When you enter my wards your first duty is to forget all your physiology. Physiology is an experimental science—and a very good thing no doubt in its proper place. Medicine is not a science, but an empirical art.

Samuel Gee (1888)¹

3  DIVIDED WE STAND: Physiologists and Clinicians in the American Context

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INTRODUCTION

By the mid-nineteenth century, European physiologists had largely won their campaign to secure the independence of their subject from medical anatomy. They had achieved this emancipation by self-consciously adopting an experimental approach toward the study of vital processes. They exploited for their own purposes recent advances in physics and chemistry, and they especially emphasized the value of vivisection experiments in the investigation of animal function. Henceforth they could claim to belong to a separate discipline no longer to be regarded as a mere “handmaiden” of medicine.²

A generation later, English and American physiologists had begun to enjoy a similar sense of independence. Yet the prospects of the new discipline remained closely bound up with the destinies of medicine and medical education. If physiology found its most receptive home in universities
(occasionally even in philosophical faculties rather than medical schools), its audience nonetheless consisted overwhelmingly of intending physicians. Without those premedical and medical students, and without the resources that came to it by virtue of its association with medicine, the newly "independent" discipline would have withered on the vine. However distasteful it may have been for some research physiologists to admit it, they remained essentially parasitic on the larger medical enterprise from which they had emerged.

But perhaps medicine was itself becoming dependent upon its new disciplinary offspring. Perhaps, to modify my earlier metaphor, it was less the case that physiology remained parasitic on medicine than that the two had entered into a symbiotic relationship. Many physiologists certainly believed (or hoped) so. The benefits that medicine derived from this symbiosis have traditionally been described (by physiologists and medical historians alike) in terms of the presumed utility of physiological theories, techniques, and instruments for the medical problems faced by practicing doctors. The word "presumed" is used advisedly, for these traditional allusions to the medical utility of physiology are disappointingly brief and vague. They tend to take for granted the point at issue. No one, to my knowledge, has yet made a sustained effort to identify the specific ways in which experimental physiology has contributed to the healing task.

Some readers may feel that the contributions of the discipline to medical practice were (and are) so obvious as to require neither detailed elaboration nor systematic defense. Yet repeated assertions as to the medical value of experimental physiology have failed to stir a remarkably persistent stream of skepticism toward the discipline on the part of practicing doctors. The evidence for this skepticism is admittedly somewhat fragmentary, and must usually be surmised from the remarks of physiologists themselves. But since medical history, like other history, reflects the views of academic elites, one may wonder whether there have not been thousands of fellow travelers in spirit for every busy doctor whose skepticism has found its way into print. Moreover, it is the persistence of this attitude, rather than its extent, that is perhaps more striking and more in need of explanation. Although this essay focuses on the division between physiologists and clinicians in the American context, it is important to recognize that there was nothing uniquely American about the situation. We do not have to do here with some trivial manifestation of the alleged pragmatism of American society. As the discipline of physiology took root in Europe, so, too, did the skepticism of clinicians toward it. When the great French physiologist Claude Bernard began to lecture on the medical significance of experimental physiology, he lamented the number of physicians who believed that "physiology can be of no practical use in medicine," that it was "but a

science de luxe which could well be dispensed with." It was presumably in an effort to change this attitude that Bernard wrote his famous Introduction to the Study of Experimental Medicine (1865). He offered there his vision of a new "scientific" medicine as the way out of the therapeutic uncertainty and nihilism of that era. Obviously irritated by the "false opinion" that medicine was not a science but a mysterious art, Bernard went on to dispute the long-standing claim that the best physiologists are the worst doctors, the "most awkward when action is necessary at the patient's bedside." To Bernard it seemed obvious that "solid instruction in physiology . . ., the most scientific part of medicine," was precisely the one thing that physicians most needed.

Skeptical doctors may have been bemused to compare Bernard's prescription with that of his contemporary, the Prussian pathologist Rudolf Virchow. For Virchow, writing in the 1840s and 1850s, the royal road to medical certainty lay not through ordinary physiology ("a respectable science but thus far a very incomplete one"), but rather through Virchow's own specialty of "pathological physiology." Unlike ordinary physiology, pathological physiology recognized that even a complete knowledge of drug action under normal conditions would be inadequate for understanding the therapeutic effects of drugs under pathological conditions. Moreover, pathological physiology "does not stand before the gates of medicine but lives in its mansion".

