“Venerate the Lancet”: Benjamin Rush’s Yellow Fever Therapy in Context

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SUMMARY: In 1793, during a yellow fever epidemic in Philadelphia, Benjamin Rush adopted a therapy that centered on rapid depletion through purgation and bleeding. His method, especially his reliance on copious bloodletting, was at first widely condemned, but many American practitioners eventually adopted it. Although the therapy struck many observers as being radical, in large part it grew from premises that had substantial support. Rush was convinced that it worked and that heroic methods were the key to conquering disease. In particular, massive bleeding became central to his therapeutics.

KEYWORDS: Benjamin Rush, yellow fever, bloodletting, therapeutics

The yellow fever epidemic that struck Philadelphia in 1793 took perhaps five thousand lives, but it was only one of several major visitations during the 1790s. In the course of the eighteenth and nineteenth centuries, New Orleans, Charleston, and other southern cities suffered outbreaks that were far more lethal. Nevertheless, the epidemic of 1793 retains an almost mythic status. Beyond the widespread death that it brought and the dislocation that resulted from the flight of more than one third of the population of Philadelphia, it is remembered for having prompted the introduction of a radical new therapy by the most prominent physician in

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the city, Benjamin Rush. The core strategy in his method was rapid depletion, through powerful purgatives and copious bloodletting.1

This epidemic in fact marked the defining moment in the therapeutics espoused by Rush, and his method would in turn have a significant impact on how disease—not merely yellow fever, but disease in general—was treated in America. Oliver Wendell Holmes, in a talk given to the Massachusetts Medical Society in 1860, commented:

If I wish [a student] to understand the tendencies of the American medical mind . . . I would make him read the life and writings of Benjamin Rush. . . . He taught thousands of American students, he gave a direction to the medical mind of the country more than any other one man; perhaps he typifies it better than any other.2

Holmes spoke not as an admirer, but as a severe critic of Rush and of the “heroic” tradition that he personified.

Rush’s reputation as a physician has turned primarily on the issue of his therapeutics, particularly as regards yellow fever. With the benefit of hindsight, the point may easily be made that the main elements in his therapy were detrimental, and for well over a century the method that he promoted has been almost uniformly condemned.3 But it is still fair to


2. Oliver Wendell Holmes, Currents and Counter-Currents in Medical Science. With Other Addresses and Essays (Boston: Ticknor and Fields, 1861), pp. 25–26, 27.

3. While not defending Rush’s method, Chris Holmes wrote that it might not have increased mortality; Shryock challenged this contention, while noting that since it was
raise the question, Was that method justified in terms of contemporary theory and practice? My aim in this paper is to place Rush and his therapy in context.

In the estimation of contemporaries, and of many historians since, the method that Rush adopted in 1793 marked a radical departure from common practice. Shortly after Rush died in 1813, his friend and fellow-physician David Ramsay observed that he had “adopted modes of treating several diseases, which had not been usual in this country. . . . Dr. Rush carried bleeding and the depleting system farther than ever had been done before by any of his cotemporary [sic] physicians.”4 Four decades later, however, Charles Caldwell, one of Rush’s oldest living students—but also a man with whom he had feuded—wrote that “Dr. Rush had not an original mind . . . (for it is not known to me that he ever made a discovery in science).”5 Ramsay’s assessment is correct, at least if we consider only publishing physicians with some claim to be seen as authorities—but the statements are not mutually exclusive, and there is much to be said for Caldwell’s assertion, as well. Some aspects of the therapy that Rush advocated were indeed far from the mainstream, but

impossible to know how many of Rush’s patients actually had yellow fever, any assessment of his records was necessarily speculative: Chris Holmes, “Benjamin Rush and the Yellow Fever,” Bull. Hist. Med., 1966, 40: 246–63, see especially pp. 247–51; Richard Harrison Shryock, “The Medical Reputation of Benjamin Rush: Contrasts over Two Centuries,” ibid., 1971, 45: 507–52, see especially pp. 541–42. As late as 1855, in a widely cited treatise, René La Roche wrote: “Of the indispensable necessity of the antiphlogistic and evacuant treatment, which in this, as in other fevers of kindred nature, consists in sanguine evacuants, sedatives—internal and external—and purgatives, there can be no doubt” (Yellow Fever, Considered in Its Historical, Pathological, Etiological, and Therapeutical Relations. Including a Sketch of the Disease as it has Occurred in Philadelphia from 1699 to 1854 . . . [Philadelphia: Blanchard and Lea, 1855], 2: 632). La Roche then provided a massive footnote (ibid., p. 632 n. 1), including more than 150 references to authors who ostensibly had written in support of depletion during the preceding century; this list, however, includes writers who allowed for bleeding only in certain cases and to only a moderate degree. Shryock’s article provides a good review of the historiography on Rush as a physician. According to Shryock (“Medical Reputation” [n. 3], pp. 524–27), Rush was generally criticized or ignored in Europe even before his death, but was seldom attacked by American writers prior to 1820, then more generally after 1840, when in some regions a reaction against heroic medicine set in.


5. The Autobiography of Charles Caldwell, M.D., ed. Harriot W. Warner (Philadelphia: Lippincott, Grambo, 1855), p. 317; Caldwell did note, however, that Rush seemed original because he gave a “new cast to the knowledge he attained” (ibid.).
even the most radical elements in his method grew from premises that were widely accepted.

The Main Components in Rush’s Therapy for Yellow Fever

In both his published account of the 1793 epidemic and his correspondence, Rush details the evolution of his therapy for treating yellow fever. As he relates, he first made use of common modes of treatment, such as gentle purging, blistering, or wrapping the patient in blankets that had been soaked in vinegar. Finding these to fail, he consulted Edward Stevens, a physician newly arrived from St. Croix, who told him that experience had caused him to abandon all evacuations in the treatment of yellow fever, for they were harmful, and instead to rely on cinchona bark, wine, and, especially, cold baths. These remedies Rush tried on four patients, but three died. Convinced that “there does not exist a disease for which the goodness of Providence has not a remedy,” he reviewed his library for sources on how best to treat the disease.6 He located a source that he already knew quite well: a letter that John Mitchell had written in 1741, providing an account of several cases of “yellow fever” that had recently struck Virginia. Shortly before his death in 1790, Benjamin Franklin had given Rush this document, and the latter had incorporated some of Mitchell’s points into his lectures.7 Mitchell reported that he had enjoyed success by purging patients, and Rush now began to employ purgation more rigorously than he had earlier. Recalling that Thomas Young, a physician in the Continental Army, had treated bilious fever with a purgative that combined ten grains apiece of calomel and jalap, Rush decided to do the same, but to make the dose even larger,


7. Rush later published the manuscript in the American Medical and Philosophical Register, 1814, 4: 181–215. That he chose to rely on this manuscript is strange, and may be explained by the fact that it was brief and that he was familiar with it. As Saul Jarcho notes, at least five of the six cases of “yellow fever” described by Mitchell could not have been that disease: “John Mitchell, Benjamin Rush, and Yellow Fever,” Bull. Hist. Med., 1957, 31: 132–36, on p. 135. Rush had been familiar with Hillary for some time, since he cited him in a lecture (though not in reference to yellow fever) in 1786: “Notes from a Course of Lectures on The Practice of Physic. by Benjamin Rush. MD.,” taken by Thomas C. James, 1786 (hereafter, “Rush Lecture Notes, 1786”), MS, College of Physicians of Philadelphia, unpaginated reference (note: this manuscript is paginated only to p. 35). Hillary would seem to have been a better source on yellow fever, though there is no reason to believe that Rush would have taken a different course in therapeutics had he relied on him instead of on Mitchell.
adding five grains of jalap. Even more than his reliance on purgation, a second initiative came to define his therapy for yellow fever: from early September onward, he made progressively greater use of venesection.⁸

Rush’s method incorporated a complex of strategies. Some were widely regarded as radical, but others were quite traditional, and perhaps for this reason they were ignored by contemporary critics and have continued to be ignored by historians. A notable example of the traditional is Rush’s use of revulsion, a practice that had its origins in humoral pathology. In justifying his use of strong purgatives in the treatment of yellow fever, Rush explained: “I had observed the fever to fall with its principal force upon such parts as had been previously weakened by any former disease. By creating an artificial weak part in the bowels, I diverted the force of the fever to them, and thereby saved the liver and brain from fatal or dangerous congestions.”⁹

Still, his core strategy, that of treating yellow fever through rapid depletion, struck contemporaries as being an innovation. And not only did Rush continue to use purgation and venesection in combating the yellow fever epidemics that struck Philadelphia in 1794 and 1797, but in the latter year he amended the method in a way that again set him against the mainstream. Almost all writers warned against the use of emetics in treating yellow fever, their objection being that vomiting weakened patients who were already debilitated, and that since nausea regularly attended yellow fever, emetics might well provoke vomiting that was uncontrollable and ultimately fatal. Rush himself had earlier shared


these concerns, but now he changed his mind. Still, the use of emetics
did not become identified with his method, and when that was discussed,
whether to praise or to condemn, assessments hinged on his use of
purigation and venesection.

Of these two, purgation was by far the less controversial. While a few
writers opposed the practice, most endorsed it, at least in cases where the
patient was not severely debilitated or purging freely and involuntarily.
Cathartics were widely used in treating all febrile diseases, often as a first
step. As the English physician John Ball wrote, “on proper evacuation . . .
consists in a great measure the art of curing Fevers.” In treating yellow
fever, even many practitioners who rejected venesection made great use
of cathartics. Two American physicians reported: “Our method was
plentiful depletion in the beginning, not by bleeding, but by purging with
Jalap, Calomel, Scammony, Aloes, or by the milder purges.”

