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Benjamin Rush from the Perspective of the Twentieth Century*

By RICHARD HARRISON SHRYOCK, PH.D.

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IT has recently been observed that "One often hears and sees Rush's name mentioned, but in a few tiresomely repeated connections; as a man and as a writer he is little known".¹ This is doubtless true so far as the general public is concerned: in contrast to his friends John Adams and Jefferson, Rush has been largely forgotten even by the well-informed. This may be explained, in some measure, by the political emphasis which characterized American historical writing until recent decades. Despite some interesting adventures in public office, Rush was never a major political figure; hence he was neglected by writers primarily interested in constitutions, parties, and affairs of state.

During the present century, scholars concerned with special phases of American history, notably with educational and literary developments,² began to give Rush serious attention; and as the interests of general historians broadened, they noted his contributions to social reform.³ In 1934 Nathan Goodman brought out the first full-length biography, which presented the whole man "vigorous in mind and body, and heroic in stature."⁴ During the last few years, renewed interest has been manifested by literary historians. Notable is the current work of L. H. Butterfield, who, in cooperation with the American Philosophical Society, is preparing an edition of selected correspondence and also a Union Catalogue which will provide the

first systematic guide to Rush's voluminous writings.⁵ It seems likely, therefore, that Rush will receive wider attention in the future from scholars at large.

We are not primarily concerned here, however, with the rediscovery of the man by general and literary historians. In contrast with such writers, physicians and medical historians never forgot the Philadelphia leader, and comments on him have continued to appear ever since his death in 1813. An interesting essay could be prepared on the history of this literature *about* Rush; and this might throw light on changing medical perspectives over the years, as well as on the physician himself. One can only suggest the features that such an essay might assume. It would relate, first, to the often-controversial contemporary writings; next to the eulogies of 1813; then to more basic criticisms appearing after 1820, which swelled to a chorus of condemnation during the '40's and '50's. Unfavorable mid-century reactions reflected the growing influence of French medicine in America, which repudiated nearly everything for which Rush had stood. These reactions were typical of most professional appraisals until our own time, save as these were tempered, in the case of Philadelphia critics, by a natural pride in a great local citizen.

A more balanced view became possible during the present century, as medicine returned in some degree to eighteenth century concepts. The attention recently accorded his work in psychiatry affords an obvious illustration of this renaissance of appreciation.⁶ In certain respects, Rush's views no longer seem so strange to present medical thinkers as they did to those of 1850.

Yet even during recent decades one still encounters those extremes of opinion, those sharp contrasts of censure and praise, which characterized earlier writings. During the first half of the last

* Benjamin Musser Lecture III, The College of Physicians of Philadelphia, read at a Special Meeting of the College, November 6, 1946, commemorating the 200th anniversary of the birth of Benjamin Rush, one of the founders of the College.

¹ Lyman H. Butterfield, *Year Book of the Amer. Phil. Soc.*, 1945, p. 191.

² As in Ellis P. Oberholtzer, *The Literary History of Philadelphia* (Phila., 1906); and the careful study by Harry G. Good, *Benjamin Rush and His Services to American Education* (Berne, 1918).

³ E.g., John Krout, *The Origins of Prohibition* (N. Y., 1925).

⁴ Univ. of Penna. Press, Preface.

⁵ Note 1, above, 190ff.

⁶ See, e.g., Clifford B. Farr, "Benjamin Rush and American Psychiatry," *Amer. Jour. Psychiatry*, Centennial issue, 1944; R. H. Shryock, "The Psychiatry of Benjamin Rush," *ibid.*, vol. 101 (Jan. 1945).

century, there had been such divergent opinions as those of Dr. Elisha Bartlett and (somewhat earlier) of Dr. Lettsom of London. The former made the acid observation that there was more "utter nonsense and unqualified absurdity" in Rush's works than in the whole vast compass of medical literature;⁷ while Lettsom went to the other extreme in claiming that the Philadelphian combined judgment and sagacity "in almost unprecedented degree."⁸ Similarly extreme contrasts of opinion appeared as late as the 1920's and '30's. Dr. Victor Robinson, for example, observed in 1929 that "the career of Rush proves that . . . a physician with a facile pen may leave behind him several volumes entitled 'Medical Inquiries and Observations'—and not one page of scientific value. . . . There are few medical writers, and certainly none of celebrity, whose works are less worthy of perusal today than those of Rush."⁹ Yet only a few years later Dr. Goodman claimed that Rush was extraordinarily successful in his treatments, and that he anticipated modern medicine in many ways.¹⁰

