Empiricism versus Rationalism in American Medicine 1650–1950

RICHARD H. SHRYOCK

CENTRAL TO MOST WRITING on the history of science is the content of the subject; that is, the advancement of man's knowledge of himself and of the world about him. Accounts of methods employed in science are included as explaining the substantive achievements. No doubt this is the most natural way to deal with the subject. Yet at times one may shift his interest from the ends of science to the means employed, from an emphasis on facts to one on method—the latter term connoting not merely techniques but also such a general procedure as quantification. Indeed, the whole way in which scientists 'approached' their fields can be considered a central theme, within which discoveries are mentioned simply as case studies or illustrations.[1]

Basic in this context is the philosophic and logical bent of scientific thought, whether of an individual or of an era. As an example of the methodologic focus, however, I would select a topic more tangible than philosophy at large but one which

[1]Dr. James B. Conant, in conferences and publications about two decades ago, encouraged teaching science to non-science 'majors' through case histories illustrating evolving methods. Other instances of the history of method are provided in G. Senn, *Die Entwicklung der biologischen Forshungsmethode in der Antike,* Veröff. Schweiz. Ges. Gesch. Med. Naturwiss. VIII (Aarau, 1933); in H. Woolf, ed., *Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences* (Indianapolis, 1961), *passim* (reprinted from *Isis*); and in T. S. Kuhn, 'The Function of Dogma in Scientific Research,' and 'Commentaries' thereon by A. R. Hall and others in A. C. Crombie, ed., *Scientific Change* (New York, 1963), pp. 347–395.

presents difficulties peculiar to its professional milieu. This is the long debate in Western culture on the relative merits of an empirical versus a rational approach to medicine—a matter which relates more to the so-called internal development of the subject than to its external or social history. Special heed will be given to American participation; but here as always one must also consider the European background, which provided not only the origins but also a continuing influence on the dialogue.

Obviously, in the past, many 'practical' men took little interest in theories or even in cumulative knowledge, and depended more on common-sense or trial-and-error gropings; this was vaguely termed an empirical approach. Others, inclined to reason about evidence and to formulate theories, were said to employ a rational or dogmatic procedure. When the two types were openly distinct, as in the contrast between modern technicians and research men, there was no problem. But in the case of physicians, the same guild included those who were, in modern parlance, either pure scientists, applied scientists, or technicians. (The less formally trained guilds—apothecaries, surgeons—are not considered here.) These distinctions were not overt among physicians until late in the last century, and even today are not always sharply drawn. Hence, while many doctors were, or thought they were, empiricists, the more learned or imaginative were devoted to rationalism. Or the same man, as will be noted, might proceed in one way in one situation, but in the other manner in some other connection. Under these circumstances sharp differences of opinion arose. What, then, were the medical settings in which the resulting discussions took form?

During the second century A.D., the Greek physician Galen compared empiricism in medicine with what he termed the rational tradition. Empiricists, he noted, claimed to depend only on 'experience', which might be accidental but could be confirmed by trial-and-error testing. What empiricists rejected was reasoning about such matters as anatomy, physiology, or the individual patient and his environment. The latter procedures, on the other hand, were the stock in trade of rationalists. Galen, in comparing the two schools, invoked a plague on both their houses since in the end both commonly used the same remedies. And he added: 'since empiricism is attacked by some dogmatists as... unscientific, while again the empiricists attack rationalism as being plausible but not true, the result is ... [an] argument... elaborated at great length as they refute and defend each charge in great detail.'[2]

Here were summarized the chief elements in a debate which would continue in medical circles for almost seventeen hundred years. In most respects Galen's teachings were finally rejected during the seventeenth and eighteenth centuries, but on this particular theme his comments were still pertinent and were in effect repeated by both European and American authors well after 1800. One finds Dr. James Maclurg of Virginia remarking in 1820, for example, that: 'It is hard to say when this quarrel began between empiricists and dogmatists, or when it will end.' He knew it was quite ancient, and like Galen, Francis Bacon, and other illustrious predecessors, he condemned extremists in both camps. Yet the Virginian predicted that the debate would never cease until 'our philosophy has acquired perfection;' and with a modesty somewhat unusual at the time, he admitted that this utopia was a distant one.[3]

As in the case of other, age-old controversies in medicine, more than one viewpoint might be held simultaneously within any group of physicians. Yet the two traditions mentioned were not merely constant factors in medical thought. On the whole, leading physicians tended to be rationalists, as befitted their learning, from medieval days until at least the eighteenth

[2] A. J. Brock, *Greek Medicine* (London, 1929), pp. 137–138. As is well known, Galen himself was dogmatic in his teleology but still made sound, empirical studies.

[3] Maclurg, 'On Reasoning in Medicine,' *Philadelphia Journal of the Medical and Physical Sciences*, I (1820), 218. (I am assuming that Dr. James Maclurg was identical with Dr. James McClurg.)

century. But it is not easy to label their views clearly, what with inconsistencies and a lack of precise terms.

Hence, it would be unwise to attempt firm definitions of empiricism or rationalism in medicine. What was usually involved in this distinction was a difference in emphasis. Or, to put it in another way, there was a spectrum of methods. At one extreme were the unlearned, crude empiricists of 'kitchen physic;' at the other, the more speculative and dogmatic rationalists. Even such extremists often confused the issue. Down-to-earth, folk practitioners would suddenly indulge in strange conjectures; while dogmatists, for their part, might claim to be 'true empiricists' just because there was *some* observational base from which their flights of fancy took off. In between, one must place (1) extreme empiricists, devoted to learning derived from sense impressions but distrustful of all intuition and theories; and (2) scientific empiricists, whose observations were guided by theories and whose theories were checked by observations.

For the present purpose, there is no need to review logical analyses of the concepts here taken for granted (theory, verification, and so on); and no attempt will be made to distinguish the several steps actually involved in present 'scientific empiricism.'[4] One may repeat that the essential issue involved was always that of procedure, but that certain other matters, such as a possible relationship between a physician's personality and his methods, also came into the picture.

Just how complex the story is, may be indicated by recalling the outlook of certain well-known heroes of modern medicine. But when did medicine first become modern? Despite the protests of Francis Bacon and others, Greek speculations about physiology, disease, and therapy persisted into the baroque

[4]Thoughtful comment on this confusing situation is given in L. S. King, *The Medical World of the Eighteenth Century* (Chicago, 1958), pp. 32–34; see also K. E. Rotschuh, *Physiologie im Werden* (Stuttgart, 1969), p. 17. On a breakbown of specific procedures involved in scientific empiricism, see the recent analysis in Paul A. Weiss, 'The Emergence of Scientific Thought in the Eighteenth Century,' *The Graduate Journal* (Univ. of Texas), VI (Fall, 1964), 384–387. *Re* logical analyses, consult E. Nagel, *The Structure of Science* (London, 1961), chapters 5 and 6.

era. These concepts were then associated, in what now seems a strange juxtaposition, with the advent of so-called 'modern' methods. William Harvey, for example, has long been viewed as a pioneer, scientific empiricist in his resort to experiments and quantification; yet he was at the same time a dogmatic rationalist in much of his thought.[5]

Harvey, moreover, was by no means exceptional in this regard: the mélange of medieval and modern ideas was typical of practice as well as of research. Sydenham, the great English clinician of the period, was admired for his careful, bedside observations, which involved identification of diseases by symptoms, but he related these data to ancient, uncomfirmed theories anent humoral pathology. And Sydenham, like Harvey, was unaware of any incongruity in combining objective and speculative approaches.

The same comment may be made on the author of the first treatise on medicine composed in the English-American colonies. In his 'Angel of Bethesda' of 1724, the Rev. Cotton Mather of Boston provided a blend of theology and science which remains unique in American medical literature. His science, even as that of Harvey and of Sydenham, now seems a bizarre combination of fact and fancy. He reported some real evidence, cited many authorities, and managed to combine mere dogmas with promising hypotheses. Notable, for example, was his advocacy of a germ theory of infections. Even more remarkable was the contrast between Mather's conviction that the cause of all illness was sin (germs were simply God's agents) and his view that the medicine of the future would relate chiefly to matter and motion. If it now appears difficult to reconcile theology and dynamics in this fashion, anyone can see how Mather did it by consulting the manuscript still preserved in the American Antiquarian Society. In all these connections, the clergy-

[5] T. C. Allbutt, *Science and Medieval Thought* (London, 1901), pp. 44–45; W. Pagel, *William Harvey's Biological Ideas* (Basel, 1967), *passim;* and the review of the latter work by E. Lesky in *Clio Medica,* III (Sept., 1968), 297–298.

man was inspired by certain European thinkers, but he displayed an extraordinary flair for seizing on their most seminal ideas.[6]

As mentioned, one's impression of the seventeenth and early eighteenth centuries is that, despite resort to some experimentation and measurements, the prevailing tone in medical thought continued to be that of medieval rationalism. This was no longer the case in physics and astronomy, wherein the problems were much simpler than those in medicine, and where there was also no such urgency and need for haste as confronted physicians. Hence, it was possible to check speculation in physics by experiments or quantification long before this became feasible in human biology.

To make matters worse, the very success of Newtonian physics encouraged further, fanciful conjectures in medicine. Classical theories were retained by some doctors and abandoned by others; but the influence of physics did encourage new, mechanistic views in physiology and eventually in pathology. Since the heart had proven to be a pump, for example, the stomach no doubt was a churn. And measurement would prove all things: hence Mather's claims about matter and motion! Certain medical men, or physical scientists, became iatrophysicists or iatrochemists, and stretched their claims about the cause or cure of illness far beyond anything which biology could then substantiate.[7] They should not be blamed for this, in view of the unrecognized complexity of the problems confronting them. Indeed, the iatrophysicists deserve some credit for pointing medicine in what ultimately proved a promising direction, but the fact that their outlook now seems prophetic did not usually help at the time.

[6] O. T. Beall, Jr., and R. H. Shryock, *Cotton Mather* (Baltimore, 1954); I. B. Cohen, 'The *Compendium Physicae* of Charles Morton,' *Isis*, XXXII (1942), 659–660. Later versions of sin-as-the-cause of all illness were not usually combined with traditional medicine; see, e.g., W. Hooker, *Physician and Patient* (New York, 1849), p. 141; Wolf von Siebenthal, *Krankheit als Folger der Sünde* (Hannover, 1950), pp. 42 ff.

[7] The value of physics and astronomy as models was explained to physicians before 1700; see, e.g., G. Baglivi, *De Praxi Medica* (Rome, 1696, English transl. of 2nd ed., London, 1723), pp. 131, 134.

Crude empiricism, at the other extreme of the spectrum, meanwhile continued to flourish. At a time when clergymen still practiced and 'doctors' were hardly distinguishable from folk healers, Americans were much impressed by the Rev. John Wesley's *Primitive Physic* (1747), which, for all its good intentions, was very primitive indeed. And even educated men might secure their remedies from the village blacksmith. Who knew what wonders might be stumbled upon, particularly since a good God might be expected to provide remedies within any environment appropriate for the diseases thereof? Yet it does seem bizarre when one observes Boston doctors of 1720 recommending 'the swallowing of leaden bullets for the miserable distemper known as the griping of the guts.'[8] Less strange was the fact that colonists were intrigued by native Indian medicine, thinking this at times more useful than their own.

The chief argument used by practical men against the learned was that research, then chiefly in anatomy, was of no use in therapy. Of what avail was it to patients to learn that a particular organ possessed a duct? Perhaps one may even observe here an early expression of anti-intellectualism in American life? At any rate, it was true that as late as 1800 nearly all helpful drugs or techniques had been found by chance or by blind trial-and-error as isolated phenomena. This was the case, for example, with cinchona bark (quinine), with fox glove (digitalis), and with inoculation against small pox; each of which had originated in folk medicine, Inca, English, and oriental, in that order. In the case of inoculation, prototype of all later immunology, the first hint of its value probably reached America from southern Libya. (Advocates of African studies, please take note.) And folk medicine, except for its magical elements, was usually equated with crude empiricism.[9]

[8] C. Mather to John Woodward, Sept. 28, 1724, MSS, Royal Society of London, Guard Books M, 2–3, no. 53. Copies in the American Philosophical Society Library, Philadelphia.

