summarize the chief points made in its defence, and the more important criticisms. It may be claimed, in defence, that the method involved a real effort to get below the surface of neurotic symptoms, and that this attempt to discover a psychic pathology and ætiology was indicated by the failure to find somatic bases for these phenomena. It would also seem that the most common criticisms of Freud's work are not the most significant. The emphasis upon sex, for example, is a matter of degree, and doubtless can be modified in the light of convincing evidence one way or the other. (It is pertinent to recall, again, that such emphasis was in the intellectual atmosphere of the times.)[3] Nor was the general criticism that Freud's work was "merely speculative" a sound one; for the latter was doubtless correct in the assertion that it was based on a large number of clinical observations.[4]

That there were some speculative elements in Freudian reasoning could hardly be denied, but it may again be pointed out that there is nothing objectionable in speculation *per se*. Freud's hypotheses were daring and ingenious, to say the least. Essential questions were: to what extent was such speculation inextricably interwoven with, and therefore indistinguishable from, conclusions based upon empirical data? and, to what extent did statements introduced as theories gradu-

[1] Freud, in the *Encyclo. Brit.*, 14 ed., XVIII, 674.

[2] As an example of clever and somewhat severe criticism, see Joseph Jastrow: *The House that Freud Built* (N.Y., 1932), p. 212, etc.

[3] For example, an essentially sexual basis for so subtle a matter as the æsthetic sense was elaborated in Santayana's delightful work, *The Sense of Beauty*. The treatment of the theme here, of course, was not quite the same as that met in Freud.

[4] Freud: *Introduction to Psychoanalysis*, pp. 63, 210.

ally take upon themselves the appearance of established facts? Each reader must answer these questions for himself. One fundamental criticism stands out clearly. The whole Freudian hypothesis, if we may call it that, was of such a nature that it was very difficult to check it *one way or the other*. It did not seem, in the nature of the case, susceptible to experimental verification.[1]

This difficulty, if it is not removed, brings into question the validity of the whole scheme of psychoanalysis. A "system" explaining in logical sequence the origin, nature, and treatment of all disorders of a given class is suggestive—within the mental field—of those general medical systems which were so typical of the eighteenth and earlier centuries. If, like the latter, psychoanalysis cannot be checked one way or the other, it represents a reversion to the system-making stage in medical methodology, and may be expected to reproduce the various features and developments typical of that level. It is suggestive that this is indeed just what has happened. Once more, within this particular field, one hears of masters and disciples, of divergent "schools", of scientific controversies in which there is an "attack" or a "defence" of a given system with more or less feeling on both sides. There is, to be sure, considerable clinical evidence supporting the divergent viewpoints; but so, too, was there such evidence in the case of the older and long-since discarded systems. There is also clinical evidence of the therapeutic values of psychoanalysis; but this gain is nothing new or decisive. There was plenty of cumulative evidence in 1800 of the value of blood-letting.

Pending the possible formulation of experimental checks on psychoanalysis (which are being attempted) [2] something may be done to investigate the validity of its therapeutics. This, after all, is what most concerns the general public. What is evidently needed is a definite diagnosis of the particular diseases involved, and a subsequent check of treatments in terms of these diagnoses and with the use of controls. When clinical statistics of the type which Louis applied to blood-letting can be employed in a study of large identical groups who have, and who have not been analyzed, then there will be afforded some real evidence of the intrinsic merits or demerits of the procedure.

The very suggestion, of course, brings up almost insuperable diffi-

[1] This point is stated in J. F. Braun: "Freud and the Scientific Method", *Phil. of Science*, I (N.Y., 1934), 323 ff. See also Carney Landis: "Psychoanalysis and Scientific Method", *Proceeds.* Amer. Phil. Soc., vol. 84 (June 30, 1941), 515 ff.

[2] See Robert R. Sears: *Survey of Objective Studies of Psychoanalytic Concepts* (Soc. Sci. Research Council, N.Y., n.d.—*c.* 1943), 135 ff.

culties. Neither the general classification of the neuroses nor the diagnoses of individual cases are usually so exact as to permit of the further abstractions of statistics. It may be said that, lacking these desiderata, we must depend on the most logical and persuasive system available. This may be true; though the thought that harm as well as good may be done by any potent treatment might give one pause. If there must be a return to more elementary methods of attacking mental diseases, then we must take our chances on the weapons employed. Meanwhile, if history is to be accepted as a guide, hope for the future will be found in further experiments with empirical treatments, and with continued investigations of the ætiology and pathology of mental diseases.

Both ætiology and pathology may prove to be ultimately somatic in nature. This would be the simplest of all possible outcomes, as it would eliminate the confusing dualism of mind and matter. Yet it may turn out that only psychic explanations will ever be found in certain cases. In the latter instance the psychic disturbances may ultimately be referred beyond the individual into his social environment. There is obviously, for example, what might be termed a social ætiology of such a somatic disease as tuberculosis. In the same manner, it is conceivable that social causes may one day be discovered for the neuroses, and even for some of the psychoses.[1] This might prove of the utmost therapeutic value, even though the actual pathological elements involved could not be revealed. A social environment productive of dementia præcox could be avoided, even though little was known of the psychological mechanisms involved in actual cases, much as miasmatic regions were shunned effectively long before there was any knowledge of the mechanism of malarial infection.

Such possibilities were by no means overlooked by psychiatrists in recent decades. It is true that they were not interested over a long period in the study of environmental factors, were not concerned with an orientation in the social sciences. In some cases there was even a lack of contact with experimental psychology. Stern has pointed out, for instance, that, despite the importance attached by Freudians to childhood experiences, they long made no studies in child psychology. But the work of psychoanalysts as well as of all other psychiatrists inevitably contributed to a growing recognition of the serious and widespread character of mental diseases. And this in turn led to an increased concern for their general social significance.

[1] See, e.g.: Edward Sapir, "Cultural Anthropology and Psychiatry", *Jour. of Abnormal and Soc. Psych.*, XXVII (1932), p. 235; A. I. Hallowell: "Culture and Mental Disorder", ibid., XXIX (1934), pp. 1 ff.

Americans were shocked to learn that the number of hospital beds devoted to mental cases in their country was approximately as great as that required for all other types of illness combined. This was startling, even after allowance was made for the slow turnover in the population of the former type of institution. Nor were various other statistical statements any more encouraging. It appeared by 1930 that about 75,000 mental cases were admitted to American institutions every year, and that at this rate about one out of every twenty-one in the total population has been or will be at some time in such institutions. There were said to be some 1,000,000 children, out of a child population of 24,000,000, who were destined to suffer from mental disease unless proper treatment was received in time.[1] As in the case of somatic diseases, such figures may have indicated improved reporting rather than any actual increase in the conditions involved. In either case, the reports alarmed the public and led to a demand for action.

A crusade was thereupon inaugurated for the proper care and prevention of mental disease. As was to be expected, it was directed along a more scientific level than had been the movements of Victorian days, but in each instance there was a co-operation of lay and medical leaders. In the United States the new movement owed much to Clifford Beers who, after emerging from mental hospitals, wrote his famous account of *A Mind that Found Itself*. He later led in the organization of the National Committee on Mental Hygiene, which was instrumental in promoting the improvement of the better hospitals and in arousing public concern for the proper treatment of all mental cases.[2]

At this point the demand for a programme against mental disease was naturally merged into the general movement for public hygiene and preventive medicine. If the whole problem of mental conditions had taken on social proportions, perhaps social remedies could be found. The current emphasis upon preventive medicine would not permit of a mere increase in "asylums" and similar custodial measures. Mental disease, like somatic disease, must be prevented as well as cured.[3] As usual, two means to this end presented themselves. The problem could be approached from the hereditary or from the environmental angles. Mental hygiene could be made primarily a matter of eugenics,

[1] J. L. McCartney: "Mental Hygiene in a Public Health Program", *Amer. Jour. Pub. Health*, XX (1930), 943 ff.

[2] Some suggestion of the scope of the movement is afforded in the *Guide to Places of Mental Hygiene Interest in the United States*, issued by the National Committee (N.Y., 1930).

[3] C.-E. A. Winslow: "A Community Mental Hygiene Program—The Next Great Opportunity", *Milbank Mem. Fund Quart.*, XIII (1935), 211 ff.

or a matter of social improvements. In Germany the biological approach was stressed; in the United States, the environmental. Neither, of course, is necessarily exclusive of the other, and an actual programme of mental hygiene may consist of an attempted evaluation of both factors in given types and situations.[1]

In recent decades clinical research in psychiatry has continued to be of a largely empirical nature. As each new lead opened up in somatic therapy, attempts were made to apply it to the treatment of psychoses. The surgical vogue for removing foci of infection was thus carried over, but with disappointing results. The latest products of pharmacology and of endocrinology were utilized, as in the employment of hypnotics and of insulin. Shock was an old procedure, and was now secured by electrical equipment as well as by drugs. Encouraging results were often secured, but there was danger as well, since effects seemed to be measured by the degree of severity employed. Some observers actually concluded that the patient was frightened out of his mental symptoms by this "modern form of torture".[2]

World War II directed attention forcibly to the need for further clinical research on the neuroses. This was occasioned by the revelations of induction [3] and by the high rate of psychiatric casualties reported by the military services. Despite a serious lack of trained psychiatrists,[4] medical officers made interesting studies of neuroses induced by army life and by combat, and promising forms of therapy were evolved. The treatment known as narcosis, for example, employs hypnotics and procedures suggestive of psychoanalysis, and seems to have been distinctly helpful.

As an aftermath to the war, there appeared a renewed concern for the condition of the nation's mental hospitals. Many of these, despite the good work of the National Committee on Mental Hygiene, had never

[1] Karl Birnbaum: "The Social Significance of the Psychopathic", in F. E. Williams (Ed.): *Some Social Aspects of Mental Hygiene* (Phila., 1930), pp. 70 ff. (Supplement to a vol. of the *Annals* of the Amer. Acad. of Polit. and Social Sci.). The whole social field of mental hygiene is reviewed in this volume. On possible hereditary factors, see Abraham Myerson: "The Inheritance of Mental Disease", in *The March of Medicine* (N.Y. Acad. of Med., 1941), 3 ff.

[2] Edward Strecker: "Pharmacological and Surgical Approaches", in Univ. of Penna. Bicentennial vol. *Therapeutic Advances in Psychiatry* (Phila., 1940), 1 ff.

[3] Of more than 4 million men rejected for the U.S. military services, 1940 to June 1944, more than 700,000 were rejected for "mental disease", and nearly 600,000 more for "mental deficiency" (including illiteracy); *Hearings*, Senate Subcom't. on Wartime Health and Education, Pt. 5 (Wash., 1944), 1618.

[4] Among about 180,000 physicians in the U.S. in 1944, there were only some 3,000 recognized psychiatrists. Of the latter, not more than about 600 were available outside of hospitals; testimony by Dr. L. S. Kubie of Columbia University, *Hearings: Medical Research*, I (before the Subcom't. noted above, Wash., 1944), 2229 ff.

risen much above the custodial level. During the war old buildings became more inadequate and it proved almost impossible to secure attendants of any sort. The Federal Government thereupon assigned many conscientious objectors to these institutions, with the result that for the first time in history really high-grade attendants were available. Such men were naturally shocked by the conditions of overcrowding and neglect which they encountered, and their protests led to informal surveys which were published in popular magazines. This group organized and published a little journal for their own use (the *Attendant*), and their demands for better care of the mentally ill received wide attention. Whether public attention will again decline—as it has periodically since the days of Dorothea Dix—remains to be seen.

Unfortunately, there is still just enough stigma associated with mental disease to make the public more indifferent to its consequences than they are to those of ordinary illness. There is a tendency to forget the victims, once they are "put away", and this makes it difficult to maintain interest in the quality of mental institutions. In the United States, moreover, it will be difficult for State hospitals to compete—for the limited professional personnel and attendants—with the improved hospital programme of the federal Veterans Bureau. Only by providing much larger funds for buildings, equipment, and salaries can the States avoid even further deterioration in this essential phase of medical care.

After all the usual phases of psychiatry have received attention, there remains one aspect of mental life which can be easily overlooked despite its universality. In most psychiatric research emphasis has naturally been laid upon the abnormal, or at least upon borderline phenomena, while relatively less heed has been given to the possible influence of normal mental states upon disease. Yet if the body can produce mental disease, the mind can be in turn responsible for physical ills. Priests and general practitioners, as well as quacks and sectarians, have often recognized in a general way the significance of the patient's "state of mind". In recent decades this has been subjected to a more careful and critical investigation, in response to the revived interest in the sick man's personality as a whole.[1] It is possible that the future hospital patient will receive a regular check of his mental as well as of his physical condition.

Emotional states within the limits usually assumed for normality may well prove of considerable importance. Fear can be as dangerous

[1] See, e.g., H. F. Dunbar: *Emotions and Bodily Changes: A Survey of Literature on Psychosomatic Interrelationships, 1910–1945*, 3 ed. (N.Y., 1946).

as some infections, and hope more useful than many drugs. Such potent factors need not necessarily be taken for granted or treated as incidental to other elements in a total situation. There is still something to be done towards solving the age-old puzzle of body and mind.

XVIII

PRACTICE IN A CHANGING SOCIETY

1880–1930

WHILE FEW developments in medicine have caused the popular flurry that has psychoanalysis, it would be difficult to say just what influence the latter exerted on the public relations of the medical profession as a whole. The fact is that economic rather than scientific influences were becoming paramount in this period. The respect accorded physicians, which was described in a preceding chapter, did not in itself imply that their contacts with the public were all that could be desired. Ironically enough, the very progress which inspired confidence also created difficulties.

In the first place, improvements in training and progress in science forced the medical student to invest far more in education than had hitherto been necessary. Expenses in practice also rose, partly because of a need for more elaborate equipment, partly because changes in urban living necessitated the rental of office space in the business district. In due time offices called for secretaries or other attendants. One would expect to find that increased expenses of this sort were passed on to consumers through higher charges. There is, indeed, evidence that fees in the United States, for example, were rising in marked fashion even before the end of the nineteenth century. But these increases must be checked against parallel changes in price levels before their meaning can be made clear.[1]

In the second place, scientific progress made for greater costs in medical care through a chain of circumstances associated with specialization. The rise of specialties after about 1870, with the organization of national societies and eventually of accrediting boards, is a

[1] The evidence, moreover, is provided by published fee bills, and practice did not necessarily correspond to these. See George Rosen: *Fees and Fee Bills: Some Economic Aspects of Medical Practice in Nineteenth Century America* (Baltimore, 1946), 64 ff. (Supplements to the *Bull. of the Hist. of Med.*, no. 6). Increased expenses in medical education after 1900 are analyzed in R. G. Leland: "The Costs of Medical Education", *Jour.* of the Amer. Med. Asso., XCVI (1931), 682 ff.; and in Abraham Flexner: *Medical Education: A Comparative Study* (N.Y., 1926), 294 ff.

story in itself. It can only be noted here that localized pathology and increasing knowledge led to specialization on certain body parts or systems. Although the medical profession at first opposed this trend, the prestige of specialists rose rapidly after 1900. They charged higher fees than did general practitioners, and the patient was likely to see more than one of them in the course of a long or baffling illness. Parallel specialization also occurred among nurses and medical technicians. The ultimate economic effect of all this is best observed in mounting hospital costs, which became burdensome to patients at the very time when the number seeking institutional care was rapidly increasing. It was this complex of services, as contrasted with the unaided efforts of a family doctor, which was primarily responsible for over-all advances in medical costs. Other things being equal, the quality of service was rising and some one had to pay for it. But it was only human for patients, worried about bills, to feel that somehow or other physicians were to blame for the situation.

The profession was meanwhile caught in the current of the times and carried in a direction which further complicated its social organization. Industrial society set up ideals of business success which inevitably influenced professional men to some degree.[1] The young doctor who went into debt in order to pay for training and equipment saw around him businessmen who acquired wealth and who set a standard of living which tempted others to go and do likewise. The physician felt that he had worked harder and sacrificed more than most businessmen. Why should he not receive as large a return as they? Why should he not charge substantial fees? Such feeling was naturally more common in the cities. And to the cities the doctors moved, along with the rest of the population.

Here the plot thickened. In the urban environment neither doctors nor patients knew each other so intimately as had once been the case. Some prosperous patients found it possible to abuse the old free-service ideal by resorting to clinics intended for the poor. Others shopped around from one practitioner to another, moving on whenever pressed for payment. On the other hand, some physicians abused the sliding scale of fees, by charging people of limited income more than they could really afford. This was not always intentional, for it was not so easy to gauge a patient's purse in city practice as it had been when the family doctor was father-confessor to his community. Despite con-

[1] There are brief observations on this theme, in the statement by Walton H. Hamilton, appended to the final report of the "Committee on the Costs of Medical Care" (*Medical Care for the American People*, Chicago, 1932, pp. 191 ff.).

demnation by medical organizations, there was fee-splitting between surgeons and general practitioners—a device which threatened the patient's health as well as his pocket-book. Thus the conditions as well as the ideals of urban life left their marks on the healing art.

Increasing charges for medical service might, in themselves, have caused a certain amount of difficulty. But it was the combination of such increases with a growing demand for medical service that made the problem of medical costs so acute in the present century. There had been little concern in earlier periods as to whether or not all members of a community received the attention of regular physicians. There is good authority for the statement that slave-owners in the United States were content to leave a large part of practice among slaves to the overseers. And poor white men made the best of such medical charity as dispensary doctors or other physicians might occasionally give them. Conditions were not very different in Europe, where appeals for medical aid were discouraged as a matter of economy in the administration of poor relief. The lack of concern about this situation may be ascribed in part to social indifference, but it was also due to a lack of public confidence in medical science.[1] Few people complained, in 1850, that they had no access to hospitals. They were only too happy to stay away from them.