Such conflicting opinions, persisting over so long a period, suggest that we are dealing here not only with changing scientific perspectives but also with an unusually positive personality. This personality was so sincere and devoted, and yet so self-assured and aggressive withal, that it has continued to make friends and enemies throughout these many years. Hence there remains room for honest differences of opinion about Rush the man.

It should be possible, on the other hand, to approach agreement concerning the doctor's theory and practice—as measured by the science of his day—and to evaluate his place in American medical history. Subjective reactions to his personality have no place in such an analysis, which relates to what may be well viewed as the most important aspect of Rush's career. Certainly the "American Sydenham" was first and foremost a physician. Important as were his political, social, and literary contributions, he was primarily devoted to medicine. In this field alone was he the outstanding national leader. In practice, he seems

to have viewed his political and social activities as those which any public-spirited physician should undertake; that is, these activities were incidental to the scientific career. Here there was an interesting contrast with his friend Franklin. For the latter, although declaring the primacy of scientific interests, seems actually to have subordinated them to public affairs.

For the sake of simplicity, the analysis of Rush's medical work may be limited to the well-rounded theory and practice of his later years—which means primarily the period following the great yellow fever epidemic of 1793. An analysis of his earlier views and of how and why these changed would not be without interest; but it was the final "system" which became the subject of controversy and on which Rush staked his permanent reputation.

Most basic to an understanding of this system was his theory of pathology, or of what he termed the "proximate cause" of disease. He began by claiming that all fevers resulted from (1) a predisposing debility; (2) a stimulus which was the immediate, inciting cause, operating upon a debilitated body to produce (3) a convulsive excitement or "excessive action" in the walls of the blood vessels. This last condition, which related more to pathologic physiology than to pathologic anatomy, was viewed as the essence of the fevers. Subsequently, he declared this "convulsive action" to be the underlying feature of all apparently distinct forms of illness. "I have formerly said," he declared to his students in 1796, "that there was but one fever in the world. Be not startled, gentlemen; follow me and I will say there is but one disease in the world. The proximate cause is irregular convulsive or wrong action in the system affected. This . . . is a concise view of my theory of diseases. . . . I call upon you, gentlemen, at this early period either to approve or disapprove of it now."¹¹

Stated as bluntly as this, the theory now sounds absurdly over-simple. Further analysis, however, shows that Rush was dealing here with basic problems which cannot be ignored in any concept of disease. Predisposing debility was analogous to the modern idea of "lowered resistance." It might be caused by fatigue, intemperance, faulty diet, emotional disturbances, and various other processes.

"Stimulus" (the immediate, inciting cause) was conceived broadly enough to include anything which would be listed today as an external etiologic

⁷ *The Philosophy of Medical Science* (Phila., 1844), 225.

⁸ *Recollections of Dr. Rush* (Lon., 1815), 12ff.

⁹ *Medical Life*, vol. 36 (Sept., 1929), 445, 447.

¹⁰ *Benjamin Rush*, 235, 254, etc. References to various opinions of Rush are appended to this work, and also to the author's article on Rush in the *Dict. of Amer. Biog.* Dr. Butterfield's finding lists will doubtless add more.