[9] Distinctions are sometimes made between the two approaches even in folk medicine, as in G. W. Harley, *Native African Medicine* (Cambridge, Mass., 1941), pp. 37 ff. But 'empiricism,' in this context, usually implies just the absence of supernatural elements. On the criticism of research, see V. Robinson, *The Story of Medicine* (New York, 1931), pp.

Thus, while dogmatists based their claims on learning and derided the ignorance of 'empirics,' the latter ridiculed the futility of their opponents. Since each of these views was partly right and partly wrong, it is not surprising that, as Galen had put it so long before, the argument continued 'at great length' and 'in great detail.' Cynics, noting only that the two traditions were equally dangerous for patients, concluded that time had stood still in the most vital of arts. Molière said as much in the seventeenth century, Voltaire in the eighteenth, and Thomas Jefferson in the nineteenth century.[10]

By about 1750, nevertheless, there were signs that, beneath surface contrasts between dogmatic and folk traditions, an underlying current moved in the direction of a sophisticated type of empiricism. The earlier success of some quantitative and experimental studies was not forgotten, and further attempts along these lines were encouraged by continued advances in mathematics and in the physical sciences. In the first place, certain discoveries had potential meaning for medicine, as in the development of statistics or in Lavoisier's work on respiration. Technology, meantime, was helpful in producing instruments or techniques applicable to medical problems; for example, improved microscopes. Even more significant was the general impact of the Enlightenment on the biological sciences, medicine included. Such supernatural elements in medical thought as theology, astrology, and witchcraft were phased out, and a subtle change occurred even within the most extravagant theories. Whereas traditional rationalism had been dogmatic in citing 'authority,' later speculation abandoned awe for the ancients and claimed to rest on direct observations of Nature.

Under these circumstances, a few, thoughtful physicians argued more clearly than had Galen or Bacon that there was a

301–302; L. S. King, 'Medical Philosophy,' in L. Stevenson and R. Multhauf, eds., *Medicine, Science and Culture* (Baltimore, 1968), p. 146. *Festschrift* honoring Dr. Owsei Temkin.

[10] *Re* cynical comments on physicians during these centuries, see, e.g., R. H. Shryock, 'Public Relations of the Medical Profession,' *Annals of Medical History*, n.s., II (1930), 308–339.

sane, mid-course between extremes in method. This was the approach here termed 'scientific empiricism.' Thus, the Englishman J. Gregory, in 1770, condemned unchecked theorizing but at the same time opposed any rejection of theories as such. For, he held: 'all physicians must reason, and the only difference among them [is] that some reason better than others.' They collect many facts and detect 'a remote view of a leading principle,' but accept this only if it can be confirmed by further observations or experiments.[11]

This view anticipated, in a general way, the outlook taken for granted in present medical studies; and it guided actual research in physiology by certain medical men of the later eighteenth and early nineteenth centuries. Ironically enough, however, procedure of this sort was not widely reflected in medical thought for nearly seventy-five years after Gregory made his statement. Some light may be thrown on the delay by recalling that medicine can be defined only teleologically—that is, as sciences and arts intended to protect men against illness. And, for this reason, it necessarily focused on disease (pathology). But the nature of disease remained a baffling question: was it just a pattern of response to adverse stimuli (such as fever), or were there distinct diseases which were real things in themselves? As will be noted, this was one of those questions which can be answered either way; but at the time it appeared that if one response was true, the other must be false.

If diseases were entities—Sydenham had viewed them as 'species' of illness, as real as plant or animal species—they must be identified. For what caused or cured one 'specific disease,' might not cause or cure another. Now the most simple way of identifying particular diseases was that which had long been used in distinguishing those which seemed obviously different (small pox, great pox, and so on); that is, by symptoms. Moreover, identifications on this basis called only for observa-

[11] Gregory, *Observations on the Duties...of a Physician and on the Methods of Prosecuting Enquiries in Philosophy* (London, 1770), pp. 110–112.

tions of patients, without resort to theories of any sort (extreme empiricism).

On the other hand, if such attempts bogged down, doctors might again seek quick solutions by speculating about some major pattern of disease from which could be deduced one, major type of cure (dogmatic rationalism). Just because they *were* physicians rather than physicists, they would be unwilling to suspend judgment too long: they had to *do something*. If all this was bad science, anxious patients as well as concerned but uncritical doctors would share the blame.

The actual course of medical method after 1750, somewhat over-simplified, illustrates these generalizations. Since Sydenham's emphasis on specificity had greatly influenced professional thought by that time, a continuing effort was made to identify specific diseases. This was done by observation of symptoms through unaided, sense impressions. Such 'bedside medicine' was certainly more empirical than the old 'library medicine' of tradition and conjecture, and American colonists provided some good examples. Thus, Dr. William Douglass of Boston described scarlet fever in 1736, and Dr. John Lining of Charleston yellow fever in 1753, so well that their diagnoses can still be confirmed.[12] The fact that these diseases were identified by a particular skin color illustrates the procedure involved.

Unfortunately, the results were not so clear when signs of illness were neither uniform nor obvious. Symptoms were numerous and their combinations almost endless, so that when each complex was viewed as an entity the results could be chaotic. What is now termed pulmonary tuberculosis, for example, was alone conceived as some twenty different diseases, and the total number of such entities in texts (nosologies) reached from one to two thousand. Obviously, the basic prob-

[12] F. H. Garrison, *History of Medicine*, 4th ed. (Philadelphia, 1929), p. 376. *Re* the need for physicians to do something, even though this involved almost routine, therapeutic experimentation without controls ('experience'), see, e.g., T. von Bischoff, *Ueber den Einfluss des... von Liebig* (München, 1874), pp. 60 ff.

lem in clinical medicine, as in contemporary botany, was to find better criteria for identifying and classifying 'species.'

While this attainment waited upon further studies, practitioners had of necessity to carry on but found nosology confusing. Diagnosis, if attempted, was often meaningless: many so-called diseases were just names for symptom combinations. Facing such difficulties, educated men still refused to admit that learning was of no value. What if the few, known cures *had* been found by chance or simple trial-and-error, that was no excuse for abandoning the use of reason—'man's noblest faculty.' After all, as more data accumulated, the greater was the need to bring order into the picture. It is not surprising, then, that reputable doctors continued to justify their practice by some rationale, even if they only took the old humoral tradition for granted.

Sensing this need for principles but rejecting ancient ideas, other leaders still looked to advancing sciences for what might be called 'instant' solutions. Professional ambitions as well as imagination and learning were involved; the physician who could find one key to open all doors might be hailed as the Newton of medicine. The most apparent clues were found, as noted, in new mechanistic ideas or in combinations of these with traditional concepts anent body fluids. The German Friedrich Hoffmann, among others, explained physiology and pathology by assumptions about the blood, 'animal spirits,' and a subtle fluid which, flowing through the nerves, affected the body in various ways. Insofar as such theories involved dynamic factors, they were later encouraged by the sound research of the Swiss Albrecht von Haller, who by about 1750 demonstrated that contractility was an essential property of muscles, and sensibility of nerves. Hence, muscle-nerve speculation invoked the latest scientific knowledge, and was soon formulated in such new medical centers as Leyden and Edinburgh.

In the latter city, the great figure was William Cullen, who attracted many American students after 1750. Cullen claimed that he reasoned only about obvious points which could be confirmed by experience. But, he added, one should always remember 'the incomplete and fallacious state of Empiricism.' Noting that chills often preceded fevers, he deduced that all such illness involved—'we suppose'—a loss of energy in the brain, which then ceased to transmit the nervous force needed to maintain tone in the vascular system. The results were spasms in capillary walls and attendant fevers.[13] Treatments should consist of measures calculated to restore nervous force, and thereby to end the aforesaid tensions or spasms. In this way, Cullen surrounded dogmatic theses with a scientific aura and passed them on to such leaders as John Brown of Edinburgh, Giovanni Rasori of Milan, and Benjamin Rush of Philadelphia.

Rush was not the only American to fall under the spell of extreme rationalism, but he undoubtedly was *the* significant figure in this context. His fellow-student Brown taught that all illness resulted either from nervous tensions or from a reverse lack of tone, and that the former type could be cured by opiates and the latter by 'Scotch,' an intriguing therapy which gained widespread popularity. Rush, however, reduced all disease to the lowest common denominator of 'excessive action' in the capillaries, and held that such tension could be relaxed by drastic bleeding and purging. These treatments were not as pleasant as Brown's, but Rush was sure they were confirmed by results: could not anyone see that the tense, feverish patient, bled long enough, *would* relax sooner or later?

All of this added up to a claim that there was but one disease and that Benjamin Rush was its prophet. Or, as he himself put

[13]On Hoffmann, see L. S. King, 'Medicine in 1695,' *Bulletin of the History of Medicine*, XLIII (Jan.-Feb., 1969), 17–29; *re* Cullen, 'Lectures on the Practice of Physic,' 1774 (MSS, Edinburgh College of Physicians), *passim*. Cullen clung to the idea of specific diseases (nosology), as so many variations in response to his one 'proximate cause' of all fevers. There were old theories about 'hollow nerves,' along which the nervous 'force' or 'fluid' postulated by Cullen and others might flow; see Edwin Clarke, 'The Doctrine of the Hollow Nerve,' in Stevenson and Multhauf, *Medicine*, pp. 123–142. Paul Weiss has recently shown photographically an actual flow of materials from the neuron along the axons, in *Proceedings of the American Philosophical Society*, CXIII (April, 1969), 142–148.

it to a class in 1796: 'I have formerly said 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.... This, Gentlemen, is a concise view of my theory of diseases.... I call upon you, Gentlemen, at this earlier period either to approve or disapprove of it now.'[14] One can sense, here, that such rationalism was not only speculative but still dogmatic in tone. Students must accept Rush's doctrine, 'or else !' A popular teacher at the largest American school and an able writer, Rush's influence spread a blight of heroic practice across the United States after 1800. It was a nice question as to who was the more 'heroic' in this setting, the patient or the doctor, since the latter was urged, in extreme cases, to remove up to three-fourths of the blood in the body.[15] Over the next fifty years, many practitioners employed such treatments and certain disciples of Rush even carried his therapy to greater extremes.[16]

Meantime, similarly speculative systems continued to appear in Europe; Rush, indeed, was viewed as just a distant expression of the trend.[17] For several decades before and after 1800, it was easy for a reputable doctor to detect 'a remote view of a leading principle' and then to proclaim it as the final word. Until about the 1820s these views were based on limited, bedside data, were made more uncertain by questionable deductions, and were often strongly opposed by other authorities. Rush, for example, observed that fevers involved a flushed face and therefore capillary distension, caused presumably by some 'nervous' force or action. From this he deduced that,

[14]Rush, 'Lectures on the Practice of Physics,' 1796 (MSS, Univ. Penna. Library, I, no. 31; II, no. 1).

[15]Rush, 'In Defence of Bloodletting,' in *Medical Inquiries and Observations* (Philadelphia, 1796).

[16]See, e.g., John Mace, *Proximate Cause of Disease* (Philadelphia, 1802), pp. 55–56; also L. P. Yandell, *A Memoir of... John E. Cooke* (Louisville, 1875), *passim*. On the other hand, a Dr. Samuel Danforth of Boston was well known for the opposite view; i.e., that illness was always caused by a *lack* of 'excitement.' Hence, he refused to bleed and gave only stimulants and adequate food; see James Jackson, *Another Letter to a Young Physician* (Boston, 1861), pp. 51–54.

[17]Ch. Daremberg, *Historie des Sciences Médicales* (Paris, 1870), p. 1141, referred to Rush only as a disciple of Brown at one of the 'extrémités du monde' !

since tension was the one thing all fevers had in common, it must constitute their essence and doubtless that of other diseases as well. Ergo, his doctrine of one 'proximate cause' or basic illness. It is this type of ultimate explanation—announced at one time as final truth—which is here termed a 'system.' No continuing program to confirm or qualify the synthesis was envisaged. A system in this sense was the hallmark of pretentious rationalism, whether in medicine or in any other scientific field.