10. Account of Yellow Fever, 1794, p. 229; Benjamin Rush, Medical Inquiries and Observa-
tions, vol. 5: Containing, An Account of the Yellow Fever, as It Appeared in Philadelphia in 1797,
and Observations upon the Nature and Cure of the Gout, and Hydrophobia (Philadelphia: Dobson,
1798), pp. 59–60 (hereafter, Account of Yellow Fever, 1797). On opposition to the use of
emetics in yellow fever, see Benjamin Moseley, A Treatise on Tropical Diseases; on Military
433–34; William Lempriere, Practical Observations on the Diseases of the Army in Jamaica, as They
Occurred between the Years 1792 and 1797 . . . (London: Longman and Rees, 1799), 2: 120;
John Holliday, A Short Account of the Origin, Symptoms, and Most Approved Method of Treating the
Putrid Bilious Yellow Fever, Vulgarly Called the Black Vomit . . . in the City of Havanna . . . 1794
(Boston: Manning & Loring, 1796), p. 1; Alexander Hosack, History of the Yellow Fever, as It
Appeared in the City of New York, in 1795 (Philadelphia: Dobson, 1797), p. 27; James Tytler, A
Treatise on the Plague and Yellow Fever. With an Appendix, containing Histories of the Plague . . .
(Salem: Cushing, 1799), pp. 527–28. A number of writers who opposed the use of emetics
in treating yellow fever, among them Moseley and Lempriere, did endorse venesection.
Emetics were sometimes used in remittent fever, in order to convert it to intermittent; John
Rollo, Observations on the Diseases Which Appeared in the Army on St. Lucia, in . . . 1778 . . . and
. . . 1779 . . ., 2nd ed. rev. & corr. (Barbados: Dilly, 1781), p. 75. Cullen almost always
prescribed emetics first for fever patients; of course, he did not treat yellow fever specifically:
Guenter B. Risse, “Cullen as Clinician: Organisation and Strategies of an Eighteenth
Century Medical Practice,” in William Cullen and the Eighteenth Century Medical World: A
Bicentenary Exhibition and Symposium arranged by the Royal College of Physicians of Edinburgh in
p. 143.

11. John Ball, The Modern Practice of Physic: or a Method of judiciously Treating the Several

12. Valentine Seaman, An Account of the Epidemic Yellow Fever, as It Appeared in the City of
the review of opinion in Tytler, Treatise on the Plague and Yellow Fever (n. 10), p. 528. William
Currie urged practitioners not to heed “theories of speculative and fanciful authors” who
warned that purgation was too debilitating, and he asserted: “there is nothing more
In general, Rush was faulted not for purging but for the nature and dosage of the cathartic. William Patterson, surgeon to the Londonderry Infirmary, asserted that

his preparation is of too drastick a nature, is comparable to arsenick, and is a dose for a horse; that, as exciting salivation, it is hurtful; that, as adding to the excoriation of the rectum, it contributes to increase inflammation of this part; that it tends to lacerate and inflame the bowels; and that being prescribed indiscriminately, it must do harm to weak habits.\textsuperscript{13}

Calomel, Rush’s purgative of choice, was widely regarded as one of the safer and milder mercurials. It was widely used as a purgative, as well as a sialogogue, and for decades it had been prescribed in the treatment of various fevers, although few writers before Rush applied it specifically in the case of yellow fever. In defending his use of it, Rush asserted that in addition to serving a therapeutic purpose as a purgative, it removed lymphatic obstructions and promoted salutary sweats. His patients who had salivated, he claimed, had in almost all cases recovered.\textsuperscript{14} By claiming multiple benefits for calomel, he exalted its appeal; increasingly, it became the purgative most widely used in treating yellow fever—and especially in American practice, this in large part reflected the influence of Rush. Isaac Cathrall, a physician in Philadelphia, reported: “This medicine was first employed here in large doses by Doctor Rush, and afterwards generally adopted by the practitioners of this city.”\textsuperscript{15}
The case was quite different regarding venesection. Opinion in Britain and the West Indies was generally negative, and in America also it stood against him, if not as uniformly so. A number of writers were critical of even a moderate taking of blood; others accepted venesection as a therapeutic for yellow fever, but felt that Rush’s call for copious bloodletting was dangerously misguided. Since this aspect of his practice was so controversial, its genesis will be the primary focus of the balance of this paper.

Rush and Authority

Despite his reputation as an innovator, Rush often manifested a respect, perhaps a need, for authority. The influences that shaped his medical philosophy were primarily British. Beginning in 1761, he served a five-year apprenticeship under John Redman, who, before returning to Philadelphia, had studied at Edinburgh and practiced at Guy’s, although his M.D. was from Leyden. While an apprentice, Rush began to study Hermann Boerhaave’s writings, but at the suggestion of Redman he soon turned to the works of Thomas Sydenham. On completing his apprenticeship, he took Redman’s advice by studying medicine at Edinburgh, receiving his M.D. there in 1768. He studied under, and acclaimed,
several professors at Edinburgh, but, like many other American students and thousands of Britons, he was particularly taken with William Cullen. He returned to Philadelphia imbued with what he understood to be Cullen’s medical philosophy and practice, and antagonistic to Boerhaave’s system, which, he would later claim, was then prevalent in the city. Soon, to his own recollection, he was courting controversy with older practitioners there by toasting the “Speedy interment of the System of Dr. Boerhaave, and may it never rise again.”

Rush would later recall: “However unpopular and offensive the System of Dr. Cullen was when first broached by me, I lived to see it adopted by all the physicians who had opposed it; nay more, I lived to see it adhered to and defended with great obstinacy when an attempt was made to alter and improve it twenty years later.” It was Rush himself who was to lead the latter campaign, but not before spending two decades as a firm Cullenist. In 1781 he published Cullen’s *First Lines of the Practice of Physic*, asserting in his preface that the author had “produced a revolution in medicine.” Until the late 1780s, he continued to defer to Cullen, as he did in a lecture in 1786: “For the predisposing and occasional causes of Fevers I refer you to Dr. Cullen as likewise for the proximate on which I have little to say.”

It may have been only after Rush read John Brown’s *Elements of Medicine*, in 1788 or 1789, that he moved away from Cullen. In the autumn of 1789, he told Ramsay that the system of Cullen was tottering; that Dr. John Brown had brought forward some new and luminous principles of medicine, but they were mixed with others which were extravagant; that he saw a gleam of light before him leading to a more simple and consistent system of medicine than the world had yet seen, and pointed out some of its leading features.


22. Ramsay, *Eulogium* (n. 4), p. 23n. In addition, Rush may have been prompted to consider his medical system more deeply when in 1789 he was appointed professor of the
The following year, in the course of a rather half-hearted eulogy of Cullen, Rush spoke of the destructive value of Brown’s work, but added that his system likewise required improvement: “Dr. Brown has proved the imperfection of human genius, by extending some parts of Dr. Cullen’s system of physic, and perhaps by correcting some of its defects. But he has left much to be done by his successors.”

Rush never gave himself over to Brown’s system as he had to Cullen’s. He certainly borrowed some concepts from him, however, and one of these was important in the chain that would lead to his yellow fever therapy. Brown claimed that the debility that characterized fever could be indirect as well as direct: if direct, it caused the disease; if indirect, it was caused by it, reflecting a system that had been overwhelmed by the action of the diseased state. Nevertheless, the issue of whether debility was direct or indirect had no influence on Brown’s therapy, for diseases of both kinds were to be treated as though they were asthenic, diseases of debility, and that meant a focus on stimulant remedies, rather than on depletory ones like venesection. Rush, on the other hand, used the doctrine of indirect debility to explain that yellow fever, while being attended by symptoms that suggested debility, was really the product of an overactive system.

Despite this instance of borrowing, Rush was not a Brunonian and resented being characterized as such. With Rush’s encouragement, in 1805 John T. Rees, his student at the University of Pennsylvania, produced a dissertation comparing Rush’s system to Brunonianism. Not surprisingly, Rees concluded that the system of his “illustrious preceptor”

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23. Benjamin Rush, An Eulogium in Honor of the Late Dr. William Cullen, Professor of the Practice of Physic in the University of Edinburgh; Delivered before the College of Physicians of Philadelphia, on the 9th of July; agreeably to Their Vote of the 4th of May, 1790 (Philadelphia: Dobson, 1790), p. 28.

24. Rush later substituted “debility from abstraction” for direct, and “debility from action” for indirect, “in consequence of the tendency which the use of these terms have had to associate his principles with those of Dr. Brown” (John T. Rees, Remarks on the Medical Theories of Brown, Cullen, Darwin, & Rush [Philadelphia: Carr, 1805], p. 67). That the appearance of debility could reflect an overactive system was widely accepted; e.g., David MacBride advised that in the case of inflammatory fever the circulatory system might be overwhelmed by the amount of fluid, resulting in “a false appearance of weakness”: A Methodical Introduction to the Theory and Practice of Physic (London: W. Strahan et al., 1772), p. 311.
was original and possessed “many advantages” over its rival. For his part, in both lecture and writings Rush pointed up his differences with Brown: “The Doctor supposes a fever to consist in debility. I do not admit debility to be a disease, but place it wholly in morbid excitement, invited and fixed by previous debility. He . . . supposes excitement and excitability to be equal in fever. My theory supposes a fever to be the reverse of this.” Furthermore, while Brown had asserted that only three of the hundred disease entities were sthenic, Rush claimed that at least half were. When applied to therapeutics, this contrast was pivotal.