¹¹ *Lectures on the Practice of Physic*, I, No. 31; II, No. 1 (MSS., University of Pennsylvania Library).

factor. Rush did not distinguish clearly between circumstances producing debility and those acting as direct stimuli; thus he listed emotions—psychosomatic relations—under both headings. But note that he viewed the genesis of each illness as involving two circumstances—the impact of an outside agent and the reaction of the body itself. This view, later lost to sight in the preoccupation with bacteria as the sole “cause” of certain diseases, is analogous to the modern idea that the “cause” of an infection is really a complex relationship between body cells and infectious agents.¹²

Where Rush departed from views already held by some in his own time, and more universally accepted thereafter, was in his concept of the stimulus factor. He insisted that this was entirely non-specific in nature. All stimuli, no matter what their character, were said to produce the same effect; that is, the pathologic process of excessive action in the vascular system. The body was capable of only this one primary form of pathologic reaction, much as the optic nerve and visual center react to any stimulus whatever in terms of sight. To be sure, the pathologic process expressed itself in different symptoms having different names—in a fever, a pleurisy, a dropsy, and so on. But any one of these clinical pictures could be produced by any sort of stimulus if the latter were strong enough. Rush knew that certain etiologic factors (for example, smallpox virus) seemed to be specific, but he expressly denied this. “My view,” he declared, “establishes the sameness of a pleurisy, whether it be excited by heat succeeding cold, or by the contagions of the smallpox and measles, or by the miasmata of the yellow fever.”¹³

This might be interpreted to mean simply that different causative factors may produce similar symptoms—a truism in itself—were it not that Rush denied specificity in pathology as well as in etiology. All external factors were able to produce the same symptoms because all of them led to the same underlying pathologic process. This process, in turn, could produce the variety of clinical pictures noted; though it is not clear in Rush’s writings why it sometimes revealed itself in a dropsy and at other times in a fever. Whence came this basic doctrine of the underlying unity of disease?

Any attempt at explaining the origin of Rush’s doctrine must take into consideration many phases

of his thought as well as the chief medical problems of his day. There was no one, determining circumstance, but rather a convergence of influences—scientific, professional, and social—which led the physician toward this extreme position.

It is to be remembered, first, that monistic pathologic theories had been inherited from classical times. Stated very simply, these had ascribed disease processes either to the condition of the body fluids (humoralism) or to an alternating tension and laxness in the solid parts (solidism). The latter hypothesis was usually explained in terms of tension and laxity in the nervous or vascular systems, since these reached nearly all parts of the body, and any irregularities in them would therefore seem to explain the wide dispersal of symptoms—the fact that the ill man often seemed “sick all over.” (It later proved difficult for the localized pathology of 1830 to explain this phenomenon.) In a general way, Rush may be identified with the persistence of this *strictum et laxum* tradition. Much interest had been displayed in it at Edinburgh where Rush was trained, and the immediate background of the latter’s theory seems to have been an elaboration of this type of “system” by his fellow-student, John Brown.

Indeed, Rush was viewed in Europe as a sort of Brunonian, though he took pains to point out differences between this school and his own. Among other distinctions, Brown viewed “debility” as a lack of tone in body systems, and held that this was one of two main types of pathology, the other being excessive tone or action. But Rush insisted, as noted, that debility was not in itself disease but only a predisposition; and that the only truly pathologic condition was tension.¹⁴ He thus reduced Brown’s dualism of laxity and tension to the strictly monistic doctrine that tension alone was the substratum—the *ding an sich*—of all illness.

Why, however, after the brilliant advent of modern methods in medicine during the seventeenth century, was there this seeming reversion to classic speculations in pathology as late as 1800? This resulted, in part, from the very advances made in biological science during the interval between Harvey and Rush. So many new facts had been discovered that confusion was threatened, unless

¹² Wiley D. Forbus, *Reaction to Injury* (Baltimore, 1943), 45ff.

¹³ Quoted by Goodman, *Benjamin Rush*, 232.

¹⁴ Owsei Temkin, Comments on the German translation of Rush’s account of Yellow Fever (MS., 1946, to appear in the *Festschrift* in honor of Dr. Victor Robinson).

these could be interrelated by some all-embracing theory. At the very time that Rush was formulating his ideas, his teacher Cullen at Edinburgh declared: "For, when many new facts have been acquired, it becomes requisite that these should be incorporated into a system, whereby . . . the whole may be rendered more complete, consistent, and useful."¹⁵ The Philadelphian took his master seriously.