Then dramatic advances in medicine made it imperative that all people secure the services of regular medicine. One question was inevitable: was society in general, and the medical profession in particular, in a position to meet this growing demand? Serious doubts arose. Declining death rates, ascribed to medical aid, showed that mortality was not a constant to be taken for granted. The very fact that certain diseases could now be prevented or cured suggested that they might actually be eliminated. But they obviously persisted. Hence there gradually evolved, in educated minds, a syllogism of some such form as this:

> Medical science can now prevent or cure certain major diseases.
> Many people continue to suffer from these very diseases.
> *Ergo*, medical science does not serve the people as it should.

The most obvious explanation was to be found in the mounting costs of service. Here, again, it is to be noted that it was the very progress which physicians had made in science, which involved them in new difficulties in the practice of their art. Technical improvement led

[1] See Chap. XIII.

to simultaneous increase in the demand for medical services and in the price that must be paid for them. And so the more that people trusted medical aid, the less they could afford it. Here was a serious and unexpected *impasse* in the public relations of the profession.

It was, of course, one thing to recognize this problem, and quite another to solve it. It gradually became apparent that certain groups which had hitherto received some medical attention were no longer able to do so under the changing conditions of modern society. This was especially true of the skilled working classes. Such workers had, since medieval days, protected themselves against the expense of illness through guild funds or benefit organizations of some sort. Workingmen's societies providing insurance protection had developed rapidly in England, for instance, at the end of the eighteenth century. It was realized at this time that many who could not pay for the heavy expense of serious illness could meet these costs if they were spread over the years by premium payments. The factory labourers and lower middle classes of the nineteenth century, unfortunately, formed groups which lacked adequate protection of this sort.[1] Their pride demanded that they pay even when they could not afford to do so, or else that they go without any medical care whatever. A certain proportion resorted to this last device, to the detriment of their individual health and that of their community. The old tradition of voluntary insurance had not been expanded to meet their growing needs, and the still older tradition of charity did not meet their needs at all. A stigma was attached to charity hospitals, just as there had once been to charity schools.

The degree to which this became a matter of public concern depended upon the general social philosophy of a given time and place. In Germany and Austria, where socialism made considerable headway after 1870, the medical needs of the poor received relatively early consideration. Bismarck wished to forestall socialism by making the poor look to the Government, rather than to the Social Democrats. He is said to have been moved also by a semi-feudal ideal, the belief that the poor should look to their overlords for personal security. Since the principle of voluntary insurance had failed to keep pace with the needs of the time, the Chancellor secured protection for the labouring population by compulsory insurance against such major risks as sickness

[1] It may be recalled that it was an interest in working-men's insurance funds which aroused Edwin Chadwick's interest in the health of the labouring class which had no such protection. See his article, "Life Assurances—Diminution of Sickness and Mortality", *Westminster Rev.*, IX (1828), 384 ff. On the early history of benefit societies and their relation to the first health insurance systems, see the publications of the International Labour Office of Geneva, notably *Voluntary Health Insurance* (1927).

(1883) and old age (1889). This class was thereafter entitled to cash benefits and to medical service from physicians retained by insurance companies, rather than faced with the alternatives of neglect or charity. Premium payments were made by both employers and employees.[1]

The original emphasis in German sickness insurance was placed upon financial protection for a minority. In this respect it was thought of somewhat as voluntary health insurance is in the United States to-day, save that the classes involved were different in the two cases. But during the first decades of operation German social insurance was extended to other classes in the population, and emphasis shifted from financial to medical protection. Eventually practically all low-income groups—clerks, agricultural workers, etc.—were included in the benefits. Cash payments amounting to about half the regular wages were allowed for a maximum of twenty-six weeks, after which pensions were provided when necessary. Medical services were extended to include the families of the insured, and to provide prenatal and children's clinics, institutional care, and funeral benefits.[2]

The German system did not provide "State medicine" in the strict sense of the term, but it was obviously set up and directed as a matter of official policy. The Government and all other groups and organizations involved in the plan—except the medical profession—were represented in its administration. Poor relief, including a minimum of medical attention from city physicians, had long been a Government function; and it is not surprising that such relief was expanded in a day when *laissez-faire* objections to Government activities were already losing ground in central Europe. The physicians in Germany seem to have at first approved compulsory health insurance, since they quite naturally hoped that insurance payments would improve their low average incomes. Its advent coincided with an era of rapid scientific advances in medicine—with the time of Koch and Ehrlich—and doctors were then preoccupied with technical interests to an unusual degree. They had little time to study the social and economic aspects of practice, and apparently had no inkling of the economic and professional implications of health insurance. Consequently they had no suggestions to offer, no programme of their own; and the well-organized and

[1] The legislation and general background of this early State socialism is discussed in Frederick C. Howe: *Socialized Germany* (N.Y., 1916), pp. 194 ff.

[2] Walter Weddigen: *Grundfragen der Sozialversicherungs-Reform* (1931), cited in *A Critical Analysis of Sickness Insurance*, p. 6 (a preliminary report by the Bur. of Med. Economics of the Am. Med. Asso., reprinted from the *Amer. Med. Asso. Bull.*, April 1934; hereafter cited as *A.M.A. Bull.*, 1934).

influential insurance societies took over from the start the planning and administration of the new system.[1]

It was not long before the insurance companies, in which physicians had so little influence, began to exert quite an influence on the physicians. The latter found their relations to patients and to society changed in a radical manner. No practitioner was forced to accept insurance practice, but the situation was such that the majority of them eventually entered the service. The number of the insured steadily mounted, from 4,000,000 in 1885 to over 22,000,000 in 1928—from about 9 per cent of the population to 33 per cent. Moreover, the percentage of a given number of the insured who resorted to physicians constantly increased with the years. In 1888 a given 100 insured were reported ill on the average of 547 days; in 1925 the same number were reported sick 1,250 days. This meant more work for practitioners, in consequence of which their numbers rose. The population of Germany increased 14 per cent between 1887 and 1898, while the number of physicians actually advanced over 52 per cent during the same interval.[2] In a word, patients received more care, and the medical profession a greater total income.

Two quite different interpretations of these developments were possible. Although it seemed reasonable, particularly to social workers, to assume that the increase in sickness reports was a desirable phenomenon, many physicians thought otherwise. They reported that a large number of the patients consulting them were not really ill. Some German authorities estimated that from 60 to 75 per cent of the time of insurance practitioners was wasted on unnecessary calls. Much of this was ascribed to the feeling of the insured that, having paid dues over a considerable time, they were entitled "to get something back" from the impersonal organization now serving them.

To make matters worse, patients were said to report in order to satisfy the still popular craving for medicines—a craving which two generations of nihilistic medicine had not been able to eradicate. This tendency, in turn, shaded over into neurasthenia, and neurotic cases were said to be increasing in amazing fashion. Poverty had at least had this virtue, that it discouraged the hypochondriac or the neurotic patient from imposing too much upon the physician; now they were able to indulge themselves to their hearts' content. It was even claimed

[1] A. M. Simons and Nathan Sinai: *The Way of Health Insurance* (Chicago, 1932), pp. 23 ff.

[2] H. E. Sigerist: *Man and Medicine* (N.Y., 1932), pp. 312 ff. Note, however, that the number of days for which sick benefits were paid under German law was doubled during this interval.

that neurotic conditions were created by the opportunity for such indulgence.[1]

These trends, plus a small amount of actual malingering, necessitated a critical check on patients in order to protect the insurance companies against excessive charges. This check was necessarily made by the examining physician, and the resulting confusion of police and medical duties was an unfortunate one. It was likely to destroy any accord between the doctor and his patient. This was true even if the doctor gave of his best.

Still other difficulties ensued, which proved of great concern to physicians. Until a sufficient number of them entered the insurance service, the patient was restricted in his free choice of doctors, and this limited his confidence in his professional adviser. On the other hand, when the supply of physicians desiring insurance practice finally became greater than the demand, they entered into a competition with each other that was both uneconomical and unprofessional. Such competition tended to lower fees, and the less these became, the more patients a practitioner had to see in order to make the work pay. This, in turn, hardly made for careful service.[2]

Some medical observers also believed that the lack of substantial reward for his services tended to decrease the physician's personal interest in his insurance cases. Doctors were only human, and were likely to pay careful attention to those who paid well in private practice. Conversely, they were likely to treat their many insurance patients, for whom only small fees were paid, in a routine manner. Was it not well known that other systems of "State medicine", such as army medical service, exhibited considerable routine indifference of this sort?

Worse than this, was there not even danger that insurance fees would become so low as to depress the income of physicians below a professional level? And in such a case the whole character of the profession would suffer. This in turn would injure the public as well as the physicians. Actually, the income of the more poorly paid physicians tended to improve under insurance practice—at least temporarily. There was some difference of interpretation of the subsequent experience in Germany and elsewhere. American investigators reported that professional incomes under insurance have averaged as high, or somewhat higher than, they did in private working-class practice before insurance was

[1] *A.M.A. Bull.*, 1934, pp. 32 ff.

[2] It may be claimed that even in private practice physicians are in competition with one another. True as this is, such competition is not usually so direct or unfortunate as it is said to have been in some forms of insurance practice.

established.[1] But, at best, there was some uncertainty among physicians as to the effects of the new scheme on their incomes, and this became a factor in their attitude towards it.

In due time other apparent disadvantages were discovered, and many physicians eventually found themselves aligned in opposition to the whole insurance system. It must be remembered that the individualistic tradition in medicine was stronger, if anything, than it was in business. This being the case, it was naturally irksome for doctors to find themselves more or less dependent on the insurance organizations, managed as they were in large part by laymen.

These organizations, it was claimed, were inclined to dominate the whole system. They displayed a bureaucratic tendency to build impressive offices and expensive institutions, which were then used as "talking points" in "selling" the system to the public. Company officials were inclined to view the insured as dependents, and the physicians as employees. More and more red tape, and more and more regulation of practice encompassed the physician, and tended to rob him of his freedom.[2] He came to fear the complete loss of his professional status, which involved not only his personal interest but the quality of his service as well. An ancient and honourable guild seemed about to be forced into the employee status of modern industrial society; and, like other guilds in the same situation, it feared not only a loss of personal prestige but also the cheapening of its product.[3]

It was natural that the German medical profession should make some effort to defend itself against this seeming menace. As difficulties increased, the Hartmann Society was organized by a medical group in 1901. This served as a sort of professional union, although the use of that term suggests the very industrial status which physicians did not desire. The society collected funds to support strikes, and succeeded to some degree in improving the position of practitioners in relation to the

[1] Cf. the statements on income in *A.M.A. Bull.*, 1934, with Simon and Sinai, op. cit., p. 206.

[2] There are various situations in which an insurance company, in order to save money, may lay down regulations which interfere with the physician's professional judgment. Thus, insurance officers may set a limit on the number of days of hospitalization to be paid for in normal obstetrical cases. This limit may be below that deemed essential by the obstetrician in charge.

[3] Strictly speaking, it is not the employee status *per se* which threatens the freedom of physicians, but rather employment in a minor capacity by a large, impersonal organization. The physician who is a full-time professor in a good medical school retains considerable freedom, since the school is not such an organization. But one employed for routine work by a large company, or by the Government, is in a different position. The contrast here is analogous to that between the university professor, again, and a teacher in a public school system.

companies.[1] After about 1924 collective agreements and conferences between interested groups furthered the same end.

So much for this aspect of the picture. There was, of course, another side. Over and against all the objections to health insurance, as it developed in Germany, were claims of social improvement on a grand scale. Those who were weakest economically could now "count upon medical aid no matter what their station in life" without becoming charity cases.[2] No longer was disease so likely to bring need and despair, for public medical care was now provided in the same manner and had the same legal status as public health work and public education.

Social workers, labour leaders, and apparently the majority of those insured joined with the carriers in the view that these values outweighed the objections raised by the medical profession. Some of these objections were denied altogether. It was questioned, for example, whether the removal of the financial relation between patient and doctor would decrease the latter's personal interest in his case. It might even improve their relationship, since the question of payment was sometimes a cause of personal difficulties.

It was denied, too, that the analogy with military (State) medicine was significant in this connection. If there was routine indifference in army medical service, for example, this might be ascribed to army conditions in general, or to the quality of the military personnel. It did not necessarily mean that a similar indifference would characterize civil insurance service. At any rate, there was already considerable routine indifference in charity practice, and poor patients would be none the worse for shifting from this to another sort of routine under insurance protection. It was also observed that poor patients had little free choice of physicians under charity practice, and hence were not injured if such choice was also denied them by the carriers. In a word, the chief class of patients to whom compulsory insurance would apply never had enjoyed much freedom or much personal attention.

There remained the professional indictment, that the steady increase in calls upon insurance doctors was unnecessary from a medical point of view. The advocates of insurance interpreted this in quite the opposite fashion; that is, as evidence of an increasing care of the people's health. The poor were now consulting doctors in the early stages of disease, when the most could be done for them; rather than delaying until it might be too late, for fear of the expense or because of an aversion to charity. Even if many of the calls proved unnecessary, this at

[1] Sigerist: *Man and Medicine*, p. 314. [2] Ibid., p. 315.

least erred on the safe side and was common in private as well as in insurance practice. This increased care was expected to bring about a decrease in morbidity and mortality rates among the poor. Such a reduction would not only serve humanitarian ends, but would also reduce unemployment due to illness, and so tend to reduce poverty. Less poverty in turn meant less sickness. Thus, the vicious circle of disease and poverty was said to be cut by systematic medical attention, and in its place was set up a useful chain of health and security.

It was difficult to prove this last contention. Mortality rates were falling in insured Germany after 1900, but they were also declining in uninsured France and in the United States.[1] Medical critics declared that the insurance system had little or no connection with the German public health service. Little preventive work was involved. The early advocates of insurance had held it would do much to further the public health movement; but once the system was in operation, they were said to have lost interest in its broader social implications. Indeed, it was claimed that the insurance system tended to overshadow the public health service and to detract from its support.[2]

These criticisms were again denied by advocates of health insurance, who were encouraged by subsequent developments in Germany. They reported that insurance services were aiding the cause of public health not only in the general ways mentioned, but specifically through the improvement of hospitals and by the development of special aids to mothers and children. By 1935 the insurance funds were said to provide for the costs of the arrival of about two-thirds of all the babies born in Germany, and they subsidized the children's clinics to which these babies were brought for subsequent care. Rather interesting was the added claim that there had been a marked decline in the patronage of quacks of all kinds, since the poor were guaranteed the attention of regular physicians. If true, this was an achievement of some magnitude, for Germany had been "wide-open" to quackery since about 1870.

What were perhaps the most sweeping claims for sickness insurance were not easily susceptible to demonstration one way or the other. During and after the First World War the vitality of the German people was subjected to great strains. Defeat, the severity of the Versailles Treaty, inflation, and other post-war developments reduced the

[1] See *A.M.A. Bull.*, 1934, pp. 39, 40; Morris Fishbein: "Sickness Insurance and Sickness Costs", *Hygeia*, XII (Chicago, 1934), 1075.

[2] A. M. Simons and Nathan Sinai: *The Way of Health Insurance*, p. 161. Cf. Barbara N. Armstrong: *Insuring the Essentials* (N.Y., 1932), p. 361.

majority to a rather desperate condition. (The personal privations that ensued may have accounted for some of the increasing demands on insurance doctors previously mentioned.) The fact that under these circumstances German vital statistics continued to be about as favourable as those of France and of the United States was credited to the efficiency of the sickness insurance service. But this is anticipating later phases of the story.

The German example in providing social insurance apparently made a decided impression upon other European States. Austria, the first to follow, adopted compulsory health insurance for those with low incomes in 1888. France and Holland continued a policy of encouraging and regulating voluntary beneficial societies; but Sweden provided in 1891 for subsidizing such organizations as well. Denmark followed suit in 1892, Belgium in 1894, and Switzerland in 1912. Italy began subsidizing voluntary societies in 1886, and established a well-organized old-age and invalidity fund two years before the end of the century.[1]

In one country such examples were carried even farther towards the complete socialization of medical service. Both medical service and research suffered severely in Russia as a result of the First World War, the ensuing revolution, and the emigration or "liquidation" of middle-class intellectuals. Some laboratories and schools managed, nevertheless, to continue in operation throughout this difficult period. With the organization of the Soviet Government, a People's Commissariat of Public Health was established in 1918. This was linked up during the next five years with research institutes in the chief cities, and with a system of State-employed physicians who supplied free service to the masses of the people. Hospitals, dispensaries, and similar institutions increased rapidly in number. Logically enough, there was little place for private practice in a communist State. Here was almost complete State medicine. Salaries were reported very low, and difficulty was naturally encountered in providing service over vast areas of sparsely populated country.[2]

In later years an improved training plan took form, and a programme of popular health control was inaugurated. The old distinction between public health work and individual medical service was eliminated—both forming part of the same scheme and being directed

[1] These earlier trends are summarized in F. A. Ogg: *The Economic Development of Modern Europe* (N.Y., 1917), pp. 623 ff. Later analyses are given in Armstrong, op. cit., pp. 312 ff.

[2] Garrison, op. cit., 799, gives them as from twenty dollars to sixty-five dollars per month.

by the same authorities. The systematic education of the masses in personal hygiene and preventive medicine was attempted through the use of various educational procedures, notably by coloured posters similar to those employed for other propaganda purposes. The educational programme included an effort, rarely made by other governments, to inform the people of the dangers of quackery and irregular service. After much progress during the 1930's in lowering the previously high morbidity and mortality rates, the Russian medical system made an impressive record in meeting the severe test imposed by World War II. Despite appalling conditions, casualties were handled with efficiency, and epidemics among the civilian population were held to a minimum.[1]

There was naturally some criticism of the German system by observers in individualistic England. Yet the pressure of industrial circumstances found that country providing limited workmen's compensation against accidents as early as 1880; a protection greatly extended in 1906. The poorer classes, somewhat better educated than they had been early in the nineteenth century, and exercising almost complete manhood suffrage after 1884, became increasingly concerned about protection against the chief financial hazards. Sickness due to ordinary causes, or to accidents, or associated with old age, was their chief anxiety. Unable to pay regular medical fees, and either unable or unwilling to appeal to charity, they resorted to various schemes for insurance or contract practice. Some doctors ran "private medical clubs" providing service for "a penny a week". Such schemes competed with one another, forcing the payment of physicians down to a low level, and the whole situation became one of economic and professional confusion.