In one respect, however, Rush went in a direction clearly marked by Brown. While it is fair to speak of “Cullenism,” Cullen’s system was quite loose: it included some precepts, but many qualifiers and exceptions to rules. By contrast, Brunonianism was highly systematic and, beneath Brown’s verbiage (which was often arcane), it was quite simple. Through much of the latter half of the eighteenth century, medical systems and those who put them forth were widely denounced in both Britain and America. Medical practice was shaped by theory to a great degree, but the balance in this period was tipped toward empiricism. Late in the century, however, rationalism became more popular, and Brunonianism fostered this tendency. Its success may have encouraged Rush to think in terms of creating a system that, like Brunonianism, would both explain disease and point to the therapy that would conquer it. But despite Ramsay’s recollection that Rush had outlined, as early as the fall of 1789, “a more simple and consistent system of medicine than the world had yet seen,” it is doubtful that it went very far in this early stage. Just as Brown gained sudden insight by conquering his gout and from this experience derived his system, so did Rush “need” yellow fever. It was from the

25. Ibid, pp. 73, 74. Rees also discusses (and ridicules) the core doctrines of Cullen and of Erasmus Darwin, but he focuses on Brown. Rush’s system is analyzed on pp. 68–74 and is distinguished from Brown’s (largely on the basis of a quotation from Rush himself) on pp. 68–70. Rees does express concern over Rush’s logic on two points, pp. 72–73.

26. “Essence of Dr. Rush’s Lectures from Notes taken by me in the winters of 1815 and 1816,” by Elias Boudinot Stockton, endorsed, “Philadelphia, 1817” (hereafter, “Rush Lecture Notes, 1815–16”), MS, College of Physicians of Philadelphia, p. 145 (obviously, these notes were not taken in class in 1815–16, Rush having died in 1813; Stockton may mean that he transcribed them in 1815–16; they appear to date originally from 1800–1810). The quotation is from Account of Yellow Fever, 1794, pp. 147–48. Rush may also have resented suggestions that his system owed much to Erasmus Darwin. He told his students that while Darwin divided diseases into those of sensation, association, motion, and volition, he divided them into those appearing in the blood vessels and viscera (the fevers) and those appearing in the vessels, nerves, and alimentary canal (other diseases): “Rush Lecture Notes, 1815–16,” p. 144.

epidemic of 1793 that his coherent philosophy of pathology and therapeutics would spring.

Possibly Rush’s theory on how to treat yellow fever, or fever in general, emerged from extended cogitation. He was to write that he had come to believe—the chronology is hazy, but certainly well before 1793—that the models for treatment that he had earlier accepted did not work in the case of fevers common in Pennsylvania. In consequence, “Many painful hours have I spent in contemplating this subject. At length light broke in upon my mind. The phenomena of fever suddenly appeared to me in a new order. I instantly combined them into a new theory.”28 After the fact, Rush would maintain that the method that he adopted in 1793 was rational: “The conquest of this formidable disease, was not the effect of accident, nor of the application of a single remedy; but, it was the triumph of a principle in medicine.”29 But however he may have seen himself, the weight of evidence runs counter to the image of a painstaking rationalist. Indeed, he exalted intuition, rather than reason, writing, “To be unchangeable, belongs only to that being who sees things in their order and relation to each other by a single act of intuition.”30 It may be that his concept of treating yellow fever grew from a moment of revelation, and it is quite probable that his method evolved during the crisis of 1793 and was not in place at the start of the epidemic. Had it been, he would not have dabbled in methods suggested by Stevens and others. Rush’s writings across the 1790s suggest that he first discovered a therapy he thought effective and then created a theoretical framework to explain its success. But he may have had preconceptions in place by 1793 that inclined him toward a depletory method.

Prior to 1793, Rush displayed some sympathy for the empirical perspective and for the common practice of observing which remedies seemed to work in treating disease, irrespective of theory. In 1789, he reported the success of certain treatments; speculating that “regular-bred” physicians would reject them because they did not accord with prevailing theory, he advised that “many things are true in medicine, as well as in other branches of philosophy, which are very improbable.”31 By the 1790s, however, he was an unabashed theorizer, taking a strong position in debate between rationalists and empiricists. In one treatise,

29. Account of Yellow Fever, 1793, p. 204.
he wrote: “Let not the reader be offended at my attempts to reason. . . . A single just principle in our science, will lead to more truth in one year, than whole volumes of uncombined facts will do in a century.”

Although the tone here may appear to disparage hard data, Rush often proclaimed his devotion to fact. “Facts are the morality of medicine. They are the same in all ages and in all countries,” he commented in 1790. However, he tended to credit as fact particularly information that accorded with his theories. The end result was a series of statements like these in his course lectures: “In highly cultivated Countries Fevers are scarcely known. In Barbadoes and St Kitts there are few Fevers except such as are produced by Intemperance”; “I know but one certain sign of death, that is the commencement of putrefaction. Persons have recovered after respiration has ceased two or three days.” A particularly damaging “fact” that he related to his students was that the human body contained 25–28 pounds of blood, about twice the actual amount. Since he also told them that they could safely remove four fifths of a patient’s blood—while healthy individuals used all their blood, he asserted, the sick used only a small fraction—the possibility was real that a student might apply these two bits of information and consequently bleed someone to death.

That Rush did not know the actual volume of blood is understandable; neither did his contemporaries. It is of interest, however, that he chose to guess. By contrast, John Hunter, in his highly influential treatise on the blood, pointedly refused to speculate on its quantity, claiming that no means existed to measure it. Many eighteenth-century authorities were likewise reluctant to conjecture on medical matters that seemed uncertain to them, though all speculated on occasion. Rush, however, seldom held back, even when he had little or no hard evidence to serve as his base. In

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34. “Rush Lecture Notes, 1786” (n. 7).
35. “Rush Lecture Notes, 1797” (n. 9), lecture #51. Rush was aware of common errors in handling data, but seems to have recalled them mainly when it benefited his argument; note his comment on “Accidental coincidence,” in Pringle, *Diseases of the Army* (n. 9), p. 79 n. 46.
one of his writings, he considered what state of atmosphere was most likely to encourage malignity. While noting that he lacked the instruments necessary to check, he wrote that an excess of oxygen was probably the key; if it appeared that oxygen was not responsible, he added, perhaps aliment or fruits of the season were responsible.  

The sources that provided Rush with his information were a varied lot. His lectures and writings are replete with anecdotal evidence that had been communicated to him second- or thirdhand, or with data obtained from individuals whose medical credentials were nonexistent. Reviewing methods for combating intermittent fever, he told his students that bleeding was an option: “We owe this Remedy in a great measure to a Clergyman in Maryland who having found that in many Cases the Bark would not effect a Cure, had recourse to bleeding which proved a successful remedy.” He also credited the statement of a German bleeder who told him that he could ascertain by the smell of a patient’s blood whether he had yellow fever. Unlike many of his contemporaries, Rush seldom experimented in any systematic way. Nor is there evidence that he performed postmortems on the victims of disease. During the yellow fever epidemics of the 1790s he seems to have relied on the findings of a select few colleagues, most notably his disciple, Philip Syng Physick. In the various treatises that he wrote during the course of the 1790s defining and defending his method for treating yellow fever, Rush’s approach was highly rational, much logical extension being based on a relatively small mass of precepts, observations, anecdotes, and “facts.” Some of his fellow-practitioners likewise portrayed their methods as rational, but several conceded that their logic might be faulty and that it was the success of their methods rather than the quality of their reasoning that justified them.

39. “Rush Lecture Notes, 1786” (n. 7).
40. Account of Yellow Fever, 1794, pp. 88–89.
41. Apparently at his request, a student of his stood in cold water, the aim being to check the impact on his pulse: Account of Yellow Fever, 1793, p. 288. But his writings reveal few experiments even as rudimentary as this.
Had Rush remained an adherent of Cullenism or applied the precepts of Brown, his therapy during the crisis of 1793 would have been far more moderate than it proved to be. Cullen, despite an overlay of theory, was rather conservative in practice. In treating fevers, he bled only about half of his patients. He took into account the age and strength of the patient in determining whether to bleed at all, and if he chose to proceed he seldom took more than twelve ounces.\textsuperscript{44} This practice probably reflected the norm among British physicians and among the better-educated medical men in America, many of whom had studied abroad. Brown, despite the seeming radicalism in his system, was even more restrained. It was mainly in the treatment of sthenic diseases, which he imagined to be few in number, that he recommended such debilitating remedies as venesection. Brown’s method was very much in keeping with a tendency, during the last third of the eighteenth century, toward more moderate therapies. No one who might have been regarded as an authority maintained that bloodletting was wrong in all cases, but critics of the practice, notably William Buchan and John Millar, vehemently claimed that it was being used far too generally and that the quantities of blood being taken were excessive—yet, the practice that they were denouncing was quite restrained compared to the one that Rush was to institute in 1793.\textsuperscript{45}