No doubt it was the prevalence of 'fevers' which inspired the neurologic or tension theories just mentioned. Indeed, any system was apt to be inspired by some condition frequently encountered by its author. Now it happened that, beside fevers, a second type of illness which had long caused concern was what was vaguely termed dyspepsia. No wonder, then, that other system-makers associated most illness with the stomach, as was done by such diverse figures as Cotton Mather (1724), S.-A. Tissot of Lausanne (*ca.* 1770), H. Clutterbuck of London (1807), and Edward Miller of New York (1810).[18]

These systems appealed to many because they claimed to solve all problems in pathology and in therapeutics. They seemed to bring order and hope out of chaos and uncertainty. No longer need doctors worry about innumerable diseases, and patients were promised cures. But since these theories could be confirmed only by the bedside claims of founders, each new system competed with the others. Their authors, self-assured and ambitious, acquired followers who defended the faith against all comers. Now and then, however, disciples differed

[18] Mather referred to the stomach as 'the main wheel' of the body, but did not integrate this with his other theories. On Clutterbuck, note his *Inquiry into the Seat and Nature of Fever* (London, 1807), pp. 70 ff.; see also S. Miller, *The Medical Works of Edward Miller* (New York, 1814), p. 161. Ackerknecht refers in *Medicine at the Paris Hospital, 1794–1848* (Baltimore, 1967), p. 79, to a number of French doctors 'intensely interested' in gastro-enteritis during the 1820s. How this emphasis later faded out is illustrated by the views of N. Chapman of Philadelphia in 1839. He still held that the stomach 'occupies, perhaps the highest rank, and possesses the widest influence' [on physiology], but refused to speculate further about this. (*American Journal of Medical Sciences*, XXV, 77.)

with masters and schisms appeared. Dogmatism in medicine was no more inclined to toleration than was dogmatism in theology. Mather ridiculed the errors of classical authorities—of Hippocrates, Galen, and Celsus—while Rush, coming down to date, questioned Cullen and John Brown. Even less kind to compatriots, he implied that colleagues stood by and let patients die for lack of heroic treatments. (One of his rivals retorted, with something less than professional courtesy, that Rush's remedy was 'a dose for a horse' !) In all this confusion and friction one observes an 'immature' science, with its meager, factual base, consequent resort to speculation, and continuing need for new revelations to replace the old. The faith in progress must be maintained.[19]

Meantime, although attempts to identify diseases merely by symptoms brought confusion and even encouraged speculation, a more empirical approach had long been maintained on another front. This was gross anatomy which, at least since the days of Vesalius, had been demonstrated in medical schools as far as facilities and legal restrictions permitted. Research in this field was inspired by pure curiosity and also by some promise of utility, most immediately in surgery and obstetrics. Anatomists advanced knowledge through sense impressions, and their findings were usually confirmed by other physicians. Simple descriptions were effective for most of this work, although sophisticated techniques, such as wax injections, were beginning to be introduced. In their research, as mentioned, anatomists had no need for theories; and they were rationalistic only in so far as they deduced functions (physiology) from the structures actually observed.

In due time, empiricism in normal anatomy led to similar approaches in morbid anatomy. Those who did dissections gradually noticed abnormalities or injuries in body parts. And

[19]The role of orthodox 'systems' in the United States, 1790–1840, is noted in Shryock, *Medicine and Society in America* (New York, 1960), pp. 54–58, 69–74. *Re* the concept of an 'immature' science, see Kuhn, 'Function', pp. 352–355.

as soon as lesions were well recognized, relationships between them and ante-mortem symptoms began to be suspected. Pondering these connections, anatomists envisaged a better criterion for identifying diseases than was provided by symptoms alone.[20]

The first, clear lead here was pointed out by Morgagni of Padua as early as 1761, when he sought identifications through a correlation of symptoms with lesions found at autopsies. Thus, he noted that certain symptoms were constantly associated with congestion ('solidification') in the lungs, a complex identified as pneumonia. Yet for thirty years thereafter, little heed was given to Morgagni's views—presumably because of such factors as (1) the tendency of scientists, like others, to resist basically new ideas,[21] (2) the limitations of communication media, and (3) the popularity of speculative theories at that very time.

Here and there individual doctors sensed the value of Morgagni's methods, though not necessarily borrowing directly from him. This was the case when Dr. Thomas Bond, founder of the Pennsylvania Hospital, proposed checking diagnoses through autopsies in 1766.[22] Again, Cullen recognized by about 1770, even while preoccupied with his own system, that morbid anatomy had much promise. More typical of the 1770s and 1780s, however, was Rush, who rarely if ever mentioned Morgagni and who seemed unaware of his viewpoint. Not until Matthew Baillie of London published a pioneer text in pathology in 1793, were there signs that this field was coming into its own.

France, at that time, was in the throes of a revolution which temporarily abolished medical institutions; but during the political adjustments which ensued under Napoleon, Paris be-

[20] E. R. Long, *History of Pathology* (Baltimore, 1928), Chapters 4–6, incl.

[21] Kuhn, 'Functions,' pp. 358–359, observes that such resistance was useful in preventing premature efforts in the physical sciences, but I suspect that in the case of morbid anatomy the delay was a sheer loss of time.

[22] Bond's essay is printed in T. G. Morton and F. Woodbury, *History of the Pennsylvania Hospital* (Phila., 1897), appendix, reprinted and ed. by C. Bridenbaugh, in *Journal of the History of Medicine*, II (1947), 10–19.

came the world's scientific capital. One aspect of this ascendancy was the appearance of reorganized medical faculties and hospitals, wherein after 1800—and particularly from 1820 to 1845—a group of clinician-pathologists pursued the correlation of bedside observations and autopsy findings on an unprecedented scale. These men were inspired not only by earlier leads and improved facilities but also by the influence of anatomically-minded surgeons,[23] and by a continued spread of objective attitudes from the physical to biological fields.[24]

Examining thousands of cases, Parisian clinicians—Bichat, Laennec, Louis, and others—defined specific diseases, as in breaking down such vague concepts as 'inflammation of the chest' or 'consumption' into the entities of bronchitis, pneumonia, and pulmonary tuberculosis. This 'hospital medicine' gradually replaced nosologic confusion with a lessened number of entities, and at the same time rejected the opposite idea of one, all-pervading disease pattern. Hence, by the 1820s, the first systematic research began to replace both confused nosology and unconfirmed speculation with a firmly-grounded, localized pathology, an orientation modified but still basic today.[25]

There were few signs, before 1820, that American physicians, still led by men trained in Edinburgh, were as yet responding to French initiative. By that date, however, the small number of native journals gave increasing heed to French publications. Doubts appeared about the omniscience of Rush: soon after 1820 a doctor read a defense of his principles before the Philadelphia Medical Society, but no one else present would support the argument. When yellow fever paid its last serious

[23]*Re* the influence of surgeons, see O. Temkin, 'The Role of Surgery in the Rise of Modern Medical Thought,' *Bulletin of the History of Medicine*, XXV (1951), 248 ff.

[24]The role of the physical sciences, and the limitations placed on clinical objectivity by ethical questions, are discussed in Shryock, *The Development of Modern Medicine* (New York, 1947), Chapters vii, viii, etc.

[25]A thorough, critical interpretation of 'the Paris School' is provided in E. H. Ackerknecht, *Paris Hospital, passim.* The phrases 'library-,' 'bedside-,' and 'hospital-medicine,' used here, are his. See also G. Rosen, 'The Philosophy of Ideology and the Emergence of Modern Medicine in France,' *Bulletin of the History of Medicine*, XX (July, 1946), 329 ff.

visit to that city, the old dogmas were no longer emphasized and the autopsies became more frequent. Two years later, in 1822, the Philadelphia Anatomy Rooms were set up for routine dissections.

There Dr. John Godman, as director in 1824, announced that 'medicine is at least two centuries behind the point we should have reached if physicians had only kept to the path of pathologic research inaugurated during the Renaissance.' As it was, he declared, Bichat had at last started physic on the right path by 1800; and his work alone was worth more than all other medical writings from Hippocrates to Rush put together! Distrusting crude empiricism as much as he did vague speculation, Godman added that a blind search for remedies had likewise 'led nowhere.' It was necessary, in short, for medical science 'to begin all over again.'[26] Although Godman's actual contributions were rather thin, here was a declaration of independence from both 'empirics' and from systematizers. But a struggle still lay ahead, before this outlook would be generally accepted either in the United States or abroad.

The revolt against speculative systems was not exclusively a matter of logic: it was also aided by disillusionment with the drastic therapy of these programs (Rush's sanguinary treatments were a case in point). Certain French clinicians, because they sought verification for all claims, became skeptical about depletion in principle; and this attitude was strengthened when Pierre Louis finally established the value of clinical statistics as checks on the efficacy of bleeding.[27] True, the new breed of clinicians, in London and Dublin as well as in Paris, had few potent remedies to offer in place of rejected panaceas. But cure was not the only goal of medicine: why not also prevention? Cold-blooded as pathology anatomy appeared, Richard Bright

[26] Godman, *Contributions to Physiology and Pathologic Anatomy* (Philadelphia, 1825), pp. 5–8.

[27] P. Louis, *Recherches sur les Effets de la Saignée* (Paris, 1835), pp. 88 ff. Ackerknecht notes that Louis was not the first to use such statistics. (There are usually precursors; i.e., those who had the idea or the device earlier, but did not completely and finally 'put it across.')

of London was prophetic when he declared that the best way to *avoid* fevers was 'by making ourselves acquainted with the nature of the mischief with which we have to contend.'

As a matter of fact, moreover, a few valuable drugs *were* introduced during the 1820s: quinine and chloroform were good examples. Significantly enough, these were now provided by chemists rather than by folk practitioners: science was beginning to 'pay off.' Under pressure to examine patients more carefully, meantime, both instrument-makers and clinicians introduced improved means of observation, notably achromatic microscopes and the stethoscope. Even Louis' 'numerical method' was in a sense a more effective, observational device. Parenthetically, this resort to quantitative procedure was inspired in part by mathematicians, and was itself another sign of valid empiricism in medical circles. There was at first opposition to quantification, as is usual in any field, and some criticism was justified by the limitations of early hospital statistics. But Louis and others finally established an elementary use of the method, a use which was refined in due time.[28]

The reaction of clinicians against heroic treatments was an aspect of their whole indictment of extreme rationalists. The latter, of course, did not capitulate at once: Rush's influence among rank-and-file American doctors apparently persisted through the 1840s. To complicate matters further, new systematists appeared and continued to take on protective coloring by garbing themselves in the latest vogues.

The Frenchman Broussais, for example, was at first a leader in the anatomic studies of the era 1810–1830, and was unusual in noting limitations in 'the Paris school.' He criticized the failure to study pathologic physiology as well as morbid structures, and also denounced the Sydenham-like tendency to view diseases as things-in-themselves (ontology). By the 1830s, nevertheless, Broussais reverted to unconfirmed speculation by again

[28] Shryock, *Development*, pp. 165–167; and 'The History of Quantification in Medical Science,' *Isis*, LII (1961), 215–237.

ascribing most disease to gastro-enteritis. He also turned to heroic measures, emphasizing bleeding almost as much as had Rush. These two resembled each other in so many ways, indeed, that Ackerknecht refers to Rush as 'a kind of American Broussais;' though in point of time this phrasing could be reversed. Intoxicated by fame, perhaps, both men exhibited the usual stigmata of system-makers—egotism, monistic doctrines, and an insistence on the true faith.[29]

By the 1830s, however, objective empiricism had become well enough established in France to encourage prompt attacks on dogmatists. N. Hallé, an able and older man, remarked of Broussais that: 'The mere scent of his style shows the arrogance of the sectarian.'[30] But it was the younger clinicians who became the most effective critics. One has only to look first at Broussais' famous *Examen de la Doctrines Médicale* (1816, 1834), in which he exposed the weaknesses of *his* predecessors, and then to read Pierre Louis' *Examen de l'Examen de M. Broussais* (1834), in order to see how the latter fared.

Yet Broussais was not the only prominent physician to maintain, or revert to, over-simplified concepts. In Germany a *Naturphilosophie*, inspired by the idealism of Schelling, promoted intriguing but unconfirmed generalizations. This was the medical version of current transcendentalism in philosophy and literature (1810–1850), and was carried to such extremes as to conceive of a 'Christian pathology,' a term suggestive of later 'Christian Science.'[31] In more moderate forms, a late-stage *Naturphilosophie* would have a stimulating effect on medicine, but at first the outlook encouraged a renewed indulgence in abstruse theories. This so-called 'romantic medicine' exerted a minor influence in the United States, where physicians lectured at times on such awesome themes as 'Nature and Nature's

[29] Ackerknecht's analysis of the complex Broussais in *Paris Hospital*, chapter 6, is especially enlightening.