No better illustration of the confused state of medical science could be had than the state of nosography—of disease identification and classification—in Rush's day. Through the centuries a number of striking clinical pictures had been recognized, such as "consumption" and certain skin diseases; but the first conscious, systematic attempt to identify distinct forms of illness came with the seventeenth century. Thereafter, the authority of Sydenham gave this search considerable impetus. Logically, it represented the first truly rational approach to medicine, for how could there be an intelligent study of causes and cures of diseases until the diseases themselves had first been found?

Unfortunately, the only criterion which first appeared for disease identification was that of symptoms. This served well enough when these signs were obvious to all, as in the case of the "great pox" or of the "small pox." But the symptomatic classification of various fevers as "remittents," "intermittents," "continuing," "putrid," etc., was not so helpful. And as enthusiasm grew for identifying all possible symptom combinations as separate entities, long lists were prepared in nosography texts which amounted to only so many names. Rush rightly felt that these texts were most confusing, and desired some simpler scheme which would really aid the practitioner. He therefore swerved to the other extreme—from the listing of innumerable, supposed disease entities to the flat assertion that there was only one.¹⁶

There was, of course, a middle way between these extremes, one that eventually led out of nosographical confusion and thereby made modern medicine possible. This was the search for a localized, structural pathology. Once local lesions

were charted and then correlated with ante-mortum symptoms, a basis for disease identification was secured which was far superior to symptom lists on the one hand or assumptions about disease unity on the other. Had the merely symptomatic nosography persisted, no one would ever have found either causes or cures for the long lists of names. Worse still, had the theories of Brown and Rush survived, there would never have been even a search for causes and cures, since there was nothing more to learn. Rush knew the one "proximate cause" of all disease and—as will be noted shortly—had the one means that would cure it. Accepting his premises, there was no need for further research. But the study of lesions by pathologists, and the correlation of these with clinical evidence, opened up the whole line of pathologic advance that led in turn to the development of medical bacteriology and immunology during the nineteenth century.

This more promising approach was getting under way in Rush's time. Morgagni had clearly indicated its possibilities about 1760, and shortly thereafter Dr. Thomas Bond actually explained them in a lecture at the Pennsylvania Hospital (1766).¹⁷ Bichat and the French school were beginning intensive work in pathologic anatomy during Rush's later years. Yet the latter apparently was quite indifferent to all this promising research—rarely if ever does one find any reference to it in his writings. Originally interested in physiologic experimentation, in which his students did pioneer work in this country, Rush seems gradually to have become all-absorbed in practice. His later indifference to both physiologic and anatomic studies cannot for a moment be ascribed to laziness or inertia; he was a most intense worker, but one who by 1790 was giving nearly all his time to the bed-side and to the library.

I recall no specific statement by Rush which questions the laboratory approach, but one senses that he had little enthusiasm for it in his later years. He expressed the humanitarian zeal of the Enlightenment, and his one great purpose was to help his patients. This could not be done by turning aside to pathologic studies of no immediate value to therapy. He certainly would have repu-

¹⁵ *First Lines of the Practice of Physick*, I (Edinburgh, 1796), 34.

¹⁶ See Temkin's quotation (*op. cit.*) on Rush's opinion of Cullen and Brown in this connection, taken from the former's *Sixteen Introductory Lectures* (Phila., 1811), 11f.

¹⁷ MS. lecture, College of Physicians of Phila. Library; printed in T. G. Morton and F. Woodbury, *The History of the Pennsylvania Hospital* (Phila., 1879), 462ff.

diated the medical nihilism that ensued after 1830. Conversely, the discovery of a "system" that promised cures for all conditions must have thrilled a man who—in the tradition of the Revolution in which he had participated—hoped to benefit all mankind.

Although Rush seems to have inherited his major thesis from classical traditions, he was not necessarily aware of indebtedness to earlier theorists. He observed that the first attempts to identify disease entities on a symptomatic basis had gone astray, and therefore cast about for some means to ending the confusion. Failing to see that the symptomatic criterion could be made really useful if correlated with pathologic anatomy, he adopted the ancient idea of disease unity,—perhaps without even considering at the moment that this *was* an ancient notion. He never supported it by appeals to authorities in a scholastic manner, but seems rather to have been impressed with the revolutionary character of his own ideas. Like other eighteenth century theorists, indeed, he would have repudiated scholasticism; for the writings of empirical philosophers and the Baconian emphasis upon inductive reasoning had long since placed a premium on direct, independent investigations of Nature.