Liberal ministries recognized by 1900 that general relief of some sort must be extended to the "labouring classes". Or, if such recognition was not spontaneous, it was at least deemed expedient by way of concession to a growing labour party, which took on a distinctly socialistic character after the turn of the century. Lloyd George, associated with the Asquith ministry, envisaged a comprehensive programme, which included land reform, further industrial regulation, and a social insurance system.[2]

The workmen's compensation legislation of 1906 was a part of this programme. Two years later old-age pensions were provided. Finally, in 1911, a national insurance act was adopted by Parliament. The ori-

[1] Henry E. Sigerist: *Medicine and Health in the Soviet Union* (N.Y., 1947), especially pp. 286 ff.

[2] Edward P. Cheyney: *Modern English Reform* (Phila., 1931), pp. 138, 162, 212 ff.

'ginal bill provided for a system of compulsory health insurance rather similar to the German, in that the business was to be handled by the "friendly societies" and the latter were to set the professional fees. These benefit societies had long provided "contract" medical care for their voluntary members, and were in close touch with the ministry which guided the Act of 1911 through Parliament. Four weeks after this bill was introduced the British Medical Association appealed to Mr. Lloyd George to provide freedom of choice of practitioners and in other ways to divorce the medical profession from any dependence upon the lay societies. The association marshalled some 26,000 physicians who promised, in 1911, not to become "panel doctors" unless these modifications on the act were made.[1] Under this pressure the Ministry accepted the proposed changes, and so certain objections of the medical profession were met at the start.

In the system finally provided, the friendly societies were utilized where available, but were not permitted to dominate administration. The State, as well as employers and employees, contributed to the costs. Taxes as well as ordinary premiums were therefore involved. The actual business of receiving and disbursing was carried on to a considerable extent through the post offices. As in Germany, the original emphasis was placed on financial relief rather than upon medical care. Thus a cash payment was granted in obstetrical cases, but no provision was made to see that this was actually used for professional services. (It was just as likely, in some cases, to be spent on the husband's beer.) In most respects, the dependents of workers were not covered by the insurance.

Once the English system had been in operation for some years, phenomena similar to those already noted in Germany appeared. The proportion of those insured who reported sick, for example, increased continuously. In 1921, only fourteen out of every 100 insured claimed sickness benefits. Six years later, the annual average had risen to twenty-three.[2] Two interpretations of this were possible in England, as in Germany. It might indicate a desirable extension of medical service hitherto denied because of poverty. Or it might simply imply a wish to "get something back" after making payments.

One of the major difficulties experienced in England related to the confusion in Government medical services. The insurance system was not, at first, correlated with the national health office then located (1911)

[1] Ernest M. Little: *History of the British Medical Association, 1832–1932* (Lon., 1932), pp. 325 ff.; J. H. H. Williams: *A Century of Public Health in Britain*, pp. 49 ff.

[2] Rep't. by the Government Actuary, 1930, cited in *A.M.A. Bull.*, 1934, p. 40.

in the Local Government Board. More serious was the fact that there remained in England, as a survival from Elizabethan days, the national poor-law administration with its charity medical service. Despite the report of a special commission to the effect that this organization was inadequate and even undesirable, it was continued after the insurance act of 1911 went into operation.[1] The resulting confusion in medical services, and the fact that the insurance principle did not in itself lend support to the preventive programme of public health officials, rendered the English system unsatisfactory in many ways.[2] It was an outstanding accomplishment, therefore, when an act of 1919 achieved the long-sought co-ordination of various State medical agencies under a national ministry of health. The Local Government Act of 1929 completed this procedure, in so far as it provided for the incorporation of the Poor Law Board medical service in the national ministry.

The establishment of this ministry made possible an increasing emphasis upon medical care, as distinct from financial protection. It also furthered arrangements to protect physicians from what they considered the surviving objectionable features of the act of 1911. It was provided, for example, that all regular physicians were eligible for insurance practice; which meant that the societies could not discriminate between practitioners. Moreover, after 1919, payment was received by the latter directly from the insurance funds of the Ministry of Health, and the amount was fixed by law rather than by the societies. Compensation was provided in proportion to the number of insured persons on the doctor's list, rather than in terms of the number of calls; and during the twenties this amounted to about 9*s*. per year per person. This was reduced 10 per cent in 1931, and at the beginning of 1934, amounted in par exchange to about $2 per year. No "panel doctor" could have more than 2,500 persons on his list; and the average panel income after 1930 was about $2,000.[3] As the British system did not include dependents, service to whom must be paid for in the ordinary manner, professional income was often increased considerably from this source and from ordinary private practice.

Bureaucratic tendencies in insurance organizations were further inhibited, in the British plan, by limiting panel service to ordinary illnesses. With certain exceptions, the system provided only for office

[1] Report of the Royal Commission on Poor Laws and Relief of Distress, Part VI, *Distress Due to Unemployment* (Lon., 1909), pp. 303 ff. See also Beatrice Webb: *The State and the Doctor* (Lon., 1910), p. 84.

[2] A critical analysis is given in Sir Arthur Newsholme: *Public Health and Insurance* (Baltimore, 1920), pp. 66 ff. See also the same author's *Medicine and the State* (Lon. and Baltimore, 1932), pp. 108 ff.

[3] *Brit. Med. Jour.*, Jan. 14, 1933, p. 1477.

calls, and not for long institutional care. While this seriously limited the service afforded to individuals, it at least prevented insurance societies from building up great institutions; and the mechanization of professional relations apparently did not go so far as in the Fatherland. It would seem legitimate, therefore, to view the British system as a compromise between the German plan and the old order of unorganized private practice. The need for medical relief so insisted upon by welfare workers was recognized. At the same time, those features of this relief which had most directly threatened professional interests in Germany—and perhaps indirectly the interests of patients as well—were to a considerable extent eliminated from the British system.[1]

This may account for the fact that, despite the various difficulties noted, both labour and professional groups in England tended after 1920 to approve the health insurance system. Labour leaders sometimes condemned the cash benefits, the confusion of administration, and the limitations in types of service; but they held that a return to unorganized private practice among the poor was unthinkable. They were emphatic in stating that health insurance had been of great service to the public health and to preventive medicine in general. Despite the long history of the public health movement in England there had been no adequate realization of the extent of disease and of the opportunities to prevent it, until this was revealed by the growing number of applications for panel medical service.[2]

There was at first some strong opposition within the medical profession. Panel doctors were for a while in some disfavour with their colleagues. In later years, however, both the panel physicians and the British Medical Association were able to exert what seemed to them a considerable influence upon the operation of the insurance system. On one occasion, in 1923, the former group threatened to abandon practice *en masse*, when the Ministry of Health announced a reduction in medical payments from 9*s*. 6*d*. to 8*s*. on the grounds of economy. This threat was effective in making possible a compromise, and in demonstrating that practitioners could not be ignored by the State machinery. Thereafter the Ministry made no changes in medical service without consulting the British Medical Association, and that body apparently accepted the system as a necessary one.[3]

[1] Americans will find a convenient statement of the British law during the thirties in the supplement to the *Jour. Mich. State Med. Soc.*, XXXIII, No. 5 (May 1934), pp. 18 ff.

[2] See, e.g., the testimony of Mary Macarthur and Margaret Bondfield, as reported in the *Labor Legislation Rev.* (N.Y., June 1919).

[3] Little, op. cit., p. 332. See also the testimony in the supplement to the *Jour. Mich. State Med. Soc.*, XXXIII, No. 5 (May 1934), pp. 13 ff.

After the First World War some sort of health insurance was established in nearly all European countries and in many non-European nations. Both the nature of the systems and their results naturally varied widely, although such trends as that from voluntary to compulsory protection, and from cash benefits to direct medical service, characterized nearly all.[1] Many physicians continued to oppose the insurance programmes in those cases where services were put in the control of insurance societies. This opposition succeeded in keeping the medical profession more or less independent of lay societies, after the British manner, in the Scandinavian countries and in the system adopted by France in 1930.[2]

As the years passed, various labour organizations, the socialist parties, and other groups interested in the poorer classes expressed satisfaction with the principle of health insurance and worked for the development of the several national systems. Some co-ordination of this support was achieved after 1920, through the organization of an international conference of sickness insurance societies. The same end was also achieved in an official and presumably disinterested manner by the International Labour Conferences and the International Labour Office connected with the League of Nations at Geneva. After submitting questionnaires to the European governments on their respective experiences, the Labour Conference adopted (1927) a "draft convention" recommending to all member nations a system of health insurance. This, the Conference advised, should be based upon self-governing societies under the administrative and financial supervision of "the competent public authority", and should not be carried on for profit. In case no such societies existed, the governments were urged to provide State systems.[3]

As the delegates to the Labour Conferences represented labour organizations, employers, and governments—but not the medical societies—these recommendations were interpreted by some medical groups as dangerous to the legitimate interests of the profession. The International Medical Association—an unofficial body—made the study of all trends affecting the social aspects of medicine a matter of

[1] See the Tenth Session, *Intern't. Labour Conf. Rep't.* (Geneva, 1927).

[2] On social medicine in France, see René Sand, *L'Economie humaine par la médicine sociale* (Paris, 1934). A chart, summarizing the systems in all nations as of 1931 (with some additional data for later years), was published in the *Revue Intern. de Med. Prof. et Soc.* (Aug. 1931), and was reprinted in *A.M.A. Bull.* 1934, pp. 8 ff., and in Morris Fishbein's article in *Hygeia*, XII (1934), p. 1071.

[3] See *Draft Conventions and Recommendations, Internat. Labour Office* (Geneva, 1930), art. 6, p. 136.

primary concern, and submitted suggestions of their own on health insurance. The serious charge was made, by one medical critic, that the Labour Office had become "a propagandist not simply for insurance but for the particular kind of insurance desired by the carriers and which gave the carriers control of the medical service".[1]

On the other hand, it was the view of those associated with the Labour Office that the Conference had recommended the utilization of the carriers simply because these were already available and were experienced in the field. There seemed to be nothing in the "draft convention" of 1927 which precluded such a control of societies as was already provided, for example, in the British system.[2] It was also pointed out that the Labour Office's "expert committee" on social insurance included ten physicians, and that the International Medical Association was represented at the 1934 meetings of this committee.

In any case, the recommendations of the International Labour Conference emphasized the general lay approval of health insurance among European peoples. Professional criticism of some of the systems continues. But it would seem to be a question whether there is any serious desire, in medical circles, to abolish the insurance principle now so generally adopted throughout the Continent.

[1] *Revue Intern. de Med. Prof. et Soc.* (Nov. 1932), pp. 109 ff., cited in *A.M.A. Bull.*, 1934, p. 49.

[2] Memorandum from the International Labour Office, July 1935.

XIX

AMERICAN EXPERIENCE

IN THE United States, with its vast potential resources and its relatively high living standards, poverty long seemed a less pressing problem than it did in the Old World. Hence a consciousness of the need for medical relief was slow in developing in this country. The individualistic economic order seemed a great success as "big business" grew rapidly bigger after 1900.[1] Even the plantation States envisaged a "New South", which would arise as soon as industrialism could be firmly established. It was quite natural that faith in a *laissez-faire* philosophy should prove more tenacious in the United States than in Europe.

Yet questions were raised despite the general optimism. Utopian reformers had long been critical of what they considered an acquisitive society. Towards the end of the nineteenth century more conservative critics began to condemn private exploitation of the national wealth. It became apparent that American natural resources could be, and indeed were being, exhausted by unrestricted competition and greed. Attention was first called to the situation by scientific men, notably by geologists and foresters, and in 1873 the American Association for the Advancement of Science began an agitation for governmental conservation. In 1891 a forestry bureau was established in the federal department of agriculture, and a national forest reserve was provided.

When Theodore Roosevelt became president, he displayed a marked interest in the conservation programme. He possessed both a scientific interest in the geography of American resources, and a desire that they should be developed in the interest of the nation as a whole. Working in co-operation with Gifford Pinchot and other leaders, the President appointed a National Conservation Commission in 1908. He also called a famous White House Conference on the subject. Here scientific men met on an equal footing with politicians in the discussion of great public questions; and an impetus was given to a movement destined to involve the conservation of human health as well as that of animal and vegetable resources.[2]

[1] Harold U. Faulkner: *The Quest for Social Justice* (N.Y., 1931), Chap. II.

[2] Charles R. Van Hise: *The Conservation of Natural Resources in the United States* (N.Y., 1916), pp. 6, 364 ff.

The analogy between preserving forests or fur-bearing animals and the guarding of invaluable human lives soon became too obvious to be overlooked. Was it not as important to save the lives of American children as it was to protect Alaskan seals? What really happened here—as it had in Europe—was that the old ideal of "sanitary reform" was reinforced by the ideal of conserving national wealth. Upon the Victorian motive of humanitarianism was superimposed the modern motive of national interest.[1]

As in Europe, the triumphs of sanitation and the more recent achievements of bacteriology and preventive medicine furnished the conservationists with their chief appeal. Declining death rates showed that mortality was not a constant, and that still further vital gains could be made in this direction. Such convictions led, a year after the meeting of the White House Conference, to the creation of a Committee of One Hundred on National Health. This group conducted inquiries and submitted reports to the National Conservation Commission on all phases of the preservation of human health as they were then understood. Most significant was a valuable *Report on National Vitality: Its Wastes and Conservation*, submitted for the Committee of One Hundred by Professor Irving Fisher of Yale University. This study surveyed the evidence of improvement in public health, and emphasized the opportunity for further progress.

Fisher included in his report a section on the role of the medical profession in "conservation through semi-public hygiene", but he was concerned here largely with technical advances in medical science. He still felt it necessary, in 1909, to assure the public that bacteriology had made great gains and that preventive medicine was coming to the fore.[2] He evidently wished to overcome the old public suspicions about medical science, and was not fully conscious of the newer scepticism soon to come—the scepticism as to the social effectiveness of medical service. Some intimation of the latter point of view, however, is to be found in the recommendations made at the conclusion of his study. Here, amid varied suggestions as to extending the public health programme, Fisher declared it to be "both bad policy and bad economy" for the State to leave protection against disease either "to the weak and spasmodic efforts of charity, or to the philanthropy of physicians".[3]

[1] See the general discussions of the latter theme in Charles A. Beard: *The Idea of National Interest* (N.Y., 1934), pp. 1, 84, 101 ff.; and in his *Open Door at Home* (N.Y., 1934), pp. 217 ff.
[2] Irving Fisher: *Report on National Vitality* (Wash., 1909), pp. 65 ff.
[3] Ibid., p. 126.

The indifference of Congress prevented the development of any immediate federal policy based on these reports, but they may have served to arouse popular interest in the problems of public health control. There was some development of "lodge practice" and of "ten-cent" organizations suggestive of similar schemes in Britain. Meanwhile the exigencies of industrial expansion necessitated, as in the latter country, certain preliminary steps in the direction of social insurance. Workmen's compensation laws were passed by some twenty States prior to 1915, and there was important federal legislation on the subject in 1908. With several million accidents occurring annually, these laws assumed some economic significance for the medical profession.[1]

When the United States entered the First World War, the national efficiency of which Theodore Roosevelt had said so much was subjected to a severe test. This proved enlightening in many ways. Among other things, the physical examinations of recruits for the national army revealed a startling amount of physical inefficiency. Some of this obviously could have been, but had not been, corrected by proper medical attention. Similar evidence had by this time accumulated as a result of physical examinations made in the public schools, or through examinations conducted by certain insurance companies of their policy-holders.[2] The conviction slowly grew that something must be done about the situation.

Just what might be done was suggested, not only by the workmen's compensation laws already adopted, but also by the examples set in several European countries. The relatively recent British insurance act had attracted wide attention in the United States, and various welfare organizations were convinced that similar legislation should be provided in there. Sentiment favourable to this view grew rapidly during the early years of the war, perhaps because the temper of the time lent itself to idealistic appeals. Although the American Federation of Labour declined at this time to approve health insurance, the State federations tended to support it. The medical profession, on the other hand, was cautious about the sudden demand for such a sweeping alteration in the whole basis of its public relations. The American

[1] I. M. Rubinow: "Social Insurance and the Medical Profession", *Jour. Amer. Med. Asso.*, LXIV (1915), 381; and the same author's *Social Insurance with Special Reference to American Conditions* (N.Y., 1913), pp. 283 ff.

[2] Alden B. Mills: *The Extent of Illness and of Physical and Mental Defects in the United States* (Abstract of publication No. 2 by the Com't. on the Costs of Med. Care, Wash., 1929). The interest of insurance companies in proper medical service was analysed by Irving Fisher: *Economic Aspect of Lengthening Human Life, passim* (Address before the Asso. of Life Insur. Presidents, N.Y., 1909).