[It receives its questions in part from pathological anatomy, in part from practical medicine; it derives its answers partly from observation by the sickbed, to this extent being a division of the clinic, and partly from animal experiment.

For all of that, however, the similarity between Bernard's campaign and Virchow's was more striking than their differences in matters of detail. Both would have medicine built upon basic science, and for both (as Virchow put it in 1847) "experiment is the final and highest court." From Virchow himself, we gain some sense of the reception his program got from medical men. A surgeon named Schuh, whom Virchow had accused of failing to grasp the real significance of the new scientific knowledge and techniques, responded by saying that he was no more competent than anyone else to undertake the task Virchow had in view. As Virchow reported it, Schuh said that "he gladly left to others the saccharine practice of dreaming and the enjoyment of infallibility; meanwhile he, as a practical surgeon, stood on the same field of observation as his forebears had for centuries."8

One can, of course, look upon such reactions as merely inevitable in an
cra when the new or emerging disciplines of experimental biology had yet to establish their relevance and value for medical practice. For physiology, as I have suggested elsewhere, the yoke of utility was especially burdensome. At least as late as the 1870s, even the most aggressive spokesmen for the discipline found it difficult to think of any physiological discovery that had made a significant, direct impact on the art of healing. In his *Introduction*, Bernard could only point rather weakly toward the experimental eradication of the “itch” (scabies). In 1874, Michael Foster, founder of what was to become the great Cambridge School of Physiology, tried to justify vivisection by claiming that it had already played an important role in the advance of the healing task. But he could offer only two specific examples, both of them dubious: allegedly experimental advances in methods of ligature had improved the treatment of aneurysms, and Claude Bernard’s work on the glycogenic function of the liver provided the only available source of illumination into the problem of diabetes, which unfortunately remained outside the therapeutic pale.9

What Bernard and Foster really sought to convey was the medical promise of experimental physiology, but their efforts won only guarded and partial acceptance from medical men. Before the 1870s, precisely because its therapeutic value was considered dubious, experimental physiology (and laboratory science in general) found no place in the English medical curriculum, which was shaped predominantly by the pragmatic demands of the London hospital schools. Foster’s colleague and compatriot, John Scott Burdon Sanderson, faced the problem candidly and directly in 1872, when he told the British Association for the Advancement of Science that the revival of English physiology depended above all on overcoming “that practical tendency of the national mind which leads us Englishmen to underrate or depreciate any kind of knowledge which does not minister directly to personal comfort or advantage.”10

Burdon Sanderson’s address came on the eve of the exciting work of Koch and Pasteur in bacteriology and immunology. Obviously encouraged by these dramatic developments in neighboring fields, some physiologists (and “pathological physiologists”) now found it easier to speak confidently about the medical utility of their research. As early as 1877, in fact, Virchow could write as follows:

> It is no longer necessary today to write that scientific medicine is also the best foundation for medical practice. It is sufficient to point out how completely even the external character of medical practice has changed in the last thirty years. Scientific methods have been introduced everywhere into practice. The diagnosis and prognosis of the physician are based on the experience of the pathological anatomist and the physiologist. Therapeutic doctrine has become biological and thereby experimental science. Concepts of healing processes are no longer separated from those of physiological regulatory processes. Even surgical practice has been altered to its foundations, not by the empiricism of war, but in a much more radical manner by means of a completely theoretically constructed therapy.11

Nonetheless, there is evidence to suggest that many clinicians continued long afterward to doubt the utility of all this newfangled science. The epigraph with which this essay begins reminds us that as late as 1888 an academic clinician could advise his ward clerks that their “first duty” was to “forget all [their] physiology.”12 Especially now, when the scientific basis of modern medicine is taken so much for granted, the long-standing split between doctors and research physiologists seems worthy of more systematic attention than it has hitherto received.

In what follows, the persistence of this division after 1870 is explored with specific reference to the American context. The latter half of the paper very briefly surveys several factors that may help to explain the phenomenon, including the possibility that the skeptical physicians may actually have had some justification for their doubts about the medical utility of laboratory physiology.

But two cautionary and qualifying remarks should be recorded now. First, this is an exploratory effort, not yet buttressed by the sort of extensive evidence we shall surely need to do full justice to the issues it raises. Second, while every effort has been made to focus on the attitudes of practicing doctors toward physiology per se, these attitudes almost invariably reflect a very similar posture toward experimental science in general. Indeed, a much longer essay (or book) might well be written on the persistent skepticism of many ordinary doctors toward experimental science from its beginnings to the present day.