This fact, that the most speculative medical thinkers of the eighteenth century rendered at least lip service to inductive reasoning, raises an interesting question about Rush's own thought. Did he base his theory on something more than tradition—which doubtless influenced him even if he was unaware of it—and on something more than the pragmatic realization that it was useful in clarifying nosographic complexities? In a word, did he cite any actual evidence from which to induce his theory in the first place?

Here, it must be admitted, the writings are rather fragmentary and unsatisfactory. Rush usually just stated his premises and reasoned from them, using such dogmatic phrases as "my view establishes," and so on. Often he seemed to accept analogies as evidence, and was quite clever in employing ones that were apt and appealing in his day. Thus he suggests that the physician who thinks different clinical pictures are really different diseases, is as ignorant as a savage who supposes that water, dew, and frost are distinct substances. Or, again, he compares the theory of disease unity with noble monotheism in religion, while a belief in distinct diseases is equated with a superstitious

belief in polytheism. Goodman declares that this was a very modern line of reasoning.¹⁸

Yet, imbedded in some of Rush's lectures, are hints of an empirical starting point. As might be expected, he took off on his speculative flights from a very circumscribed field of bed-side observations. He had made the common observation that a flushed skin is associated with fevers. This was believed to indicate a distension of the capillaries. Since such "convulsive action" in the walls of these vessels was observed in *all* fevers, Rush concluded that it must be the essential pathology of these conditions.¹⁹ Conversely, phenomena associated with some fevers but not with others—such as various symptoms or lesions—could not be essential to fevers as such. The underlying fallacy here was the initial assumption that "fever" was a sort of entity, but this was commonly held at that time. Once this was assumed, the reasoning was logical enough though just the reverse of that now employed. Rush would have held that the intestinal lesions, now viewed as the chief feature of typhoid fever, were mere after-effects of capillary or arterial distension; while we view as a secondary phenomenon the very capillary distension which he considered the essence of the disease. The doctor had the cart before the horse here; but in any case, he had them linked neatly together.

In attempting to explain the origin of Rush's "system," mention has now been made of the traditions behind it, of its pragmatic appeal in a time of confusion, and of the limited degree to which it was based upon actual clinical evidence. These explanations would be incomplete without additional reference to certain qualities of mind that lent themselves to the elaboration of ingenious hypotheses. The doctor had a keen interest in abstract and speculative questions, and was most at home in quasi-philosophical discussions. These qualities, combined with personal kindness, gave him an almost unique popularity with students; so that he probably contributed more than any other one man to making Philadelphia the chief national center of medical training.

Rush's imagination was stimulated by wide reading, and this suggested all sorts of plausible hypotheses—many of which were sound enough when applied to the social and historical problems of that epoch. Historians, who still fall back on

¹⁸ Benjamin Rush, 235

¹⁹ Richard H. Shryock, *Development of Modern Medicine* (Phila., 1936), 28, 29.

their imagination at times, may appreciate these qualities more highly than do natural scientists. To put it more accurately, the historian is still likely to depend on his imagination in situations not admitting of final verification. He presents what seems the most convincing hypothesis; and unless quite careful, he soon writes as though the thesis were actually confirmed by the incomplete evidence available. Rush's thought operated on much this same level; that is, he began with some very limited evidence and on this basis formulated an hypothesis that was logical enough within its own structure. Then, since the thesis seemed plausible and promised cures, he accepted it enthusiastically and interpreted his clinical experience as verifying it.

All the steps in this chain of thought are sound enough until we come to the last—the failure to seek adequate verification. The treatment which Rush advocated was literally a cure-all—was good for what ailed you, since by definition the same thing ailed everyone. The procedures—which were largely bleeding and purging—will be discussed by Dr. Pepper. They followed logically from Rush's premises; for if disease consisted of vascular tension, then it could be cured by relieving that tension through bleeding. Anyone could see that, if the patient were bled copiously enough, he would relax—sooner or later! But such a superficial observation was far from providing a real verification of the soundness of the theory employed.