Medical Association manifested some interest in the general problem, but refused to endorse compulsory insurance. This position was maintained on several occasions between 1916 and 1920; and in the latter year the Association formally resolved that it should oppose compulsory insurance against illness "provided, controlled, or regulated by any State or the Federal Government".[1]

Leadership in the effort to secure immediate provision for health insurance was assumed by the American Association for Labour Legislation. This body appealed both to public opinion in general and to State legislatures in particular. Pamphlets were issued which featured such startling announcements as the following:

> Of the young men in the country examined in the draft, one-third were rejected on physical grounds.
>
> Wage-earners of the country lose 250,000,000 working days a year because of sickness, making, at $2.00 a day, a wage loss of at least $500,000,000.
>
> More than 15,000 American women die every year from causes connected with childbirth, and 250,000 babies die in the first year of their lives.
>
> Sickness surveys in many cities show that about one-third of those too sick to work are without medical care.
>
> Sickness is the largest single factor in dependency.[2]

The Association for Labour Legislation drew up a model act for a State system, which it sought to bring to the attention of State legislatures. Official studies of health insurance were made in eight States between 1915 and 1918. As usual, the great size of the country and the federal system of government made it difficult to secure prompt action one way or the other. Special commissions appointed in Massachusetts, California, and four other States approved the principle of health insurance, but the legislatures failed to act. A bill based on that suggested by the Association finally passed in the New York Senate in 1919, but was defeated in the Assembly.[3]

It is interesting to observe that the Association's bill followed in some respects the British system, but departed from it in important particulars. There was provision for compulsory insurance of low-income

[1] A.M.A. *Index and Digest of Official Actions*, p. 53.
[2] *Amer. Labour Leg. Rev.*, IX, 276–80 (1919).
[3] See John B. Andrews: "Health Promotion through Legislation for Health Insurance" (reprint from the *Proceeds.* of the Conference of State and Provincial Health Authorities, N.Y., 1919), p. 3: Barbara N. Armstrong: *Insuring the Essentials* (N.Y., 1932), pp. 370 ff.

groups, for joint contributions and control by employers and employees, and for the free choice of physicians. There was much more emphasis in the New York bill, however, upon the local control of administration. There was also more definite provision for the possible interests of the medical profession. A physician was to be a member of the State commission of three intended to supervise the whole system, and the State medical societies were to choose a medical advisory board which had to be consulted by the commission on all medical disputes. Insurance companies operating for profit were excluded from acting as carriers, and minimum fees for medical service were to be determined by the county medical societies in co-operation with the medical member of the State commission.[1]

In these and other ways a definite effort was made to meet the more serious objections of the medical profession to the German and the British systems. Indeed, some of the provisions followed specific recommendations made by the British Medical Association. The New York bill also went farther than British law in providing for institutional care, and even for funeral expenses. It was highly praised by such British labour leaders as Margaret Bondfield as superior to the legislation in her own country.

On the other hand, the American system seemed to threaten the interests of large insurance companies. Its proponents claimed that the opposition to social health insurance came largely from such companies and from employers' associations.[2] Whatever the source, various charges were made against the whole movement, chiefly on the grounds that it would involve great public expense, and that it was essentially a product of that German *Kultur* with which the United States had so recently been at war. This, in 1919, proved a potent indictment!

The failure of the New York bill came at an unfortunate time for the advocates of health insurance. For in addition to the normal inertia of the public in matters of social change, the years which followed were marked by the conservatism of the Harding and Coolidge administrations. Dominant business interests were opposed to social insurance in general. The issue could no longer be pressed vigorously in State legislatures or other Government bodies. In consequence, the movement continued largely in the form of private investigations and experiments, pending the day when it might again be brought up for political action.

During the twenties and early thirties considerable interest was mani-

[1] *Health Insurance: Standards and Tentative Draft of an Act*, N.Y. (1916); J. B. Andrews, op. cit., pp. 4 ff.

[2] J. B. Andrews, op. cit., p. 10; Armstrong, op. cit., p. 371.

fested in health insurance by the large private foundations, notably by the Milbank Memorial Fund. Most special studies or reports made between 1918 and 1928 reported that the basic cause of inadequate medical service was simply the poverty of large numbers of the population. Social investigators recommended, as had Fisher two decades before, that medical care of the poor be no longer left to the philanthropy of physicians.

Here and there experiments were made in providing organized medical service with a view to securing economy of operation, and also in order to insure proper care of a given group. Universities provided student health services during the post-war period, based on health insurance financed by special fees. Industrial concerns established a medical service for their employees, on a basis of premiums paid from wages; and such systems were occasionally extended to the families of workers, and even to whole communities. A number of interesting experiments of this sort were conducted in Southern mill towns, where such a programme seemed a natural product of the paternalism obtaining in employer–employee relationships.[1] In a few instances foundations or charitable organizations provided small communities with various forms of organized medical service along similar lines.

The claims made for and against these local insurance schemes were much the same as those made in connection with Government systems in Europe. On the one hand, it was said that the quality of medical service was poor, that the personal relationship was lost, and that malingering or a desire to "get something back" was encouraged. While many physicians thus "viewed with alarm" these experiments in health insurance, other observers "pointed with pride" to their accomplishments. In student health service, for instance, it was claimed that much serious illness was prevented by being caught in time, since students were said to report promptly when no fee was involved.[2]

Meanwhile, another type of organized medical service not relating primarily to medical relief was being developed by physicians themselves. This was the "private group clinic", which arose chiefly in the Middle West after 1918. Perhaps the plan was suggested to physicians

[1] *Surveys of Organized Medical Service*, pp. 5, 23, 39 ff. (Com't. on the Costs of Med. Care, abstract of publications Nos. 17–21, Wash., 1932).

[2] Ibid., p. 36, in my own opinion, it is very difficult to generalize here, as in so many other phases of the health insurance problem. I have observed student health service informally in a number of universities, and the quality of service seemed to vary widely. On the general danger to quality of medical service in some forms of health insurance, see the *Final Report of the Commission on Medical Education* (A. Lawrence Lowell, Ch., N.Y., 1932), pp. 380 ff.

by their experiences in the Government service during the war.[1] Such clinics were usually composed of not more than ten physicians and dentists who used common equipment, were jointly responsible for patients, and pooled their income. Financial affairs were commonly handled by a business manager. The group's relations to hospitals and free service were usually those of ordinary private practitioners. The majority of the clinic patients seemed to be people of moderate means, and the fees were "about the same" as those charged by other local practitioners. Clinic managers claimed that they reduced medical costs by improving the quality of service without increasing fees, or by saving the patient's time. The former were convinced, by 1930, that their business was steadily increasing. There were then about 150 such clinics in the country, with a personnel of 2,000 physicians.[2]

Although this type of organization did not provide free service to any extent, it presented some of the same problems already encountered in insurance service. Like the latter, it was said to involve a mechanized relationship between doctor and patient. The private clinic was resented by some individual practitioners, since it tended to cut into their income.[3] On the other hand, it seemed to represent a more efficient form of practice, both in regard to technical efficiency and to business management. The American Medical Association therefore deemed it unwise to take a uniform position either for or against group practice of this type. Local medical societies were advised to determine their attitude towards group and contract practice in terms of such principles as fair charges and free choice.[4]

The appearance of the private clinic suggested a possible com-

[1] Frank Billings: *The Trend of the Practice of Modern Medicine* (Wash., 1922), pp. 5 ff.

[2] Rufus Rorem: *Private Group Clinics* (Abstract of publication No. 8, the Com't. on the Costs of Med. Care, Wash., 1931), p. 3 ff.

[3] The possible effects of socialized medicine upon professional income was naturally a matter of great concern to American physicians, as it had been to their European colleagues. The A.M.A. created a Bureau of Medical Economics to study all phases of this and other financial problems; and a journal appeared, devoted entirely to the business interests of the profession. The A.M.A. Bureau reported, in 1931, that the lowest average gross income among physicians was received in public health work, while the highest was received in surgery. Thus the average gross income for the public health physicians was reported as about $5,000, while that of surgeons was about $13,000. Salaried men in general received less than those depending primarily on fees, although insurance practice averaged distinctly better than public health work or teaching. Both the average and the median income of salaried men lay in the interval between $4,500 and $5,500. About 25 per cent of the profession reported income less than $3,500. It looked as if most of this latter group would profit, at least for a time, by the assured income of a health insurance system; while the majority of the profession would find no gain, and possibly a loss, in receiving their income primarily from such a source. See R. G. Leland: 'Income from Medical Practice", *Jour. A.M.A.*, XCVI (1931), 1683 ff.

[4] Note action of the Judicial Council of the Association, 1927; *Index and Digest of Official Actions*, pp. 106 ff.

promise between the extremes of individual practice and State medicine. Could not such a clinic serve the poor as well as the propertied classes, on a basis of health insurance? Or could not the patients already resorting to these clinics protect themselves from sudden costs, and their physicians from bad debts, by paying in a like manner? This would retain the control of medical organization in the hands of private practitioners. A further advantage could be gained, it was suggested, by combining all these features with a system of periodical examinations for the clientele of each clinic. This would protect the health as well as the funds of patients, at the same time that it would not necessarily reduce the income of the practitioners.[1]

For a year or two before the economic crash in 1929 the problem of medical service was pushing to the front pages in American discussions of social welfare. The sudden descent of depression upon all classes made the question more pressing and more acute. By the time the Roosevelt administration came into office, in 1933, the whole problem of social security had become of paramount importance. Health insurance, as well as unemployment insurance and old-age pensions, was demanded by many as emergency legislation. Considerable space was given to the subject by the more important journals of opinion, from such general magazines as *Harpers* and the *Atlantic Monthly*, to the more specialized *Forum* and *Survey*. Medical editors and public health authorities contributed an increasing number of articles to professional periodicals.[2]

Meanwhile, another type of literature was becoming available. In 1926 some fifteen leaders in medicine, public health, and the social sciences had inaugurated a series of conferences which led to the creation of a national Committee on the Costs of Medical Care. This body consisted of fifty members drawn from various interested groups ranging from private practitioners to economists. Dr. Ray Lyman Wilbur,

[1] See especially Evans Clark: "A Cure for Doctors' Bills", *Atlantic Monthly* (Boston, Oct. 1930). The plan was elaborated and much other pertinent data given in the same author's *How to Budget Health* (N.Y., 1933), especially Chaps. VII–XI, incl.

[2] See the interesting list of articles of 1928 and 1929 in *The First Two Years' Work of the Committee on the Costs of Medical Care* (Wash., 1929), p. 21. In the *Am. Jour. of Pub. Health* note especially XXII, No. 9 (N.Y., Sept. 1932). The issues of the *Jour. of the A.M.A.* for 1932 contain much pertinent material; e.g., those for May 28 (p. 1895), June 11 (pp. 1060 ff.), July 9 (p. 156), Sept. 3 (p. 836), Sept. 10 (p. 918), Oct. 8 (p. 1264), Oct. 15 (p. 1356), Oct. 22 (p. 1431), Nov. 5 (p. 1609), etc. Increasing public concern was also reflected in the reports of relief organizations and special foundations; e.g., the Milbank Memorial Fund, *Quarterly Bull.*, X, No. 3 (N.Y., July 1932) contains articles by R. H. Britten and by Ray Lyman Wilbur on the economics of public health and medical care. A general study of the whole problem of medical economics appeared in Hugh Cabot: *The Doctor's Bill* (N.Y., 1935).

Secretary of the Interior in President Hoover's Cabinet, served as chairman. An intensive programme of investigation into the national situation was conducted during the next five years, under the direction of Dr. Harry Moore and with the financial support of a group of large foundations.[1] The United States Public Health Service co-operated; and the American Medical Association, the American Dental Association, and several other organizations conducted supplementary studies which were "of great value to the Committee's programme of study".

The committee carried out a nation-wide survey of sickness and medical service among nearly 9,000 white families. Their reports revealed, by 1932, a direct correlation between income and all types of medical service, and tended in general to substantiate the claims made by the advocates of health insurance more than a decade before. The lowest income group (under $1,200 per year) received more of certain types of care—presumably due to charity services—than did the next two higher groups ($1,200 to $3,000); but in general these classes all received much less service than those whose incomes were above the last-named amount. Thus the group with the lowest amount of service received only 50 per cent as many days of hospitalization, and only 41 per cent as many medical calls as did the group with the highest amount of service. In every case the latter was the highest income group.[2]

The highest group itself received less medical service than the standard which the majority of the committee considered essential to good care. Nearly one half the individuals in the lowest group received no professional medical or dental attention of any kind. Such complete neglect was reported most common in the poorer States of the South, in four of which the financial resources of the people as a whole appeared insufficient to meet the cost of adequate medical and dental service to their inhabitants. An illustration of what this meant in a specific instance was afforded by a sample of 500 Negroes in Tipton County, Tennessee, who were suffering from syphilis. When investigated, only 3 per cent of them had been given both neo-arsphenamine and mercury, and only 14 per cent had received any professional medical treatment whatever.

The committee's investigators thought that the inability of many to secure medical attention was due to general lack of income, but they also blamed the uncertain and uneven costs of illness. They concluded that about 90 per cent of the entire population could afford the cost of

[1] *Medical Care for the American People* (Chicago, 1932), pp. vi ff.
[2] Ibid., pp. 5 ff.

medical care, provided this was distributed over a considerable time by some form of insurance.[1]

Taking quite another view of the situation, the Bureau of Economics of the American Medical Association conducted an investigation which ". . . revealed the fact that there are few, if any, people in the United States really suffering from lack of medical care. The mayors of a hundred cities of various sizes testified to the fact that there was no neglect of the poor because of their inability to pay." [2] The difference between such findings and those cited above may possibly be explained in part by the denotation of the term "poor". The Committee on the Costs of Medical Care found that the very poor did indeed receive more hospital care, for example, than did the groups just above them in income.

It should be noted that even if the poor received an adequate number of calls from physicians, or sufficient days in hospital, their poverty might still prevent them from preserving their health. For the victims of poverty could not always afford to act on medical advice, even when they received it. Physicians knew how to eliminate hookworm disease in the South, and much good work in this respect was done by the Rockefeller Foundation, but the disease was not entirely eliminated. All that was needed in many cases was to wear shoes, but who was to provide the shoes? Or, again, physicians knew how to control diabetes with insulin. But who was to pay for the life-giving insulin, month after month, and year after year? Health could indeed be purchased, but some had not the price.[3] Southwood Smith had seen this just as clearly a century ago as health officers and conscientious clinicians do to-day. The situation was rooted deep in the social order, and seemed beyond the control of the most well-intentioned profession. Yet it was necessarily of the greatest concern to society.

The whole problem involved in the United States was one of great complexity. Besides the basic question of poverty, there were others of considerable significance. Despite the relatively large numbers of physicians there, there was the unequal distribution of their services over large rural areas.[4] In such regions it was difficult to find a

[1] *Medical Care for the American People*, pp. 7–10; Louis S. Reed: *The Ability to Pay for Medical Care* (Abstracts of Pub. No. 25, Com't. on the Costs of Med. Care, Wash., 1933).

[2] Morris Fishbein: "Sickness Insurance and Sickness Costs", *Hygeia*, XII (1934), 1074.

[3] This was even true of the urban poor who had access to large clinics. See Nathan Sinai and A. B. Mills: *A Survey of the Medical Facilities of the City of Philadelphia* (Abstracts of Pub. No. 9 of the Com't. on the Costs of Medical Care, Wash., 1929), p. 17.

[4] Garrison (*Hist. of Med.*, 4 ed., 1929, p. 763) comments on the "amazing overplus" of American physicians, noting an average of one to 691 of the population, as compared to one in 1,940 in Germany, one to 2,120 in Austria, and one to 2,834 in France.

doctor or a hospital, even if patients could afford to pay for them. A study by the Duke Endowment, made in 1927, showed that while there was one physician for every 772 persons in the United States as a whole, there was only one for every 1,244 in North Carolina, and only one for every 1,409 in South Carolina. In Berkeley County, in the latter State, the ratio was as low as one to 4,512.[1] The natural preference of physicians for urban locations was the obvious explanation of this situation. The same preference explained the similarly uneven distribution of nursing and of other forms of medical service.[2]

Various efforts were made to provide medical service in neglected rural areas. In the Canadian province of Saskatchewan, rural communities levied taxes of from $7 to $10 per family to engage the services of full-time physicians at salaries ranging from $3,000 to $5,000. Both the physicians and the communities were said to approve this form of State medicine.[3] In other cases State boards of health or private foundations established a "frontier nursing service", somewhat similar to that provided by the Highlands and Islands Medical Board and the Queen's Nurses in the British Isles, or by the Swedish Red Cross nursing service in Norrland. This last provided not only the public health nursing available in progressive communities, but also brought into isolated regions the first well-trained midwives and adequate hospital facilities. While the Swedish Government was able to serve its whole frontier area in this fashion, American efforts were limited to experiments in a limited number of localities.[4]

Another major factor in the American situation was the ignorance or gullibility of the public, which resulted in diverting an estimated $500,000,000 a year from legitimate medical channels into the coffers of quacks, sectarians, and the makers of secret medicines.[5] More than 70 per cent of the total bill for medicines was paid for self-medication; that is, for secret medicines and home remedies. Physicians were writing fewer prescriptions, and the consumption of ready-made drugs was increasing so rapidly as to encroach seriously upon the professional

[1] *Annual Rep't. of the Hosp't Section, The Duke Endowment, 1927* (Charlotte, N.C., 1928).

[2] *Nurses: Production, Education, Distribution, and Pay* (published by the Committee on the Grading of Nursing Schools, N.Y., 1930), pp. 20 ff.

[3] C. R. Rorem: *The "Municipal Doctor" System in Rural Saskatchewan* (Abstract of Pub. No. 11, Com't. on the Costs of Med. Care, Wash., 1931).

[4] Cf. Anne Winslow: *The Frontier Nursing Service* (Abstract of Pub. No. 10, Com't. on the Costs of Med. Care, Wash., 1932), and *Social Work and Legislation in Sweden: Survey Published by Order of the Swedish Government* (Stockholm, 1928), pp. 119 ff.

[5] The figure of $485,000,000 is given in *Medical Care for the American People*, p. 15. Other estimates vary.

work of pharmacists. The latter, as a result, devoted only part of their time to pharmacy, and the rest to a retail trade in various household goods.[1] Hence there appeared that curious American phenomenon, the "drug store", which seems to sell almost everything except drugs. The complaint about quack remedies was nothing new, to be sure; but each generation apparently rediscovered the extent of the evil for itself.

