American Attitudes toward the Germ Theory of Disease: Phyllis Allen Richmond Revisited

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FEW works have had such a long and influential reign in the scholarly corpus of medical history as Phyllis Allen Richmond's "American attitudes toward the germ theory of disease, 1860–1880," which appeared in the pages of this journal in 1954. Her contention that American physicians were especially slow to accept the germ theory of disease has been repeated as fact in countless works of scholarship, and the original article is still frequently assigned in courses. Rarely has one short work had so great an impact on the historiography of our field. For more than forty years, Richmond's thesis has rarely been challenged, a record that surely has few precedents in the field.[1]

1. Phyllis Allen Richmond, "American attitudes toward the germ theory of disease" (1860–1880), *J. Hist. Med. Allied Sci.*, 1954, *9*, 58–84. Sample course syllabuses appear in Education Committee of the American Association for the History of Medicine, *History of Medicine in the Undergraduate Curriculum* (n.p.: the Association, 1991). Another article that achieved a similar unquestioned place in the historiography of American medicine was Frances Kobrin, "The American midwife controversy: a crisis of professionalization," *Bull. Hist. Med.*, 1966, *40*, 350–63.

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Based on a review of the published medical literature, Richmond asserted that "Little attention was paid to European bacteriology during the 1870s." According to her, the American profession was so "completely hostile to the new views" of disease that "there was no partisan bickering over the hypothesis as one might have expected," a "uniformity of opinion" that was strictly "an American phenomenon." Moreover, even after 1875, when American physicians did begin to discuss the germ theory, they did so in a highly unscientific manner, employing imprecise terminology and manifesting deep allegiances to old-fashioned miasma theories. As Richmond said of one author, he exhibited "a fine misunderstanding of all the questions of his day," from the zymotic theory of disease to Pasteur's work on fermentation. But "the chief complaint against the American medical profession in this period (1860 to 1880)," in Richmond's estimation, was "the lack of any attempt at experimental work to prove or disprove the theory."[2]

To explain the profession's backwardness, Richmond pointed to the general lack of support for science in nineteenth-century America. Unlike in France and Germany, she noted, there was no governmental support for basic research. The proliferation of commercial medical schools contributed to low standards throughout the whole profession. As a result, American physicians had little appreciation for pure science. Intensely pragmatic in their interests, they regarded the germ theory of disease as inconsequential because it offered no quick therapeutic returns. In Richmond's view, the germ theory controversy served as "a striking example of American neglect of research in basic science."[3]

The tone of Richmond's critique was well-suited to the intellectual climate of the mid-1950s. Even before the 1957 Sputnik crisis, American science educators and policy makers were lamenting that the

2. Richmond, (n. 1), pp. 64, 75, 76, 61.

3. Ibid., p. 83. Richmond's portrayal of American resistance to the germ theory received powerful reinforcement with the publication of another equally influential article, Lloyd Stevenson, "Science down the drain: on the hostility of certain sanitarians to animal experimentation, bacteriology, and immunology," *Bull. Hist. Med.*, 1955, *29*, 1–26. Stevenson's title neatly conveyed the tone of his argument: that the moralistic orientation of mid-century public health reformers, known collectively as "sanitarians," let to their rejection of animal experimentation, bacteriology, and immunology. Among the casualties of this unscientific, moralistic world view was the germ theory of disease. He emphasized religious and moral values more than Richmond did, but otherwise their arguments were quite compatible.

United States had fallen dangerously behind the Soviet Union in the scientific cold war. The rising star of post-war biomedicine, newly funded by the National Institutes of Health, served as an inspiring example of what "democratic science" might produce, if generously supported by the federal government and freed from expectations of immediate results. Richmond's depiction of the once pitiful state of nineteenth-century American medicine, as evidenced by its failure to appreciate one of the greatest discoveries in medical history, pointed to the direction post-World War II biomedicine should take toward a basic science purified of narrowly pragmatic, provincial concerns.[4]

Medical historians' historiographic preoccupations changed dramatically over the next three decades, yet Richmond's thesis remained quite persuasive. As a new generation of social historians documented the factionalism, commercialism, and therapeutic uncertainty that plagued mid-century practitioners, her portrait of nineteenth-century American physicians as an unscientific lot, blinded by both pragmatism and moralism, made all the more sense. With few exceptions, that portrait has prevailed to the present, and the assumption of American exceptionalism continues to dominate modern historiography.

Yet Richmond's argument left a puzzling question, which she herself noted. Given that American physicians were so resistant to both germ theory and experimentalism up to the early 1880s, Richmond acknowledged that the "sudden appearance of the germ explanation" in the mid-1880s was "one of the more interesting mysteries of the whole American scene." As she put it, "One year there is scarcely a word about the theory in any of the journals, and the next year it is included as an accepted fact as if it had been known all along." The usual explanation for this quick reversal is the revolution in

4. On American science and the Cold War, see Roger Geiger, *Research and Relevant Knowledge* (New York: Oxford University Press, 1993), and Stuart W. Leslie, *The Cold War and American Science* (New York: Columbia University Press, 1993). For a good descriptive account of post-World War II medical optimism, see James T. Patterson, *The Dread Disease: Cancer and Modern American Culture* (Cambridge: Harvard University Press, 1987), esp. pp. 137–200. Richmond's article was drawn from her doctoral dissertation at the University of Pennsylvania, completed under the direction of Richard Shryock, whose own work emphasized American physicians' indifference to basic medical research. See Richard H. Shryock, *American Medical Research, Past and Present* (New York: The Commonwealth Fund, 1947). He also endorsed the "backward" hypothesis in Richard H. Shryock, "Germ theories in medicine prior to 1870: further comments on continuity in science," *Clio Med.*, 1972, *7*, 81–109.

bacteriological technique begun in the late 1870s, most notably by Robert Koch and his students. Yet if, as of 1880, American physicians were as uniformly uninformed about European developments or distrustful of experimentalism as Richmond portrayed them, these improvements in laboratory method should hardly have made such a dramatic difference in so short a time.[5]

Moreover, in strictly pragmatic terms, the germ theory of disease had little more to offer general practitioners in 1890 than it did in 1870, yet by the last decade of the nineteenth century, it had achieved widespread legitimacy within the upper ranks of the American medical profession. To be sure, applications of the germ theory paid more immediate returns in the fields of surgery and public health. By the late 1880s, many American surgeons had taken up Listerism and the antiseptic method, although not necessarily the germ theory itself; in public health, the laboratory discoveries of the 1880s clearly strengthened the movement already underway toward more vigorous programs of disease control. Paul Starr has concluded that the successful incorporation of germ theory into surgical and public health practice was sufficient to give the whole of American medicine greater professional authority in the 1880s and 1890s.[6]

But even if this observation is true, the puzzle Richmond identified in 1954 remains: if the 1870s were the intellectual "doldrums" she styled them, how do we explain the relatively rapid transformation in American attitudes toward the germ theory? And if the profession was so resolutely pragmatic in its orientation, why did the theory gain so much ground by 1885, well before its utility to either surgery or the public health movement had been clearly demonstrated?

5. Richmond, (n. 1), p. 82.

6. Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982), esp. 134–139. For the term "doldrum," see Richmond, (n. 1), p. 67. In a more general sense, Gerald Geison has argued that the rise of the experimental sciences gave late-nineteenth century American medicine "a new and now culturally compelling basis for consolidating its status as an autonomous 'learned profession,' with all the corporate and material advantages that such status implies." See Gerald Geison, "Divided we stand: physiologists and clinicians in the American context," in Morris Vogel and Charles E. Rosenberg, eds., *The Therapeutic Revolution* (Philadelphia: University of Pennsylvania Press, 1979), pp. 67–90. Quote is on p. 85. On the impact of germ theory on surgery, see Thomas P. Gariepy, "The introduction and acceptance of Listerian antisepsis in the United States," *J. Hist. Med.*, 1994, *49*, 167–206; and Gert Brieger, "American surgery and the germ theory of disease," *Bull. Hist. Med.*, 1966, *40*, 135–144. On the germ theory and the public health movement, see Howard Kramer, "The germ theory and the early public health program in the United States," *Bull. Hist. Med.*, 1948, *22*, 233–247; and John Duffy, *The Sanitarians: A History of American Public Health* (Urbana: University of Illinois Press, 1990).

In this article, I address these questions, and revisit the subject of American attitudes toward the germ theory of disease from the very different historiographic ground of the late 1990s. Phyllis Allen Richmond's article still inspires respect for its exhaustive research and attempts to place medical thought and practice in a broader cultural context. But after the past decade's developments in the social studies of science and post-modernist critiques of "knowledge production," the flaws in her approach to the germ theory debate are glaringly apparent.[7]

What might be called the "myth of American backwardness" rests upon two ahistorical premises that are fundamentally at odds with modern historiographical trends in the history of science and medicine. First, Richmond invoked the phrase "germ theory of disease" as if it had an existence independent of a specific historical context. In her mind, the theory was a fixed and transcendent scientific truth, a yardstick against which past discourses about disease could be judged as properly scientific or lamentably muddled. Second, Richmond endowed experimentalism with a similarly transcendent authority, labeling American physicians as backward for refusing to accept the knowledge claims of "European bacteriology," as if its superiority should have been clearly evident in the 1870s.