One should therefore pause to examine this weakest point in Rush's line of thought. How valid was his claim, for example, that he could almost always cure yellow fever by bleeding—provided the cases were not too far advanced—and that this proved the soundness of his underlying theory? In answer, it must be noted that (1) Rush's own clinical data were fragmentary—he kept no real statistics; (2) even if he had done so, the figures would have been based only on his own claims of cures. The idea of using "controls" to check a method of treatment does not seem to have occurred to him; and under these circumstances, his claims were suspect in terms of the *post hoc, ergo propter hoc* fallacy; (3) even if the physician had kept careful records, and checked his own treatments by the use of "controls," the limited experience of one observer was quite inadequate for the final demonstration of a sweeping hypothesis.

It is easy to show in this fashion that Rush's procedures did not measure up to present canons of

scientific method, or to those which Bichat would have demanded in that day. Yet even here, one can feel some sympathy for the former's difficulties. The Philadelphian was by no means visionary in seeking clues in pathologic physiology rather than in pathologic anatomy; yet it happened that the first field offered more difficulty than the second in the way of verification. Bichat could verify his thesis that lesions were common to certain types of tissues with relative ease; whereas—given the state of physiology at the time—there was just no way for Rush to test whether a tension in capillaries caused what he viewed as mere after-effect in the form of organic lesions.

Hence he was under great temptation to seek confirmation by the only apparent means—the appeal to clinical experience. It is only fair to recall that innumerable physicians had reported, through the centuries, that bleeding did give excellent results in all manner of conditions. Doubtless Rush felt that the evidence in favor of his treatments—and therefore of his theory—was cumulative even if incomplete.

It is also to be recalled that he was no more careless in handling clinical data than were the great majority of medical men in his day. Perhaps a few individual physicians had suggested the keeping of accurate records, and had been prevented from doing this both by lack of time and lack of enough data to make it worth while. Large hospital services, bringing hundreds of similar cases under the view of a single clinician, were still pretty much in the future. Statistics and the calculus of probabilities were young sciences in Rush's day, moreover, and it was not until three years before his death that the mathematician Laplace called attention to the value of these disciplines for medical studies.²⁰

Strangely enough, the first attempt to apply statistics in therapy which I have encountered is that of William Cobbett—an attempt made in the course of the pamphleteer's famous controversy with Rush. Cobbett had no real clinical statistics; but he went to the local bills of mortality to prove mathematically that the doctor was killing yellow fever patients rather than curing them.²¹ In relation to the history of scientific method, this episode deserves more attention than it has received. While Rush was then in no mood to accept any

²⁰ *Théorie Analytique des Probabilités*, 3 ed., (Paris, 1920), 420ff.

²¹ *The Rush Light* (N. Y., Feb. 28, 1800), 49.

suggestion coming from Cobbett, it is also true that physicians in general ignored this early appeal to quantitative procedures.

Within a decade after Rush's death, the French school had firmly established approaches to medicine which were the antithesis of his own. Research was concentrated on the correlation of structural lesions with associated clinical data, in the effort to identify specific disease entities. This brought with it a demand for all the more accurate methods of observation—instruments, statistics—which Rush and most of his contemporaries had ignored. As a logical corollary, the whole theory of the unity of disease was repudiated.

This theory, to be sure, has been revived from time to time down to our own day. The thrill of finding the *one* pathologic condition, with its promise of one cure, continued to appeal to certain theorists and to many laymen. No doubt Hahnemann and the early homeopaths; "Doctor" Still and the osteopaths, and Mrs. Eddy and the Christian Scientists, all felt the same exaltation in proclaiming one cause and one cure as Rush once had in announcing his revolutionary "system." But the latter worked in a day when the weaknesses of this approach were not yet clear; whereas the others proclaimed their views when medical science had advanced to a position making them plainly untenable. Hence Rush was a great physician in good standing, while the others were excluded from regular medicine and became the founders of modern sects.