Rush himself was not at first an avid bleeder. In an early essay, he discussed the therapy that he had used in treating an epidemic fever in 1780:

\begin{quote}
I have taken no notice of blood-letting. Out of several hundred patients whom I visited in this fever, I did not meet with a single case, before the 27th of September, in which the state of the pulse indicated this evacuation. It is true, the pulse was \textit{full}, but never \textit{hard}. . . . I heard of several cases, in which bleeding was followed by a fatal termination.\textsuperscript{46}
\end{quote}

In 1786, he cautioned his students: “According to Facts related by Sydenham we \textit{learn} that Inflammatory diseases do not bear bleeding as well now as formerly.”\textsuperscript{47} Nevertheless, when standard practice encouraged venesection, he could be quite aggressive. He told the same class that when as practitioners they were confronted by inflammatory fever,

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\item \textsuperscript{44} Risse, “Cullen as Clinician” (n. 10), p. 144.
\item \textsuperscript{46} Benjamin Rush, “An Account of the Bilious Remitting Fever, as It Appeared in Philadelphia in the Summer and Autumn of the Year 1780,” in \textit{Medical Inquiries and Observations} (n. 31), 1: 115–16.
\item \textsuperscript{47} “Rush Lecture Notes, 1786” (n. 7).
\end{itemize}
“After recommending bleeding you should not be deterred from putting it in practice solely by the sinking of the pulse as it will sometimes subside from Fear or Horror.” 48 During 1783–86, he bled each of three pregnant women, at least two of whom suffered from pleurisy, between eleven and sixteen times; all recovered, he reported, and bore healthy babies. 49

By 1789, Rush was beginning to theorize in a direction that would encourage more liberal use of venesection. “How shall we reconcile the practice of bleeding in intermittents, with our modern theories of fever?” he wondered; “May not such congestions be formed in the viscera, as to produce the same species of inflammatory diathesis which occurs in several other inflammatory diseases?” 50 Rush was beginning to perceive inflammation where most writers did not. The significance of this would become apparent in 1793.

Rush vacillated in judging his own role in defining the strategy that he adopted in treating yellow fever. At times, as in his conversation with Ramsay, he characterized his general system as being innovative, perhaps revolutionary. During the epidemic of 1793, he often discussed his practice in writing and by implication claimed responsibility for a novel method of treating the fever. Nevertheless, in a letter that he wrote to a fellow physician, John R. B. Rodgers, he protested:

> It was extremely unfortunate that the new remedies were ever connected with my name. I have no other merit than that of having early adopted and extended a mode of treating the disorder which I had learned in the year 1762, from my first preceptor in medicine, Dr. Redman, and which is strongly recommended by Hillary, Moseley, Mitchell, Kirby, and many other writers on this fever. 51

The key word in Rush’s disclaimer is “extended.” On many key points associated with establishing his method, Rush could justly point to writers who had taken a similar path. But how much did he go beyond what they sanctioned? It appears that he departed from them in significant ways. Furthermore, many of his authorities were rather minor figures, and some of them never treated yellow fever. In defining his method of

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48. Ibid.


51. Letter dated Philadelphia, 3 October 1793: Rush, *Letters* (n. 8), 2: 698. Four of the individuals noted in Rush’s letter are referred to in this article. None of them went as far as did Rush in bleeding in yellow fever (if they treated it) or in other fevers deemed malignant. The fifth writer, Timothy Kirby, was quite obscure: apparently his only publication was his doctoral dissertation, *De febre putrida maligna*, which was published in Leyden in 1773 and again in 1774. Seldom if ever is the dissertation mentioned in the medical literature of the period, and it is unclear how Rush became aware of it.
treating the disease, Rush was far more innovative than he chose to acknowledge.

Removing the Safeguards

In devising the method that he employed to treat yellow fever, Rush challenged or ignored several traditional safeguards that were generally affirmed in contemporary practice. In all cases, however, other professional practitioners took the same route. Rush was often in a minority, but he was never alone.

The first of these safeguards was reliance on nature as a guide to therapy. Most contemporary authorities who expressed themselves on the subject argued that in treating disease practitioners should rely on the healing powers of nature, interposing themselves only at such times and in such ways as nature seemed to favor. Jean Devèze, a French physician who practiced in Philadelphia during the epidemic in 1793, wrote that “any one who, seduced by the brilliancy of a system, will force nature by the rules of the method he has adopted, he, I say, is a scourge more fatal to the human kind than the plague itself would be.”52 Those who shared his perspective were in general heavily influenced by Hippocratic philosophy and practice.

Although Cullen seldom referred to nature per se, he believed that an inner vital force had the will, and generally the power, to cure. But even before Rush broke with Cullen’s system he was expressing doubts regarding the value of nature in healing. In 1786, he stated in a lecture: “Much has been said on the powers of Nature in curing Diseases; in certain Diseases it is true her powers are very great but as artificial Diseases increase the efforts of Nature cease or become injurious &c it is therefore the Business of a physician to direct & regulate her Actions.”53 As he moved through the 1790s, he came to dismiss entirely the notion that nature, as reflected in the human body, was somehow wise or benign. In 1797, he told his students: “By Nature in the present case I mean nothing but Physical Necessity. This at once excludes every thing like inteligence [sic] from her operations: these are all performed in obedience with the

52. Devèze, Enquiry (n. 15), p. 50. Devèze may well have been writing with Rush in mind. Besides the fact that his practice differed greatly from Rush’s, the latter had ridiculed him for it: see Account of Yellow Fever, 1793, pp. 319–20.
53. “Rush Lecture Notes, 1786” (n. 7), pp. 7–8. Rush distinguished between natural diseases—mostly fevers, which he thought were decreasing in severity—and artificial ones, which were products of human behavior or practices.
same laws which govern Vegetation in plants.” Like Sydenham, he believed that nature caused disease by its reaction to a proximate cause, but unlike him he did not regard nature as being anxious to cure and, if it were possible, able to cure as well. Hippocrates he characterized as “nothing more than an attentive observer of Nature.” Although he chose on occasion to be moderate in tone, a disdain for nature, and in large part for its advocates, became one of the hallmarks of Rush’s medical thought.

Devotion to nature may have been common in eighteenth-century medical thought, but so was a countervailing view: that nature was unable to cure serious diseases. When confronted by a highly mortal disease like yellow fever, many physicians disregarded nature as a factor in therapy. Robert Jackson, whose writings on tropical fevers were highly influential, commented of his practice: “I exerted myself, from the first moment that I was called to the patient, to change the genius and natural tendency of

54. “Rush Lecture Notes, 1797” (n. 9), lecture #41.
55. On Rush’s differences with Sydenham, see Thomas Sydenham, The Works of Thomas Sydenham, M.D. on Acute and Chronic Diseases; with Their Histories and Modes of Cure. With Notes, Intended to Accommodate Them to the Present State of Medicine, and to the Climate and Diseases of the United States, by Benjamin Rush, M.D. (Philadelphia: Kite, 1815), p. iv. Late in life, Rush observed that the health of Philadelphians was improving in part because of “the rejection of the supposed healing powers of nature in all diseases of great, and feeble morbid action, and the preference which is given to the more prompt, and safe method of curing them by means of artificial remedies” (Benjamin Rush, “An Inquiry into the Comparative State of Medicine in Philadelphia, between the Years 1760 and 1766, and the Year 1809,” in Medical Inquiries and Observations, 5th ed. [Philadelphia: Finley, 1819], 4: 246–47). Rush sometimes made mildly favorable comments on nature; e.g., “It may be said in favour of Nature, that she removes extraneous Substances by creating an inflammation & Suppuration. That She checks hemorrhages by occasioning Syncope. That she diminishes our appetite, and calls for cool drink and Air in Fevers” (“Rush Lecture Notes, 1797” [n. 9], lecture #41). He was, however, consistent on the point that nature could not cure serious diseases; see, e.g., his notation on Pringle, Diseases of the British Army (n. 9), p. 60 n. 39. Rush’s disparagement of nature brought an often-cited complaint by Victor Robinson: “In every age, opposition to the cardinal doctrine of the healing power of nature, promulgated by Hippocrates, has been a sign of the obstructionist in medicine. Rush was conspicuous among those who sought to overthrow the Hippocratic maxim that nature heals and that the physician is only nature’s assistant” (Victor Robinson, editorial introduction to Joseph McFarland, “The Epidemic of Yellow Fever in Philadelphia in 1793 and Its Influence upon Dr. Benjamin Rush”; the reference for the introduction is Med. Life, 1929, 36: 445–48, quotation on p. 447). Not mentioning Robinson, Hendrie Lloyd James wrote a strong defense of Rush’s perspective: “Benjamin Rush and His Critics,” Ann. Med. Hist., n. s., 1930, 2: 470–75, on p. 471.
the disease; or, if I may be allowed the expression, to take the business, as speedily as possible, totally out of the hands of nature.” Rush’s method of treating yellow fever, like Jackson’s, ignored nature from the outset. Not only did Rush disparage nature as a curative, but he regarded it as slow to act. Like many other writers, he stressed the importance of early treatment, not only of yellow fever but of acute diseases generally, and such practice left no time to see which way nature tended. He intended to destroy yellow fever early, before it could “form”; once it had formed—that is, once the more violent or definitive symptoms appeared—there was, he acknowledged, little that medicine could do to affect the outcome.