Some patients might conceivably have paid fees if they had not spent their money on self-medication. The funds so invested were more than half as large as those paid to physicians.[2] Here the medical profession, which was by this time being criticized by social investigators for its opposition to health insurance, felt that it had a real grievance against the leaders of public opinion—past and present. For a century or more the medical profession had protested against the public patronage of all forms of irregular practice, only to be viewed as an interested party hawking its own wares. Now social observers recognized, in the resulting situation, one of the causes of inadequate medical service.

It became apparent, while the studies of the Committee on Medical Costs were under way, that physicians also objected to the implied criticisms of the profession included in various lay studies. They were naturally aggrieved by the indictment of medical service, just as their older colleagues had once resented attacks on medical science. Were lay critics familiar with the history of the profession? As a matter of fact, was there any profession which had behind it such a record for self-sacrifice in the public interest, and which therefore deserved as much consideration where matters of public welfare were concerned?

As a distinguished American surgeon put it:

> Why he [the doctor] should refrain from forcible collection of his unpaid bills; why he does not patent some of his prescriptions, inventions and discoveries and make a fortune; why he should continue to counteract the spread of diseases he has painfully and at great educational expense learned how to diagnose and treat; why he should so strenuously oppose year after year the efforts of anti-vivisectionists and anti-vaccinationists with their Christian Science allies to cripple research and to annul statutes already on the books, knowing how calls would increase did they have their way; and why at the

[1] C. R. Rorem and R. P. Fischelis: *The Costs of Medicines* (Abstract of Pub. No. 14, Com't. on the Costs of Med. Care, Wash., 1932).

[2] In Philadelphia, for example, about $104,000,000 was spent in 1928 for all forms of medical service—nearly $54 per capita. Of this total, physicians received 26 per cent, and drugs and medicines cost 20 per cent. Nathan Sinai and Alden B. Mills: *A Survey of the Medical Facilities of the City of Philadelphia: 1929* (Abstract of Pub. No. 9, The Com't. on the Costs of Med. Care, Wash., 1931).

same time he should continue to work longer hours for less pay during a shorter life of activity than most people is an enigma to a hard-headed business man.[1]

The traditional answer was that, in return for such service and sacrifice, practitioners achieved prestige, received at least a fair income, and enjoyed a high degree of individual freedom. All these values, many physicians felt, were threatened by the plans for insurance company or Government practice.

For their part, there were social observers who believed that physicians were either ignorant of or indifferent to public needs. It was pointed out that individual doctors and some medical societies objected even to the work of State health departments.[2] A lack of adequate training in preventive and social medicine was pointed to as a cause of professional indifference to the social implications of practice.

It is interesting that those representing each side in the discussion expressed alarm, lest the other had succumbed to the commercialism of the age. Lay observers were convinced that many physicians were primarily interested in financial success, while the latter suspected their critics of wishing to regiment medical practice along industrial lines.[3]

After publishing a series of preliminary studies, the Committee on the Costs of Medical Care submitted its final report in 1932. This took the form of a majority and a minority report, besides a number of individual statements.[4] The majority recommended a general plan for medical service which represented a compromise between individual practice and State control. Besides suggesting the extension of public health work, and urging certain improvements in the training of medical personnel, they recommended that medical service should be furnished primarily by groups of physicians, dentists, and nurses organized

[1] Harvey Cushing: "Medicine at the Crossroads", *Jour. of the Amer. Med. Assoc.*, C (1933), 1567 ff.

[2] C.-E. A. Winslow cites the recorded opposition (1927) of ninety-seven out of 142 county medical societies in eight States, against existing clinic and hospital activities of medical schools. Again, forty-two out of fifty-four societies reporting were opposed to the programme of their own State boards of health. Reports were collected by the Minnesota State Medical Association; *The Road to Health*, pp. 126, 127. The views of social welfare advocates who desire an immediate inauguration of some form of socialized practice are presented in Abraham Epstein: *Insecurity: A Challenge to America*, 3 ed. (N.Y., 1936), pp. 399 ff.; and in the same author's *The Case for Health Insurance* (N.Y., 1935).

[3] Cf. the statements of Dr. J. Bentley Squier to the American College of Surgeons, and that of John A. Kingsbury of the Milbank Fund (*re* the Com't. on the Costs of Medical Care) in the *N.Y. Times*, Oct. 10, 1933.

[4] *Medical Care for the American People: The Final Report of the Committee on the Costs of Medical Care* (Chicago, 1932). Seventeen physicians and all the laymen on the Committee, except three, made up the majority. Eight of the physicians signing represented private practice.

around hospital centres. Medical costs, they held, should be met through the use of voluntary, group insurance or taxation, or both.[1]

A number of medical members of the committee dissented from this report, and in a minority statement declared that such group practice would set up medical hierarchies, destroy personal relationships, and not necessarily lower costs. They objected to group payment and voluntary insurance on the ground, first, that insurance led to such abuses as competition in rates, the interference of lay managers in the conditions of practice, and the provision of impersonal and inferior service. Second, it was claimed that voluntary insurance always led sooner or later to compulsory insurance, and that as this system was still on trial in Europe, it was unwise to rush into it in the United States.[2]

The American medical profession seemed, in general, to take the same view of the situation as that outlined in the minority report. The majority report was condemned in some medical societies; and in certain cases an appeal was made to all members to oppose it. Several departments of the Association, however, continued to study the whole problem. So many new plans for medical service were appearing throughout the country and so much discord was developing within the medical profession with regard to them, that in 1934 the Association's House of Delegates adopted ten fundamental principles to guide the profession in all such matters. Their nature has already been suggested in the discussion of the general attitude of the profession.

At the same time, the Association's Bureau of Medical Economics reported that local medical societies throughout the country were attempting to meet social needs by organizing "a more equitable distribution" of services. More than 200 such experiments were reported under way, which included a wide variety of arrangements—such as reduced rates, pre-payment for hospital care, and so on. The bureau felt that the local societies were now striving earnestly to improve conditions, and that this indicated that the medical profession recognized its responsibility and had accepted leadership in providing medical service.[3]

Special action was taken by certain professional societies. The American College of Dentists made a study of health insurance, and approved it in principle. In 1931 the Michigan State Medical Society undertook a study of all aspects of "medical economics" in that State, and secured the social co-operation of several of the science depart-

[1] *Medical Care for the American People*, p. xvi.

[2] Ibid., pp. 153 ff. Eight physicians and one layman signed the minority report.

[3] *Special Rep't. of the Bur. of Med. Econ.*, A.M.A., 1934; *Official Statement to the House of Delegates by the Bd. of Trustees*, A.M.A., Feb. 15, 1935.

ments and the Department of Public Health of the University of Michigan.[1] This co-operation of medical, public health, and social science groups was unusual and of considerable interest in itself. The subsequent report made to the Medical Society was approved (including a recommendation to subsidize practitioners in thinly populated areas), except for the recommendation of a health insurance system. Desiring more information on this, the Medical Society joined with the American College of Dentists to send Dr. Henry A. Luce and Dr. Nathan Sinai abroad, in order to study [illegible] experience. On the basis of their findings a plan was drawn up for a State Mutual Health Service, and this was formally adopted by the society in 1934.[2] Of the seventy physicians who voted on the issue, only nine were opposed to the plan.

Efforts were made, in the Michigan plan, to guard against the chief dangers that the medical profession felt were inherent in health insurance. It was emphasized that the State rather than the local medical society was in the best position to deal with the situation. Commercial companies were excluded, cash benefits were prohibited, and both the central and local control was placed largely in the hands of the medical and allied professions. Insurance was not to be compulsory for the lesser income groups, although some effort was to be made to extend it to them on an experimental basis. The funds were to be collected from employers and employees rather than by taxation. The system was not immediately established, however, in view of the possibility that Congress would provide health insurance on a national scale.

To return to the majority report of the Committee on the Costs of Medical Care, it may be observed that this received considerable attention in the Press; and it was praised in some instances by physicians of note. One medical editor pointed out that the majority programme was by no means so revolutionary as were the State systems in Europe, and suggested that its adoption might actually serve to avoid such extremes. The majority report, he observed, would continue the control of socialized practice in the hands of professional groups, rather than in those of the State or of insurance societies. Taking the same view, Dr. Wilbur declared that: "The doctor must recognize that whether he likes it or not, something is going to be done. It is better to have it done by him than to him." [3]

[1] Nathan Sinai: "Medical and Dental Economics", *Jour. of the Amer. Col. of Dentists*, I, No. 4 (Oct. 1934), 110 ff.

[2] Supplement to the *Jour. Mich. State Med. Soc.*, XXXIII, No. 5 (May 1934). See also the "Report of Com't. on Med. Ec.", ibid., Nov. 1934.

[3] *New Eng. Jour. of Med.*, Dec. 22, 1932 (reprint).

Many physicians in the United States were inclined, by 1935, to view such warnings seriously. Local medical societies appeared in New York, Philadelphia, and other cities, devoting themselves entirely to medical economics.[1] One of these societies, the Medical League for Socialized Medicine of New York City, strongly advocated compulsory health insurance under professional control. More conservative in attitude, a number of State medical societies—among which that of Michigan was a pioneer—finally set up medical prepayment plans under their own control. By 1943 some twenty-four of these bodies had made such arrangements, covering about 1,000,000 subscribers.

Most society plans limit protection to hospitalization and surgery, but those of Washington and Oregon give almost complete medical coverage. All these plans employ individual practitioners and pay fees for service. Hence, unlike the industrial plans to be noted below, they provide no means for improving the prevailing quality of medical care. Medical societies have opposed compulsory health insurance under State as well as under Federal auspices, and to date (1947) no State has adopted such a programme.

The failure of the States to follow certain Canadian provinces, in experiments with health insurance, was not simply the result of professional opposition. Advocates of compulsory insurance were themselves more interested in the possibilities of a Federal system. President Roosevelt and Congress, during the New Deal regime, favoured social security legislation, and health insurance was widely viewed as a logical part of this programme. In 1934 the American Federation of Labour approved such insurance, and other liberal and radical organizations rallied to its support during the years that followed. The unofficial United States Chamber of Commerce, however, declined in 1934 to approve health insurance in principle; thus indicating that conservative forces would align themselves with the professional opposition.

Meanwhile, the interest of welfare and philanthropic bodies in health problems continued to mount, and this tended to increase non-political concern with the insurance question. By 1935 there were more than 100 national agencies dealing directly with health problems, and many of the latter impinged in one way or another upon social medicine.[2] Thus, many influences converged to make health insurance a national issue during the decade that followed 1935. The literature on the subject be-

[1] See, e.g., the statement for the "Physicians' Equity Association of New York City", prepared by its secretary, Dr. Seymour Fiske, in the *N.Y. Times*, May 13, 1935.

[2] *Social Work Year Book of the Russell Sage Foundation* (N.Y., 1935).

came so large that librarians displayed concern lest it be lost or not properly organized.[1] On the other hand, further research was called for in all the social and economic aspects of medical service.[2]

During the greater part of the depression period of the thirties a sort of State medicine was provided under the emergency Federal relief system. Fixed payments to physicians were made from national funds for all patients who were on the Federal relief rolls. But this was usually viewed as a temporary arrangement, and as one that in any case was not sufficiently comprehensive to cover the needs of large parts of the population.

In 1934 President Roosevelt announced a programme of social security legislation. He appointed a Committee on Economic Security, composed of Cabinet members and a technical staff, to prepare the programme. This body had the services of a Medical Advisory Board, on whose staff were representatives of the American Medical Association. No recommendations were made by the committee regarding social medicine, although the principles of health insurance desired by Cabinet members were communicated to the President. The security legislation submitted in 1935 and enacted the next year included old age pensions and unemployment insurance but no provision for insurance against illness. The President declared that he was not "at this time" recommending such a plan, but said that groups representing the medical profession were co-operating with the Government and that "definite progress" was being made.[3]

It was commonly believed that leaders in the medical world persuaded the President to give up any contemplated health plan, and public attention began to focus on the attitude of the profession. In 1937 the American Foundation issued a report on medical opinion throughout the country. This revealed that many individual physicians were concerned about the crisis in medical economics and were favourable to some degree of so-called socialization.[4] During the same year a group known as the Committee of Physicians, numbering over 400, advocated a national health programme. This committee drew its leadership from the medical schools and teaching hospitals, while the views of the majority of the profession seem to have been represented

[1] A. F. Kuhlman: "Neglected Categories of Research Materials Bearing upon Social Aspects of Medicine", *Social Forces*, XIV (1936), pp. 373 ff.

[2] Michael M. Davis: *Milbank Mem. Fund Quart.*, XIII (N.Y., 1935), pp. 339 ff.

[3] *Official Statement to the House of Delegates*, by the Bd. of Trustees, Amer. Med. Asso., Feb. 15, 1935.

[4] *American Medicine: Expert Testimony Out of Court*, I (N.Y., 1937), pp. 19 ff. See also *N.Y. Times*, April 4, 1937.

by the conservative *Journal* of the American Medical Association.[1] The latter opposed the Committee of Physicians in no uncertain terms.

Tension within the ranks of the profession increased as efforts were made by local or State medical societies to discourage group-practice units from using prepayment plans. The economic depression, combined with rising medical costs, had led to a proliferation of all sorts of voluntary health insurance plans after 1935. These exhibited a wide range of forms in relation to sponsorship, financial arrangements with subscribers and medical staffs, medical coverage, personnel coverage, and so on. Some were commercial, others non-commercial in nature; the first usually paid cash benefits, the second often provided medical care. This care ranged from the custodial (hospitalization) to relatively complete service.

Most successful of the voluntary plans was the Associated Hospital Service (Blue Cross). Four years after the first unit of this type was founded at Dallas in 1929, total membership was only 6,000—at which time there was still some professional opposition to the organization. As this declined and as economic difficulties continued, enrolment grew, reaching nearly 1,000,000 by 1937 and climbing to about 17,000,000 by 1945.[2] Blue Cross usually offered only custodial care, but during the forties it began to offer catastrophic (surgical) coverage in certain States. Only a small percentage of its subscribers, however, acquired this type of protection.[3]

Meanwhile, certain industrial plans had been set up, which provided comprehensive medical coverage to employees of large corporations on a prepayment basis. Particularly significant was the combination of prepayment with group practice in these organizations—the employees being served by a group of physicians usually associated with a single hospital. For while prepayment aided subscribers in distributing costs, it did nothing to improve the quality of services, whereas group practice tended to provide this additional benefit. Outstanding examples of this type of plan were the Ross-Loos Clinic in Los Angeles, and the Permanente Foundation in other West Coast cities. In 1943 the

[1] A.M.A. *Journal*, vol. 109 (Oct. 16, 1937), 1280 ff.; *N.Y. Herald Tribune*, Nov. 6, 1937; Esther E. Lape: "Medical Education as Discussed in 'American Medicine'," *Jour. of the Asso. of Amer. Med. Colleges*, XII (Nov. 1937), pp. 359 f.

[2] Louis H. Pink: "The Story of Blue Cross", *Public Affairs Pamphlets*, no. 101 (1945), pp. 4, 5.

[3] Such coverage was offered in thirteen States by 1943; but only about 750,000 had taken it out of nearly six million subscribers; Helen H. Avnet: *Voluntary Medical Insurance in the United States* (N.Y., 1943), p. 33.

Federal Social Security Board reported on 113 industrial plans which served nearly 1,500,000 persons.[1]

While the organized medical profession accepted Blue Cross and does not seem to have seriously opposed industrial plans, strong objections arose to group-practice units that offered prepayment service to the public or even to such large elements as Government employees. In certain cases, local or State medical societies denied membership and the use of hospital facilities to physicians who served plans of this type. Efforts were even made to apply professional penalties to medical men who defended these organizations.[2] This raised questions of economic and personal freedom. In the famous case of the Group Health Association in Washington, D.C., the issue was taken into the Federal courts; and a decision was finally obtained against the American Medical Association as having violated Federal statutes against operations in restraint of trade.[3]

Apparently as a result of this, the Association became less active in direct pressure against either local or national plans. It was replaced, or at least supplemented in this connection, by a new Association of American Physicians and Surgeons organized at Gary, Indiana, in 1944. The latter body declared its approval of prepayment health plans under professional control, but expressed determined opposition to the types of plans already condemned by medical societies. Soon the Association found itself primarily concerned with opposition to new programmes for Federal legislation.[4]

The failure to secure compulsory national health insurance in 1936 had been followed by further efforts in this direction both in Federal bureaux and in Congress. The President appointed an Interdepartmental Committee to Co-ordinate Health and Welfare Activities, and this body reported that "some action and financial support from the Federal Government is essential if a sound and comprehensive [health] programme is to develop".[5] In consequence, a National Conference on Health and Medical Care was called at Washington in 1938, and to

[1] *Bureau Memorandum* No. 55, "Prepayment Medical Care Organizations"; see also Margaret C. Klem: "Voluntary Medical Insurance Plans", *Medical Care*, vol. 4 (Nov. 1944), pp. 263 ff. The latest study in this field is that by Nathan Sinai, O. W. Anderson, and M. L. Dollar: *Health Insurance in the United States* (Commonwealth Fund, N.Y., 1946).

[2] See *New England Jour. of Med.*, vol. 218 (Boston, 1938), p. 50.

[3] R. H. Shryock: "Freedom and Interference in Medicine", *Annals* of the Amer. Acad. of Polit. and Soc. Sci. (Phila., Nov. 1938), pp. 22 ff.

[4] The nature and programme of this Association are stated in *News of the Assoc. of Amer. Phys. and Surgeons*, vol. I, no. 1 (Gary, January 22, 1944).