In summing up, the most serious criticism of Rush seems to be that he persisted in a traditional approach to the problem of disease, in a day when more promising procedures were already being adopted in European centers. While his reasoning was ingenious and generally consistent, his temperament and other factors were such that he pushed his approach to extremes in practice. Yet this does not mean that the praises heaped upon him were unmerited. He was, in some respects, a keen clinical observer, and note how his imagination served him in such a matter as the recognition of focal infection. His work in psychiatry has been duly appreciated in recent years, as interest in a psychological orientation in that field has revived. Here he was at his best, for the subject called only for clinical observation, and involved reasoning about matters that were abstract and obscure. His theories about local miasmas, as disease-producing stimuli in epidemics, were no more one-sided than were those of his opponents; and they

had the added merit of encouraging the great sanitary reform movements which were inaugurated during his own life-time. His very indifference to laboratory research may be interpreted as the defect of his virtues; since it seems to have resulted from his earnest desire to aid suffering humanity, rather than to bury himself in investigations remote from the needs of his friends and patients. And though the influence of his "system" probably delayed such research in this country, he was only one of many clinicians who then and subsequently displayed little interest in laboratory investigations.

It has already been implied that, although Rush looked backward in his theory of disease, he was modern in the sense that certain of his eighteenth-century views have been revived during our own time. His concentration on "excessive action" in the vascular system seems analogous to the concern now accorded the whole "hypertension" complex. (Incidentally, current theories regarding this major problem still divide along much the same lines that separated the two theoretical schools of the earlier centuries; that is, between humoral and tension hypotheses.)²² Even Rush's doctrine of disease unity had the merit of viewing the patient as a whole—a merit that was lost during much of the nineteenth century when emphasis upon specificity was carried to extremes. His concern with psychosomatic relations is an excellent illustration in this connection.

Rush illustrates, finally, two other eighteenth-century traits or practices which were largely lost thereafter, only to be revived in recent years. These relate not to physicians in particular but to scientists in general, among whom Rush—with his wide interests ranging from chemistry to psychology—would certainly have arrayed himself. The first trait was this very versatility which was so typical of many seventeenth and eighteenth century thinkers. At this point we usually think at once, in this city, of Franklin; but versatility was also characteristic of Rush, of Jefferson, of Benjamin Thompson, and of William Charles Wells—to name only some of Franklin's own countrymen. True, it was easier to be versatile when science and learning were relatively non-technical. With the growth of knowledge during the 1800's, progress seemed possible only through specialization. But

²² See, e.g., Eugene M. Landis, "Pathogenesis of Hypertension in Man", Univ. of Penna. Bicent. Conf. on *Hypertension* (Phila., 1941), 9ff.

the resulting divisions between fields must now in some way be overcome, so that something of that earlier access from one area to another may be revived. Rush's type of mind would be most helpful today in what is termed "cross-fertilization" or "the breaking down of departmental barriers."

The second practice which Rush well illustrated in his day, and for which there is now a renewed demand, was the active participation by scientists in public affairs. He considered it his duty, as a physician and an intellectual, to take part in the Revolution, and it was no accident that he became a signer of the Declaration of Independence. Although he held no major political positions, he gave himself thereafter to all sorts of social reforms. In like manner the two greatest American physicians of that era, Benjamin Franklin and Benjamin Thompson, each became²⁰ an outstanding public figure; the one in his own country and the other—as an exiled Loyalist—in Bavaria.

Subsequently, something of a divorce between

science and public affairs ensued. By the later nineteenth century, few scientists were active in public life, and governments took small interest in science—at least in English-speaking lands. I need not labor the point of how all this has changed of late, and of how scientists are once more becoming public figures and governments are taking them seriously—or at least more seriously than they did but a generation ago. The atomic bomb only dramatized trends which had already set in in this direction.

The moral of all this is that Rush was more modern in many ways, as a medical man and as a scientist, than we might at first suppose upon dipping into his lectures and essays. The most famous American physician of his time, he did much to establish a great medical tradition in this city. Despite scientific and human limitations, he remains a striking figure in whom Americans in general and Philadelphians in particular may well take an unaffected pride.