Richmond's narrative of resistance ceases to be so convincing if we read the same debates with a different, and, I believe, more defensible, set of premises: that the phrase "germ theory of disease" had no fixed meaning as of the 1860s, but rather emerged through a complex process of scientific debate, especially in the period from 1870 to 1885; and that in the process, the very meaning of what constituted a scientific discourse changed as well. Starting from these assumptions, I argue that, far from being unproductive muddles, the debates of the 1870s laid the foundation for the "mysterious" acceptance of a more clearly articulated and persuasive germ theory of disease in the mid-1880s. Moreover, when compared to their European counterparts, the upper ranks of American physicians do not appear exceptionally backward in their understanding of the germ theory debates. Contrary to the usual stereotypes, those who criticized

7. For overviews of these trends, see Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992); and Jan Golinksi, "The theory of practice and the practice of theory: sociological approaches in the history of science," *Isis*, 1990, *81*, 492–505.

the germ theory were not uniformly hostile to experimentalism, and often employed experimental evidence in their critiques. And far from portraying the germ theory as irrelevant, its advocates represented it as a powerful source of guidance in the *preventive* and *hygienic* aspects of their work, an aspect of medical practice too often overlooked by medical historians.

When reread today, what is immediately striking about Richmond's argument is the extraordinarily *early* time frame she used for pronouncing American responses to the germ theory debate backward. Starting her literature review with 1860, Richmond's portrayal of American physicians as tardy in their appreciation of European discoveries rests on the fact that the medical press paid little attention to the germ theory debates until the mid-1870s. Yet the phrase "germ theory of disease" does not even show up in the English-language medical literature until around 1870, so far as I have been able to determine. Under the heading "germ theory" in the U.S. Surgeon General's Catalogue, the first article with that phrase in the title, a piece by the British physician Jabez Hogg in the *Medical Times and Gazette* of London, was dated 1870.[8]

That 1860 is far too early to begin looking for any discussion of a "germ theory of disease" is amply reinforced by recent studies of Louis Pasteur and Joseph Lister, the two figures most closely associated with early formulations of the modern germ theory. Put simply, neither had a coherent "germ theory of disease" in mind much before the mid- to late 1860s, and even then, both were initially more interested in understanding the processes of fermentation and putrefaction than enunciating a sweeping model of disease causation.

Pasteur voiced speculations on the relationship between microbes and epidemic disease in the late 1850s and early 1860s, but his initial focus was on what Gerald Geison refers to as a "germ theory of fermentation." With his experiments on pébrine, conducted between 1865 and 1870, Pasteur clearly began moving in the direction of a more comprehensive theory of disease, but not until the mid-1870s did he "make the infectious diseases the focus of his research," Geison

8. *Index Catalogue of the Library of the Surgeon-General's Office, United States Army*, 1st series, 5 (Washington, D.C.: U.S. Government Printing Office, 1880–95), 385–88. Note that there are few items listed under this heading that date before the late 1860s. I suspect Richmond's decision to start her survey in 1860 reflected her extensive work on earlier animacular hypotheses, which she more or less equated with the modern germ theory of disease.

writes.[9] Likewise, Lister's celebrated papers on the antiseptic method in surgery appeared only in 1867, and what he articulated then, as Christopher Lawrence and Richard Dixey have recently argued, was also a "germ theory of putrefaction" rather than a germ theory of disease. Lawrence and Dixey show that Lister and his followers later recast the original premises of Listerism to fit the Germanic "germ theory of disease" that emerged in the wake of Robert Koch's brilliant experimental successes of the late 1870s and early 1880s.[10]

Thus, recent historical work suggests that what came to be debated under the heading "germ theory of disease" did not emerge as a distinctive set of propositions until at least the late 1860s, and even then, the meanings ascribed to the phrase evolved dramatically between 1870 and 1885, as the focus of attention broadened from Pasteur and Lister to encompass Koch and his disciples.[11] Richmond's finding that the American medical press began to discuss the theory seriously only in the mid-1870s and that leaders of the profession reached little consensus about its validity or implications for the practice of medicine until the early to mid-1880s, seems far less exceptional from the standpoint of these recent studies of the germ theory's leading architects.

Moreover, Richmond's claims for American backwardness rested on the belief, asserted rather than supported in the article itself, that European physicians showed an earlier and more informed appreciation of germ theory and experimental evidence in general. Yet even a cursory survey of the French, German, and English medical communities in the 1870s and 1880s reveals that they too had heated arguments over exactly the same issues that vexed their American contemporaries. In France, Louis Pasteur continued to battle with advocates of spontaneous generation into the mid-1870s, and his work received relatively little notice among his fellow scientists until the early 1880s, according to Bruno Latour. In Germany, the senior medical statesman

9. Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton: Princeton University Press, 1995), esp. pp. 32–33, 90–91. Quote is on p. 32. See also Bruno Latour, *The Pasteurization of France* (Cambridge: Harvard University Press, 1988), esp. pp. 16, 26.

10. Christopher Lawrence and Richard Dixey, "Practising on principle: Joseph Lister and the germ theories of disease," in Christopher Lawrence, ed., *Medical Theory, Surgical Practice: Studies in the History of Surgery* (New York: Routledge, 1992), pp. 153–215.

11. My own research on the United States confirms the importance of distinguishing between the first version of the germ theory, which was heavily influenced by sanitarian assumptions, and the later, more bacteriologically informed version that developed in the 1890s. Nancy Tomes, *The Gospel of Germs: Men, Women, and the Microbe in American Life, 1880–1930* (Cambridge: Harvard University Press, in press).

Rudolf Virchow dismissed Robert Koch's work on anthrax as "improbable" in 1878, and expressed reservations about the germ theory well into the 1880s. As late as 1892, Max von Pettenkoffer drank his celebrated "cholera cocktail" to prove that the cholera bacillus alone did not cause the disease. In England, John Tyndall and H. Charlton Bastian debated the spontaneous generation question into the late 1870s, and the surgeon Lawson Tait ridiculed Lister and his antiseptic method. In short, from the late 1860s well into the 1880s, advocates of the germ theory met with serious resistance in Europe as well as in the United States.[12]

This is not to deny that the American context of the debate was distinctive in some respects. Richmond was certainly correct in pointing out that Americans lagged behind Europeans in making *original* contributions to the debate over disease causation, and that American science was poorly funded in comparison to Germany or France. Moreover, it seems likely that the low standards and commercialized nature of American medical education meant that the majority of American doctors remained less cognizant of the debates over germ theory than did their European counterparts.

Still, American physicians by no means existed in the kind of intellectual isolation that Richmond's portrayal of the "doldrums" period suggests. The upper ranks of the profession, including the faculty and students at the better medical schools, the members of local medical societies, and the regular subscribers to the medical press, followed European developments in disease theory far more closely than her characterizations would lead us to expect. If, prior to 1880, few American physicians produced original contributions to the germ theory debate, many were avid readers and critics of European medical literature. The breadth of awareness is evidenced in lengthy reviews of European developments, including experimental work, which appeared in journals such as the *Boston Medical and Surgical Journal*, the

12. See, for example, on France, Geison, (n. 9) *The Private Science*, esp. pp. 110–29, and Latour, (n. 9) *Pasteurization of France*, esp. pp. 29–30. Latour expressed surprise at finding so few references to Pasteur in the *Revue Scientifique* from 1871 to 1880. On Germany, see Thomas D. Brock, *Robert Koch: A Life in Medicine and Bacteriology* (Madison, Wis.: Science Tech Publishers, 1988), p. 82; William Coleman, "Koch's comma bacillus: the first year," *Bull. Hist. Med.*, 1987, *61*, 315–342; and Richard J. Evans, *Death in Hamburg* (New York: Viking Penguin Books, 1990), esp. 490–507. On England, see Terrie Romano, "The cattle plague of 1865 and the reception of the germ theory in Mid-Victorian Britain," in this issue. See also John Farley, *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore: Johns Hopkins University Press, 1977).

New York State Medical Journal, the New York *Medical Record*, and the *Philadelphia Medical Times* in the early to mid-1870s.[13]

Another gauge of American interest in European developments can be found in medical school theses from the mid- to late 1870s. Ranging from the perfunctory to the competent, these student essays provide a good cross-section of what constituted common medical knowledge at any point in time. A sampling of thesis collections at the better medical schools suggests that the debates over the germ theory of disease attracted considerable student interest. To give but one striking example, in an 1876 thesis from the University of Pennsylvania, a student named Edwin Bertolet argued for the infectiousness of tuberculosis six years before Koch's isolation of the tubercle bacillus. His thesis included references to more than twenty experimental studies in three languages and were drawn from both medicine and veterinary science. Bertolet also reported that he had tried to inoculate guinea pigs with the disease, but failed to get any clear results because the animals died from other causes.[14]

Thus, Richmond's portrayal of American physicians in the 1870s as a monolithic group united in their disinterest toward the value of experimental work and the possible role of microbes in causing disease is clearly overdrawn. Contrary to her claim that no "partisan bickering" developed over the germ theory, from 1870 onward, a small but growing number of American physicians seriously debated the theory's credibility and the scientific standards by which its validity should be judged. Their debates coincided with, and increasingly contributed to, a much broader discussion concerning the relative value of scientific knowledge derived from the bedside versus the laboratory.