[30] *Ibid.*, p. 64.

[31] See e.g., P. Diepgen, *Deutsche Medizin vor 100 Jahre: Ein Beitrag zur Geschichte der Romantik* (Leipzig, 1923), *passim.*

God', thereby concealing neglect of research behind an impressive, metaphysical facade.

A second German contribution, which had a more specific impact on the States by the 1830s, was still another medical system, that of the famous Hahnemann. So much has been written on homeopathy that one need only recall that its one, 'proximate cause' of most illness was said to be psora (the Itch), and the chief rationale of treatment the claim that 'like cures like'—that a drug which causes a fever will also cure it, particularly if used in high dilutions. Hahnemann was an erudite physician, and his theories were as respectable in the 1820s as were those of Rush or of Broussais. From the present viewpoint, indeed, his theories were preferable to the earlier systems. His followers agreed with skeptical clinicians in opposing bleeding and purging, yet they had an advantage over the latter in also promising cures. Actually, homeopathic drugs were probably of no more use than was the regular pharmacopeia, though they encouraged patients and had at least the virtue of doing no harm.[32]

Conservative leaders, nevertheless, viewed Hahnemann's ideas with increasing disfavor after 1840, and O. W. Holmes of Boston cited various authorities who had tested homeopathic remedies and found them wanting.[33] Sharp attacks were made on the system by other prominent doctors during the next three decades, but this had also been true in the case of earlier systematists. By the 1840s, however, a new phenomenon appeared: most physicians displayed an aversion to association with Hahnemann's disciples. Gradually, the latter, voluntarily or otherwise, ceased to be members of 'regular' medical societies

[32] L. S. King gives a clear account of the origin of Hahnemann's ideas in *Medical World*, chapter 6. *Re* the early status of homeopathy as another 'system,' see, e.g., J. J. Reuss, *Die Medizinischen u. Heilmethoden der neusten Zeit* (1831), pp. 269 ff.; T.C.E. Auber, *Traité de Philosophie Médicale* (Paris, 1839), p. 534. In 1837, G. B. Wood of Philadelphia remarked that it was a new system which had not 'laid hold' of many American doctors, *Medical Essays* (Philadelphia, 1859), p. 132. See also S. R. Kirby, *The Introduction... of Homeopathy in the United States* (New York, 1864).

[33] 'Homeopathy and Its Kindred Delusions' (1842), in *Currents and Countercurrents in Medical Science* (Boston, 1861), pp. 126–129.

and instead organized their own. Homeopathic colleges and journals likewise appeared, and the orthodox guild was in time confronted by a minority transformed into a rival profession.

Nothing so extreme had occurred in relation to earlier systems. One possible explanation of this contrast is simple enough: Hahnemann just lived too long. His *Organon* was published in 1810 when its author was 55 years old, and when the rules of evidence in science were not yet as strict as would soon be the case. Hahnemann did perform therapeutic experiments but not under what would now be viewed as controlled conditions. The sharpness of the attack on Hahnemann may be ascribed, as suggested by King, to the weakness of regular practice: had orthodox medicine possessed effective remedies, it might have viewed rivals with less concern.[34] But the reverse argument also seems plausible; that is, the tactics of regulars may have reflected growing confidence. By way of comparison, note that organized medicine in the States was in a much stronger, scientific position by 1905–1935, and this was when its most aggressive attacks on quackery were undertaken.

The truth is, I believe, that it was during just the decades (1820–1850), when homeopathy was getting under way, that attitudes in medical centers became increasingly hostile to unconfirmed generalizations. This is apparent when one reads the better journals of the time, with their insistence on confirmation of theories as well as of facts, and with their dawning distrust of 'experience', that is, of one man's personal findings. Many American as well as European authors cited the physical sciences and Comte's positive philosophy as inspiring their objective outlook. They were aware that biology (including medicine) was, as Charles Gillispie puts it, at last becoming assimilated 'to the objective posture of physics.'[35]

[34]King, *Medical World*, pp. 186–191. On Hahnemann's methods and records, see H. Henne, ed., *Hahnemanns Krankenjournal* (Stuttgart, 1963–68), nos. 1–4, e.g. his *Einleitung*, no. 4, pp. 38–39.

[35]On changing attitudes in the better journals, see, e.g., in the *American Journal of Medical Sciences*, the review of 'Bright's Reports,' I (1828), 409; H. L. Hodge, 'Ob-

Physicians were encouraged in this view, despite the weakness of their armamentarium, by a faith that objective science would eventually do more for human welfare than could monistic theories. And even by the 1850s, intimations of this had appeared in the United States as well as in Europe; as in the effectiveness of a few new drugs, the introduction of anesthesia, and progress in such branches of surgery as dentistry and gynecology. British sanitary reform was meanwhile demonstrating results, German progress in physiology was pointing toward biochemistry, and various physicians, Davaine in France, Henle in Germany, Bassi in Italy, and J. K. Mitchell in the United States, once again had an inkling that pathology would lead into medical bacteriology.[36] To none of these areas had the systematists, as far as I can recall, made any major contributions. It is against this background that one may view the changing image of homeopathy, from the dignity of a system to the status of a sect, as a turning point in medical thought.

Homeopaths survived, as sectarians, by virtue of a learned background and because of the appeals already mentioned. But their isolation from the regulars (or, as homeopaths termed them, 'allopaths') increased through the 1840s and 1850s, as the latter came increasingly under the spell of scientific empiricism. This trend was evident not only in journals but also in general works on 'medical philosophy' which appeared in both Europe and the United States.

Apart from such technical discussions as those by Louis, one recalls various French treatises of this nature from Cabanis' *Du Degré de Certitude* (1798) to T. C. E. Auber's *Traité de Philosophie Médicale* (1839); in England there was, for exam-

servations on Sedation,' X (1832), 92 ff.; and the review of W. Beaumont's work on gastric digestion in XIV (1833), 120. In the latter, system-makers are said to 'advance heresies for the gratification of a morbid desire to be distinguished... as the head of a new sect.'

[36]For a summary of this progress, *ca.* 1820–1860, see, e.g., Shryock, 'Nineteenth Century Medicine: Scientific Aspects,' *Journal of World History*, UNESCO, III (1957), no. 4, 880–908. Recent studies of the men noted here, particularly those by J. Théodorides on Davaine, are enlightening.

ple, Sir Gilbert Blane's *Elements of Medical Logic* (1819); and in Germany, F. Oesterlen's *Medizinische Logik* (1852). Interest in such works was doubtless both cause and effect of a continuing debate, among thoughtful physicians, on how medical men could adopt the attitudes of natural scientists. (Ironically, this reversed an earlier pattern, by which many of these very scientists had been introduced to their own fields by a preliminary medical training.) The debate mentioned was at times a lively one and culminated in a sense in Claude Bernard's classic on experimental medicine (1865).

In the United States, although certain of Rush's papers had a bearing on theory (1790–1812) and Nathan Smith's *Practical Essay on Typhous Fever* (1824) was an empirical reaction thereto, the first substantial book on medical philosophy was Samuel Jackson's *Principles of Medicine* (1832). Herein, the Philadelphia professor—though a former student of Rush—deplored dogmatism, and ridiculed as mere impressions the 'evidence' on which this was often based. He urged medicine to emulate the physical sciences, regretted that the former had never known 'the fertilizing influence of the inductive logic,' and cited Dugald Stuart as another philosopher whom physicians should follow.[37] In 1838 and 1840, in lectures to students, Jackson went further in deploring the failure of American physicians to employ critical methods in research. Aware by this time of German work on pathologic histology as well as of French gross pathology, he abandoned his early deprecation of microscopes. (In 1832 he had echoed John Locke's criticism of microscopy as useless: an attitude then still common in France.) But by 1840, Jackson realized that the Germans were forging ahead in histology and that Americans had much to learn from them. Yet, he added, none of his medical compatriots were really seeking 'scientific fame.'[38]

[37] *The Principles of Medicine* (Philadelphia, 1832), pp. x-xiv, xix. See also King, 'Medical Philosophy,' pp. 144–145.

[38] *On the Methods of Acquiring Knowledge: An Introductory Lecture* (Philadelphia, 1838), pp. 7–31; *Address to Medical Graduates... April 3, 1840*, (Philadelphia, 1840),

Without clearly analyzing its nature, Jackson considered induction an essential part of scientific method. Yet even this type of reasoning was distrusted by Elisha Bartlett of Rhode Island. His *Essay on the Philosophy of Medicine* (1844) carried French clinical zeal to its logical conclusion. He emphasized only 'facts,' which, one gathers, would almost speak for themselves. Apparently, no intuition or even working hypotheses were to be allowed. Here one observes extreme empiricism, in sharp contrast to the dogmas still in vogue only three decades before. Bartlett was therefore even more severe in his judgment of the speculative Rush than Louis had been in attacking Broussais. The New Englander, in recalling Rush's essays, declared 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!'

Although our sympathies today may extend more to Bartlett than to Rush, there was complacency in rigid empiricism as well as in dogmatic rationalism. The former's rejection of all reasoning seems as illogical as was the latter's *devotion* to this process. Better balanced than either of these extremes, parenthetically, was Oesterlen's *Logik*, which appeared eight years after Bartlett's work. This was apparently indicative of the broadening outlook of German research, already recognized in Jackson's writings. Meantime, it is easy to point out other weaknesses in 'the Paris school' and among its American followers. The relative indifference to physiologic research and to histology are obvious illustrations. There was also the debatable issue over 'ontology' (as implied in French clinical work), in contrast after 1850 with a revived concept of disease as bodily behavior. Despite concomitant emphasis on 'posi-

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also printed in *American Journal of Medical Sciences*, XXVI (1840), 119 ff. Locke had remarked that 'though we cut into the inside, we still see but the outside of things,' cited in P. Romanel, 'Locke and Sydenham,' *Bulletin of Medical History*, XXXII (1958), 298 ff.; while Jackson in 1832 had said that 'minute anatomy can reveal...only the exterior side of life,' in *Methods*, p. 21.

tivism,' this debate seemed to echo overtones of a philosophic nature (realism versus nominalism.)[39]

One may add that American empiricists, as well as European, exaggerated a bit the sterility of medical research in the United States. True, few if any Americans were devoted to systematic investigations, and Jackson may have been correct in ascribing this to 'the commercial spirit of the age.'[40] Research which cast doubts on therapy made little appeal to a 'practical,' self-governing people. (Had not Jackson himself admitted that doctors often could only 'amuse' patients while Nature performed the real cure? Why then, bother with research in such a useless science?) There were also other circumstances, such as the management of hospitals, which did not encourage original investigations in the States.

The native record, nevertheless, was not an entire blank: the introduction of anesthesia and advances in surgery mentioned above were largely American achievements. Although based on trial-and-error procedures, these reflected more than crude empiricism. Individuals trained in Paris also did some basic research in the Gallic manner, as when W. Gerhard of Philadelphia distinguished typhus from typhoid fever in pathologic terms (1837). Later Americans erred in claiming priority for this study, but it did at least place Gerhard in the van of those who finally made the puzzle clear.[41]

[39]For the quotation from Bartlett *re* Rush, see his *Philosophy of Medical Science* (Philadelphia, 1844), p. 225; note also the less extreme essay by W. Hooker of Yale University, 'The Present Mental Attitudes and Tendencies of the Medical Profession,' *New Englander*, X (n.s., IV, 1852), 548–568. Ackerknecht views Bartlett's book as the most complete expression of the Paris-school tradition, in his 'Elisha Bartlett and the Philosophy of the Paris Clinical School,' *Bulletin of the History of Medicine*, XXIV (1950), 34–60. On Oesterlen, see K. E. Rothschuh, 'Friedrich Oesterlen (1812–1877) u. die Methodologie der Medizin,' *Sudhofs Archiv*, Band 52 (Juni, 1968), 105–123.

[40]Jackson included England in this generalization in *Methods*. Relevant here is Shryock, 'American Indifference to Basic Science During the Nineteenth Century,' *Archives Internationales d'Historie des Sciences*, V (1948), 50–65; but cf. Phyllis A. Richmond, 'The Nineteenth Century American Physician as a Research Scientist,' *International Record of Medicine*, CLXXI (1958), 492.