The aggressiveness of Rush and Jackson was shared by many practitioners, who argued that when a patient was confronted by a disease that was likely to prove fatal, the physician should interpose himself as strongly as possible. It is quite common in the medical literature of the period to read some variant of the aphorism of Celsus, “Better a doubtful remedy than none at all.” The yellow fever epidemics that struck Philadelphia during the 1790s tended to encourage extreme therapies. During the epidemic of 1793, Rush’s longtime rival, William Shippen, wrote to commend him for his devotion to the people of Philadelphia. Shippen may have been unaware that Rush considered his method to be highly successful, for he expressed regret that Rush was stymied by the raging violence of the fever (which Shippen believed to be the plague). He advised, “You know that desperate diseases often require & Warrant the use of desperate remedys,” and recommended that Rush replace the jalap in his pill with gamboge, adding: “after a dose or two as occasion may require you should bleed your patient almost to death at least to fainting.”

Like reliance on nature, a safeguard against extreme practice that was often appealed to in eighteenth-century medical literature was that of common sense. No medical system could provide definite answers to

59. Shippen to Rush, 13 October 1793, Rush MSS, vol. 36: “MSS. Correspondence of Dr. B. Rush, Yellow Fever,” part 2, #114, Library Company of Philadelphia (on loan to the Pennsylvania Historical Society); Rush later published most of this letter (*Account of Yellow Fever, 1793*, p. 250n), ignoring Shippen’s admonition, “N:B: This is intirely inter nos.” Note Chisholm’s reference to Celsus: *Essay on the Malignant Pestilential Fever* (n. 12), p. 163. Even Devèze, who advocated the doctrine of nature as curative, wrote of his treatment of yellow fever in 1793: “it was better to try uncertain means, than to let a patient die for want of an endeavour, however desperate the case might be” (*Enquiry* [n. 15], p. 70).
every issue affecting therapeutics. Symptoms might conflict, the disease might be poorly defined, the constitution of the patient might not seem equal to the therapy that the physician preferred. In such cases, practitioners were supposed to rely on judgment. Indeed, it was considered the mark of a superior physician that he judged ably. Limits were often suggested in terms of common sense, rather than strict guidelines. As the French physician Jean Senac wrote, “No one but a madman will, in treating a febrile complaint, persist long in attempting to overcome it by very profuse bloodletting.”

Nevertheless, to many practitioners, common sense seemed to dictate that violent diseases could best be conquered by violent therapies. Rush applied this principle in the case of yellow fever in 1793, and subsequently defended it in writing: “There has always been a prejudice against what are called heroic, or energetic remedies, and it is common to say of them, they are worse than the disease. It is necessary they should be so,—that is, more powerful than the disease, or they cannot overcome it.” But he was certainly not alone in approaching yellow fever this way. Félix Pascalis-Ouvière, a French physician who practiced in Philadelphia, advised copious venesection in treating the fever. If in the course of this treatment the patient stabilized and normal evacuations resumed, he noted, bloodletting should cease; but recovery or death were the only ways to end the treatment. He commented: “Happy those who arrive at that period, without having been prompted to the absolute annihilation of that last share of organical life and circulation, without which no diseases are cured, nor health recovered!”

Another traditional limit on venesection was the condition of the patient. Of his critics, Rush complained: “It was said, that blood-letting was prescribed indiscriminately in all cases, without any regard to age, constitution, or the force of the disease. This was not true as far as it relates to my practice.” In fact, however, his caveats were few. The most important was his practice of not bleeding patients who entered his care more than three days after contracting the illness and had not been bled or at least rigorously purged. Even here, he made an important excep-

61. Works of Thomas Sydenham (n. 55), p. 239n.
63. Account of Yellow Fever, 1793, p. 277.
64. Ibid., p. 266.
tion: patients who had regularly given blood before the fourth day might be bled again, so long as their illness or inflammation continued. The most extended programs of bloodletting were justified on this basis. Physick, for example, was bled (not by Rush, though he approved) twenty-two times in ten days, to a total of 176 ounces.65 Physick was young, strong, and apparently quite willing to undergo the rigorous treatment. By contrast, many of Rush’s patients were frail and, by his own testimony, were reluctant to give blood. Nevertheless, bloodletting, usually copious, was standard in his therapy, and for those who accepted the regimen, there was really no way out. He enumerated ten states of the pulse that indicated bloodletting. The first—full, hard, tense—would have been accepted as an indicator by most physicians. But he also included states that were rapid, intermitting, depressed, or imperceptible. He concluded: “If all the states of pulse which have been enumerated, indicate bleeding, it must be an affecting consideration to reflect, how many lives have been lost by physicians limiting the use of the lancet only to the tense or full pulse!”66 And just as, to his mind, the state of the pulse seldom discouraged venesection, neither did the state of the patient. In one treatise he noted categories of people who some asserted should be bled less or not at all, such as infants, the elderly, and pregnant women. He advocated bloodletting for all groups, and for several, he insisted that the respective state actually called for more bleeding, rather than less.67

The Nature of Yellow Fever as a Key to Therapy

During the late eighteenth century, the identification of diseases was heavily influenced by two contrasting tendencies. One trend was to emphasize perceived variations and to assert that even the slightest difference in symptoms marked off separate diseases. Many writers were upset by this tendency. A British physician asserted: “To lay hold of the occasional symptoms which arise from the differences of constitution and other circumstances, and erect them into new diseases with terrifying names, burthens the memory, and tends to darken rather than

66. Account of Yellow Fever, 1794, p. 227; analysis of the states of pulse, pp. 223–26. Rush did offer criteria for distinguishing whether a weak pulse resulted from indirect debility (if so, venesection was appropriate) or direct (in which case it was not); “the other states of the pulse indicate bleeding in every stage of fever, and in every condition of the system” (pp. 225–26).
67. Ibid., pp. 195–205.
Further clouding the picture was the fact that, across Europe, diseases were called by a number of names. Consequently, there was a drive to designate a single name for each disease, noting the variants. Cullen did this in his highly influential work on nosology; but despite his effort to provide an orderly nomenclature, he still allowed for 1,387 diseases.69

How diseases were classed made a great difference in the therapy that authorities recommended. In his influential treatise on fevers, William Grant wrote of typhus:

>This is the only fever which we ought to distinguish by the name of putrid; the words putrid fever, of late years, have had no definite idea affixed to them; but are used to express every ill-conditioned fever, of what species soever it may be. . . . this mistake of terms must, of necessity, produce fatal errors in practice.70

As had been the case since the classical period, virtually all eighteenth-century medical authorities categorized fevers, at least in part, by the duration of their extreme symptoms. They could be intermittent (regular cycles of paroxysms and intermissions); continued (not marked by remissions that were complete or extended); or—recognized by most but not all writers—remitting (symptoms occasionally abated, but not so far as to establish perfect or regular intermissions). Cullen, who knew yellow fever only from reports, classed it as a form of typhus, because it was attended by lassitude. More commonly, it was referred to as a remittent fever. Other authorities regarded it as a type of continued fever, minimizing the significance or reliability of remissions. And many writers gave it compound names reflecting what they regarded as its nature, most notably bilious, putrid, malignant, or pestilential. Rush himself first wrote of the “bilious remitting and intermitting yellow fever,” later calling it simply “yellow fever”; according to his reckoning, during the epidemic of 1794 the disease was referred to not only as yellow fever but by thirteen other names.71 This reflected, he believed, confusion over its nature.

Uncertainty regarding how diseases were to be categorized, a problem compounded by the profusion of names, encouraged a number of writ-

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ers, Brown being among the most prominent, to put forward models for identifying diseases that did not rely on their symptoms, since these were regarded as too variable. The models always allowed for a sharp reduction in the number of recognized diseases. Brown acknowledged only one hundred diseases, and even these, he believed, fell into just two classes, sthenic and asthenic.

By stages, Rush also broke with traditional nosology. His move seems to have come at about the same time as his general break with Cullenism. In 1786, he was still telling his students that Cullen’s categorization of fevers was the best. By within ten years, however, he had come to strongly disagree with the practice of differentiating among diseases by symptoms, as nosologists did. Symptoms, he argued, were ephemeral; he preferred distinguishing by an essential quality that he called a “state.” In the end, however, he distinguished states by symptoms, and the boundaries as he perceived them were in many cases rather vague. Dispensing with the classes of diseases in Cullen’s nosology, he enumerated forty states of fever. For the traditional nomenclature, he increasingly expressed contempt. “Time,” he told his students, “will bury the Names of Diseases in the same Grave with the Heathen Gods; then, and not till then will Medicine be a perfect Science”; his only compromise was to use conventional names as an easy reference to symptoms and in order to make it easier for audiences to follow.

Likewise influencing Rush’s concept of disease was a belief that was very common in his period: it was generally thought that some diseases were closely related to others, and that they could transmogrify into those that were similar in nature. Rush was simply recognizing the same slipperiness that his peers did when he asserted that “the bilious fever often appears in the same person in the form of colic, dysentery, inflammation of the liver, lungs, and brain, in the course of five or six days.” Likewise, diseases were sometimes imagined to be amalgams. Referring to what he called “Boullam Fever,” Colin Chisholm commented: “When this disease first appeared here, and for some time after, the prevalent opinion was, that it was the yellow-fever of the West-Indies, engrafted on the European jail-fever.”