13. "The influence of the lower organisms in the production of infectious and contagious diseases," *Phila. Med. Times*, 1875, *28*, 761–64; E.P. Hurd, "On the germ theory of disease," *Bost. Med. Surg. J.*, 1874, *91*, 97–110; Thomas Satterthwaite, "Bacteria: their nature, and relation of disease," *Med. Rec. (N.Y.)*, 1875, *10*, 833–36, 849–55; Lewis A. Stimson, "Bacteria and their influence upon the origin and development of septic complications of wounds," *N.Y. Med. J.*, 1875, *22*, 113–145. Although he does not explicitly mention Richmond's thesis, I think my general argument here is quite consistent with the one made by James H. Cassedy, "The microscope in American medical science, 1840–1860," *Isis*, 1976, *67*, 76–97.

14. Edwin B. Bertolet, "An essay on the inoculability and infectiousness of tuberculosis" (M.D. thesis, University of Pennsylvania, 1876). Special Collections, Van Pelt Library, University of Pennsylvania [hereafter UPA]. On the medical thesis as an indicator of common medical knowledge, see John Harley Warner's foreword to the Medical College of South Carolina's unpublished bibliography of antebellum student theses.

As John Harley Warner has shown in a series of superb studies of nineteenth-century medical thought, the very standards for judging medicine scientific were being heatedly contested among the American medical elite during the 1870s and 1880s. The traditional historical narrative of a declining French clinical empiricism versus an ascendant German laboratory ideal does not do justice to the complexity of these debates. When American physicians first began to go to Germany, Warner has shown, they were not in search of laboratory training, but rather the same kind of clinical experience that had motivated the Paris migration. The identification of Germany with the laboratory came about only gradually in the 1870s and 1880s, as medical migrants to Germany were introduced to new methods of experimental science and returned home to advocate their usefulness.[15]

This point is especially important to an understanding of early debates over the germ theory. Richmond's invocation of a "European bacteriology" that Americans were supposedly ignoring in the 1870s constitutes one of the most ahistorical aspects of her argument. At the point when American commentators first began to debate the relationship between microbes and disease, the powerful symbiosis of Germany and bacteriology had yet to develop. Robert Koch was still an unknown private practitioner in Wollstein; the experimental evidence being offered by Louis Pasteur, John Tyndall, and other investigators was sketchy, to say the least; and the American hegira to study German bacteriology had yet to begin. In short, American commentators became engaged in a lively debate over the role of microbes in causing disease well before the scientific discipline of bacteriology came into existence.[16]

15. See John Harley Warner, "The fall and rise of professional mystery: epistemology, authority and the emergence of laboratory medicine in nineteenth-century America," in Andrew Cunningham and Perry Williams, eds., *The Laboratory Revolution in Medicine* (New York: Cambridge University Press, 1992), pp. 110–141; *The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America 1820–1885* (Cambridge: Harvard University Press, 1986); and *Against the Spirit of System: The French Impulse in Nineteenth-Century American Medicine* (Princeton: Princeton University Press, in press). On the German influence and the debate over science in medicine, see also the excellent article by Russell C. Maulitz, "'Physician versus bacteriologist': the ideology of science in clinical medicine," in Vogel and Rosenberg, (n. 6) *Therapeutic Revolution*, pp. 91–107.

16. On the American hegira to Germany, see Thomas Bonner, *American Doctors and German Universities: A Chapter in International Intellectual Relations* (Lincoln: University of Nebraska Press, 1963), esp. pp. 110–120. Maulitz, (n. 15), makes much the same argument I do about timing. The early career of William Welch illustrates the need to be careful

This prebacteriological discourse on the germ theory was conducted by a generation of physicians whose professional experiences and intellectual orientations do not map neatly onto the traditional battle lines of French empiricism versus German experimentalism. Consider four of the first and most eloquent voices in the debate, Joseph Richardson and James Lawrence Cabell, who spoke in favor of the germ theory, and Edward P. Hurd and Thomas Satterthwaite, who questioned it. Of the four, only Cabell and Satterthwaite had any European training, and it was the Paris-trained Cabell, not the Viennese-trained Satterthwaite, who championed the germ theory's cause. Richardson was a dedicated microscopist; the other three seem to have been drawn into the debate largely because of their interest in pathology and surgery.[17]

Reading through the pre-1880 debate, there is no gainsaying Richmond's general observation that it was beset by confusions in terminology and interpretation. Yet in this regard, American commentators seem little different from their European contemporaries, who were no less confused in the 1870s. To begin with, in the Anglo-American literature, the phrase "germ theory of disease" was initially used for two different propositions, one, the English microscopist Lionel Beale's theory that the agents of disease were degraded particles of bioplasm, produced by the body itself, and the other, the Pasteur/Lister hypothesis that living microorganisms were the agents of infectious disease. The latter was often referred to as the "parasitic germ theory," to distinguish it from the former.[18]

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about reading the later dominance of German bacteriology back into initial discussions of the germ theory. When Welch first traveled to Breslau in 1877, he went there to study experimental physiology and pathology, and took little interest in the work on bacteriology going on in the same laboratories. His conversion to the importance of both the germ theory and bacteriology came *after* his first trip to Germany in the late 1870s; only in 1884 did he return to Germany specifically to learn the new bacteriological methods. See Bonner, pp. 112–114.

17. "Edward Payson Hurd" and "Joseph G. Richardson," in Howard A. Kelly and Walter L. Burrage, eds., *Dictionary of American Medical Biography* (Boston: Milford House, 1971. reprt. of 1928 ed.), pp. 622–23, 1032; "James Lawrence Cabell" and "Thomas E. Satterthwaite," *National Cyclopedia of American Biography* (New York: James T. White and Co., 1904), *12*, 452, 298. Cabell was the oldest, born in 1813; the other three were born between 1836 and 1843. Richardson and Cabell were professors at the University of Pennsylvania and Virginia medical schools, respectively; Hurd and Satterthwaite were in private surgical practice.

18. The different variants are laid out neatly in Thomas Satterthwaite, "The present condition of the evidence concerning 'disease germs,'" in John Ashurst, Jr., ed., *Transactions of the International Medical Congress of Philadelphia, 1876*, pp. 1011–1028. See also F.A.P. Barnard, "The germ theory of disease and its relations to hygiene," in *Public Health Reports*

Efforts to sort out these different positions were further complicated by the loose construction of the word "germ," which was often used to designate a discrete contagious particle without necessarily specifying whether it was chemical or microbial in nature. For example, in an 1875 thesis at Yale on "Preventing the spread of contagious disease," a medical student named Franklin D. Clum referred to the exciting cause of contagion as "minute particles of poison or disease germs." While his subsequent discussion of their life cycle suggests Clum believed these particles were living organisms, he did not use more specific terms such as "parasite" or "bacteria" that would have clearly signaled his allegiance to what one commentator called the "germ theory proper." Likewise, in an 1876 article in the *Boston Medical and Surgical Journal*, a Boston physician named Arthur Nichols discussed the role of schools in spreading contagion, using the terms disease "poison" and "germ" in an interchangeable fashion.[19]

Even among those discussants who engaged the parasitic germ theory more directly, the terminology used to identify the microbial agents of disease varied dramatically in the 1870s; "vibrio," "algae," "fungi," "cryptograms," "microzymes," "animalcula," as well as the more familiar modern word, "bacteria," were all in common use. To muddy usages further, the word "germ" was sometimes employed to designate the spore stage of bacterial development. While regretting this confusion in terminology, Lewis A. Stimson wrote in the *New York Medical Journal* for 1875 that "it is easily explained by the great difficulties in the way of accurate observation, study and description." In this regard, New World commentators were no more confused than their European counterparts. Not until the early 1880s did there emerge a more standardized scientific language for describing the world of "the infinitely little," as Edward P. Hurd called it.[20]

A final complication of early debates over the parasitic germ theory was its varied application to different diseases. By the 1870s, many American physicians believed that the so-called specific fevers, such as smallpox, cholera, and typhoid, were caused by a ferment or poison

and Papers Presented at the Meetings of the American Public Health Association in the Year 1873 (New York: Hurd and Houghton, 1875), pp. 70–87. Barnard compares Beale's germ theory to what he terms the "germ theory proper," on p. 72.

19. Franklin D. Clum, "Preventing the spread of contagious disease" (M.D. thesis, Yale University School of Medicine, 1875) Fulton Medical Library; Arthur H. Nichols, "Schoolchildren and dangerous communicable disease," *Bost. Med. Surg. J.*, 1876, *94*, 319–23.

20. Stimson, (n. 13), p. 114; Hurd, (n. 13), p. 97.

unique to them alone. The more "specific" the disease appeared to be, especially if its contagion seemed portable, as in the case of smallpox or yellow fever, the easier it was to imagine as a hypothetical "germ disease." Thus a physician might willingly accept the germ theory applied to yellow fever while hotly contesting its relevance to diphtheria or tuberculosis. In other words, commentators were not only debating the validity of a general germ theory of disease, but also the germ theory as applied to individual diseases.[21]

Despite these various confusions and complications, American commentators on the germ theory demonstrated a clear sense of the central question under debate in the 1870s: was the causal agent responsible for infectious disease a chemical substance or a living organism? Most American physicians accepted the doctrine that zymotic diseases were the result of a chemical process of fermentation. In 1861, when Pasteur published his work demonstrating that living organisms, carried chiefly by the air, were responsible for fermentation, he suggested an alternative, biological view of disease causation, a view that gained a growing number of adherents in the late 1860s and early 1870s. By 1873, when F.A.P. Bernard surveyed the medical scene, he clearly perceived two general camps of opinion, one in favor of a chemical, the other of a biological, identity for the disease ferment.[22]

As Richmond herself admitted in the 1954 article, those opposed to the germ theory had some formidable arguments on their side. Microscopic examination of the air, water, and the human body revealed that microorganisms were everywhere, including the systems of the healthy, which diminished their credibility as pathological agents. Bacteria were known to play a central role in decomposing organic matter; thus it seemed probable that they appeared in the wake of the chemical process of disease, feeding on the by-products

21. For a good exposition of this point, see Margaret Humphreys [Warner], "Hunting the yellow fever germ: the principle and practice of etiological proof in late nineteenth-century America," *Bull. Hist. Med.*, 1985, *59*, 361–82.