[41]E. Long, *History of American Pathology* (Springfield, Illinois, 1962), p. 61, states that Gerhard credited H. C. Lombard (in *Dublin Journal of Medical Science*, 1836) as a precursor. See also the account of the long effort involved in finally working out this

As implied, moreover, extreme empiricism had at least the merit of ridiculing highfalutin notions. A reviewer in the *Transactions* of the American Medical Association, for example, condemned a prominent doctor in 1851 by accusing him of 'almost transcendental abstractions, or theories which are the plausible refinements of erudition swayed by fancy.'[42] In such an atmosphere, both in Europe and in the United States, regular physicians still harboring 'systems' quietly abandoned them. They were either convinced by a lack of evidence or feared professional rejection. This was true in the case of certain neurogenic-tension doctrines which had continued to appear; and a similar, better-known fate awaited the revival of a humoral pathology by Carl Rokitansky of Prague and Vienna as late as the 1840s.[43]

There was a significant contrast, however, in the behavior of Rokitansky in this case, and that of Broussais in Paris durin the 1830s. Both men were distinguished pathologists before reverting, as it were, to speculative theses. Like a true systematist, however, Broussais never recanted and his system achieved some temporary popularity, whereas Rokitansky gave up his theory after the German Virchow demonstrated that it was untenable. The contrast may be ascribed in part to personalities, but also resulted from the changing climate of opinion. Broussais made a sort of 'last stand' for dogmatism as late as 1835, but Virchow's attack on Rokitansky came a decade later when empirical pathologists finally insisted upon valid evidence. Only in rare cases after 1850, as when a learned man

problem, in G. Ongaro, 'Evoluzione Storica del Concetto di "Tifo",' *La Riforma Medica* (Napoli, 1967), and 'Le Prime Descrizioni Anatomo-Pathologiche del Tifo Addominale,' *Minerva Medica* (1966).

[42] *American Medical Association Transactions* (Philadelphia, 1851), p. 485.

[43] G. Rath, 'Neural Pathologies: A Pathogenic Concept of the 18th and 19th Centuries,' *Bulletin of the History of Medicine*, XXXIII (1959), 526 ff.; on Rokitansky, see Erna Lesky, *Die Wiener Medizinische Schule im 19. Jahrhundert* (Gratz-Köln, 1965), pp. 134–135; and also J. J. Nierstrasz, 'General Pathology and Therapy of Inflammations in the 1860s,' *Janus*, LIV (1967), 173–174. Nierstrasz states that Rokitansky's and Virchow's views 'lived on together quietly' for a time, and that there were of course partial truths in humoral pathology.

became indifferent to professional standing, might he maintain doctrinaire views in pathology or therapy. The most striking example of this was the Frenchman F.-V. Raspail, a remarkable scientist who anticipated some of the later, German achievements. Unfortunately, his politics as well as informal education prevented professional recognition, and he devoted his later years to providing a sort of sure-cure (camphor) to the people of France.[44]

Empirical progress does not seem to have been retarded, in the States, by Bartlett's extreme views. Some clinical studies such as those made on wound surgery during the Civil War, were of the trial-and-error type; but there was rarely any objection to reasoning as such.[45] An illustration of the latter point was the comment made by J. J. Woodward, the best known pathologist in the Union Army, on the etiology of malaria. Recalling J. K. Mitchell's ideas as well as the current military experience, he decided in 1863 that a germ theory would indeed explain some peculiarities of that disease but added that there was no evidence to *prove* this.[46] In other words, the epidemiologic data were suggestive but their implications had not been confirmed in wards or 'labs'—the same obstacle that had long prevented acceptance of animalcular hypotheses in general. Woodward's procedure here was within the tradition of scientific empiricism: he reasoned well from observed facts but declined to accept conclusions not fully validated.

[44] Dora B. Weiner, *Raspail: Scientist and Reformer* (New York, 1968), cf. chapters 4 and 6. On the spread of Broussais' system, consult an anonymous work, transl. from French as *Conversations on the Theory and Practice of Physiological Medicine* (London, 1825), pp. 297 ff.

[45] Shryock, 'A Medical Perspective on the Civil War,' *American Quarterly*, XIV (1962), 161 ff.; reprinted in *Medicine in America* (Baltimore, 1966). On Bartlett's limitations, see King, 'Medical Philosophy,' pp. 156–159. Contemporary reviews of Bartlett's work were generally but not entirely favorable. 'G.C.S.' [G.C. Shattuck?], in the *American Journal of Medical Science*, n.s., X (1845), 143, e.g., agreed with his criticisms of dogmatists—mentioning Gallup, Miner, and Thomson in this connection—but thought he went too far in not recognizing 'intuitive powers' [working hypotheses?] whose insights might be confirmed.

[46] J. J. Woodward, *The Chief Camp Diseases of the United States Armies* (Philadelphia, 1863, Reprint, Hafner, for New York Academy of Medicine, 1964), p. 36.

One could hold, no doubt, that an empiricist might *assume* some final fact needed for a new synthesis—as did such luminaries as William Harvey, Dmitri Mendeleev, and Charles Darwin—but good method in itself hardly *required* venturesome behavior of this sort. Unless guided by genius, this way might lead back into uncritical speculation.

It is obvious enough why medical scientists could not be reduced to the unthinking role prescribed by Bartlett. First, as Gregory had remarked, it was difficult for thoughtful men not to reason in one way or another. Moreover, when research transcended the passive observation of bedside and postmortem phenomena and moved into etiology, physiology, and other complex fields, it was impossible to proceed without imagination and working hypotheses.[47] Doubts about such research in Paris retarded French medicine after 1840, despite the brilliance of such men as Magendie, Raspail, and Claude Bernard. It is true that French 'hospital medicine' had provided the first firm, localized pathology and hence the weakening of speculation in that central field. But 'laboratory medicine,' through the use of microscopes, expanded descriptive horizons to include minute as well as gross phenomena. This was done in terms of what is usually called 'Virchow's cellular pathology.'[48]

It is relevant to note, here, that cellular pathology owed something to earlier rationalism as well as to sound empiricism. Thus, late stages of the *Naturphilosophie* involved a theory that pathology was altered physiology (rather than simply altered anatomy), and this view was taken over by German scientists. Although the latter proceeded empirically, the *Na-*

[47] C. V. Daremberg pointed out as early as 1850 that physiology would revolutionize traditional medicine, as no advances in anatomy could do: *Essai sur La Determination et les Charactères de l'Histoire de la Médecine* (Paris, 1850), p. 40.

[48] These trends are traced with unusual insight by Knud Faber of Copenhagen, in his classic *Nosography* (New York, 1930). Virchow, of course, had precursors, including the Frenchman Raspail as well as German colleagues, see Weiner, *Raspail*; similar claims were once made *re* Dutrochet, see A. R. Rich, 'The Place of R.J.H. Dutrochet in the Development of the Cell Theory,' *Bulletin of the Johns Hopkins Hospital*, XXXIX (1926), 330.

turphilosophie seems to have provided working hypotheses: awareness of a theory now guided efforts to prove it. Earlier rationalism also inspired other ideas which promoted subsequent research.[49]

Medical bacteriology, about which there had been some reasoning for at least one hundred and fifty years, was finally established during the 1870s. This achievement followed on the convergence of a number of trends: on general advances in micro-biology, on observations of large parasites, on the postulates of Jacob Henle (1840), and on a shift in focus from the field (epidemiology) into the laboratory. Bacteriology demonstrated causal factors in infections—then the most feared conditions—and so revealed an exciting panorama of future prevention and cures. At the same time, these developments again encouraged 'ontologic' concepts: had it not turned out that diseases *were* real things, incarnate in microorganisms, rather than just bodily reactions to adverse stimuli?[50]

The importance of bacteriology here, however, is not its bearing on the nature of illness, but rather that it rounded out, at least for the time being, a valid synthesis of many morbid phenomena. This synthesis supplemented both gross and cellular pathology, in that the identification and possible prevention or cure of specific diseases was greatly advanced by awareness of causal factors. Bacteriology finally provided rational knowledge of infectious diseases—a need which had been met in the past only by a denial of contagion or by bizarre conjectures about 'fevers.' Fanciful reasoning was no longer necessary: hence, with one possible exception, scientific empiricism was not seriously challenged in regular medicine after 1875.

[49] Martin Müller noted this general relationship in *Über die Philosophischen Anschauungen des...Johannes Müller* (Leipzig, 1927); and Diepgen also commented on it in *Medizin*. See especially W. Pagel, 'The Speculative Basis of Modern Pathology,' *Bulletin of the History of Medicine* (1945), 3–40. The latter states on pages 38–40 that the German F. Jahn outlined the cell theory clearly in 1843, in the spirit of the *Naturphilosophie;* and that Virchow just filled in empirical data needed to make it acceptable 'in a scientific age.'

[50] O. Temkin has pointed out that either of these views may be employed, according to circumstances; see his paper in Crombie, *Scientific Change*.

It is quite true, as Lloyd Stevenson has pointed out, that both cellular pathology and bacteriology themselves constituted systems of a sort; and that, in this respect, even Virchow or Pasteur could be termed system-makers. Both men established syntheses which promised general understanding of pathology and therapy, much as had Rush or even Broussais in his day. Here again, nevertheless, there seems to be a methodologic distinction. The latter two, as noted, presented a claim to final truth: there was no tolerance of criticism nor expectation of revisions. No doubt neither Virchow nor Pasteur *enjoyed* criticism, but both must have expected it and also looked forward to continued investigations. Neither held that he had come up with one ultimate cause or cure: what they thought was that they had found the most promising *clues* to future research. They envisaged acceptance of their syntheses in terms of scientific evidence rather than of authority or faith.

It did not follow that unchecked speculation disappeared altogether from the American scene after 1850 or even after 1900. Not only did homeopathy and a vaguely-defined sect known as eclecticism retain distinctive characteristics, but a series of healing cults flourished throughout the nineteenth and into the present century. These emphasized one cause or condition of illness, and/or one type of cure, and in this respect resembled earlier 'systems' *within* regular medicine. But, lacking technical knowledge, the cults were in some ways closer to crude empiricism. Although several such heresies originated in Europe, equalitarian America offered a lush soil for their growth.[51] Thomsonianism (botanic medicine) and hydropathy ('the water cure') have been most discussed, though 'psychology' (mesmerism), chrono-thermalism, and Sylvester Graham's program (the 'Graham crackers' appeal) had their followers. These movements shared the merits of mild therapy,

[51] See, e.g., Shryock, *Medicine and Society*, pp. 144–146. Russian medical science is not under consideration here, though some Western critics viewed official Russian physiology as reverting to dogmatism by 1950.

and Grahamism was salutary in its personal hygiene. The Chrono-Thermal system, inspired by Dr. Samuel Dickson of Scotland, was interesting in that its one basis of illness was body temperature and its one clue to cures the use of quinine.[52] But Thomsonianism remains, on the whole, the most intriguing of the lot.

It seems paradoxical that the American founder of botanic medicine began his career as a 'mere empiric' and yet later reverted to ancient theories. But, as noted, all thoughtful men must reason at times, and perhaps Samuel Thomson was in his own way a thoughtful man. Interested as a farm boy in plants, he found species which made perfect remedies—the old approach of herbalists, into which his enthusiasm infused new life. Unlike most folk practitioners, Thomson attracted a wide following after publishing an account of his discoveries. This work was remarkable, not so much for its trial-and-error gropings as for its unintended tribute to speculation; that is, as evidence that even a man who disdained formal learning might seek theoretical underpinnings.