72. “Rush Lecture Notes, 1786” (n. 7).
73. Account of Yellow Fever, 1794, pp. 149–78. The year before this work was published, Rush referred to his new system in a letter to John Redman Coxe, 8 December 1795, in Rush, Letters (n. 8), 2: 765.
74. “Rush Lecture Notes, 1797” (n. 9), lecture #43.
75. Account of Yellow Fever, 1794, p. 150.
The belief that a disease could shift heavily influenced therapeutics. If diseases were essentially the same, a treatment that appeared to work for one might well do so for another. Perhaps most often expressed was the belief that since intermittent fever could usually be cured by cinchona, remittent fever, being similar to it, would be as well. Practitioners routinely attempted to transform dangerous diseases into related ones that were milder or more treatable, while on the other hand they strove to prevent moderate diseases from shifting into dangerous ones. Few would have challenged, and none would have thought strange, Rush’s assertion that “an intermittent has been changed by the premature use of the bark into a remitting fever, and a common remittent, has, by improper regimen or violent exercise, been excited into a yellow fever.”

While Rush after 1793 increasingly saw the disease names as being irrelevant, he believed that diseases did have a proper distinguishing essence, that being proximate cause. Soon, he was maintaining that all fevers had the same proximate cause, irregular arterial action:

There is but one fever, despite variations in predisposing, remote, or exciting causes, because there is only one proximate cause. . . . All ordinary fever being seated in the blood-vessels, it follows of course, that all those local affections we call pleurisy, angina, phrenitis, internal dropsy of the brain, pulmonary consumption, and inflammation of the liver, stomach, bowels, and limbs, are symptoms only of an original and primary disease in the sanguiferous system.

From this, it was only a small step to a statement that Rush made to his students 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. The proximate cause of disease is irregular convulsive . . . action in the [vascular] system affected.”

Classifying disease by its causes and recognizing only one cause made Rush, as he defined himself, a “Unitarian.” It followed logically that if there were only one disease, a single mode of therapy might cure it. Rush himself said, in a lecture: “The remote, predisposing and exciting causes of disease are all An Unit. . . . In the great majority of diseases it is only necessary to know their grade & duration in order to cure them. We give similar remedies in similar circumstances for every disease.”

The suggestion that a single treatment might be successful against all disease was generally rejected by contemporary authorities. With Rush obviously in mind, William Currie, another veteran of the epidemics in

Philadelphia, strongly criticized the belief that “a convulsive state of the arteries is the proximate cause of every fever, whether pestilential, typhus, intermittent, or remittent, and that copious blood-letting and purging are the best remedies . . . in every form that can possibly occur.”

For his part, Rush vacillated in applying the logical end of his theory to the therapeutics that he taught. In 1809, he was giving his students advice that, as far as it went, was in keeping with the norms of contemporary medical teaching:

Diseases of warm & cold climates require different treatment. . . . The season of the year should be attended to. Epidemics differ. . . . The dress & moral habits should be attended to. The inhabitants of Egypt require stronger purges than other persons. The nation of which a person is a native should be attended to.

By and large, however, his teaching and writing after 1793 favored applying a small number of remedies to treat a wide range of diseases.

Further encouraging a standard therapy was Rush’s growing tendency to see most diseases as inflammatory. Again, his defining moment came during the epidemic of 1793. While neither Cullen nor Brown had any firsthand knowledge of yellow fever and neither was alive by that time, both of them had asserted that most fevers were caused by debility. The Rush of 1786, still under Cullen’s influence, deferred to him when he told his students: “Debility and Spasm are the proximate Causes they produce that reaction wch constitutes all the phenomena of Fever.”

Seven years later, he would see the cause very differently.

There was a cardinal rule regarding venesection: inflammation called for bloodletting, often in copious quantities, or at least to the point where inflammatory symptoms abated. The rule went back to Galen and Celsus and was seldom disputed. Even Hippocrates, who had been critical of bleeding in putrid and bilious conditions, endorsed moderate venesection to counter inflammation. Prior to 1793, however, yellow fever was generally seen not as an inflammatory disease, in which the patient’s system was overcharged, but rather as one of debility, often

83. “Rush Lecture Notes, 1786” (n. 7).
debility to a dangerous degree. Beyond being classed as remittent or continued, it was likely to be thought of as putrid, malignant, or bilious, or some combination of the three. Most practitioners refrained from bleeding, at least bleeding copiously, when treating diseases in any of these categories, because the patient was regarded as being so weakened by the disease itself that the process might cause him to sink irrevocably. In the West Indies, the opposition to letting blood in yellow fever was so widespread that Jackson, an advocate of venesection, reported, “A great majority of British practitioners deprecate the use of the lancet.”

The rules, however, had become somewhat less clear during the preceding century. Sydenham had bled patients with confluent smallpox, even though this disease was often classed as malignant, and major authorities like Sir John Pringle and John Huxham had advocated moderate bloodletting in the early stages of most fevers. Had Rush chosen to prescribe limited venesection for yellow fever in 1793, he would still have gone against general practice in America and among British practitioners in the West Indies, but he would have been going down a path that was already fairly well marked out. What really set him apart was his call for copious venesection. Nevertheless, he did not prescribe it in the case of a disease that he considered putrid or malignant. To him, yellow fever was highly inflammatory.

Rush recognized that his position was quite unusual and that in consequence few initially agreed with his aggressive bloodletting. He wrote:

The habitual association of the lancet with [pleurisy] has often caused me to rejoice when I have heard a patient complain of a pain in his side in a malignant fever. It insured to me his consent to the frequent use of the lancet, and it protected me, when it was used unsuccessfully, from the clamours of the public, for few people censure copious bleeding in a pleurisy.


86. Ball reported: “Though most authors, as well ancient as modern, generally agree, that Malignant Fevers by no means admit of bleeding, yet the ingenious and successful practitioners, Sydenham and [Jean] Astruc, indiscriminately order it in all cases; and Huxham and Pringle allow it may be performed, with advantage, under certain circumstances, especially in the beginning” (Ball, Modern Practice of Physic [n. 11], 1: 120). In this, he exaggerated somewhat Sydenham’s tendency to bleed, but represented the others accurately. Rush was familiar with at least some of Huxham’s work well before 1793: in 1786, he told a class that his study of nervous fevers was “one of the best treatises ever written” (“Lecture Notes, 1786” [n. 7]).

87. Account of Yellow Fever, 1794, p. 214. Similarly, he wrote: “In pneumonies which affect whole neighbourhoods in the spring of the year, bleeding is the universal remedy. Why
In the wake of the epidemic of 1793, Rush justified his resolution to bleed in yellow fever on several grounds. He recalled that patients seemed to be tending naturally toward letting blood, for they were obtaining relief through spontaneous hemorrhages throughout the body. Although Rush did not expand on the significance of this observation, he probably took the tendency toward hemorrhage as a sign of plethora, for this connection was widely seen by contemporary authorities. Rush also put forth an empirical defense, noting the favorable outcome of the case of a boy whom he had bled for the fever on 20 August. According to his account, analogy also prompted him to bleed: “The symptoms of congestion in the brain resembling those which occur in the first stage of hydrocephalus internus, a disease in which I had lately used bleeding with success.”

The defense of his decision to bleed rested heavily on indications that were traditionally associated with inflammation. He reported having observed in patients “the appearance of a moist, and white tongue on the first day of the disorder, a certain sign of an inflammatory fever!” The diseases that had preceded the yellow fever in Philadelphia had all been inflammatory, he noted, so this one was probably so, as well. Finally, he argued, authority helped to establish that yellow fever was inflammatory. Noting that the weather in Philadelphia had been warm and dry prior to the outbreak, he recalled that Sydenham had observed that the same circumstance had preceded a smallpox epidemic, and that William Hillary reported that West Indian yellow fever was always inflammatory after a very hot season. Rush recognized Hillary as an expert on yellow fever, as he did Benjamin Moseley, author of the influential Treatise on Tropical Disease: “The authority of Dr. Moseley had great weight with me in advising the loss of blood, more especially as his ideas of the highly inflammatory nature of the fever, accorded so perfectly with my own.”

Two signs were generally looked to as indicating inflammation, and both went back to Galen. One was the appearance of the blood removed in phlebotomy. If it was sisy—glutinous, clumped—and threw off a buff layer, inflammation was present. The second was the patient’s pulse. If it was full and throbbing, this, too, revealed inflammation. Other signs, such as redness, heat, and topical pain, might corroborate the diagnosis.

should it not be equally so, in a fever which is of a more uniform inflammatory nature, and which tends more rapidly to effusions, in parts of the body, much more vital than the lungs?” (Account of Yellow Fever, 1793, p. 277).

89. Ibid., p. 259.
Rush rejected the appearance of the blood as a test. Reporting that the blood of some yellow fever patients was indeed sisy and covered with a buff coat, but that in other cases it was dissolved and loose, dark, lacking a crust, or otherwise distinct from the classic indicator, he interpreted each appearance as manifesting inflammation. He also claimed, however, that the pulse was a better guide. He was seen by admirers to be a master at interpreting the pulse, and in lecture he enumerated nineteen different types. Of the respective merits of blood and pulse in diagnosis, he observed: “When time, and more attention to this index of the state of the system in fevers, shall have brought to light all the knowledge that the pulse is capable of imparting, the appearances of the blood in fevers, will be regarded as little as the appearances of the urine.”

Rush did not conclude merely that yellow fever was inflammatory in nature, but that among the fevers it was second only to plague (a disease that, of course, he knew only from reports) in terms of the level of inflammation. And feeling that it would be necessary to remove a great deal of blood to counter the inflammation, he argued that bleeding needed to be copious, rather than moderate.