[5] I. S. Falk: "Roads Ahead in Health Security", *Survey Graphic*, XXVII (July 1938), p. 383.

this were invited various interested groups—medical societies, welfare bodies, organized labour, and Federal agencies. The conference recommended that an annual budget of some $850,000,000, half contributed by the States and half by the Federal Government, should be devoted to a national health programme. This included the care of the "medically needy", the lightening of the medical financial burdens of self-supporting elements through taxes and insurance systems, and other special features, but did not demand a complete system of compulsory insurance. The programme was generally endorsed by the representatives of all groups present save those of the American Medical Association.[1]

In response to the National Conference, Senator Wagner of New York introduced in the Senate the first of a series of health bills; and Congress thereafter became the centre of the national issue. This issue, it became increasingly clear, was no longer one between advocates and proponents of health insurance as such. Rather was it one between those who favoured voluntary insurance and those who desired a compulsory, national plan. Organized medicine and conservatives in general supported the first; organized labour, liberals, and radicals the second.

The debate proceeded along lines already described. Conservatives were encouraged by the rapid growth of the voluntary plans; while their opponents claimed that these could never reach the masses of the poor. Support for the latter view was provided in reports made by the National Resources Planning Board and the Social Security Board, and also in the so-called Beveridge Plan on social insurance published in Britain in 1942. Advocates of compulsory insurance seemed to have the backing of the Federal administration. They were also favoured after 1941, as had been the case in 1918, by the war atmosphere which encouraged sweeping, idealistic measures.

Although given less heed in public discussions, a compromise programme was also advocated by certain relatively liberal professional groups. These granted that voluntary insurance in itself could not reach the poorer masses, but held that it could do so if subsidized when necessary by State or Federal funds. This procedure was viewed as preferable to compulsory Federal insurance, since it could be combined with group practice to raise the quality of services, and would avoid dangers feared in the establishment of an enormous Federal programme.[2] It was further pointed out that most major social reforms

[1] See *N.Y. Times*, July 19, 1938.

[2] This was the tenor of discussions in the Committee on Medicine and the Changing Order of the N.Y. Acad. of Med., which met between 1943 and 1946. See its final report,

had been adopted by Congress only after extensive experimentation by the States, and that it might provide a comparison of the relative merits of voluntary and compulsory insurance if certain States would experiment with the latter principle. Only if and when such ventures proved more successful than voluntary insurance should a national compulsory system be established.[1]

It is unnecessary here to analyze in detail the various Wagner bills—known in 1943–1946 as the Wagner–Murray–Dingell bills—other than to state that they included compulsory Federal health insurance as a part of the social security system. The bills provided for medical care and for cash benefits in the case of disability, and applied to virtually the entire working population. There was to be free choice of physicians. The latter were to be paid from Federal insurance funds through fees-for-service, or upon a *per capita* basis, or by salary—the choice between these methods being left to the majority of local practitioners. The whole system was to be under the general direction of the Surgeon General of the Public Health Service.[2]

The bills had additional features of interest, notably a provision for channelling 1 or 2 per cent of the insurance income into medical education and research. Such a fund might have reached $40,000,000 or more annually, and would have involved a major Federal research programme.

Despite all the favourable circumstances noted, none of the Wagner health bills were passed in Congress during the period between 1939 and 1946. Chances for their passage appeared best in 1945 and early in the next year. Thereafter, the end of the war, changes in Federal policy after President Roosevelt's death, and the sweeping political victory of the Republicans in November of 1946, all pointed to a conservative reaction against the New Deal tradition. The issue of voluntary versus compulsory health insurance remains before the country, but prospects of the early enactment of a Federal programme seem less imminent than was the case only a short time ago.

Medicine in the Changing Order (N.Y., 1947). The wide range of interest and ideas on the controversy is indicated in the *Complete Handbook on State Medicine* published by J. Weston Walch (Portland, Maine, 1946), *Supplement* (1947).

[1] It may be objected that health conditions within any given State are beyond its own control, that certain States are too poor to finance health insurance, that difficulties would be presented by migratory groups, etc. Similar objections may be made to state unemployment insurance, but much State experience was nevertheless acquired in this field.

[2] See, e.g., the analysis of the 1943 bill in "The New Wagner–Murray–Dingell Social Security Bill", *Lawyer's Guild Review*, III, No. 6 (N.Y., Jan. 15, 1944); Franz Goldmann, "Health Bills Pending in Congress", *New Eng. Jour. of Med.*, vol. 234 (May, June, 1946), pp. 655 ff.

XX

SOME CONTEMPORARY QUESTIONS

CONTROVERSY AND uncertainty have promoted some pessimism about medical services in the United States. There is a natural tendency to say that scientific problems have been solved, or are in process of solution, and that the pressing obligation now is to make existing knowledge more widely available. In medicine, as in the physical sciences, technical advances seem to have outstripped social adjustment. No one has advocated that a moratorium be placed on medical science while society catches up with it, as was urged with reference to physical science during the 1920's. But liberal critics of American medicine are inclined to view it as technically more advanced and socially more backward than that of almost any other country.

There is no question about the seriousness of the problem of medical care in the United States. But emphasis upon this, desirable as it is, should not distract attention from equally serious difficulties in medical science. After all, some medical service is now available to the greater part of the American population, while in most other progressive countries systematic insurance plans and associated medical care are provided. Yet the value of all this is strictly limited by the present state of medical knowledge.

The dazzling progress made in science during the past century has blinded many to the persistent difficulties—to the areas in which few or no advances have been made. For the recent decades, the real question was not whether the public acquired confidence in medicine, but rather whether they expected too much of it. Critics of contemporary medical science felt it necessary to warn against superficial optimism.[1]

Such optimism was most natural in the United States, because here the progress in organizing institutional and professional support of medical science had been sudden and spectacular. Prior to the present century, factors deep-rooted in American civilization had inhibited medical research, and thereby made for a colonial dependence on Euro-

[1] See, e.g., C. C. Furnas: *The Next Hundred Years* (Baltimore, 1936), pp. 22 ff.

pean centres. Notable was the indifference of a commercially-minded people to apparently useless, pathologic studies, as a result of which neither the universities nor Government supported "pure science" of this sort.[1] At the end of the last century, however, research reached a level at which it was of obvious value to public hygiene and to therapy. This value was particularly clear in the case of certain infectious diseases which had most terrorized the public up to that time. At this point the very emphasis upon immediate utility, which had hitherto discouraged medical science, became a factor in its favour. Now that medicine was useful, Americans would rally to its aid with unusual fervour. It was fortunate that this change in outlook coincided with the accumulation of national wealth during the industrial revolution that followed the Civil War. As a result American institutions were in a relatively strong position for supporting medical research, in a day when this was becoming an ever more costly process.

Space will permit only a brief account of the professional innovations which made possible a real programme of research in the United States after 1900. The most immediate influence at work was that of German medicine, but this took hold only because of the fundamental change in attitudes towards the medical sciences.

In 1901 the American Medical Association was reorganized and began to give more attention to research. This called in turn for a reform in medical education, since schools and teaching hospitals were the most obvious centres for scientific work.[2] Foundations established by wealthy philanthropists proved of great aid in this connection, as these were able both to recommend changes and to assist in bringing them about. Thus Abraham Flexner's report to the Carnegie Foundation for the Advancement of Teaching (1911) inaugurated sweeping reforms in medical education,[3] and these were implemented thereafter by large grants made to certain schools by the Rockefeller Foundation and the General Education Board.[4] Guided by Welch and other leaders, Rockefeller had earlier set up the independent Institute for Medical Research in the European manner. By 1940 the Rockefeller Foundation alone had contributed about $169,000,000 to the fields of medicine and public health—exclusive of the activities of its International Health

[1] R. H. Shryock: "Trends in American Medical Research during the Nineteenth Century", *Proceeds.* of the Amer. Phil. Soc., vol. 91 (1947), pp. 58 ff.

[2] On growing professional interest in research in the United States, see *Journal* of the A.M.A., vol. 15 (1890), 543 f.; vol. 34 (1900), pp. 1445, 1557.

[3] See Abraham Flexner: *I Remember* (N.Y., 1940), pp. 113 ff.

[4] Ibid., 257 ff., see also James and Simon Flexner: *William H. Welch* (N.Y., 1941), pp. 325 ff.

Division.[1] In that same year these fields were clearly the chief beneficiaries of all the Foundations, and received from the sixty-three largest ones a total income of about $9,500,000.[2]

While foundations were stimulating medical research, this was further encouraged by a rapid expansion of universities and the growth of their endowments. Medical schools were aided by their own institutions as well as by outside organizations. Most of the small private hospital schools disappeared, and those in universities benefited by association with other scientific departments. The calibre of medical faculties, hitherto drawn from local practitioners, was improved by adopting the general university practice of calling men from all over the country. Full-time chairs became the rule in the preclinical subjects, and were in some cases set up even for clinical fields; this at last set men free for systematic investigations.

Meanwhile the development of immunology and of experimental pharmacology had promoted the evolution of small drug concerns into large pharmaceutical houses. The latter, following the general pattern of corporation research then unfolding, undertook extensive studies of their own. These were commercially motivated and usually emphasized direct utility; but they were closely correlated with work in the medical schools and were sometimes of a basic nature.[3]

Last but not least, the growth of research enthusiasms influenced the attitude of government as well as of private institutions towards science. The reform of State university medical schools, particularly in the Middle West, paralleled that of the private schools. The Federal Government supported original work in the Army and Navy medical services on an increasing scale after 1900. Notable was the evolution of the Public Health Service, after about 1910, into a genuine national health agency. With the development of its own laboratories at Bethesda, Maryland, in recent decades, this has become one of the chief medical research organizations in the United States.[4]

[1] *Remarks of Mr. John D. Rockefeller, Jr. . . . at the Rockefeller Foundation* (April 3, 1940), pp. 1–9.

[2] Geneva Seybold (Ed.): *American Foundations and their Fields*, Raymond Rich Associates (N.Y., 1942), pp. 38, 39.

[3] A critical discussion of these relationships is given in Francis Boyer: "The Pharmaceutical Manufacturer and Scientific Research", *New Eng. Jour. of Med.*, vol. 228 (1943), 529 ff. Trends in the history of a drug firm, as seen by the Company, are outlined in "One Hundred Years in the Conservation of Life", issued by Sharp and Dohme (Phila., 1944). For the present scale and methods of pharmaceutical research, see the account of the Abbott firm in *Fortune*, vol. 22 (Aug. 1940), pp. 110 ff.

[4] See Robert D. Leigh: *Federal Health Administration* (N.Y., 1927) for the story until that year. Later developments in the Public Health Service can be found in the annual reports of the Surgeon General; e.g., Thomas Parran: "The Aims of the Public Health Service", *Ann. Rept.* for 1937, pp. 463 ff.

During the Second World War the Committee on Medical Research of the Office of Scientific Research and Development (O.S.R.D.)[1] organized and co-ordinated applied medical investigations in laboratories all over the country.[2] So successful was the work of the O.S.R.D. that the end of the war brought a demand for the continuation of such a programme in some peace-time Federal organization. The famous Bush Report to the President (1945) recommended the creation of a National Science Foundation, whose medical division was eventually to direct the annual spending of about $20,000,000 on medical research.[3] Legislation to provide such a foundation was before Congress in 1945 and 1946, at the same time that the Wagner–Murray–Dingel bill (providing even larger sums for medical research) was being considered. Both of these programmes failed to pass, but their serious consideration indicated the possibility of future Federal support for medical and other scientific studies on a scale transcending anything that had been possible heretofore.

Research in the United States seemed to be moving, during the early 1940s, in the direction of the European tradition of Government support. In this, however, it encountered opposition from the older pattern of private control. One reason for objections to the proposed National Science Foundation was the fear among scientists that Government aid might threaten Government control, and that this in turn would inhibit the freedom essential to original work. Here the conflict in ideals between statism and individualism was reflected in the scientific world. This is well illustrated in present contrasts between the Russian system of State control and the American methods of supporting science.[4]

While the prestige of medical science was steadily mounting during

[1] During the war emergency, the O.S.R.D. was organized with the co-operation of the National Research Council; somewhat as the N.R.C. had grown out of the National Academy of Science during the First World War, and as the Academy had originated in the emergency of the Civil War.

[2] The Committee on Medical Research worked in close co-operation with the medical departments of the Army and Navy, and with appropriate committees of the N.R.C. For a summary of the achievements of the Committee on Medical Research, see the testimony of its Chairman (Dr. A. N. Richards, Univ. of Penna.) in the report on *Medical Research*, I (G.P.O., 1945), pp. 2193 ff., issued by the Subcommittee on Wartime Health and Education (Pepper Subcommittee) of the Senate Committee on Education and Labour.

[3] *Science: The Endless Frontier* (Gov't. Print. Office, 1945), 28 ff. An analysis of this report appeared in the *N.Y. Times*, July 19, 1945.

[4] For left-wing criticism of the organization of science in the Western democracies, see J. D. Bernal: *The Social Function of Science* (N.Y., 1939), *passim*. On individualistic ("democratic") objections to the control of science by the State, see M. Polanyi: *The Contempt of Freedom: The Russian Experiment and After* (Lon., 1940); P. W. Bridgman: "The British Society for the Freedom of Science", *Science*, vol. 100 (1944), pp. 54 ff.

the present century, and its support from private and Government sources was increasing, research itself was running into serious difficulties. Achievements in medical bacteriology and related fields had revolutionized public hygiene, surgery, and even therapy between 1875 and 1910, and all this promoted an optimism that was akin to that of 1800. In that earlier day, certain major diseases—scurvy, smallpox, leprosy, the plague—seemed to be disappearing; but then came the cholera, and tuberculosis grew apace.[1] In like manner, by 1910 the chief infectious diseases were about to be wiped from the earth—typhoid, typhus, diphtheria, yellow fever—and life expectancy at the earlier ages was rapidly improving. But then came the influenza pandemic of 1918, and the apparent growth of the chronic, degenerative, and mental diseases. Even within the area of the infections, all was not plain sailing in the decades that followed.

There was, first, some disillusionment concerning certain overenthusiastic claims by the bacteriologic pioneers. Bitter experience with Koch's supposed cure for tuberculosis is a case in point.[2] More serious were difficulties encountered in dealing with infections with which no bacteria seemed to be associated. By 1910 it was known that filterable viruses were involved in a whole series of serious diseases such as measles, influenza, poliomyelitis, and the common cold—but the nature of these substances was obscure. Unlike bacteria, they could not be cultivated outside animal hosts, and this made it difficult to develop protective vaccines. The same difficulty was met in dealing with the rickettsiæ. That these were causative factors in typhus and spotted fever was demonstrated by American investigators—Ricketts, Goldberger, and others—after 1910. The rickettsiæ were similar to bacteria in appearance, but their behaviour was more like that of viruses.

The fact that recovery from certain virus and rickettsial infections brought immunity, led to attempts to prepare vaccines from killed viruses, but these lost all antigenic (immunity-producing) properties. Fortunately, control of certain rickettsial and virus infections was facilitated by the discovery of their insect vectors, but no protection was available against other virus conditions such as measles and the common cold. These were communicated as respiratory infections, and no real protection against this process was developed until recent years. In the case of poliomyelitis, it is still uncertain whether the virus is trans-

[1] See Chap. IV.

[2] For similarly sober, second thought concerning some of Pasteur's work, see M. J Van Stockum: *New Principles of Anti-rabies Treatment and Rabies Statistics* (The Hague 1935), p. 56.

mitted primarily by insect vectors, by food and drink, or as a respiratory infection.

Immunology relating to bacteria itself proved more complicated than was at first realized. The discovery of different types, within a species of pathogenic organisms, required refinements in the preparation of vaccines and serums. It proved necessary, for example, to develop a number of type-specific anti-pneumococcal serums. Even a given type of organism might display a wide range in virulency, so that epidemiologists came to speak of different races or strains and of cocci "in an epidemic mood". Some bacteria tended to develop resistances of their own to drugs or serums, which made the wide use of such substances potentially dangerous. Much remained to be learned about the natural history and life-processes of bacteria, as well as of the resistance mechanisms of animal hosts. A persistent problem was how to kill bacteria or viruses, without so destroying their antigenic properties as to make them useless as vaccines. If, on the other hand, attenuated living vaccines were used in order to preserve antigenicity, how could these be made entirely safe? [1]

While search for further methods of avoiding infections or of protecting against them by immunization was slowed down by the difficulties noted, there was continued progress in surgery and therapy. Technical advances were marked in the former field after 1910, notably in thoracic and neurologic specialties. Surgery was more closely correlated with basic research in physiology and pathology, as is well illustrated in the late Dr. Harvey Cushing's work in brain surgery and its correlation with endocrinology.[2] Surgery benefited from advances in anæsthesia, which included the use of new procedures as well as of new drugs. To the spinal and nerve-block techniques, for example, was now added the use of avertin [3] as a rectal anæsthetic. Progress was facilitated by intensive work in pharmacology, during which innumerable drugs were tested for their anæsthetic properties.[4]

General research in pharmacology had first related largely to the analysis of botanic drugs, but moved on into synthetic chemistry as that field developed. Like surgery, pharmacology became closely cor-

[1] R. E. Dyer: "The Role of Immunization in War-time Advances in Medicine", and R. J. Dubos: "Trends in the Study and Control of Infectious Diseases", *Proceeds.* Amer. Phil. Soc.: *Symposium on War-time Advances in Medicine* (Phila., 1944), pp. 182 ff., 208 ff.

[2] The authorized biography of Cushing by John F. Fulton has recently been published (1946).

[3] Tribromethal alcohol, Butzengeiger (1926); see Josiah C. Trent: "Surgical Anæsthesia, 1846–1946", in *Jour. of the Hist. of Med. and Allied Sciences*, I (Oct. 1946), pp. 512 f.