Thomson's discoveries, announced as early as 1822, were reminiscent of Galenic doctrines which had doubtless seeped into folklore over the centuries. To begin with, he held that the best drugs were of vegetable origin. More basic was his view that there were just four elements in Nature—earth, air, fire, and water, with the corresponding qualities of hot, cold, and so on. But what first demanded medical attention was cold, since that caused all illness. Thomson, like most sectarians, was intent on demolishing 'regular' medicine, and he therefore illustrated his principles by noting the damage done by orthodox bleeding and purging: 'Taking away the blood', he noted, 'reduces the heat and gives power to the cold...and the coldness

[52]Shryock, 'Sylvester Graham and the Popular Health Movement, 1830–1870,' *Mississippi Valley Historical Review*, XVIII (1931), reprinted in *Medicine in America* (Baltimore, 1966); S. Dickson, *The Fallacy of the Art of Physic* (1836); S. J. Rose, *The Reformed Practice of Medicine* (Philadelphia, 1845); C. S. Bryan, 'Dr. Samuel Dickson,' *Bulletin of the History of Medicine*, XLII (Jan. 1968), 24–39.

of the stomach causes canker; the physic drives all the determining powers... inwardly, and scatters the canker through the stomach...which holds the cold on the inside.' Perspiration then ceases and a settled fever occurs. But 'my experience taught me to give hot medicine to drive the cold out.'[53] At this point, the 'mere empiric' had moved full circle from haphazard observations to theoretical doctrines: the 'practical' man had adopted dogmas of whose long history he was probably unaware.

Thomson was more original in organizing his program: he apparently was the only American who ever patented a form of medical practice. Later followers modified or abandoned his theories and so became less sharply distinct from 'regulars.'[54] Whether 'botanics' were in any way absorbed into orthodox practice is uncertain, but in effect their sect died off gradually after 1850. The earlier apparatus of an organized guild, schools, societies, and journals, withered away.

The same thing may be said of hydropathy, which as a dogma never rose much above crude empiricism but which tended to combine with other fringe sects in a curious eclecticism. A college which merged water cures with the latest hygienic enthusiasms was chartered by New York State as late as 1861, with a right to grant the M.D. degree.[55] Vestiges of hydropathy persisted in some spas, but became hard to distinguish from modern hydrotherapy. Another drugless sect appeared later in the form of naturopathy, which set up several schools but now seems to be disappearing. Whether this program was akin to *Naturheilung*, popular in Germany under the Nazi regime, is not clear.

Scientific empiricism weakened the more naïve cults and was not without influence on sects which continued to survive.

[53] S. A. Thomson, *Narrative* (Boston, 1822), p. 27.

[54] A. Berman, 'Neo-Thomsonianism in the United States,' *Journal of the History of Medicine*, XI (1956), *passim*.

[55] This 'college' had first been a water-cure establishment and then a 'hygienic institute.' Its presiding genius was T. H. Trall, who—even as Thomson and the later Andrew Still—finally proclaimed a theory of pathology and cure. Trall was picturesque as well as imaginative; see Shryock, 'Graham,' pp. 180–181.

When bacteriology led to asceptic surgery after 1880, and also combined empirical sanitation with a rational public-health program, 'the wonders of modern medicine' began to dawn on an educated public. These values also became apparent to homeopathic and eclectic practitioners: Homeopaths in particular, who maintained their own hospitals and schools, were adopting regular medicine by the early 1900s. Along the way, the dogmas of an earlier day disappeared so gradually that it is hard to say when the process was completed.[56]

In view of these trends, one might have thought by 1890 that sectarianism was on the way out. Yet at about that time two new and relatively successful cults appeared in the United States. Meeting different needs, these represented opposite extremes of the methodologic spectrum. Osteopathy originated in the mundane, trial-and-error efforts of Andrew T. Still, while 'Christian Science' in contrast was based on Mrs. Eddy's idealistic metaphysics. Both were reminiscent of earlier movements. Still was similar in some ways to Thomson;[57] while Mrs. Eddy's views transcended transcendentalism and also echoed remote forms of religious healing.[58]

The basic doctrine of Doctor Still, who had had some medical training, was that 'all the remedies necessary to health exist in the human body... they can be administered by adjusting the body in such condition that the remedies may naturally associate themselves together... and relieve the afflicted.' This was the positive aspect of a view that structural difficulties in bones and joints, which in turn affected various organs, were the chief causes of illness. Treatment originally emphasized a manipulation of the skeleton—particularly of the spine. Here,

[56]As a plausible date, however, recall the statement of Dr. W. J. Mayo in 1921 that 'today homeopathy is a part of regular medicine,' *Journal of the American Medical Association*, LXXVI, 923.

[57]See the account of early experiences in A. T. Still, *Autobiography* (Kirkville, Missouri, 1897), *passim.*

[58]There are favorable biographies by Christian Scientists. For a critical interpretation, see E. S. Bates and J. V. Dittemore, *Mary Baker Eddy: The Truth and the Tradition* (New York, 1932).

again, was a monistic emphasis upon one body system, albeit the focus was no longer on such old favorites as the nerves or the gastro-intestinal tract. Formal teaching of the program began at the American College of Osteopathy, chartered at Kirksville, Missouri, in 1892.[59]

The subsequent spread of this sect, with the founding of several other schools, was surprising in view of the growing strength of medical science. Certain possible explanations, however, may be suggested: (1) the demise of Thomsonianism, and the merging of homeopathy with regular medicine, left unmet the needs of those who sought moral support from another 'reformation'; (2) there were doubtless some patients who were helped by this type of orthopedic specialization; and (3) osteopathy gradually absorbed, after the first generation of practitioners, the advances made in scientific medicine. In this last respect, the late arrival of Dr. Still's movement proved an advantage: orthodox practice would have had little to offer it before 1885.

Osteopathy thus resembled Thomsonianism in beginning as crude empiricism and then resorting to over-simplified conjectures. In borrowing from regular medicine, however, osteopathy followed the example of homeopathy. By mid-twentieth century, osteopathic colleges appeared to outside observers as grade B medical schools. There is no study of just how the transformation was accomplished within either homeopathy or osteopathy, but this is a matter of professional history rather than of medical thought.

In the case of these major sects, what is here termed borrowing has been viewed by some as a merging of principles. Each of these programs, it is held, gave at the same time that it received. Homeopaths, for example, may claim priority in using high dilutions, or in relation to 'like-cures-like' phenomena in

[59]For sympathetic comments, see G. W. Northup, D.O., *Osteopathic Medicine: An American Reformation* (Chicago, 1966); and D. B. Thorburn, 'The Case for Osteopathy,' *American Mercury*, LXX (Jan., 1950), 32–42. 'Regular' criticism of the sect is expressed in J. D. Wasserug, 'The Medical Position: A Reply,' *ibid.*, 42–50.

vaccines. Such claims are dubious, since most drugs are not effective in infinitesimal doses and vaccines are not cures. Yet the latter do involve a principle that 'like *prevents* like,' which might be considered a late corollary of homeopathy. Moreover, the latter's encouragement of milder therapy, already mentioned, was doubtless of real service; indeed, homeopaths probably accomplished more in this manner than did nihilistic clinicians. Similar comments may be made on other sects. The Grahamites, for instance, anticipated in part the value of vitamins; while osteopathy may have pressured some regulars into giving more heed to orthopedic practice.

In striking a balance, however, it may be repeated that sectarians made few if any scientific contributions; and that there was less excuse for doctrinaire teachings after 1850 than there had been before that time. Whether competition with sectarians helped or hindered well-trained doctors, whose practice was based increasingly on scientific empiricism, is difficult to say. In some cases, this competition probably weakened support for orthodox institutions; but, on the other hand, it may have indirectly improved the work of physicians by denying them a complete monopoly. One says 'complete' here because the regulars had long composed the great majority of practitioners. By 1930, for example, there were in the United States some 121,000 physicians, but only about 36,000 recognized sectarians. Among these irregulars were 10,000 religious healers, 7,700 osteopaths, 2,500 naturopaths, and 16,000 chiropractors, the latter employing a type of practice similar to that followed originally in osteopathy.[60] The proportion of sectarians as well as of poorly-trained regulars was probably smaller by this time than it had been a century before, what with advances in scientific knowledge and reforms in licensing procedures.[61]

[60] *Medical Care for the American People:* Final Report of the Committee on the Costs of Medical Care (Chicago, 1932), p. 4.

[61] Jacob Bigelow estimated in 1844 that at least three-fourths of New England people were in the hands of the regulars, 'Introductory Lecture, The Medical College, Boston

The one instance in which unverified speculation seemed to reappear, *within* regular medicine, was involved in Dr. Sigmund Freud's formulation of psychoanalysis. The term 'seemed' is used advisedly, since the value of both theory and practice in this specialty are still matters of opinion. Discussions, pro and con, have usually related to scientific method as such and to the values or dangers revealed in clinical experience. These issues are indeed central but they have usually been examined without reference to historical backgrounds. Few analysts or critics seem to have read such earlier systematists as Cullen, Rush, or Brown as a means of approaching Freud. If the latter is first encountered in this manner, however, the reader feels that he is still immersed in the medical reasoning of 1800. Obvious differences will be noted, of course, between the thought of Rush or of Cullen and that of Freud a century later. Yet there are such seeming analogies in their claims, personalities, and methods as to justify the question: Can Freud's work in psychiatry be viewed as a throw-back to, or a persistence of, earlier modes of medical speculation?

The main difficulty in psychiatry was obvious: it was by definition caught up in the dualism of body and mind. In consequence, either a physical or a psychologic approach could be employed, and emphasis in the field swung back and forth after 1800 between one pole and the other. (J. C. Whitehorn refers to these extremes as, respectively, psychophobia and psychomania.) Prior to about 1820, most of the mentally ill had been treated, so far as medicine was concerned, by the same methods employed in general practice. Because these methods proved ineffective, and were associated with much neglect and brutality, reformers turned to a psychologic approach in the form of

[Harvard],' *Boston Medical and Surgical Journal*, XXX (1844), 344. This would place the percentage of sectarians only slightly higher than that indicated in 1930, but New England was not necessarily typical. The concept of 'regular,' moreover, was less sharply defined in 1844 than in 1930. On licensing after 1875, see Shryock, *Medical Licensing in America: 1650–1965* (Baltimore, 1967), pp. 43–76.

'moral treatment' (kindness); but the high hopes entertained for this were only partially realized.[62] Meantime, by 1850, somatic medicine was making marked advances in pathology; and psychiatry, following this lead, adopted a neurologic approach. But this is turn proved disappointing by 1890: no brain lesions were found to correlate with such phenomena as depressions or paranoia. As a result of the further disillusionment, most doctors just referred psychotic patients to asylums and dismissed neurotic symptoms as mere 'nerves.' Unfortunately, such indifference did not make the victims go away. Indeed, there was even a popular impression by the early 1900s that mental illness was increasing. Since something had to be done, another return to psychologic strategy seemed indicated.

The term 'psychologic' is used here, of course, only in a broad sense as non-somatic: it is not to be equated with psychology as a discipline. One major aspect of the story, indeed, was the natural lack of early contacts between subjective psychiatry and objective psychology[63], to say nothing of the gap between the former and traditional neurology. The situation was further complicated by professional factors: 'practicing psychologists' began to work independently from psychiatrists. The whole area was extraordinarily complex and ill-defined: neither the somatic nor the non-somatic were ever entirely ignored, and attempts were made to overcome dualism altogether, as in Adolph Meyer's 'psychobiology.'[64]

In a broad sense, nevertheless, one may repeat that a return to non-somatic approaches was in order. Revivals of religious healing in the United States, particularly Christian Science but

[62] See J. K. Hall, G. Zilboorg, and H. A. Bunker, eds., *One Hundred Years of American Psychiatry* (New York, 1944), especially W. Malamud, 'The History of Psychiatric Therapies;' J. C. Whitehorn, 'A Century of Psychiatric Research;' and T. V. Moore, 'A Century of Psychology.' Also, G. N. Grob, *The State and the Mentally Ill... 1830–1920* (Chapel Hill, 1966), chapters 2 and 6.

[63] Note O. M. Marx, 'American Psychiatry Without William James,' *Bulletin of the History of Medicine*, XLII (Jan-.Feb., 1968), 52–61. Also, Moore, 'Century,' pp. 471–477; but cf. D. Shakow's review of this volume in *Psychological Bulletin*, XLII (July, 1945), 427–431.

[64] Whitehorn, 'Research,' p. 172.

also the so-called Emanuel Movement, may be viewed as lay gestures in this direction. Within medicine, even while neurologic orientations were still dominant, there was a revival of hypnotism in treating hysteria, by this time as a scientific rather than as an occult or quackish procedure. It was this revival which first attracted Freud into the field.