22. Bernard, (n. 18). See also, Hurd, (n. 13), who contrasted the "chemical theory of contagium" to the "vegetable parasite theory." My understanding of the American debate is heavily indebted to several older works on mid-nineteenth-century disease theory: John K. Crellin, "The dawn of the germ theory: particles, infection and biology," in F.N.L. Poynter, ed., *Medicine and Science in the 1860s* (London: Wellcome Institute of the History of Medicine, 1968), pp. 57–76; John M. Eyler, *Victorian Social Medicine: The Ideas and Methods of William Farr* (Baltimore: Johns Hopkins University Press, 1979); and Margaret Pelling, *Cholera, Fever, and English Medicine, 1825–1865* (New York: Oxford University Press, 1978).

it produced. The presence of bacteria in the bodies and discharges of the sick could be a consequence, not a cause of, the underlying disease. As Edward P. Hurd wrote of microorganisms in 1874, "There is no proof that all that have as yet been found are not accompaniments, or effects, and not causes of the diseased conditions with which they are found associated." To prove a causal relationship, an investigator needed to complete what Hurd referred to as "the cycle of proof." It proved nothing, he wrote, to show that "the supposed cause A always exists with the disease B, and hence B is the effect of A. Into a preexisting set of circumstances where B does not exist he must introduce A and produce the disease." Until the investigator accomplished this demonstration, Hurd concluded, "his speculations are of little worth."[23]

At least a few American physicians tried to use experiments to decide the merits of the two competing theories. Edwin Bertolet's attempts to replicate the French physician Jean-Antoine Villeman's experiments in inoculating guinea pigs with tuberculosis were by no means unique. Prior to 1880, references to original research crop up in journal articles and medical school theses more often than the "backward thesis" would lead us to expect.[24]

To detail but one interesting example, in 1871, Frank Davis, the assistant editor of the *Chicago Medical Examiner*, published an account of experimental investigations he was inspired to make after reading about the germ theory. He began by observing that "As the most rational and probable explanation that has been offered to account for the occurrence of these phenomena, this theory has been very generally accepted by the scientific world," a characterization that hardly squares with Richmond's portrayal of American opinion in this period. But Davis continued, "Microscopists have failed . . . as yet, to demonstrate the existence of such specific germs holding a definite relation to the origin of disease," a criticism of germ theory

23. Hurd, (n. 13), p. 102.

24. Richmond cites Salisbury's palmella experiments in the late 1860s as the only real effort at experimentation. While I have not systematically set out to find other such examples, I have encountered sufficient references to individual efforts to suggest a budding interest in experimental method, however primitive. See, for example, "Observations and experiments on living organisms in heated water," *Am. J. Med. Sci.*, 1868, *55*, 283–84; Joseph Richardson, "Experiments showing the occurrence of vegetable organisms in human blood," *Am. J. Med. Sci.*, 1868, n.s. *56*, 291–94; Satterthwaite, (n. 13), pp. 851–854; and "Tyndall and Bastian on the germ theory of disease," *Bost. Med. Surg. J.*, 1876, *94*, 249–251.

frequently made before the microorganisms of anthrax and relapsing fever were isolated in the late 1870s.[25]

To address this question, Davis conducted, in his own words, "a number of microscopic examinations of the atmosphere of rooms" in which patients "laboring under different diseases, contagious and otherwise were lying." To create a sort of control group, he sampled the air of rooms occupied by healthy people as well. In each room, Davis exposed slides holding a drop of glycerin for twenty-four hours; he also held slides to the nostrils and mouths of patients suffering from various zymotic diseases, including typhoid and scarlet fever. "On a careful comparison of the slides exposed only to normal, healthy atmospheres, with those which had been exposed to the contaminated or contagious atmospheres, we were unable to detect the slightest particle of any kind in one, which was not equally present in the other." From these results, Davis concluded that his experiment did not confirm the relationship between airborne germs and diseases.[26]

Compared to the bacteriological techniques Robert Koch and his peers developed in the late 1870s and early 1880s, Davis's methods were admittedly crude, yet by the standards of his own time, his experimental imagination does not suffer so badly in comparison. After all, during the late 1860s and early 1870s, Louis Pasteur was traveling to Alpine mountain tops to capture vials of pure air, and John Tyndall was warming flasks of turnip broth in a Turkish bath. My point here is not that a horde of great New World experimentalists has been overlooked, but rather that Richmond's portrait of American physicians as entirely uninterested in original research is overdrawn. Obviously, inquisitiveness found outlets in experimentation on both sides of the Atlantic.[27]

To be sure, American commentators did not *privilege* laboratory evidence over other sorts of scientific knowledge. In this sense, the germ theory debates amply confirm John Harley Warner's point that most physicians in the 1870s were reluctant to exalt experimental evidence over and above other forms of observation and investigation.

25. Frank J. Davis, "Atmospheric germs and their relation to disease," *Chi. Med. Examiner*, 1871, *12*, 191–99, quote from p. 195.

26. Ibid., pp. 197–98.

27. I discuss the rough quality of early experimental proofs of the germ theory in Tomes, (n. 11), chpt. 1.

Sweeping, programmatic claims for the superiority of laboratory knowledge over clinical observation were met with sustained resistance, and when specific experimental results appeared to contradict bedside evidence, they were likely to be ignored. But at the same time, the judiciously cited experiment, especially when employed in concert with more familiar forms of clinical and epidemiological reasoning, was increasingly accepted as a legitimate element in medical argumentation.[28]

In striking testimony to how definitions of scientific medicine were expanding in the post-Civil War period, both converts and skeptics regarding the germ theory employed experimental evidence in arguing for their positions. Their disagreements centered not on the *relevance* of laboratory insights, but on their *interpretation*, that is, what specific experimental results actually *proved*. Physicians who expressed reservations about the parasitic germ theory did not necessarily reject experimental method as a source of scientific knowledge. Indeed, for many of them, it was the theory's *failure* to meet what they regarded as minimal standards of experimental proof that fueled their skepticism.[29]

A case in point is Edward P. Hurd's 1874 article in the *Boston Medical and Surgical Journal*, which painstakingly sifted through microscopic observations of fungi and bacteria, reports of animal experimentation, and Pasteur's studies of fermentation. Hurd's reservations about the parasitic germ theory were based largely on its failure to pass what he understood as the acid test of the experiment: "before the animated pathology can be established on a scientific basis, it must be shown . . . that the infected atmosphere contains spores identical with those of the fungi obtained by the culture of the bacteria, and that the same spores are in every way like the elementary corpuscles contained in the morbid products," he reasoned. Having set those terms of proof, he concluded, "It will now be seen what a hiatus remains to be filled, before the animated pathology can be accepted as accounting for the origin of contagious diseases in general."[30]

28. Warner, (n. 15) "Fall and rise."

29. My argument here is quite consistent with the work of Margaret Humphreys on yellow fever, which implicitly challenges the Richmond thesis on similar grounds. See Margaret Humphreys [Warner], (n. 21); and *Yellow Fever and the South* (New Brunswick, N.J.: Rutgers University Press, 1992).

30. Hurd, (n. 13), p. 102. Richmond cites Hurd as a "cautious" supporter of the germ theory, but I would categorize him as a definite critic.

Testifying to the fact that the significance of specific experimental results was by no means obvious, arguments for and against the germ theory frequently invoked the same laboratory work as evidence. In rejecting the germ theory as unproven, Hurd and other skeptics cited the very research by Davaine, Pasteur, and Burdon Sanderson that was routinely claimed by the other side. As Terrie Romano demonstrates in her work on England, this pattern of cross-citation was common there as well; it reflects not a deficiency in the Americans' scientific understanding, but rather the unstable meanings readers in general derived from laboratory reports.[31]

For their part, supporters of the germ theory quite willingly admitted the shakiness of their experimental grounds. Hurd's perception of the germ theory as a work in progress, a hypothesis far from conclusively proven, was echoed in their writings as well. Converts pleaded their case not by insisting that the available experimental proof was compelling, but rather by insisting that its gaping holes would soon be filled. An 1875 editorial in the *Philadelphia Medical Times* admitted, "Though the evidence in its favor may be as yet comparatively meagre, conflicting in some details, and in many respects contradictory, yet it is constantly increasing by the labors of a host of conscientious and able workers." Similarly, in an 1878 address on the germ theory, Joseph Richardson admitted in his "plea" for the "doctrine," that "no really skilful microscopist" had yet found "any definite relationship" between microbes and disease. "Nevertheless," he continued, "the presumption that such causal relation does exist, is, in my opinion, so strong that I intend to ask you to accept my judgment that it will be demonstrated in the near future."[32]