[4] Chauncey D. Leake: "Historical Notes on the Pharmacology of Anæsthesia", ibid., pp. 576 ff.

related with physiology and pathology. Each possible drug had to be studied in terms of absorption, specific affinities for tissues, toxicity, and habit formation. Thus morphine, an old standby during the nineteenth century, was valuable because it had both hypnotic and analgesic effects; but it was dangerous in relation to habit formation. When veronal was substituted, this was found to have no analgesic properties. Hence, drugs like allonal were created, in which was combined a barbital-like hypnotic and the analgesic pyramidon. Apart from the therapeutic value of producing needed rest, these products had obvious humane significance. Synthetic pharmacology also provided drugs of value for conditions other than those affecting the nervous system. Continued refinements of digitalis products, for example, proved useful in heart treatments, while certain drugs like cincophen were found helpful in kidney disorders.[1]

In most cases, however, these remedies provided relief from symptoms rather than an essential cure. More basic, therefore, was the search for a chemotherapy that would actually promote recovery from serious infections. Here the experience of the decades between 1915 and 1935 proved even more discouraging than that in immunology. Ehrlich had demonstrated the power of his "magic bullet" in 1910, and predicted in 1913 that the next five years would witness "advances of the highest importance in this field". Yet by 1920, despite constant search, only salvarsan and several anti-malarials and trypanocidal compounds had been found—all relating to protozoal diseases. One authority after another, during the 1920s, declared the outlook barren. The failures of these "doldrum years" resulted from Ehrlich's lead in seeking only drugs which actually killed pathogenic organisms without harming the host. The possibilities here were very limited, since most protoplasmic poisons are dangerous to man as well as to micro-organisms. Progress in such chemotherapy had to await new clues that were uncovered only during the last decade.[2]

More serious than the failure to prevent or cure certain infections was the increase in so-called chronic illnesses (for example, arthritis), degenerative diseases (cancer, hypertension), and the mental disorders. As partial victory over infections advanced life expectancy among the young, a much larger percentage of the population survived to middle and old age. They then became the victims of the chronic and degenerative diseases to which people of that age are most susceptible. In

[1] See the articles by Arno B. Luckhardt, E. H. Volweiler, and Mary M. Rising in Julius Stieglitz: *Chemistry in Medicine* (N.Y., 1928), pp. 445 ff., 460 ff., 473 ff.

[2] Iago Galdston: *Behind the Sulpha Drugs* (N.Y., 1943), pp. 111 ff.

a sense, the increase in this type of disease was a natural though distressing sign of medical progress. More accurate reporting was also a factor in the rising mortality curves. Even after allowance was made for these circumstances, however, the evidence indicated that there was a positive increase in the morbidity and mortality rates for diseases of the older age levels. Thus the cancer mortality for a given number of men aged fifty to sixty, for the decade 1930–1940, was found to be higher than that for a similar group *of the same age* for earlier decades.

Leading Causes of Death

New York City...Five-Year Period, 1941-1945

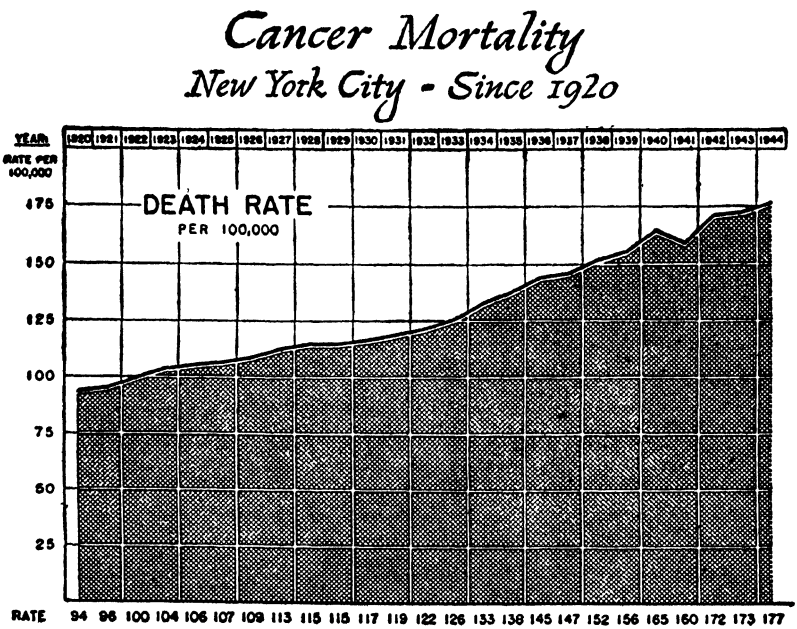
CAUSE	ANNUAL AVERAGE DEATHS	RATE PER 100,000
HEART DISEASES	29,403	386
CANCER	13,169	173
CER. HEM., ARTER'L DIS.	4,957	65
ACCIDENTS	3,824	50
TUBERCULOSIS	3,585	47
PNEUMONIAS	3,345	44
DIABETES	3,131	41
NEPHRITIS	3,081	40
CONG DEB., MALF'N.	2,815	37
CIRRHOSIS OF LIVER	1,052	14
SUICIDE	907	12
SYPHILIS	857	11
DIS. OF STOMACH	660	9
HERNIA, INTEST. OBST'N.	611	8
LEUKEMIAS	608	8
DIS. OF PROSTATE	600	7
APPENDICITIS	441	6
NON-MALIG. TUMORS	400	5
DIARRHEA, ENTERITIS	316	4
ALCOHOLISM	301	4
OTHER CAUSES	4,419	67
ALL CAUSES	78,382	1,028

The total situation relating to this disease is most disturbing. In fourteen years, between 1910 and 1924, the mortality rate in the United States rose 21 per cent for females and 47 per cent for males. Within only five years, 1939–1944, the average national rate for both sexes advanced from 119·3 to 129·1 per 100,000![1] Similar increases occurred in other so-called progressive countries. Much the same trends were observable in diseases of the heart and circulatory system, which became the chief cause of death during the present century. At the present time,

[1] For early studies of cancer mortality, see Frederick L. Hoffmann: *The Mortality from Cancer Throughout the World* (Newark, N.J., 1915), pp. 28 ff. On recent American statistics, see the reports of the U.S. Census Bureau; Louis I. Dublin: "Trends in Longevity", *Annals Amer. Acad. Polit. and Soc. Sci.*, vol. 237 (Jan. 1945), pp. 123 ff., etc.

these diseases cause about 30 per cent of all deaths in the United States.[1] More exact pictures of these trends in degenerative disease mortality—in contrast to declining rates from infectious diseases—can be seen in the included charts showing the experience of New York City.

One obstacle to adequate studies of the degenerative diseases was the fact that research was largely concentrated on infectious conditions. Some momentum had to be overcome before the focus could be shifted. In recent years in the United States, only 17 cents has been spent per death per year on studies of heart disease; while—to go to the other



extreme—$525 is spent per death per year on poliomyelitis research. About $2.18 is thus spent on cancer.[2] Work on infections was relatively attractive to many scientists, because here more promising leads were available from earlier investigations.

Plans are now under way, however, for raising large funds and making necessary institutional arrangements for an intensive study of chronic and degenerative diseases. In the United States this is especially true in the case of cancer, where the American Cancer Society is collecting a

[1] See, e.g., *Special Release*, U.S. Bur. of the Census, Oct. 21, 1945 ("Heart Diseases Cause Three Deaths in Ten").

[2] Henry L. Sims: "The Future of Medical Research", paper read before Section L of the Amer. Asso. for the Advancement of Sci., Cleveland, Sept. 11–16, 1944.

national fund and certain special hospitals and institutes are devoting themselves primarily to this problem.[1]

The basic difficulty in dealing with the two major degenerative conditions, the hypertension complex and cancer, is the obscurity of their ætiology. These conditions, unlike most infectious diseases, can still be identified only in terms of symptoms and lesions.[2] It is true that empirical research on cancer can seek by trial and error for substances that may destroy tumours, and considerable work of this nature has been done in Europe. In the United States, however, emphasis has been placed on a search for causative factors.[3] If these can be found, rational means for prevention or cure can be anticipated, just as the knowledge of bacterial ætiology led to rational prevention and cure of infectious diseases.

Much has been learned about the pathologic nature of cancers. It became clear after 1890 that they involved the uncontrolled, rebellious growth of immature cells which retained full power of multiplication, lost some of the ability to differentiate, and escaped altogether from the forces making for bodily organization. Normal growth is limited to the needs of the whole body, whereas malignant growth never stops, and eventually destroys itself along with the entire organism. This suggested that if more were known about growth and cell organization, clues might be found for the factors that brought about abnormal proliferation. Unfortunately, the physical or chemical mechanisms which control growth are at present unknown.[4]

Some technical advances were made, nevertheless, in methods for studying this disease. Following the leads of bacteriology, the German Hanau first successfully inoculated (transplanted) cancer from one animal to another in 1898. The availability of experimental animals thereafter made possible systematic research on the nature and causes of malignant tumours. Although a search for bacterial agents proved fruitless, it was shown in recent decades that irritant substances (carcinogens) and viruses definitely did cause cancers of certain types. Considerable work has also been done on possible endocrine factors, and on the theory that proteins produced in normal metabolism may also have carcinogenic properties. More sweeping theories about environmental

[1] Notably the Memorial Hospital Cancer Centre in New York City, and the National Cancer Institute of the Public Health Service at Bethesda, Md.

[2] Excepting heart and vascular conditions of known infectious origins.

[3] Peyton Rous: "Concerning the Cancer Problem", *Amer. Scientist*, vol. 34 (July 1946), p. 335.

[4] Stanley P. Reimann: "The Cancer Problem as it Stands Today", *Trans.* of the Phila. Col. of Phys., 4 ser., vol. 13 (1945), p. 14.

origins—such as changes in the food habits of the last generation or two—have been encouraged by the rapid rise in mortality, which is difficult to explain on biologic grounds. At the other extreme, however, is the present interest in seeking possible hereditary factors.[1]

The present situation in cancer studies is therefore exceedingly complex, although empirical clinical improvements have been made in treatment through early surgery and by the use of radium and X-rays. Since intensive research is now getting under way, it is to be hoped that the whole problem may eventually prove to be less complex than now appears to be the case.

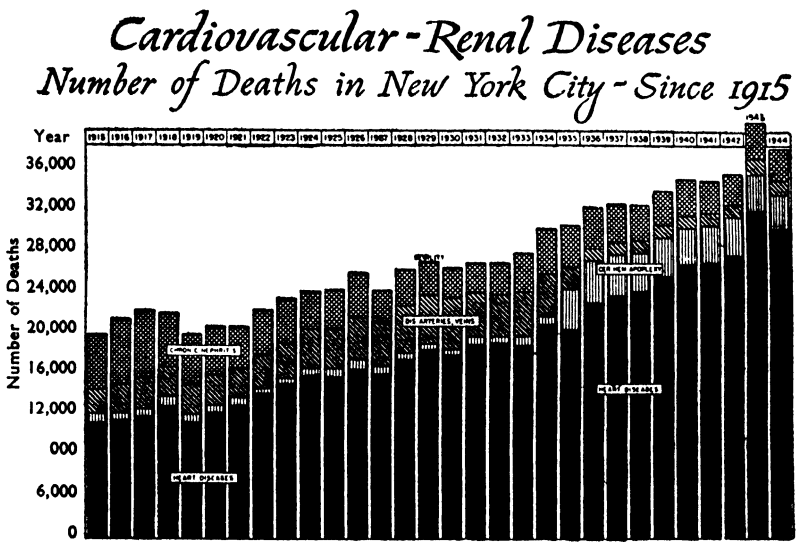
The search for causative factors in hypertension has proved similarly baffling up to the present time. This is not a disease entity nor even a uniform syndrome, but it is a useful concept that sums up a series of related, pathologic developments. Increased arterial pressure or peripheral resistance seems causally related to a series of degenerative changes which involve the heart and kidneys, and which ultimately reveal themselves in the more specifically conceived diseases of these organs. It may be recalled that it was these terminal processes which had first been identified by early nineteenth-century pathologists, as in the case of Bright's disease of the kidneys, which had been partially recognized by the Anglo-American William Charles Wells about 1800 and more clearly defined by Bright a generation thereafter. Subsequently this condition was found to be associated with the circulatory changes noted—a nice illustration of the whole trend from localized back to a generalized pathology.

In seeking the cause or causes of the most common and puzzling form of hypertension (essential benign), it proved difficult to find where the vicious circle starts. It was long believed that over-activity of the sympathetic nervous system produced peripheral resistance in the splanchnic (visceral) arterioles and that this in turn led to elevated blood pressure. Studies made in the United States between 1936 and 1940 raised doubts about this theory of abnormal, neurogenic "tone". C. J. Wiggers and others suggested that the uniformity of high blood pressure throughout the body indicated rather the presence of toxic substances uniformly distributed in the blood itself. Hormones like epinephrine (adrenalin) or kidney products like renin have been suspected,

[1] For recent developments concerning cancer, see the articles by Peyton Rous, Baldwin Lucké, and others in the Univ. of Penna. Bicent. Vol. on *Cancer* (Phila., 1940). A standard general work is that of Charles Eberling: *The Riddle of Cancer*, transl. from the French by W. H. Woglum (New Haven, 1944).

but have not been clearly demonstrated as the villains of the drama.[1]

The historian cannot refrain from pointing out that these current views of hypertension—the theory of "nervous tone" and that of "blood impurities"—are basically those long employed by the speculative tension and humoral pathologies of earlier centuries.[2] Here again, pathology returns to the approaches of the eighteenth century, albeit with far more knowledge, better means for investigation, and a much clearer realization of the necessity for verification. At the present time



both theories continue to be investigated, but the pathogenesis of hypertension remains obscure.

In contrast to the sombre background of the degenerative disorders, a brighter picture has recently unfolded in the fields of immunology and of chemotherapy. Bacteriologists began to make headway against the viruses and rickettsiæ, even as they had against bacteria a generation before. During the 1930's ferrets were found susceptible to influenza virus, and this ended a long impasse in work on that disease. Techniques for cultivating various rickettsiæ and viruses in animal hosts were improved by extensive experiments, and this at last made possible the production of vaccines. During the early forties apparently safe and effective vaccines were developed against influenza, typhus, and yellow

[1] Eugene M. Landis: "Pathogenesis of Hypertension in Man", Univ. of Penna. Bicent. Conf. vol. on *Hypertension* (Phila., 1941), pp. 7 ff.

[2] Cf. Chap. I.

fever[1] in time to protect military personnel during World War II. In consequence of this, and of such other measures as the use of insecticides, there was no repetition of the influenza and typhus epidemics which had proved so devastating during the First World War.

Basic work on the viruses was advanced in 1935, when W. M. Stanley isolated that of a plant disease (tobacco mosaic) in crystalline form and so made possible its study outside the host. Shortly thereafter, the study of viruses—as well as of all other micro-organisms—was aided by the introduction of the electron microscope. The limit of magnification by instruments using ordinary light had long since been reached, since this is determined by the length of light waves. The new microscope focused beams of electrons by the use of an electromagnetic field, and this secured enormously increased magnification.[2] Even the single particles of viruses became visible. Once again, as in the days of Leeuwenhoek, men could see what human eyes had never seen before. It is still too early to predict the nature of findings which the electron microscope will make possible, but its potentialities are obvious.

Meantime, the viruses, as examined outside the bodies of hosts, proved quite inert. Hence some discussion arose as to whether or not they were living materials. Since life can only be defined in terms of purposeful behaviour, the answer would seem to be that if viruses act at times exactly as if they were alive, they *are* alive. But subsequently it was found that the units of certain viruses were single molecules, so that it may be necessaryt o alter considerably the picture of living organisms as ordinarily envisaged.

Of more immediate concern to medicine than these general biologic questions was the light shed by a further study of viruses on means of infection. During the 1930s it was found that certain virus infections (measles, chicken-pox) were actually conveyed through wards by the air. W. F. Wells, at the University of Pennsylvania, then demonstrated

[1] A living virus was employed against yellow fever, attenuated by its growth in chick embryos. In the case of typhus rickettsiæ, on the other hand, it was found possible to kill them (after cultivation in egg yolk-sacs) by the use of formalin, without destroying their antigenicity. Formalin also proved useful, when combined with diphtheria toxin, to provide a "toxoid" which was safer than the old toxin-antitoxin immunization and just as effective. Similar results followed the treatment of tetanus toxin with formalin in 1942; and the use of the resulting toxoid was so effective that up to 1944, not a single case of tetanus following combat injuries had been reported in the United States Army.

[2] For the history and a relatively non-technical account of the electron microscope, see Gessner G. Hawley: *Seeing the Invisible* (N.Y., 1945), pp. 84 ff. Many other instruments of value to medicine were developed over the last two or three decades, such as Chevalier Jackson's bronchoscope and other devices used in clinical work. On the significance of new instruments to research and practice, see George Rosen: *The Specialization of Medicine* (N.Y., 1944), pp. 26 ff.

that bacterial infections also might be carried by air, and advocated the sterilization of public rooms by the use of ultra-violet rays. Experimental tests along this line in schools and in army barracks proved encouraging, and suggested possibilities in checking hitherto uncontrolled respiratory infections. Meantime Dr. Deryl Hart, of Duke University, found that surgical infections were decreased by the sterilization of the air in operating rooms. Here medicine was returning in some degree to the old ætiologic theory about air-borne poisons (miasms), against which there had been an extreme reaction after the demonstration during the 1870s of the role of bacteria. True, infections were not ordinarily spread by vague, atmospheric poisons; but they could indeed be transmitted by invisible bacteria or viruses wafted under certain conditions through the atmosphere.