The manner in which the Viennese doctor arrived at his theories is too well known to bear repetition here. One need only recall that he found hypnotism less effective with neuroses that were reminiscenses of patients in so-called 'free associations.' Since much that had been lost to conscious memory came to the surface in these sessions, and because the content so recovered often seemed to relate to sex, Freud arrived at an unusual explanation. Neurotic persons, he held, had suppressed early sexual memories because of cultural inhibitions; but these thoughts had then taken refuge in 'the unconscious mind,' whence they later emerged furtively in dreams and in neurotic behavior. Most exciting was the seeming confirmation of this theory in practice; that is, the discovery that when some patients *did* recall—under proper management—the unconscious was apparently relieved of tensions and improvement ensued.

Although few of these ideas were entirely new and Freud has been accused of not acknowledging precursors, his general synthesis was novel and imaginative. It also proved in tune with the times. There certainly was need for help in relation to neuroses, though it would have been more promising if analysis could have envisaged aid for psychoses as well. But apart from strictly medical results, there were other aspects of analysis which encouraged its acceptance. The boldness of the doctrine, to say nothing of a vocabulary which was striking even if unnecessary, impressed various laymen. Moreover, the emphasis to which some persons objected, at the same time attracted others. The very fact that Edwardians disliked what seemed an obsession with sex, doubtless intrigued those who wished to appear daring and sophisticated. In this setting, analysis was encouraged by franker attitudes which had gained ground over the preceding half-century.[65]

Freud conceived his theories by about 1895. Working at first in isolation, he was joined between 1902 and 1908 by a small group of followers; and the first conference on psychoanalysis was then held and the first journal founded. At the invitation of the psychologist G. S. Hall, Freud lectured at Clark University in 1909, and his ideas infiltrated into the States thereafter. By the 1920s analysis became a vogue: it was rather fashionable to 'have an analysis,' and the couch became a guild symbol in psychiatry even as was the stethoscope for medicine at large.

All this was more true of the United States than of most European lands; and labored efforts to explain this pointed to the much-abused 'Puritan heritage,' as well as to such other factors as the influence exerted by women or the peculiar need of middle-class Americans 'to find themselves.'[66] The social history invoked here, by the way, was none too good: apparently Puritanism was confused with Victorianism. The real Puritans had rarely hesitated to call a spade a spade!

Despite the favorable attitudes mentioned, the reaction of many scientists and laymen to analysis was most skeptical. The chief reason for this was the fact that Freud offered no such confirmation of either theory or practice as had long been demanded in medical research.[67] This was not necessarily Freud's fault: rather could it be ascribed to the relatively puzzling nature of mental illness. Psychiatric nosology was still vague in 1900 (as somatic nosology had been in 1800), since diseases

[65] Recall, for example, the educational tracts of Sylvester Graham before 1850, Walt Whitman's verse of the 1860s and 1870s, and such later scientific writers as Kraft-Ebing and Havelock Ellis.

[66] See, e.g., H. M. Ruitenbeek, *Freud and America* (New York, 1966), pp. 17, 34; J. C. Burnham, *Psychoanalysis and American Medicine, 1894–1918* (New York, 1967), chapters 1 and 2.

[67] Among others, J. F. Braun, 'Freud and the Scientific Method,' *Philosophy of Science*, I (New York, 1934), 323 ff.; C. Landis, 'Psychoanalysis and Scientific Method,' *Proceedings of the American Philosophical Society*, XLVIII (1941), 515 ff.

could be identified only by confusing symptoms. In consequence, firm clinical or public-health statistics were unavailable. So subjective, moreover, were the data of analysis that experimentation was unusually difficult.

Hence Freud and his followers had to justify themselves, at first, simply by claims about their own practice. Such evidence is often unreliable, unfortunately, or, if trustworthy, irrelevant. It is a truism that an enthusiastic healer can often secure at least temporary, favorable responses regardless of his methods. Perhaps for this reason, there was no world-wide consensus among psychiatrists as to the significance of Freud's cases. He could, nevertheless, have presented his view as a working hypothesis offered in the absence of anything more promising, a procedure expected in scientific empiricism. Instead, Freud and his friends insisted that he had discovered one basic, pathologic situation and one type of cure deduced therefrom, which were applicable to most neurotic conditions. In so doing, he had at once established truth, had brought about a reformation in his field. The similarity of this pattern to that of earlier systematists or sectarians seems obvious enough.

This similarity becomes even more obvious when one recalls attendant circumstances; for example, the appearance of disciples devoted to the true faith, the subsequent defections, and Freud's condemnation of such backsliders.[68] Even the fact that his supporters became known as Freudians, a term reminiscent of earlier 'Brunonians' or 'Thomsonians,' may have some significance. In contrast, one does not discuss Pasteurians or Oslerites. Nor did men refer to anatomic or immunization 'movements' in medicine, as Freudians referred to 'the psychoanalytic movement.' True, the terms 'Darwinians' or 'Darwinism' were used for a time, but only for so long as many able men viewed Darwin's ideas as unconfirmed.

[68]As in his comments on Adler, in *History of the Psychoanalytic Movement*, transl. by A. A. Brill, Nervous and Mental Disease Monograph Ser. No. 25 (New York, 1917), p. 43. I am indebted to Mrs. Julianne Pearson for notes on this work; and to Dr. David Musto for stimulating discussions of Freud and of other systematizers.

In view of all these phenomena, one may say that the spirit of speculative rationalsim, banned from general medicine after 1850, returned after 1900 to haunt a specialty in which more promising procedures were not yet at hand. In even more figurative language, to quote a statement I once made in this context, analysis could only advance by reverting to 'more elementary scientific methods that had been tried and superceded in somatic medicine—a reversion . . . 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 . . . [fighting] in the manner of preceding centuries.'[69]

Unlike sectarians, however, Freud and those doctors who upheld him were never excluded from the medical profession. They did found certain institutes in the States which were independent of medical schools, an unusual gesture by that time, but what this meant was not always clear. The survival of psychoanalysis *within* American medicine can be plausibly ascribed to the lack of promising alternatives, and also to the fact that this program, unlike homeopathy or osteopathy, never threatened the regular profession as a whole.

The survival of analysis within the profession also may be ascribed to adjustments which ensued. Freud and his colleagues were aware, as early systematists could not be, of the sort of verification demanded in scientific medicine. They therefore took note of such criticisms as have been mentioned. In his *History* of the movement, as in the revised edition of his *Introduction*, Freud's self-assurance continued to verge on dogmatism but he did make some modest gestures. He stated, for example, that he had never proposed to give 'a perfect theory of human life' and that analysis was careful not to become a 'system,' though Adler's theory was just that! One is tempted to view this last remark as a choice example of what analysts

[69] *The Development of Modern Medicine* (Philadelphia, 1936), p. 347.

themselves called 'projection.' Freud also noted that some American doctors had opposed his views because of a lack of experimental proofs, to which he retorted that neither did astronomy enjoy such support. He did, nevertheless, welcome the first claims that experiments had confirmed certain of his concepts.[70]

Many attempts were indeed made, between the 1930s and 1950s, to meet the usual canons of research by experimentation, on both animal and human subjects. Typical of these were ingenious tests of dream symbolism, once again employing hypnotism, which came up at times with plausible results. Elements of conjecture or uncertainty, however, still persisted.[71]

Meantime, perhaps in response to criticism or else because of limitations encountered in practice, some psychiatrists reacted against emphasis upon subjective experience by embracing objective, behavioristic concepts. Others began to modify or supplement analysis with other types of treatment, as in the introduction of new drug therapy which had ameliorative if not curative values.

The view adopted by many psychiatrists in the 1960s implies that analysis did provide some concepts and treatments still found useful. Yet Freud is no longer, in American psychiatry, the great father-figure that he once was. It is recognized that, although psychoanalysis may leave a residuum of continuing value, there is much that is doctrinaire and exaggerated in its teachings. These conclusions are now expressed by both critical psychiatrists and by medical historians and there have also been signs of popular disillusionment.[72]

[70] See n. 68, above; also Freud's *New Introductory Lectures on Psychoanalysis*, transl. by W. J. H. Sprott (New York, 1933), e.g., pp. 25, 36, 38–39.

[71] Interesting here are the essays in E. Pumpian-Midlen, ed., *Psychoanalysis as Science* (Stanford, 1952), especially E. R. Hilgard, 'Experimental Approaches to Psychoanalysis' and the editor's 'The Position of Psychiatry in Relation to the Biological and Social Sciences,' e.g., pp. 3–16, and 125–50.

[72] See, e.g., J. C. Whitehorn, *Psychiatric Education and Progress* (Springfield, Ill., 1957), pp. 15, 18–19, 39, etc.; E. Ackerknecht, *Short History of Psychiatry*, transl. by S. Wolff (New York, 1959), p. 84; 'Pop-Psych,' *Time*, Oct. 7, 1966; J. Leo, 'Psychoanalysis Reaches a Crossroad,' *New York Times*, Aug. 4, 1968.

Although Freud ceased to be the father-figure in psychiatry, and although analysis continues to be ridiculed by many able physicians,[73] his influence outside medicine has been phenomenal. This aspect of the story is of only tangential interest here, but cannot be ignored because there are no complete analogies in the history of earlier, medical systems or sects. Mesmerism may offer some parallels, but the nearest analogy is that of nineteenth-century phrenology. The latter, originating in medicine, did not become a healing cult, and hence has not been discussed here, but it was at first welcomed by many scientific and literary leaders of its era.[74] Finally abandoned because of absurdities as a popular cult, it nevertheless provided the first attempt to correlate brain structure and thought processes; and it may be viewed as having posed, eventually, the whole problem of cerebral localization.[75] Thus, even as psychoanalysis, it can be in retrospect either condemned or praised.

The somewhat similar responses to phrenology and to psychoanalysis, despite an interval of more than half a century, can be accounted for by the fact that each offered what, in over-simplified terms, was a new theory of human nature. The analytic view, of course, was the more sophisticated of the two, involving as it did motivations determined for the individual by a non-rational 'unconscious' of which he was not even aware. What a break with free-will doctrines, yet also with the intellectual traditions of an Age of Reason! The many irrationalities of human conduct, even among those generally considered sane, were brought more into the open and reinterpreted.

Naturally enough, professions which sought to deal intelligently with human nature—authors, social scientists, clergy-

[73] Thus, Sir Peter Medawar of London declared recently that, before looking into the matter, he had had no idea of what 'treasures of nonsense' could be found in the analytic literature. Perhaps the most devastating critique is Percival Bailey's 'The Great Psychiatric Revolution,' *American Journal of Psychiatry*, CXIII (1956), 387–406.

[74] J. D. Davies, *Phrenology, Fad and Science: A Nineteenth-Century American Crusade* (New Haven, 1955), *passim;* K. M. Dallenbach, 'Phrenology versus Psychoanalysis,' *American Journal of Psychology*, LXVIII (Dec., 1955), 512–525.

[75] O. Temkin, 'Gall and the Phrenological Movement,' *Bulletin of the History of Medicine*, XXI (1947), 275–321.

men—reacted for or against the new revelation. Men within these fields who favored Freudianism, not being subject to restraints imposed in natural science, carried their psychomania to extremes. Note the manner in which some literary figures eventually reveled in a blatant exploitation of sex. Various factors were involved in such reactions but analysis was one of the most obvious.

These trends entertained an 'emancipated' generation and may not have been without impact on the social environment as a whole. Most specific, perhaps, was the influence exerted by analysis upon social scientists. Some of these scholars probably reacted as did literary lights: they welcomed identification with the latest avant-garde. But in certain fields, notably in anthropology, Freudian ideas did seem to offer clues to the understanding of puzzling behavior.[76] Such clues might be followed up among advanced as well as within primitive societies, and so interested a number of historians; though attempts to 'analyze' persons long deceased did not impress most members of that guild.[77] Meanwhile, a few ambitious, somewhat Freudian syntheses within anthropology or sociology seemed analogous to the thought of doctrinaire psychoanalysts.

Not all social scientists who accepted analysis first demanded evidence that its doctrines, originally medical in nature, were medically sound. Perhaps they assumed that since it had the imprimatur of many psychiatrists and therefore, presumably, of the medical guild, its credentials need not be questioned. Such an attitude would have been encouraged by the growing enthusiasm for interdisciplinary studies, characteristic of the decades after 1920. Or it may be that some social thinkers were

[76] Recall, e.g., E. Sapir, 'Cultural Anthropology and Psychiatry,' *Journal of Abnormal and Social Psychology*, XXVII (1932), 235; A. I. Hallowell, 'Culture and Mental Disorder,' *ibid.*, XXIX (1934), 1 ff.; and the summarized interpretation in C. Kluckholn, 'The Influence of Psychiatry on Anthropology in America,' in Zilboorg et al., *Hundred Years*, pp. 589–617.