The nature of yellow fever was in fact the key to therapy, as contemporaries noted. James Tytler observed:

The cure of the yellow fever hath been attempted in various ways, according to the theories laid down by different physicians concerning its nature. Dr. Cullen considers it as of the nature of typhus fever, and of consequence would have it treated with antimonials. . . . Dr. Rush, from his opinion that it is the highest degree of inflammatory fever, recommends powerful evacuants, and large blood-letting. . . . Dr. Brown, who would have considered it as a disease of debility, would of course have prescribed opium and other stimulants. . . .

Rush himself maintained that in 1793 there were in Philadelphia two primary modes of treating the fever: “the one dictated by an opinion that

91. “Rush Lecture Notes, 1797” (n. 9), lecture #4. Of an observation by Sydenham regarding the appearance of blood in inflammation, Rush wrote: “It is a great mistake to suppose, as our author does, that a buffy coat upon the blood is the only call for bleeding” (Works of Thomas Sydenham [n. 55], p. 54n). Ramsay rated highly Rush’s knowledge of the pulse: Eulogium (n. 4), pp. 23–25. Hunter observed that while blood tended to be sisy with a buff coat in an inflammatory condition, this was not always the case; he also commented, however, that the sense of pulse texture was dependent on the tester: Treatise on the Blood (n. 37), 2: 39–40, 43. As Pepper points out, bloodletting would in fact promote size, by causing anemia: “Rush’s Theories” (n. 36), p. 125.


93. Ibid., p. 153.

the disease was highly putrid, and the other, that it was of a highly inflammatory nature."95

Attack and Counterattack

Rush’s yellow fever therapy was developed in an extraordinarily stressful circumstance. In the wake of the epidemic, he wrote at length about his travails and the mental state that they induced. He lost his sister to the fever, as well as friends and apprentices. Each day he saw many patients, sometimes more than a hundred, and was forced to turn away others who begged for help. Seldom did he sleep for more than a few hours, and even then he was beset by nightmares. Early on, he gave up meat and wine, living on only a small quantity of vegetables. He lived in fear of contracting yellow fever and was convinced that in his weakened condition he could not survive an attack. Three times he fell ill with fever, letting blood at least twice.96 The personal history that Rush published provided fodder to his critics. Patterson commented, “A physician, who is constantly busied, sees too much, and does not think enough.”97

The therapeutics first enunciated by Rush during the epidemic of 1793 is best explained by reference not only to contemporary authorities and practice, but also to his character and personality. He was high-strung and combative, easily offended and slow to forgive. During the course of that epidemic he published several letters attacking physicians who followed methods other than depletion and he showed little collegiality toward them, providing at most a patina of respect. He had some allies, but he felt betrayed by friends who adhered to other practices and he also feared that rival methods would win over the people, thousands of whom would die in consequence.98


96. Any biography of Rush is likely to include extensive coverage of his experiences during the epidemic, but his “Narrative of the state of the Author’s body and mind during the prevalence of the fever” (Account of Yellow Fever, 1793, pp. 339–63) is an exceptional piece of its kind and should be consulted by anyone seriously interested in this topic. Rush’s correspondence from the core period of the epidemic (Rush, Letters [n. 8], 2: 644–726, passim) is a valuable supplement. Rush believed the fevers that attacked him to be yellow fever but he specifies this in only one episode: letter to Rachel Rush Montgomery, Philadelphia, 18 September 1793; Rush, Letters (n. 8), 2: 666; cf. Ibid., 670, 704n3, 742–43.


98. Rush discusses the contention in Account of Yellow Fever, 1793, pp. 207–43. Although the coverage is obviously from his point of view, he does quote at length letters to the press that were written by his rivals.
The verbal assaults that Rush launched in 1793 reflected his general tendency toward invective. Yet despite this aggressiveness, he viewed himself as the innocent victim of persecution. Even when he believed that he was winning converts to his method for treating yellow fever, he complained: “This victory . . . has not been a cheap one. It has been purchased at the expense of much labour and obloquy. The number of patients who died under my care, has been much exaggerated, and the most affecting stories have been circulated of their dying under the immediate use of my remedies.”99 The sense of persecution remained with him. Late in life, he wrote: “My persecutions have often been such as to subject me to the pity of my friends, and even of my enemies,”100 and in a letter to John Adams he likened himself to Jeremiah, in that he suffered for speaking the truth.101

Although he regarded himself as an injured party, Rush appears to have been sensitive to criticism that he was adamant in his opinions and unwilling to endure dissent. On occasion, he did change his mind, and he asserted that this was a virtue.102 He claimed that he was flexible enough to moderate his method when he could do so without reducing its effectiveness and even claimed that because of the prejudice against bleeding, he had thought to forgo it, only to realize that yellow fever was too dangerous to permit this.103

Rush may have been thin-skinned, but he certainly had his critics. Many of the most vehement attacks came from British writers at home and in the West Indies, for few of them endorsed the application of copious venesection in the case of yellow fever or of other fevers that were not traditionally accounted inflammatory. Rush had a ready explanation for their stance: Francophobia. The French often bled for fevers, and it was for this reason, he asserted, that the British rejected venesection. Their prejudice, in turn, was adversely affecting American attitudes:

Here then we discover the source of the prejudices and errors of our countrymen, upon the subject of blood-letting. They are of British origin. They have

99. Account of Yellow Fever, 1794, p. 117.
101. Rush to Adams, 26 December 1811, in Rush, Letters (n. 8), 2: 1116.
102. Account of Yellow Fever, 1794, p. 123. Nevertheless, in the most significant case of his changing an opinion—stating in 1809 that yellow fever, contrary to his previous argument, was not contagious—he did so grudgingly, and a rumor later spread that on his deathbed he changed his mind yet again: Edward Nathaniel Bancroft, A Sequel to an Essay on the Yellow Fever . . . Intended to Prove . . . that the Fever, called . . . Pestilential, has no Existence as a Distinct, or a Contagious Disease (London: Callow, 1817), pp. 467–74.
been inculcated upon us in British universities, and in British books; and they accord, as ily with our climate, and state of society, as the Dutch foot stoves did, with the temperate climate of the Cape of Good Hope.\textsuperscript{104}

He also warned his students not to be drawn in by British texts: “In setting out then, be independent of \textit{British Books & British Universities} as guides in \textit{American diseases} and \textit{never lose sight} of the \textit{climate} & \textit{peculiarities} of yr. own \textit{country}.”\textsuperscript{105}

Prior to his break with Cullenism, virtually all of the medical authorities that Rush referred to in lecture or in print were British. After 1793, however, he began to cite Continental writers. Notable among these was Leonardo Botalli, or Botallus, a sixteenth-century Sardinian physician who was seldom mentioned in contemporary literature. Rush used him for one purpose only: as an authority to endorse copious bloodletting. In 1797, he told his students: “Botallus says one hundred thousand men perish from the want of blood letting, or from its being used out of time, to One who perishes from too much bleeding, prescribed by his physician. I wish Gentlemen I had this Sentence wrote in Capitals over the door of our room.”\textsuperscript{106}

In the end, however, Rush remained a product of British medical education. Late in life, he attempted to legitimize his method by demonstrating that it drew on recognized authority, and the standards that he looked to were British. He referred to this effort in a letter to his son, then a student at Edinburgh, in May 1810: “I am now preparing notes upon Sir John Pringle’s \textit{Diseases of the Army} . . . Such publications serve as vehicles for my principles in medicine.”\textsuperscript{107} Rush eventually annotated four works: \textit{The Works of Thomas Sydenham, M.D. on Acute and Chronic Diseases; with Their Histories and Modes of Cure} (1809); George Cleghorn,
Observations on the Epidemical Diseases of Minorca (1809); William Hillary, Observations on the Changes of the Air, and the Concomitant Epidemical Diseases in the Island of Barbadoes (1811); and Pringle’s Observations on the Diseases of the Army (1812).

Of the four authors, Sydenham presented the greatest difficulties. This was ironic, given that Rush had for many years cast himself as a disciple of his. Toward the close of his treatise on the epidemic of 1793, Rush offered a defense of his method: “My principal aim has been to revive, and apply to it, the principles, and practice of Dr. Sydenham, and however coldly those principles, and that practice may be received by some physicians of the present day, I am satisfied that experience in all ages, and in all countries will vouch for their truth and utility.”

Still, Rush certainly knew that on many key points his practice differed markedly from Sydenham’s. As recently as 1790, he had observed that only his facts had “preserved the works of the immortal Sydenham from being destroyed by their mixture with his absurd theories.” Pringle was perhaps the most accommodating of the four. He had promoted venesection more generally than had Sydenham, and although he never observed a case of yellow fever, the third edition of his work—the edition that Rush published—including a lengthy letter in which Richard Huck discussed his practice in treating the disease, which he encountered in the West Indies. During the early 1770s, Huck had been a mentor to Rush, and the latter continued to write of him fondly and with admiration. Furthermore, Huck endorsed bloodletting in the treatment of yellow fever. Hillary served Rush as an authority on yellow fever, while Cleghorn wrote of various fevers that he had treated in Minorca during the 1740s. Each of the four works was respected, though Cleghorn was no longer widely read and Hillary was not cited on yellow fever to nearly the extent that several other authorities were. All four authors endorsed bloodletting in fevers, but not as generally as Rush did, nor in equivalent quantities. While Rush criticized each for practices that he did not regard

108. Account of Yellow Fever, 1793, p. 337. When in November 1793 Rush resigned from the College of Physicians of Philadelphia, he donated a copy of Sydenham’s Works, protesting thereby the reluctance of various members to adopt his practice. His letter announcing his resignation and his donation is addressed to John Redman, 5 November 1793, in Rush, Letters (n. 8), 2: 740.