While both camps cited experimental evidence, neither relied upon it exclusively, but rather employed it as one piece of a larger battery of arguments. Since Richmond's day, medical historians have been so inclined to look at the germ theory controversy in the light of an emergent laboratory ideal that they have paid scant attention to the other interesting forms of scientific evidence and reasoning used in the debate. In fact, a wide range of evidence drawn from outside the laboratory's walls was offered for and against the germ theory. Using

31. Romano, (n. 12).

32. "The influence of the lower organisms," (n. 25), p. 762; Joseph Richardson, *The Germ Theory of Disease, and Its Present Bearing Upon Public and Personal Hygiene* (Philadelphia: Philadelphia Social Science Association, 1878), p. 4.

the microscope in search of the distinctive microorganisms of specific diseases, physicians reported the results of pathological examinations of tissue and blood. They called upon the direct observation of communicable and epidemic diseases, both at the bedside and in the community, as evidence about how the diseases originated and spread. Last but not least, discussions of the germ theory invoked the work of naturalists who were trying to order the bewildering world of microorganisms into the sort of taxonomies used to classify the higher organisms.[33]

In organizing these diverse sources of insight, commentators had frequent recourse to analogies and metaphors of disease whose power transcended any one piece of evidence. For advocates of the chemical point of view, the models of combustion and poison seemed to offer convincing alternatives to the germ theory. Chemical research had shown that given the right conditions for its development, a tiny amount of catalyst could produce dramatic results. Using this analogy in 1874, Karl Liebermeister likened the chemical understanding of fermentation to combustion: "A burning shaving can set fire to a house and an entire city. The chemical process of burning multiplies itself ad infinitum, so long as combustible material and oxygen are present under favoring circumstances." The combustion metaphor worked nicely to explain the sometimes random pattern of infection, for it required both appropriate tinder and spark to ignite. As an editorialist in the *Philadelphia Medical Times* explained in 1876, "The match or the flying spark is harmless if the gunpowder be moist, or if any one of its constituents be missing."[34]

Another analogy frequently cited as an alternative to the microorganism was organic poison, a substance so toxic that even in minute amounts it caused paralysis and death. Thomas Satterthwaite referred to the cobra, noting that its poison might be kept dried for months and yet retain its virulence, "qualities which are extraordinarily like those observed in the contagious principles of putrid matter." Similarly, Edward Hurd discussed at length the poisonous products of

33. On medical microscopy, see Cassedy, (n. 13), and Deborah Jean Warner, "Medical microscopy in antebellum America," *Bull. Hist. Med.*, 1995, *69*, 367–386.

34. Karl Liebermeister, "Introduction," in Hugo Von Ziemssen, ed., *Cyclopaedia of the Practice of Medicine*, 20 vols. (New York: William Wood, 1874–78), *1*, *10*; "The prophylaxis of cholera," *Phila. Med. Times*, 10 June 1876, 444. Note that Liebermeister was not a critic of germ theory, but in giving a dispassionate summary of the two competing theories, used this image, which was often repeated in other accounts.

fungi and puffballs, and likened the cause of malaria to snake poison; depending on the patients' "vital powers" and the size of the dose, the disease would be more or less dangerous.[35]

By using such comparisons, skeptics suggested that the chemical theory was still a better explanation of the observed behavior of zymotic diseases than the germ theory. Hurd wrote, "Till, then, more convincing experiments shall have been performed, the poison theory of the older pathologists will hold against the living ferment theory of the newer." He concluded, "In rejecting the Germ Theory as untenable, we have either to confess our ignorance of the causes of all febrile and inflammatory contagious diseases (and it were better to rest content with ignorance than entertain beliefs that are not true), or, guided by analogy, to accept the alternative that the principle of contagion is a subtle chemical ferment, an organic poison, generated in the body of the diseased individual."[36]

Advocates of the germ theory also argued for their point of view by likening the workings of the microbial world to other, better-known natural phenomena, but in place of references to combustion or snake venom, they preferred botanical and agricultural allusions. In a speech to the San Francisco Medical Society in 1880, William Mays compared the specificity of disease germs to the principle of "an oak coming from an oak, a grape from a grape." In disputing the *de novo* origin of germs, Joseph Richardson made an even more direct allusion to the Gospel of Luke, in which Jesus referred to knowing the tree by its fruits: "The fact is, as I firmly believe, that (inverting the Scriptural aphorism) we can no more gather thorns from grapes, or thistles from figs, than we can have, for instance, the germs of yellow fever growing from clean cotton, or those of cholera developing from uninfected rice."[37]

A related image used repeatedly in early explications of the germ theory was the venerable "seed and soil" metaphor of disease. Advocates of the germ theory often appropriated this ancient medical

35. Satterthwaite, (n. 13), p. 853; Hurd, (n. 13), pp. 102, 104–105.

36. Hurd, (n. 13), pp. 105, 109.

37. William H. Mays, "On the supposed identity of the poisons of diphtheria, scarlatina, typhoid fever, and puerperal fever," *San Fran. W. Lancet*, 1880–81, *ix*, 110–15, 110; Richardson, (n. 32), p. 11. The Biblical quote appears in Luke 6:44; there is a similar image in Genesis 1:11–12 as well. "Then God said, 'Let the earth produce fresh growth, let there be on the earth plants bearing seed, fruit-trees bearing fruit each with seed according to its kind." (New English Bible transl.)

analogy to explain why it was people exposed to the same germs did not all become sick. Here again the New Testament furnished a useful parable, that of the sower, which Richardson employed in his 1878 lecture to explain the capricious nature of disease spores: if the seeds of disease, which he likened to "noxious weeds," do not "fall upon *good* ground," meaning favorable conditions for disease, they will not flourish.[38]

For a generation of American physicians who were both Protestants and often only one generation off the farm, these familiar analogies from agriculture and scripture were well chosen. The reminder that a farmer grew corn only by sowing its distinctive kernel, or broadcast only wheat grains to get wheat, neatly illustrated what was then a controversial scientific doctrine; namely, that every case of an infectious disease came only from a prior case of the same ailment. The seed and soil metaphor had the added merit of allowing advocates of the germ theory to reconcile their argument with venerable traditions of constitutional disease and preventive hygiene, which helped explain why resistance varied so strikingly among different individuals and groups.

In addition to their different choices of master metaphors, the two camps in the germ theory debate also invoked different conceptions of the place of theory and analogy in medical thinking generally. In urging a critical view of the germ theory, skeptics appealed to a deep-seated suspicion of oversimplification and speculation. Since the early 1800s, American practitioners had been taught to beware the "spirit of system" as inimical to true scientific progress; common medical wisdom held that having preconceived notions of disease all too easily led observers to distort the meaning of the facts before their eyes. For many medical commentators, the germ theory seemed a perfect example of medical "ultraism," as one author termed it, that is, reducing a complex disease process to a single underlying cause. In an 1874 address to students at the Long Island College of Medicine, later published in the *Sanitarian*, Jarvis Wight warned against medical

38. Richardson, (n. 32), p. 11. The parable of the sower is in Matthew 13:18–23. On the seed and soil metaphor, see Vivian Nutton, "The seeds of disease: an explanation of contagion and infection from the Greeks to the Renaissance," *Med. Hist.*, 1983, *27*, 1–34. William Osler claimed to have originated the "seed and soil" phrase in the 1890s, but in fact it was frequently used in earlier writings on the germ theory. See William Osler, "The home and the tuberculosis problem," *First Annual Report of the Henry Phipps Institute*, 1903, 141–54, 146.

"monomania," pointing out that to the student of the yeast plant, yeast became an obsession. "Then he looks through his yeast ideas at the universe," interpreting all phenomena, from the planets' orbits to the mysteries of disease, from that point of view, "so that if we live or die, it is fermentation." But, Wight warned, "The fact is, we must not put a part for the whole. Disease results from a variety of causes, and the widest generalization gives us the best definition."[39]

In response, advocates of the germ theory liked to remind their audience that some great scientific truths were indeed very simple. To underscore that point, they often drew explicit parallels between the germ theory and other great scientific discoveries. At the opening of his 1880 speech, William Mays proclaimed, "I will state at the outset that I am an ardent germ-theorist, viewing any doctrine that conflicts with that theory much as I would an attempt to controvert Newton's law of gravitation." Even more frequent were references to Darwin and Spencer's theories of evolution, which appeared at roughly the same time as the germ theory, and were still being hotly debated during this period. In the 1870s and 1880s, commentators frequently invoked the example of evolution in pressing the viability of the germ theory, apparently confident that their audience would understand the parallel.[40]

Evolutionary theory was not the exclusive preserve of the germ theory's advocates, it should be noted. In England, for example, both

39. G.M.B. Maughs, "Medical ultraisms," *Trans. Med. Assoc. Mo.*, 1880, *23*, 18–29; Jarvis S. Wight, "Relations of hygiene to practical medicine," *Sanitarian*, 1874, *2*, 51. See also the conclusion of Hurd's paper, in which he warned, "The scientific world is inebriated with speculation; the fogs and mists of error blind honest searchers after true knowledge, and the torchlight of induction shines dimly where clear light is needed." Hurd, (n. 13), p. 109. For the larger context of this fear of system, see the works by John H. Warner cited in (n. 15).