Further possibilities await the immunology of the future. If enough can be learned about the natural history of pathogenic organisms, coming changes in virulency may be predictable, and this would be most helpful in avoiding epidemics. If more can be discovered about the factors in these organisms which produce disease, moreover, it is theoretically possible that synthetic, chemovaccines may one day be prepared against them.[1]

Much as was accomplished in immunology after 1930, success in therapy was even more spectacular. There was a resurgence of chemotherapy in the treatment of bacterial infections, which was highlighted by the discovery of the sulphonamides and of penicillin. During all the difficulties of the 1920s, the sulpha drugs "literally stole up on an unsuspecting and indifferent world". German chemists, between 1908 and 1917, first produced sulphanilamide, used it as a dye, and noted that its compounds killed bacteria *in vitro*. But not until 1932 did Mietzsch and Klarer patent such a dye compound under the trade name prontosil, claiming that it could destroy bacteria *in vivo*. In the same year Gerhard Domagk of the I. G. Farben Industrie found that prontosil would protect mice against hitherto fatal doses of streptococci. This was not announced until 1935, along with the first reports of its successful use with human subjects. French chemists now broke down the prontosil compound and synthesized sulphanilamide. Careful checks on its clinical possibilities were made in 1936 by Colebrook and Kenny, for the British Medical Research Council,[2] with surprising re-

[1] R. J. Dubos: "Trends in the Study and Control of Infectious Diseases", *Proceeds.* Amer. Phil. Soc., Symposium on Wartime Advances in Medicine (1944), pp. 208 ff.

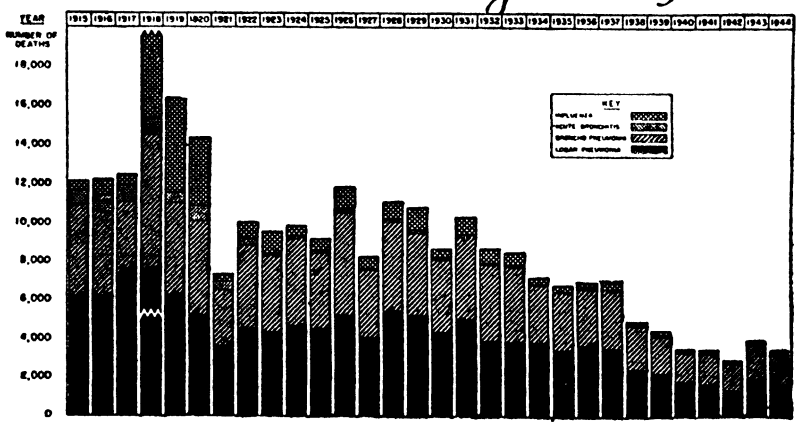
[2] A government body, organized after World War I as part of the programme to secure government aid for the natural sciences.

sults. The use of the drug reduced the death rate in puerperal infections by as much as two thirds. This aroused wide interest, and clinical testing was taken up by Perrin H. Long and Eleanor Bliss of the Johns Hopkins University during the same year.

Sulphanilamide and related sulpha drugs soon were found useful against a whole series of pathogenic bacteria, and especially against the vicious cocci group. The list of diseases in whose treatment they are of great value includes many which were largely resistant to serum therapy or other treatments, such as erysipelas, peritonitis, mastoid infections, and common forms of pneumonia and meningitis.

Pneumonia, Influenza, and Bronchitis

Deaths in New York City - Since 1915



Initial enthusiasm over these results was tempered by warnings against over-optimism. Medicine had a long history of disillusionment with cure-alls. The sulpha drugs did exhibit toxic effects, there were resistant cases, and in most instance viruses were beyond their reach. Nevertheless, sulpha therapy brought remarkable results. The average death rate from pneumococcal pneumonia was reduced from 30 to about 10 per cent or less; that from the chief forms of meningitis from almost 100 down to 50 per cent. The drugs proved of great value in diseases which were rarely fatal but which produced much suffering, such as gonorrhœa. They were also found helpful in preventing complications in various diseases, and in controlling wound infections.[1]

While the public was excited about these achievements, research men

[1] Iago Galdston: *Behind the Sulpha Drugs*, 142 ff.; E. M. K. Geiling, "Chemotherapy", in Wm. H. Taliaferro (Ed.): *Medicine and the War* (Chicago, 1944), pp. 40 ff.

became interested in the mechanism of sulpha action. Instead of killing bacteria, as Domagk had held, the drugs were found to combine with, and render unusable, substances necessary to the nutrition of these organisms. Hence the latter are "caused to starve in the midst of plenty". The sulphonamides are not protoplasm poisons, and so usually are not of serious danger to the host. Their mode of action explains why they are effective against so many pathogenic bacteria, and the knowledge of it gave clues for the discovery of related and even more effective compounds. The search for these bacteriostatic drugs is therefore proving more fruitful than Ehrlich's quest for bactericidal "bullets" could ever have been.

It may be added that circumstances made for a rapid introduction of the sulpha compounds into general practice. Much of what is called medical history is really only the history of research in main centres, for practice has often lagged decades behind. This has been true of diagnostic techniques, of preventive and immunizing methods, and even of surgery. The time lag in practice, however, has been gradually reduced by modern means of communication; and probably never was so great with drugs for which striking claims could be made. The public, often indifferent to preventive procedures, is more concerned about cures, and will demand the latest thing in medicines. The ease of administering sulphonamides also made for their prompt introduction. Indeed, it was so easy for any practitioner or patient to employ them that they were sometimes prescribed without due caution, with serious effects in individual cases.

Physicians had hardly adjusted themselves to the sulphonamides before another amazing drug was announced. Instead of employing a chemical check on bacterial nutrition, this involved the distinct principle of setting one type of organism against another. The history of penicillin, as of most other scientific developments, goes back farther than is usually realized. As long ago as 1877, Pasteur and Joubert found that certain air-borne organisms inhibited the growth of the anthrax bacillus, and they even suggested that this process of "antibiosis" might be useful in the treatment of infections. Attempts to apply the suggestion in 1899 failed, apparently because the particular organisms employed did not prove effective. Nothing further happened along this line for more than twenty years—a rather typical example of the lag between original suggestions and the first verification—but the idea was not forgotten.

In 1922 Dr. Alexander Fleming, of St. Mary's Hospital in London,

described lysozyme as a rather potent antibiotic. He continued to work in this field, and in 1928 occurred the famous contamination of one of his cultures by a pencillium mould. Instead of throwing the culture away as a matter of routine, Fleming collected a broth filtrate from the mould and termed it "penicillin". Finding that this inhibited gram-positive, pathogenic bacteria, he employed it in culture work, showed that it was non-toxic to hosts, and used it to clear up large infected surfaces in man. But it was difficult to purify the filtrate—it was somewhat unstable—and here the work paused. Fleming was working largely alone, and lacked technical or financial assistance with which to overcome the difficulties encountered. This, it will be recalled, was during the "doldrum years".

During the early thirties interest in the antibiotics was manifested by able men in Britain and in the United States, although there was no widespread enthusiasm for the field. René Dubos of the Rockefeller Institute demonstrated that a substance (gramacidin), derived from a soil bacillus, would destroy the protective capsules of pneumococci and thus expose the latter to attack by the white cells of a host. Here was a rational approach based on a knowledge of the mechanism involved; but unfortunately gramacidin proved toxic to body cells and therefore could not be employed against human pneumonia.[1]

Further developments came from empirical procedures, despite a slow pace in testing many materials. Obstacles encountered were too great to be overcome by the single investigator, and only when team work was substituted was there marked progress. Indeed, the whole success story here illustrates the current need for well-subsidized, co-operative research programmes.

In 1938 Chain and Florey at Oxford, assisted by Rockefeller funds, undertook a systematic study of antibiotics. Adequate facilities and able associates were provided. Their lead seems to have been supplied by lysozyme rather than by gramacidin,[2] but in any case they included penicillin in their programme. Small amounts of a relatively stable filtrate were secured—sometimes by purification through human filters —and these proved effective in clinical tests against the deadly staphylococcus aureus. Yet, the difficulties in securing sufficient quantities seemed insuperable. Nearly 100 litres of the mould brew were required, to produce enough of the drug to treat one patient for one day.

[1] Colin M. MacLeod: "The Past, Present and Future of Chemotherapy", in *The March of Medicine* (N.Y. Acad. of Med., 1945), pp. 43 ff.

[2] Leslie A. Falk: "The History of Penicillin", Corr. in *Jour.* of the A.M.A., vol. 124 (April 22, 1944), p. 1219.

In 1941 the Rockefeller Foundation brought Florey to America, where he was anxious to secure aid in solving the production problem. At this stage the matter was taken over by the O.S.R.D. Committee on Medical Research and by the appropriate committee of the National Research Council. With the co-operation of drug companies, large-scale production was evolved. This in itself was a major achievement.[1] Meanwhile there were extensive clinical testing and continued attempts to produce the drug synthetically. This was finally achieved in 1946. The history of penicillin well illustrates three stages in current medical research: the work of the individual scientist, of the team, and of the large-scale programme. Each was essential to the final outcome.

The effectiveness of penicillin, especially against gram-positive organisms, is now common knowledge. To some extent its values overlapped with those of the sulphonamides—as in its use against pneumococcal pneumonia or septicæmia—but its lack of toxicity often made it the drug of choice.[2] In other cases, as in dealing with the spirochætes of syphilis, it showed greater promise than did the sulpha drugs, and was safer and more easily employed than salvarsan.

Of course limitations appeared as clinical reports accumulated from all over the world between 1941 and 1946. Rapid bodily elimination created technical difficulties in maintaining the necessary level of the drug in the blood serum, which have been partly solved by methods of oral administration. Penicillin is apparently non-effective against the tubercle bacillus and other gram-negative organisms, nor does it seem of much use against viruses. It was reported in 1945 that its use tends to promote thrombosis.[3]

On the whole, however, penicillin is so widely effective, and so little toxic as to outshine even the sulphonamides in the therapeutic firmament. Neither of these, moreover, is necessarily the final or most potent antibiotic. Many other substances are being sought and tested, such as streptomycin—which has been heralded during the past two years as having value for use even against the stubborn tubercle bacillus. Few could have predicted, only a decade ago, the present promise of chemotherapy.

No account of recent advances in medicine is complete without reference to the achievements in war medicine, though these can only be

[1] W. E. Herrell: *Penicillin and Other Antibiotic Agents* (Phila., 1945), pp. 1 ff.
[2] Ibid., pp. 118, etc.
[3] L. F. Moldavsky, W. B. Hesselbrook, and C. Cateno: "Studies in the Mechanism of Penicillin Action", etc., *Science*, n.s., vol. 102 (July 13, 1945), pp. 38 ff.

recalled here in passing. Remarkable in preventive medicine, for example, was the checking of typhus epidemics (like that threatened in Naples) by the use of the insecticide "D.D.T.". The success of new immunization procedures has already been mentioned; though it may be noted that in one case—the use of yellow fever vaccines—apparent contamination with the virus of epidemic jaundice led to serious difficulties among American troops. Reductions in wound infections, in comparison both with past wars and with German practice in the recent war, may be credited to prompt evacuation, better surgical techniques, and the use of chemotherapy. The latter also provided improved anti-malarial drugs, which had prophylactic as well as therapeutic value. Last but not least, promising methods were found for dealing with certain types of mental casualties resulting from combat or even from ordinary army life, and a clearer realization of the seriousness of mental health problems was attained as a result of the experience with national induction and military service.

Since much medical as well as other scientific research was necessarily secret during the recent war, information on it is now only being gradually released.[1] There is no question that the total record, in terms of morbidity and mortality among both military and civilian populations, was far better than that of World War I—even as the record of the latter was superior to that in preceding conflicts. Certain of the advances in war medicine, such as improved vaccines, insecticides, and drugs, should prove of permanent peace-time value. But it may again be observed that war, while speeding up applied medicine, tends to retard basic studies by diverting research to immediate military needs. While insecticides and anti-malarials were developed, cancer research languished.

The net effect of renewed progress against infectious diseases, and of continued failure to prevent or cure chronic and degenerative conditions, has been the encouragement of contrasting attitudes towards medicine as a whole. Those who focus on the first phase maintain and even transcend the optimism inherited from the past century, while those who ponder the mounting toll of degenerative diseases may turn pessimists and question the reality of medical progress. These contrasting attitudes have their counterparts in the surrounding social world, where

[1] The over-all picture of war medical research in the United States, however, is provided in Dr. Richard's summarized reports for the C.M.R., and in the statements of military medical authorities in the same series of hearings. See note 2, 351. p. A history of scientific work under the O.S.R.D., by James Phinney Baxter, III, has been published under the title *Scientists Against Time* (Boston, 1946).

some look forward with hope to a new order, while others sense impending doom and the downfall of the Western world.

In the case of medicine, as well as of civilization at large, there is much valid evidence to support either viewpoint. Indeed, the evidence on both sides is so evenly balanced that the observer can only tip the scale one way or the other by the application of his own philosophy. It may be held, for example, that medical advances have saved primarily the young, and that once in or beyond the middle years, men must meet the same fate as did their most benighted forefathers. It is pointed out that if one must die of either an acute infectious disease, as did the Victorians, or of a lingering disorder in the modern manner, perhaps the first process is the more merciful of the two. At best, the end is but put off for a time. The dance of death, observed a thoughtful physician, has lengthened but the measure is still the same.[1]

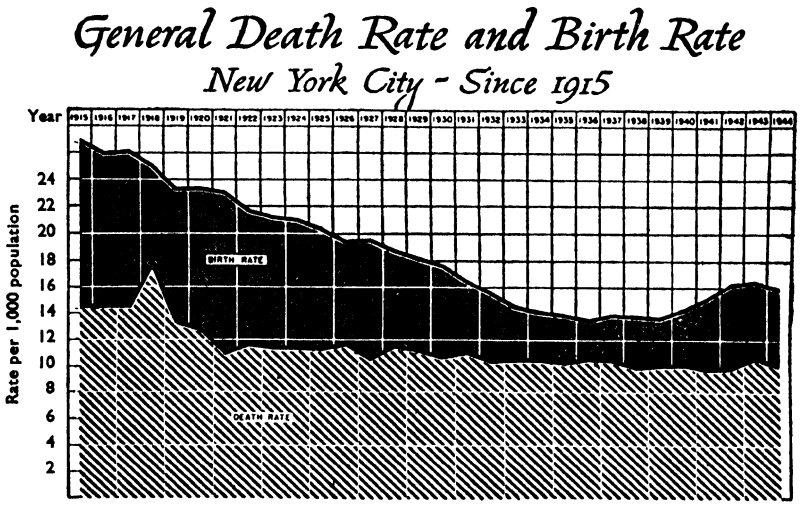
Such sombre comments are quite in order as a check to effusive optimism, yet they hardly represent the last word for most physicians or moralists. Many would agree that an increase in the life-span is desirable in itself. Is it not the purpose of medical practice, under most circumstances, to delay death as long as possible? Certainly this is an ethical end, if the increasing life span is combined—as it frequently has been—with a declining disease rate. Men not only lived longer lives after 1900; over a large part of the life span they lived healthier lives as well. And as health is usually essential to happiness, this means that science had done something to aid man in controlling himself, as well as his environment.[2] Such achievements seem essential to any conception of progress primarily concerned with the realities of this world. Unless one has faith in transcendental values, indeed, it seems unlikely that a more satisfactory idea of progress can be found than that suggested by the development of modern medicine. "*The measure of man's co-operation with man in the conquest of nature,*" wrote an American philosopher, "*measures progress.*" [3]

And who knows but what Condorcet, writing more than a century ago, may yet prove correct in his prediction of further triumphs? A victory over one important degenerative disease, comparable with that already achieved against certain infections, would in all likelihood extend the average life expectancy of older age groups by some years. The work of an American biologist, Ross C. Harrison, showed that the

[1] Peyton Rous: *The Modern Dance of Death* (Cambridge, 1929), p. 48.
[2] Cf. John K. Shryock: *Desire and the Universe* (Phila., 1935), pp. 14 ff.
[3] Edgar A. Singer, Jr.: "Progress", in *The Contented Life* (N.Y., 1936), pp. 59 ff. See also Carl L. Becker: *Progress and Power* (Stanford University Press, 1936), pp. 76 ff.

living cells of animals are potentially immortal, and suggested that even the problem of old age itself might some day fall within the reach of science.[1]

Though this may seem utopian, it is obvious that the actual accomplishments of twentieth-century medicine would have appeared utopian had they been predicted to our great-grandparents. Is there not a sense in which man has, during the past half-century, at least assured himself of attaining to old age?[2] It is a bold prophet who would set limits to the future advance of medical science, provided society con-



tinues to support research at least as well as it does at the present time. And if the day ever comes when medicine further extends the average life expectancy by as much as has already been achieved in this direction since 1890, the most momentous consequences may be anticipated.[3] For better or for worse, the future of society will then turn to a considerable degree on developments in medicine—just as the future of medicine once depended on certain trends in the evolution of society.

[1] Raymond Pearl: *The Biology of Death* (Phila., 1922), 67. For the original papers of Harrison, see *Anat. Rec.*, I (1907), 116; *Jour. Exper. Zool.*, XI (1911), pp. 787, 848. This did not mean that actual immortality could be secured for the higher animals. Indeed that was expressly denied, because of the complexity and interdependence of their parts. Paul de Kruif: *The Male Hormone* (N.Y., 1945) discusses recent attempts to extend the life span.

[2] Interesting discussions of the general problem of lengthening the life span will be found in Irving Fisher: "Lengthening of Human Life in Retrospect and Prospect", *Amer. Jour. of Pub. Health*, Jan. 1927; Louis I. Dublin: *Health and Wealth* (N.Y., 1928), pp. 340 ff.; and Dublin and A. J. Lotka: *Length of Life* (N.Y., 1936).

[3] This intriguing situation is envisaged in J. B. Haldane's *Daedalus: or Science and the Future* (N.Y., 1924).

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