109. Rush, Eulogium in Honor of Cullen (n. 23), p. 27.

110. On Huck’s contribution and Rush’s attitude toward him, see Pringle, Diseases of the Army (n. 9), pp. 177–81; Rush, Autobiography (n. 1), pp. 54–55. During the period 1771–75, Huck was a mentor to Rush and sought to win his admission to the Royal Society; letters #162–66, “MSS Correspondence of Dr. Benjn. Rush,” Rush MSS, Pennsylvania Historical Society.
as “modern,” he also interjected himself to praise statements that accorded with his views. But while he seems to have hoped to promote a sense that great authorities—and at least Sydenham and Pringle were in this class—endorsed his therapeutics, they in fact stopped far short of this.

Although some British authors, and even a few in the West Indies, expressed admiration for Rush and his method, it was rather in America that his influence was greatest. Even during the epidemic of 1793, despite the stressful and dangerous circumstances that he faced daily, and despite his battles with a number of other physicians, he sensed that his method was winning out in Philadelphia. During the last two decades of his life, his heroic style would have a major impact on American practice, and it would remain significant, if somewhat less influential, well beyond 1850. Certainly, the rise of heroic medicine was not the work of any one individual, but there is much to be said for Holmes’s observation that Rush “gave a direction to the medical mind of the country more than any other one man.”

The American Therapeutic

The image of Rush during the 1793 epidemic may be that of an over-worked, overstressed individual, desperate in his search for answers. Perhaps, indeed, the circumstances help to account for the drastic nature of his therapy. But this is only the case in the first instance. Rush lived on for twenty years, through tranquil as well as stressful periods, and far from moderating his method, he maintained it, made it in some respects even more severe, and spun around it a web of theory that justified similar therapeutics in confronting a wide range of diseases. Clearly, then, this was the method that he preferred.

The simple reason for Rush’s continuing advocacy of depletion was that he was convinced it worked. Many medical men of the period published records to support claims about the validity of their practice.

111. A number of letters written by Rush to his wife in 1793 refer to the growing popularity of his method and the general rejection of alternatives: 18–19 September; 29–30 September; 2–3 October; 23–24 October, 1793, in Rush, *Letters* (n. 8), 2: 669, 685, 693, 723–24. See also Rush to John Redman Coxe, 5 May 1795, ibid., p. 759.


113. Holmes, *Currents and Counter-Currents in Medical Science* (n. 2), p. 27.
Rush insisted that the numbers overwhelmingly supported his yellow fever therapy as opposed to others—and, if taken at face value, they did. According to his reckoning, during the first weeks after he introduced depletion in 1793 he lost less than 1 percent of his patients; the percentage then increased, but only slightly, and even the few losses he explained away. It is appropriate to note that he admitted difficulty in keeping track of his many patients. But he did not concede that his therapy failed to save any patient whose case was not yet formed. As he saw it, the method was virtually infallible.

Indeed, far from being despondent regarding the potential for medicine to handle desperate diseases like yellow fever, Rush was very much an optimist. While some medical writers were asserting that yellow fever was beyond the power of medicine, Rush claimed that it was easily curable. Again, his perspective was not unique. At the turn of the nineteenth century, many British and American medical authorities took the occasion to assess how far medicine had progressed in the recent past, how far it had to go, and how quickly it might arrive. A number were optimistic. Even the English physician Thomas Beddoes, who was often a gadfly, wrote: “A boundless region of discovery seems to be opening before us.”

Rush was optimistic regarding medicine in part because of his sense of national destiny. In 1790 he asserted that the United States, by perfecting civil government and encouraging independence of mind, was positioned to promote original thinking on all subjects. It was obliged, therefore, to lead the way in forming “a complete system of medicine.” He believed that he and his method could help to achieve this goal.

Like Boerhaave and Cullen, Rush spread his system largely through his students, who practiced it, wrote of it, and taught it throughout the United States. As Holmes pointed out, he had “thousands” of students—

114. Account of Yellow Fever, 1793, pp. 308–11. In all probability, many of those who consulted Rush were imagining their symptoms. At least fifty of Nassy’s patients, he reported, were convinced that they had yellow fever, when this was not the case; when he could not dissuade them, he fabricated a remedy: Nassy, Observations on the Cause (n. 16), p. 18n. Cf. [Richard Folwell,] A Short History of the Yellow Fever, that Broke Out in the City of Philadelphia, in July, 1797, with a List of the Dead, 2nd ed. (Philadelphia: Folwell, 1798), p. 24.

115. Among those who doubted the power of medicine to cure, note Blane, Observations on the Diseases of Seamen (n. 16), 2: 113; Lempriere, Practical Observations (n. 10), 2: 91–92. Of course, Rush mainly claimed an ability to prevent the disease from forming.


117. Rush, Eulogium in Honor of Cullen (n. 23), p. 29.
2,250, according to Ramsay’s estimate. Students and young practitioners of the time were stereotyped as being easily confused and as having difficulty with complex cases, and in some respects, Rush may have compounded their confusion. As has been noted, he on occasion encouraged his students to consider such factors as climate in determining the appropriate regimen. In many cases he provided rather broad guidelines: “You will often be under the Necessity of drawing from One to 200 oz of blood; but in this as in other Cases, let the state of the system govern the use of your lancet.” And he often cautioned them not to prescribe for the names of diseases, though many practitioners found that simple. At times, however, he provided specific guidance. Late in his career, he told a class that purgatives were forbidden “if the system is below the par of excitement—by disease or simple debility, by Typhus &c. & in convalescence from all acute diseases.” In a few instances, he even cautioned against venesection; when dealing with the “Typhoid state of fever,” he advised his students, they should bleed “with great caution & in very small quantities & in habits debilitated by great fatigue or low living it will not do.”

On the whole, however, Rush provided as much direction and consistency regarding therapy as did most professors of the time. And despite allowing for special cases, he was quite consistent on the issue of bloodletting. Even the most bewildered student would have recognized that venesection was the key to his therapeutics. During the last two decades of his life, he applied massive bloodletting to the treatment of progressively more diseases, and, as he saw it, conquered each in turn. Venesection became his panacea. As he told one class:

I cannot dismiss this Subject Gentlemen without calling upon you to Venerate the Lancet. It is the Magna gratia Coeli, The great gift of Heaven. To its other triumphs over diseases, we can now add one more. It has robbed the dreadful Hydrophobia of its Mortality.

118. Ramsay, Eulogium (n. 4), p. 19. Ramsay claimed that during Rush’s early years as a professor, his enrollment averaged only 16–30, but that it grew steadily afterward and stood at 430 in 1812 (ibid.). Rush himself estimated his class in 1795 at over 100, noting that his rival, Adam Kuhn, had “his usual 35” (Rush to John Redman Coxe, 8 December 1795, in Rush, Letters [n. 8], 2: 765). Binger cites enrollments of 45 in 1790, 332 in 1812, and 2,872 total in the period 1779–1812; as he notes, Rush also had a number of apprentices, as many as thirty at the peak of his career: Binger, Revolutionary Doctor (n. 1), p. 184.

119. “Rush Lecture Notes, 1797” (n. 9), lecture #42.

120. Ibid., lecture #52.


122. Ibid., p. 153.

123. “Rush Lecture Notes, 1797” (n. 9), lecture #52.
Rush detected rapid progress in medicine, and he saw disease being eradicated in the foreseeable future. Characteristically, he believed that wiser and more widespread use of venesection would help to bring this about. He hoped to see bloodletting employed in treating a greater range of diseases. He even expressed the hope that arteriotomy, which had not been widely used in professional practice for centuries, could be restored, assuming that it could be controlled.\(^{124}\)

Rush made things simple, for both students and citizens: a few rules, an all-embracing concept of disease, therapy that was similar if not identical from patient to patient, and, above all, the lancet. The practice of Rush reflected his politics. He was a fervent republican, with strong egalitarian principles, and as such he exalted his therapy as being sufficiently simple that people could practice it on their own families, even on themselves.\(^{125}\) The simplicity of his method, he argued, would even be of use to missionaries, as they set out to Christianize the world:

> It will be sufficient for our missionaries to know, that a powerful epidemic chases away, or mixes with all other diseases, to be acquainted with the different states of the pulse, to be able to open a vein, to administer a few strong doses of purging physic, and to gratify the calls of nature in their patients for cold water and cool air.\(^{126}\)

In the final analysis, the radicalism in the method put forward by Rush came from the man himself. On virtually all issues that fed into his therapy for yellow fever, he could legitimately point to authorities who anticipated his positions. What he provided was the drive to incorporate the strands into a potent package. His therapy was extreme because he believed in extreme therapy. In his mind, virulent disease was the enemy, and only violent therapy would work against such violence. This therapy, he firmly believed, would conquer disease and usher in a golden age.

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124. *Account of Yellow Fever, 1794*, pp. 241–48. In “An Inquiry into the Comparative State of Medicine” (n. 55), pp. 225–49, Rush also detailed and sought to account for the dramatic advance against various diseases in Philadelphia, although he noted (pp. 248–49) that to continue the advance it would be necessary to conquer yellow fever.

125. Rush’s egalitarianism was also manifested in antiprofessionalism and even animosity toward the medical profession; this and his hope to make his method available to all is discussed in *Account of Yellow Fever, 1793*, pp. 325–37.