40. Mays, (n. 37), p. 110. Charles Darwin's *Origin of Species* appeared in 1859; Herbert Spencer's early writings on the social dimensions of evolution appeared in the 1860s. On American responses to Darwin and Spencer, the range of scholarly opinion is nicely captured in two books: Richard Hofstadter, *Social Darwinism in American Thought* rev. ed. (Boston: Beacon Press, 1955) and Robert Bannister, *Social Darwinism: Science and Myth in Anglo-American Social Thought* (Philadelphia: Temple University Press, 1979). Hamilton Cravens, *The Triumph of Evolution* (Philadelphia: University of Pennsylvania Press, 1978), has written an excellent study of American scientists and the heredity–environment controversy in the early 1900s. But so far as I know, outside the fields of neurology and psychiatry, there has been relatively little work done on the impact of Darwinian and Spencerian theory of American medical thought in the 1870s and 1880s. For an exception, see the interesting discussion of Chicago physicians's responses to Darwinian theory and Social Darwinism in Thomas Bonner, *Medicine in Chicago: 1850–1950*, 2nd ed. (Chicago: University of Illinois Press, 1991), pp. 209–213.

John Tyndall and H. Charlton Bastian were avowed supporters of Darwin, and employed evolutionary arguments to support their opposing positions regarding spontaneous generation. Nor did supporters of the germ theory agree upon the import of evolutionary principles for microbial life, especially concerning the vexed issue of spontaneous generation. Some advocates of the germ theory hewed to the Pasteur–Tyndall line that a germ always came from a preexisting germ, while others argued that the laws of adaptation and mutation did not rule out the possibility of spontaneous generation. As an Albany medical student insisted in 1882, the "truth of the germ theory does not depend on germs arising only from other germs," and asserted that they could develop *de novo* under the proper atmospheric conditions.[41]

While both camps in the germ theory debate employed evolutionary theory, its growing prestige probably favored the advocates more than the skeptics. The language of evolution lent powerful credence to the advocates' vision of "higher" and "lower" forms of life, interconnected and competing with one another in a microbial "survival of the fittest." The idea that the microscopic agents of disease could be understood in terms of the same broad evolutionary principles that applied to all forms of life had enormous appeal to physicians seeking to become more scientific in their thinking. Henry Gradle, whose *Bacteria and the Germ Theory of Disease* (1883) was the first book-length exposition of the theory, began with just such a Darwinian statement of faith: "In the light of the germ theory, diseases are to be considered as *a struggle between the organism and the parasites invading it.*" He noted approvingly that this doctrine "eliminates the factor

41. James S. Dornet, "Germ theory" (M.D. thesis, 1882, Albany Medical College [hereafter AMC]). Albany Medical College Archives, no. 81-10-49, Schaffer Library of Health Sciences. For an example of an antigerm theory skeptic appealing to evolutionary theory, see the case of P.W. van Peyma discussed in George E. Haddad, "Germ theories, scientific medicine, and the Buffalo medical community," unpublished paper in the author's possession. I thank Dr. Haddad for sharing this work with me. My thanks also to James Strick for clarifying my understanding of the Bastian–Tyndall debate.

The diversity of opinion about the meanings of evolution for microbial life has been noted by William F. Bynum, "Darwin and the doctors," *Gesnerus*, 1983, *40*, 43–53. English advocates of the germ theory also split over the spontaneous generation issue. For example, in 1883, an English physician used Darwinian principles to argue against the one germ, one disease model of specificity. Unlike William Mays, he believed germs capable of causing one disease could mutate into those of another disease, given the right conditions of natural selection. Kenneth W. Millican, *The Evolution of Morbid Germs* (London: H.K. Lewis, 1883).

'accident' from the consideration of disease, and assigns disease a place in the Darwinian program of nature."[42]

By the early 1880s, the invocation of both Darwinian and Spencerian theory had become commonplace in medical student writings on the germ theory. For example, an 1884 medical thesis written by Albert Norton, a medical student at the University of Pennsylvania, used the concept of natural selection to explain the behavior of both microbes and their human hosts. By showing that germs required favorable "nutriment, temperature, air supply" and other conditions to flourish, natural selection provided a "clue to the explanation of many phenomena in epidemiology heretofore deemed most mysterious." Norton continued, "It must also be born [*sic*] in mind the powers of natural selection not only as it affects the germ but the recipient of that germ if we are to understand the periodicity of these affections." A generation exposed to an epidemic will develop a "power of resistance," he explained, and transmit it to their offspring, albeit in a weakened form. After several generations, the disease will return in full force, starting the cycle again. "The same follows in a less degree by the laws of natural selection and heredity."[43]

Belief in the germ theory's compatibility with seemingly fixed evolutionary laws went hand-in-hand with an optimistic view of medicine's future. Advocates of the germ theory argued that as humankind came to understand those laws better, the profession would eventually be able to conquer even the most fearsome diseases. William Hill concluded his 1885 thesis on the germ theory with a rousing tribute to the principle of progress:

After centuries of silent resignation, mankind enlightened by science at last begins to recognize its relentless and hitherto mysterious enemies. We see by undeniable examples that we can triumph over them and make them disappear. Shall we then continue indefinitely yielding up the inumerable [*sic*] victims that yearly succumb to the attacks of foes whose only force lies in their minuteness? No! Man is no more made to become their prey

42. Henry Gradle, *Bacteria and the Germ Theory of Disease* (Chicago: W.T. Keener, 1883), p. 2. The impact of evolutionary theory on the germ theory debate needs a great deal more sustained analysis than I am able to do here, but my impression is that advocates of the germ theory laid more frequent and persuasive claims to evolutionary theory than its critics did.

43. Albert Eugene Norton, "An essay on the relationship of micro-organisms to disease" (M.D. thesis, UPA, 1884), pp. [7, 62–63].

than that of the wild beasts among whom he had to fight his way in the infancy of the race and whom he has conquered or destroyed by his industry, intelligence, and work.[44]

As Hill's reference to the "infancy of the race" attests, commentators on the germ theory often described the microbial order in terms of social as well as biological evolution. The frequent references to the animal and vegetable "kingdoms" and the different "tribes" of bacteria underscored the parallels between the micro- and macroscopic worlds. In an 1882 article in the *Boston Medical and Surgical Journal*, Hugo Engel referred to microorganisms as "destroyers of the animal kingdom, and especially of the enlightened human race, and of the domestic animals so necessary for the existence of civilized mankind." The highly charged adjectives often used to describe pathogenic disease germs, such as "base," "murderous," and "cunning," resonated with the conceptions of civilization and savagery so common in the budding social sciences of the late nineteenth century.[45]

The frequent recourse to evolutionary theory in early discussions of the germ theory suggests that the new view of disease gained credibility because of its compatibility with the evolutionary doctrines so dominant in the disciplines of biology, sociology, and anthropology. The conception of the microbial world as part of a larger natural order, ruled by immutable laws of development, was a large part of the theory's scientific appeal. Quite independently of the growing authority of experimental method, then, the germ theory gained stature because of its harmony with the grand outlines of evolutionary thinking in the late nineteenth century.

But while appeals to evolutionary theory deepened the scientific credibility of the germ theory, ultimately what tipped the balance toward its acceptance was a more persuasive body of experimental evidence. Examining how and why the grounds of argument began to shift between 1875 and 1885 points out both the importance of the new laboratory ideal, closely identified with Robert Koch, and its debt to the debates of the preceding decade. As of the late 1870s, advocates and skeptics were basically agreed that for the theory to

44. William Preston Hill, "An essay on the origin of the germ theory" (M.D. thesis, UPA, 1885), pp. [29–31].

45. Hugo Engel, "Some facts explained by modern pathology," *Bost. Med. Surg. J.*, 1882, *107*, 244.

be proven, some experimentalist had to conclusively link a specific microbe with a specific disease. That turning point came with Robert Koch's work on anthrax, which in the eyes of many commentators completed the "cycle of proof" that opponents of the germ theory had demanded. Although few of his papers were translated into English, his demonstrations concerning anthrax in the late 1870s, followed by his investigations of wound infection, cholera, and tuberculosis in the early 1880s, were widely summarized and commented upon in American journals. Pasteur's researches during these years were also followed closely, yet Koch's name figured more prominently in the growing favor American commentators bestowed on the germ theory from the late 1870s onward.

The high regard bestowed on Koch's work resulted from the long, drawn-out debates of the 1870s, which had served to clarify and stabilize a collective understanding of what constituted experimental proof. By 1880, those engaged in the germ theory debates had developed a much greater appreciation of the "differences between scientific demonstration, working theory, hypothesis, and mere suggestion," in the words of William H. Brewer, an instructor in sanitary science at Yale. The greater certainty of the 1880s would never have emerged had not the previous generation of physicians laboriously worked out what were sufficient grounds to believe that a specific germ caused a specific disease.[46]

Two commentaries written in the mid-1880s reflecting on the rapid acceptance of the germ theory suggest how important Koch's work was in shifting the American debate to new ground. The first, an 1883 editorial written by Walter Mendelson for the *Archives of Medicine*, began by acknowledging Pasteur's importance as the pioneer in the field. In a comparison of French and German science that soon became standard, Mendelson wrote of Pasteur, he "has done great and original work, and if all his statements are not marked by the accuracy that distinguishes his German collaborator, and if he is at times given to somewhat bold flights of fancy, due allowance should be made for the national peculiarities of a Frenchman." But it was Koch who got the lion's share of praise. "His thorough scientific honesty, his truly Darwin-like patience in waiting to thoroughly test

46. William H. Brewer, "Syllabus of a course of lectures on sanitary science," *Fifth Annual Report, Connecticut State Board of Health*, 1883, p. 88.