[77] See, notably W. L. Langer, 'The Next Assignment,' *American Historical Review*, LXIII (Jan., 1958), 283–304; Freud's interest in applying his views to historiography is noted on p. 389.

simply practical: they thought analysis useful and so did not worry about its rationale. Who cared if resort to it proved right for the wrong reasons, as long as it 'worked' (crude empiricism?). On a more intellectual level, social scientists or literary men may have decided that Freudian views were persuasive in themselves, regardless of whether their original, clinical validity had been confirmed or not.[78]

Intellectuals who followed Freud were not always consistent, among themselves, in reasons given for their enthusiasm. Some held that his medical findings remained the chief justification for the whole program, and that the validity of these efforts must either be assumed or defended in principle. Others, although convinced of the soundness of doctrines, implied that this could not be verified by the sort of evidence expected in natural science. Rather should it be tested by such partial evidence and general persuasiveness as are still accepted in much social science and historiography. Freud was said, for example, to have attained a status similar to that of Karl Marx rather than to that of Newton, and the success of psychoanalysis was even ascribed to disenchantment with Marx![79]

This view, however, seems to give the case away. Few Western scholars, other than devoted socialists, would now hold that Marx's doctrines were ever *scientifically* confirmed. In other words, why claim that interpretations in such fields as psychoanalysis, history, or some aspects of behavioral science can yet transcend, except in limited areas, the non-quantitative and non-experimental levels on which these disciplines usually operate?[80] By way of comparison with analysis, in this connection, consider the case of historical writing itself. As long as

[78]E.g., Ruitenbeeck, *Freud, passim;* F. J. Hoffman, *Freudianism and the Literary Mind*, 2nd ed. (Louisiana State Univ. Press, 1967), especially chapters 1, 2, and 4; and P. Roazen, *Freud: Political and Social Thought* (New York, 1968), *passim.*

[79]Ruitenbeeck, e.g., *Freud*, insists on the validity of the original clinical work; while Roazen, equally positive, draws the analogy with Marx, *Freud*, pp. 5–7.

[80]This is the view implied in Hilgard, 'Approaches'. Note also O. Cope, *Man, Mind, and Medicine* (Philadelphia, 1968), pp. 86 ff., who states that psychiatry requires more theory than does somatic medicine.

the latter served mainly as a form of philosophy or literature, scientific criteria were simply irrelevant. Even today, although science has influenced history to an increasing degree, the field remains 'immature' if judged by scientific standards—divided, as it still is at times, between imaginative theories and extreme empiricism.[81]

Psychoanalysis, nevertheless, is in a less advantageous position than is historiography. The latter has an established position among the humanities, whether or not it can at times also assume a scientific stance. Analysis, in contrast, evolved within an area in just the reverse position; that is, medicine remains primarily a bio-physical field which overlaps only here and there, however significantly, with behavioral science or the humanities. In other words, to say that analysis is sometimes helpful even if not a firmly established specialty, does not enable it to rise above the historic role of a 'system,' with all the attendant risks and limitations. The only apparent means for escaping from this situation are (1) to achieve more objective verification than has yet been secured, or (2) to question modern scientific methods in general—the latter a sort of defensive, flanking movement in epistemology which, it is said, has been attempted.[82]

In conclusion, one may summarize the respective values and limitations of the three approaches to medicine which have now been considered; that is, of crude empiricism, of dogmatic rationalism, and of scientific empiricism. The last of these can be dismissed briefly, since it is now usually followed in all research as far as is feasible. Just because it is taken for granted,

[81] Most interpretations of 'the meaning' of world history have necessarily become 'systems;' see, e.g., P. Geyl, 'Toynbee's System of Civilizations,' in M.F.A. Montagu, ed., *Toynbee and History: Critical Essays* (Boston, 1956), especially pp. 44–45. In recent decades, a belated but promising interest in quantitative methods in historiography has finally gained some momentum. But cf. Arnold Toynbee's remarks, 'Can We Know the Pattern of the Past—A Debate,' in P. Gardiner, ed., *Theories of History* (Glencoe, Ill., 1959), p. 318.

[82] As in M. Grene, ed., *Toward a Unity of Knowledge*, Psychological Issues, VI, monograph 22 (1968), Parts I and II.

however, one needs to recall again that *the* scientific method (now so-called) is not fool proof; that a claim that it has been followed may be dubious at times; and that, in any case, circumstances are still encountered which necessitate resort to crude empiricism at one extreme or to unconfirmed speculation at the other. Something approaching the former is evident, for example, whenever a pharmacologist observes that 'we know nothing about the modus operandi of this drug' and can therefore 'use it only in an empirical manner.' Examples of unconfirmed speculation, at the other extreme, can also be recalled in twentieth-century medicine; for instance, Ehrlich's formulation of his elaborate side-chain theory in immunology.

Each of these illustrations suggests the historic and even continuing value of over-simple approaches in cases where scientific empiricism was not, or still is not, available. Obviously, men were indebted to crude empiricism, to accidents or to trial-and-error gropings now usually forgotten, for the early discovery of certain valuable drugs. As noted earlier, most substances which first appeared within folk medicine—the opiates, various purges, the specifics mercury and cinchona bark—can be included within this category. So true is this that efforts have been made, in the present century, to examine more carefully some of the materials reported in folklore.

What at least looks like folklore also made contributions to preventive measures, as in the awareness of contagion, inherited from both ancient and medieval experience, and resulting notification and isolation procedures. As late as the last century, such folk medicine was sometimes at odds with 'scientific' opinion in many countries. Between about 1820 and 1880, for example, physicians in northern Europe and in most of North America rejected earlier contagion doctrines, and ascribed even tuberculosis to hereditary factors. In southern Europe, however, popular belief in the infectious nature of that disease persisted; and it was this view which was finally confirmed by Koch and others in 1882.

There is something ironic in the spectacle, during the mid-nineteenth century, of British or American doctors solemnly informing patients that phthisis was inherited; while the plain people of Spain still insisted that it was infectious and demanded the destruction of a victim's belongings (fomites), as in the famous case of Chopin and George Sand in Majorca.[83] More spectacular, as an illustration of crude empiricism in a preventive program, was the procedure of innoculation against smallpox. When this became well known in the Western world about 1720, it elicited the first theory and practice in the whole field of immunology.

Valuable as such contributions were, however, one cannot forget the narrow limitations within which crude empiricism operated effectively. Thus, although useful drugs were occasionally turned up, the remedies employed in folklore provided only a little sense within a great mass of nonsense. The same statement, indeed, may be made of learned pharmacopoeias until at least 1850; since much of their contents derived simply from the 'experience' of doctors and this involved little more than trial-and-error adventures in practice.

Moreover, progress based on crude empiricism was extremely slow. This was partly a result of the multiplicity of remedies proposed. It was difficult, under the circumstances, to select the few worthwhile items until scientific empiricism, in the form of controlled experiments, took over. In consequence, promising remedies or procedures were sometimes ignored or later lost, because they did not stand out amid a chaos of claims. Buried in Cotton Mather's long list of drugs in 1725, for instance, was a recommendation to use citrus juice as a prevention of scurvy, the value of which was not finally demonstrated by the British navy until late in that century. In surgery, to cite a very different case, the value of maggots for cleansing wounds was stumbled upon by Civil War surgeons,

[83] R. and J. Dubos, *The White Plague* (Boston, 1952), *passim;* Shryock, *National Tuberculosis Association, 1904–1954* (New York, 1957), pp. 43–44.

then forgotten and rediscovered during World War I. Further examples of this sort are numerous.

The greatest weakness of crude empiricism, even when its findings were later proved valid, was the fact that these claims did not fit into any recognized synthesis. If the germ theory had been widely accepted in 1850, for instance, the occasional hint that epidemic cholera might be checked by drinking only sterile water might have been taken seriously.[84] As it was, the idea was lost amidst a welter of useless proposals. On the other hand, even a theory which was not fully confirmed might encourage promising research, as long as this was conducted within a plausible frame of reference. Thus, Ehrlich's side-chain hypothesis, while not actually verified, did become the starting point for Wassermann in establishing his test for syphilis.[85]

One should note that the term 'theory,' as just used, is not to be equated with dogmatic rationalism. Theories, as proposed in medicine since 1850, have usually been viewed as tentative and as subject to further checks. Even some of the speculations of the *Naturphilosophie*, as noted, suggested later empirical studies. Ideas which now seem fantastic occasionally played such a role; as when diseases were viewed as parasitic entities and the German Schönlein, following up this hint, demonstrated as early as 1839 that a skin disease (favus) actually was caused by a living parasite.[86]

When a theory was presented as absolute truth, as by most of the systematists and sectarians discussed above, it rarely stimulated investigations. Here, of course, was the great danger of dogmatic rationalism: In claiming final solutions, it tended to discourage further studies except for minor revisions of the original doctrine. Much the same results might ensue if a theory was so vague or subjective as to make tests difficult or

[84]See, e.g., *Southern Medical Reports* (New Orleans, 1850), report on Natchez.
[85]Garrison, *History*, p. 709.
[86]George Honigmann, *Geschichtliche Entwicklung der Medizin* (München, 1925), pp. 67–70.

impossible. What can be done, for example, with the following statement made by the psychiatrist Carl Jung in 1958?

> So long as a thing is in the unconscious it has no recognizable qualities and is in consequence merged with the universal unknown, with the conscious All and Nothing.... But as soon as the unconscious content enters the sphere of consciousness it has already split into the 'four,' that is to say it can be an object of experience by virtue of the four basic functions of consciousness [thinking, feeling, sensation, intuition].[87]

One is torn between viewing this pronouncement as obvious or as meaningless, but it is hard in either case to envisage reactions in psychologic research.

From time to time, imaginative scientists proposed ideas so long before there was any way to prove them, that they exerted no traceable influence on later developments. They were, it is always said, 'ahead of their time.' When Cotton Mather accepted the germ theory, for example, he promptly foresaw the promise of chemotherapy.[88] But here he simply made a logical deduction, and it would be difficult to show any connection between his 'potent worm killer' of 1725 and the eventual discovery of Ehrlich's 'magic bullet' (salvarsan) of 1910. If an unproved idea *continued* to appear, on the other hand, it seems likely that even intermittent speculation, combined perhaps with unsuccessful tests, kept the concept alive. This was probably true of sporadic discussions of the germ theory itself, from the late seventeenth down to the mid-nineteenth century. Continuity is even more clear when discussions and attempts at proof became more definite and frequent, as the final 'discovery' approached. This was apparently the case with theories about antibiotic drugs between about 1880 and 1935. Sequences might also be found in other connections, if serious efforts were

[87] Jung, *Flying Saucers* (New York, 1969), p. 109, from first Amer. ed., New York, 1959, transl. from German ed. (Zurich, 1958.) This work, called to my attention by Dr. L. U. Condon, presents a psychoanalytic type of interpretation. The quotation, although out of context, does not seem atypical of the work as a whole.

[88] Beall and Shryock, *Mather*, p. 90.

made to trace them. In a word, both continuity and non-continuity appear within the history of science.

A final word may be said in appreciation of those medical thinkers who, over the last three centuries, gradually learned to strike a methodologic balance between the blindness of 'empirics' and the fantasies of dogmatists. As suggested, moreover, even the latter did at times have something to contribute: 'empirics' were not always totally blind, nor were dogmatists always dogmatic. All this was realized, moreover, by able observers of the medical scene a century or more ago. Thus, Dr. Worthington Hooker of Yale University noted, in 1850, some of the very comments expressed here; for example, the criticism of unchecked theories, the explanation of their frequent popularity, and the qualifying caution that even a wild surmise might have its merits.[89] Indeed, the unexpected often awaits a scholar who, having worked out interpretations for himself, later encounters these same views within the period under consideration. Perhaps, a sobering thought, we exaggerate if we assume that an historian is always more far-seeing than was the wise, contemporary observer.

[89] W. Hooker, *Physician and Patient* (New York, 1849), Chapters 5, 6, 8, and 9.