by proof and counter-proof the accuracy of his observations, before announcing them to the world in print, his ripe judgment and clear reason, have all borne their fruits," Mendelson wrote. Extending the comparison to Darwin, he predicted that the germ theory "will, of course, like the theory of evolution, be combated for a number of years to come, especially by the strongly conservative members of the profession, who find it hard to throw off the traditions of years." But "Time, which healeth all things, will cure this disposition too," Mendelson concluded.[47]

Two years later, Abraham Jacobi offered another, more critical view of the germ theory's progress in his famous inaugural address to the New York Academy of Medicine. This speech, especially its denunciation of what Jacobi termed "bacteriomania," has often been cited as evidence of American reservations about the germ theory. Yet a close reading of Jacobi's remarks reveals how far the theory's advocates had pushed the boundaries of debate by the mid-1880s. Jacobi clearly acknowledged that in raising questions about the theory, he was fighting a rear-guard action. In the United States, he observed, both general practitioners and specialists "have readily accepted the new gospel with but few exceptions." As became increasingly the case with skeptics in the 1880s, he did not categorically deny the possibility that microbes might be linked with disease, but rather disputed the role of specific bacteria in causing specific diseases, such as diphtheria, where the experimental proof was not yet convincing. Since E.P. Hurd's day, the debate had shifted from the general truth of the germ theory to its validity as applied to individual diseases.[48]

Jacobi's stance toward experimentalism also bears emphasis. His complaints about bacteriomania were directed chiefly at the researcher who published too hastily, from fear "of having his celebrity snatched away from him by the next door microscopist." Likening the results to a city full of amateurs trying to play the piano, he commented, "When bacterio-microscopy in the hands of beginners becomes noisy like piano-playing—noisy in books, pamphlets and journals—a gentle protest is permissible." Yet Jacobi made clear that his criticism was "not meant for the masters who know how to wait and to mature." In particular, he emphasized, "I do not speak against Robert Koch

47. Walter Mendelson, "The germ theory of disease," *Arch. Med.*, 1883, *10*, 176–86, Quotes are from pp. 177, 186.

48. Abraham Jacobi, "Inaugural address," *Med. Rec. (N.Y.)*, 1885, *27*, 169–74. The term "bacteriomania" is on p. 172.

and his peers, who all of them are more modest than their followers." These were the "great lights, whose rays are always welcome." Jacobi's comments constituted not a categorical rejection of bacteriology but rather a plea to continue investigating other aspects of disease causation. Organic chemistry was as important a source of insight as bacteriology, he concluded, and urged his hearers to investigate entities such as the "cadaveric poisons" and "ptomaines" as nonmicrobial agents of disease.[49]

Jacobi's address thus anticipated possibilities for reconciling the chemical and microbial models of disease causation that became increasingly appealing in the 1880s. For example, through their investigations of ptomaines, early bacteriologists such as Frederick Novy and Victor Vaughan demonstrated that some pathogenic microbes *manufactured* chemical toxins, which in turn produced the characteristic symptoms of disease. In addition, in yet another variant on the seed and soil analogy, advocates of the germ theory portrayed infection as a process that required both the parasitic agent and the appropriate chemical "nidus" for its development. In this fashion, disease theory in the 1880s moved toward a compromise position that one editorialist in the *Boston Medical and Surgical Journal* referred to as a "chemico-biological science."[50]

By emphasizing the medical profession's increasing importance as "natural advisers in all matters concerning sanitation and health," Jacobi suggested another important ground for reconciliation between critics and supporters of the germ theory. From the mid-1860s onward, elite physicians were increasingly being taught to include hygienic instruction in their repertoire of patient care. In urban practice, where both the rates of infectious diseases and sanitary offenses were high, physicians assumed new responsibility for advising families about household plumbing and cleanliness, as well as isolating the sick and disinfecting their poison-laden wastes.[51]

49. Ibid., quotes are on pp. 172, 174, 173.

50. "Ferments and fermentation," *Bost. Med. Surg. J.*, 1885, *112*, 634. On ptomaine theory, see Victor Vaughan and Frederick Novy, *Ptomaines and Leucomaines* (Philadelphia: Lea Brothers and Co., 1888). Note that by the third edition, published in 1896, they had replaced the term "ptomaine" with "bacterial poisons." The parasitic germ/chemical "nidus" formulation is clearly stated in Alexander R. Becker, "Typhoid fever: its causes and sources, as explained by the germ theory of disease," *Bost. Med. Surg. J.*, 1879, *100*, 668–677, 697–704.

51. Jacobi, (n. 48), p. 170. On preventive hygiene, see Nancy Tomes, "The private side of public health," *Bull. Hist. Med.*, 1990, *64*, pp. 509–39. Several scholars have recently called attention to the *domestic* context of late nineteenth-century medical practice, which made the focus on "house diseases" and the management of infectious illnesses in the home

At first, many older sanitarians and physicians, among them such worthies such as Benjamin Richardson, Florence Nightingale, and Elizabeth Blackwell, felt that acceptance of the germ theory threatened the high status of preventive hygiene. To counter that argument, advocates of the germ theory sought to show that the new explanation for infectious diseases was perfectly consistent with the disease control measures that clinical observation and experience had already proved to be efficacious. The germ theory, as Joseph Richardson insisted in 1878, only worked "to strengthen the urgent recommendations of sanitarians."[52]

Far from being simply a promissory note for the future, converts to the germ theory presented it as a valuable hygienic tool that practitioners could immediately put to use. The new etiology was presented as a more precise and therefore powerful explanation for why sanitarian strategies worked. In an 1879 article on typhoid published in the *Boston Medical and Surgical Journal*, a California physician, Alexander R. Becker, argued that "By considering contagia in this light, as parasites, we can explain many points heretofore clouded in mystery." The recognition that the germ responsible for the disease was given off in the patient's bowel discharges, which in turn contaminated the air and water, allowed the physician to explain the otherwise capricious course of the disease. Becker concluded:

When we arrive at a full comprehension of this source of typhoid fever, our first feeling is one of dismay at the difficulties to be encountered in

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all the more important. See Annmarie Adams, *Architecture in the Family Way: Doctors, Houses, and Women, 1870–1900* (Montreal: McGill-Queens' University Press, 1996); and Judith Walzer Leavitt, "'A worrying profession': the domestic environment of medical practice in mid-nineteenth-century America," *Bull. Hist. Med.*, 1995, *69*, 1–29.

52. Richardson, (n. 32), p. 10. On Benjamin Richardson, see Stevenson, (n. 3); on Nightingale, see Charles E. Rosenberg, "Florence Nightingale on contagion: the hospital as moral universe," in *Explaining Epidemics and Other Studies in the History of Medicine* (New York: Cambridge University Press, 1992), pp. 90–108; on Blackwell, see Regina Morantz-Sanchez, *Sympathy and Science* (New York: Oxford University Press, 1985), pp. 186–191, and "Feminist theory and historical practice: rereading Elizabeth Blackwell," *Hist. Theory*, 1992, *31*, 51–69.

I develop this argument about the appropriation of sanitarian thought at more length in Tomes, (n. 51). My interpretation here is similar to Latour's argument about the French hygienists. Latour seems to think that the French case differs in essential respects from the American and British; see (n. 17), p. 26. While it is certainly true that the Americans and British did not focus so exclusively on the figure of Pasteur, Latour's general point, that supporters of the germ theory translated the hygienists' precepts into their own terms and adopted their sanitary projects, holds true generally. I agree with Latour that the synthesis of sanitary science and germ theory strengthened both. See Latour, (n. 9) *Pasteurization of France*, esp. pp. 25–26, 34.

tracing the epidemics of large cities, and still more in securing efficient preventive measures; but our second feeling is one of relief, in the fact that we have at last discovered a principal source of this insidious group of diseases, and thus know where we must direct our energies.

He concluded, in terms that would have gladdened the most fervent sanitarian, that "Prevention, to be effective, must be radical."[53]

The conviction that the new knowledge of microorganisms would give the physician greater "confidence and certainty" was frequently repeated in medical student theses. As a student at Albany Medical College, David Fleischman, asserted in 1881, "The originators and promoters of the germ theory are not visionaries, not theorizers, but philanthropists, benefactors," whose "discoveries give zest and direction to preventive medicine." His fellow student William Peddie concurred in an 1882 thesis, proclaiming that "The physicians' relation to the public is not only as a dispenser of drugs for the palliation or cure of disease, but also as a guardian and preserver of the public health."[54]

Peddie's thesis went on to detail more specific ways that the physician should incorporate disease prevention into his own professional behavior. When treating potentially communicable diseases, Peddie recommended that the practitioner avoid touching objects in the room, and wash his face and hands with carbolic acid solution after leaving it. His garments should be buttoned up during the visit, and changed before seeing the next patient. Since the "disease germs" were so often present in feces, he was to make sure nurses removed and disinfected such wastes immediately. "When calling on smallpox patients," he added, the physician "should not sit down."[55]

Peddie's conception of the physician's preventive duties also extended to inspecting household sanitary arrangements. If a zymotic disease prevailed in a household, he believed the doctor should inspect its drains, toilets, and cellars, to see "that all are kept clean and no foul gases or fluids are escaping in the house, or near it, and that no animal or vegetable decomposition is going on in or about such house or locality." Likewise, the model physician examined the purity of food and water, and counseled families on the distinctions among

53. Becker, (n. 50), pp. 671, 703.

54. David Fleischman, "The habitat of germs" (M.D. thesis, AMC, 1881), pp. 33, 32; William J. Peddie, "The prevention of disease" (M.D. thesis, AMC, 1882), pp. 19–20.

55. Peddie, (n. 54), pp. 13, 31, 32.

"disinfectants, deodorants, and germ destroyers," to which Peddie included a convenient guide in his thesis.[56]

Peddie's guide to "germ destroyers" points to a concrete area of preventive practice where the germ theory had an immediate impact. Giving instructions about the disinfection of sick rooms and bodily wastes had become a vital part of late nineteenth-century urban practice; some physicians even wrote prescriptions for disinfectant solutions in the same way they did for drugs. Not surprisingly, then, one of the first large-scale professional projects undertaken in light of the germ theory's growing acceptance was the rigorous testing of the germicidal action of commonly used disinfectants. In 1884, the American Public Health Association announced the formation of a committee to do this work, noting "It is important, equally for practitioners of medicine, for boards of health, and for the general public, that the highest attainments of science in this department of sanitation should be formulated for easy reference by all who need it for practical application."[57]

Another area in which the germ theory fostered highly visible changes in physician behavior and attitudes was in the personal practice of cleanliness. The arguments William Peddie made in his thesis anticipated the growing influence of a bacteriologically based model of cleanliness, especially as it developed in surgery, on general codes of professional conduct. His classmate James P. Newton put the matter simply in the title of his 1882 thesis, "Cleanliness: a fundamental law of practice." Discussing the implications of the germ theory for medical practice, Newton observed that the better class of patients were embracing higher standards of cleanliness, and that physicians must likewise become more exacting: "We owe it to the cultured and true gentlefolk because we find them so; and none the less do we owe it

56. Ibid., pp. 34–35.

57. American Public Health Association, *Disinfection and Disinfectants: Their Application and Use in the Prevention and Treatment of Disease* (Concord, N.H.: Republican Press Association, 1888), p. 5. After testing hundreds of solutions, the committee published its results in 1887.

I am focusing my argument in this article only on preventive hygiene and its relation to private practice. But it is important to recognize the more strictly therapeutic interest in what historian John Crellin refers to as "internal antisepsis"; that is, the ingestion of chemicals by mouth and by inhalation to counter pathogenic microbes. Even though most of the cures proposed in the 1880s would prove ineffective, the concept of internal antisepsis stimulated great interest and enthusiasm in the 1880s and 1890s. See John K. Crellin, "Internal antisepsis or the dawn of chemotherapy?" *J. Hist. Med. Allied Sci.*, 1981, *36*, 9–18.

to the 'lower classes' as exemplars of what they should be." He ended his thesis with the suggestion that success in private practice would become increasingly dependent upon observing this "fundamental law of practice."[58]

Of course, adherence to higher standards of professional cleanliness was far from uniform. Many physicians continued to wear soiled coats when they visited patients and to pay less than careful attention to personal hygiene. But the growing prestige of germ theory meant that those who openly transgressed this "fundamental law of practice" were subject to harsher and harsher criticism. To give but one striking example, in 1887, at a meeting of the Louisville Medico-Chirurgical Society, a physician mentioned in passing that he had seen a child with erysipelas before attending three women in childbirth, clearly without stopping to disinfect his hands or change his clothing. A fellow physician responded with a blistering attack on his carelessness, saying "What amazes me is that a man of Dr. Baily's reputation as a teacher and a practitioner of medicine would at this late date suffer himself to seem to antagonize the germ theory of specific diseases," and concluded, "I trust no other member of this society will follow his implied example."[59]

In the 1890s, the development of aseptic technique in surgery, with its exacting regimen of scrub suits, face masks, and a blistering level of skin cleansing, strongly reinforced the association of the germ theory with a new code of physician cleanliness. One highly visible way young male physicians signified their allegiance to "modern" views of cleanliness was by abandoning the full beards popular among older doctors. Applauding the fact that his fellow surgeons had set

58. James P. Newton, "Cleanliness: a fundamental law of practice" (M.D. thesis, 1882, AMC), [pp. 1–2, 16]. D.W. Cathell spoke to this impulse in his classic *The Physician Himself*, first published in 1881, when he counseled that the trappings of science had practical uses when competing for business. Besides displaying a microscope in their office, he advised up-and-coming young physicians to take exacting care in their personal cleanliness and to maintain strict management of patients with infectious disease. "After visiting contagious diseases, always disinfect your clothes by walking in the open air; also wash your hands with very hot water, or hold them over the fire; also use disinfecting lotions, etc., according to apparent need; if necessary, take a warm bath, or even a Turkish bath," Cathell warned. D.W. Cathell, *The Physician Himself* (New York: Arno Press, 1972; reprt. of 1881 ed.), p. 78. For an overview of the rising expectations of cleanliness in American society, see Suellen Hoy, *Chasing Dirt: The American Pursuit of Cleanliness* (New York: Oxford University Press, 1995), esp. pp. 59–121.

59. Minutes of the Medico-Chirurgical Society of Louisville, 1884–88, entry for June 1887, Historical Collections, Kornhauser Health Sciences Library, University of Louisville.

an example by "sacrificing their whiskers on the altar of Hippocrates," one observed that as of the early 1900s, "a bushy-whiskered American surgeon is almost as rare as a dodo or an Irish elk."[60]

In these various ways—by providing a convincing explanation for the nature of infection, a guide to preventive regimen, and a model for personal conduct—the germ theory had real relevance to the everyday practice of medicine in the late nineteenth century. This is not to deny that many physicians were disappointed by its therapeutic limitations. When the hopes of developing internal antiseptics and "magic bullets" were not immediately fulfilled, the divide between physician and bacteriologist, bedside and bench, certainly began to grow. But we should not forget that the ability to explain the fundamental laws of disease and to offer advice on avoiding its ravages were important aspects of the nineteenth-century physician's professional role. Thus it may be that in the late 1800s, elite physicians began to inspire greater confidence not because they could suddenly cure infectious diseases, but because they seemed better able to *explain* and *prevent* them. Their supervision of elaborate home isolation regimens, their greater knowledge of disinfectants and plumbing, and their own observance of greater personal cleanliness, all may have contributed to what Paul Starr has termed the growing "cultural authority" of the American medical profession.[61]

CONCLUSIONS

Thus, with all due respect to Richmond's long-venerated thesis, this reexamination of American physicians' grapplings with the germ theory suggests it is high time to abandon the belief in American exceptionalism that has so long prevailed among medical historians. Compared to their European counterparts, the better educated and more professionally active American physicians did not exhibit an exemplary resistance to the germ theory. The theory's critics were not necessarily hostile to experimentalism as a form of scientific inquiry, nor was the germ theory itself seen as irrelevant to late nineteenth-century medical practice.

60. Edwin F. Bowers, "The menace of whiskers," *McClure's*, 1916, *46*, 90.

61. Starr, (n. 6), pp. 13–15. On the bench/bedside divide, see Maulitz, (n. 15). On the importance of explaining disease and offering guidance on its management, see Charles E. Rosenberg, "The therapeutic revolution: medicine, meaning, and social change in nineteenth-century America," in Vogel and Rosenberg, (n. 6), pp. 3–25.

Central to American debates over the germ theory was the question of what constituted scientific discourse. Well before the methodological triumphs of German bacteriology, a generation of physicians had schooled themselves in the complexity of etiological debate. Read with less ahistorical eyes, the lamentable muddle Richmond found in 1954 constitutes a much more interesting tangle of ideas, in which doctors were working out the "differences between scientific demonstration, working theory, hypothesis, and mere suggestion," to repeat William Brewer's phrase. From this perspective, the acceptance of the germ theory in the 1880s and 1890s ceases to be such a mystery, but rather the fulfillment of trends already evident by the 1870s.

This account of American debates over the germ theory also draws attention to how narratives of historical change are created, both by contemporary participants and the historians who study them retroactively. The work of Bruno Latour on Pasteurism, Christopher Lawrence and Richard Dixey on Listerism, and John Harley Warner on the Paris School reminds us that witnesses to periods of great change, in medicine or any human enterprise, often revise and edit their memories of the past to harmonize with subsequent developments. Likewise, witnesses to the American debate over the germ theory generated their own, increasingly explicit, historical narratives of its progress. These narratives changed dramatically between the mid-1870s and the mid-1880s, from a story in which Louis Pasteur, Joseph Lister, and John Tyndall held the place of prominence, to another in which Robert Koch assumed dominance. That process of revision embodied the creation and celebration of the laboratory ideal that came to inspire the leaders of American medicine in the 1890s.

But this paper also points to the dangers of projecting the end of the story backward and of viewing the early history of the germ theory only through the lens of an ascendant German bacteriology. Like Jarvis Wight's parody of the student of yeast, historians from Richmond's day onward have tended to take at face value retrospective histories of the germ theory that tied its acceptance or rejection only to the laboratory ideal. In fact, as I have shown, the articulation of the germ theory involved a range of scientific arguments and metaphors, among which the most powerful was the theory of evolution, a doctrine that at the time had no link with the laboratory at all.

The richness of this early discourse about disease underlines the

complex processes by which the phrase "germ theory of disease" came into being and achieved widespread acceptance among American physicians. The fact that Phyllis Allen Richmond failed to perceive this richness forty years ago reflected not her skills as a scholar, but the intellectual climate of medical history in general. Our conceptions of medical science have changed dramatically since then, enabling us to view the germ theory debates in a different way. Only time will tell if our modern reading of that discourse proves as durable as hers.