THE PERSISTENCE OF THE DIVISION IN THE UNITED STATES AFTER 1870

Like his colleagues in Europe, Henry Newall Martin, first professor of biology at the new Johns Hopkins University (from 1876 to 1893), insisted that physiology “should be cultivated as a pure science absolutely independent of any so-called practical affiliation.” Yet he could scarcely ignore the fact that his discipline’s raison d’etre “in the mind of even the educated public rested on its relation to medical instruction.” Martin therefore
I, some exposure to human physiology, few indeed would perceive any special seemed very far removed from the immediate problems of practicing doctors. There was as yet, in the 1870s and 1880s, precious little evidence of its pragmatic value, and no obvious reason to suspect that the situation might soon change. In a sense, American doctors probably agreed with Martin that experimental physiology, if it were to be pursued at all, must indeed be pursued independently of pragmatic medical concerns.

That assessment was clearly shared by those most directly responsible for medical education. For while many medical students might want or need some exposure to human physiology, few indeed would perceive any special need for the sort of experimental training emphasized by the emerging band of professional investigators in the discipline. Insofar as these students did represent a natural (or even captive) audience for physiology, their primary need was for oral (rather than laboratory) instruction in the settled points of functional human anatomy. Thus, even as medical schools did begin increasingly to employ professional, laboratory-trained physiologists, they did not really expect their students to repeat the most salient features of the training those physiologists had themselves received.

Even at Harvard Medical School, which had established an assistant professorship in physiology as early as 1870 and had provided a laboratory for the first incumbent (H. P. Bowditch), two decades passed before students were required to take a course in laboratory physiology. By then, laboratory courses had been established at two other major medical schools (the University of Michigan, in 1887, and Columbia University, in 1891), but both courses were elective, rather than required. The course at Columbia did not become required of medical students until 1902.14 Partly, no doubt, because it was so expensive to provide, laboratory physiology found precious little place in the American medical curriculum before 1900. The process by which it finally did become a regular part of medical education has gone virtually unexplored, though it seems likely that laboratory physiology was one of the chief beneficiaries of that more general reform movement in medical education associated with the activities of the American Medical Association’s Council on Medical Education (established in 1904) and with the famous Flexner Report of 1910.15

Even then, the AMA’s classification of the discipline suggests some continued uncertainty about its precise relationship to medicine. From 1901 until at least the 1930s, physiology was placed in the AMA’s section on “Pathology and Physiology,” having previously been relegated to sections on “Medical Jurisprudence, Hygiene and Physiology” (1847–73), “Practical Medicine, Materia Medica and Physiology” (1874–91), and “Physiology and Dietetics” (1892–1900).16 Professional physiologists doubtless preferred their new union with the pathologists to those earlier sectional associations, but they may also have wondered whether their own research interests would now be subordinated to the interests of pathologists, “pathological physiologists,” or bacteriologists. Perhaps that concern helped to motivate the provision, adopted at the outset, that a physiologist would serve as chairman of the new section every third year.17

Implicit, at any rate, in the curricular neglect of laboratory physiology was the assumption that practicing doctors had little or no need of it. Occasionally, that assumption found explicit verbal expression as well. In the 1890s, when the American Physiological Society almost collapsed from lack of interest, even some spokesmen for “scientific medicine” were heard to say that “physiology had done all it could for medicine”.18 In the 1902 edition of his spectacularly popular Book on the Physician Himself, and Things that Concern His Reputation and Success, D. W. Cathell claimed that the new scientific knowledge might actually be damaging to his primary readership, ordinary general practitioners. He warned his readers not to be “biased too quickly or too strongly in favor of new theories based on physiological, microscopical, chemical or other experiments, especially when offered by the unbalanced to establish their abstract conclusions or preconceived notions.” To submit too readily to the appeal of theoretical science “may impair your practical tendency, give your mind a wrong bias and almost surely make your usefulness as a practicing physician diminish.” Scientific curiosity was all well and good for those “scholars and scientists” who did not depend upon practice for their “bread and butter.” But the “first question for you, as a practitioner, seeking additional and better tools, to ask yourself in everything of this kind is ‘What is its use to me?’ ”19 Very different in tone, but not remarkably different in its conclusions about the direct utility of experimental physiology, was the complaint of clinician S. J. Meltzer, in 1904, that internal medicine had received little benefit from physiology “because this science is keeping aloof from medicine and its problems.”20

As laboratory physiology became an established part of the medical curriculum, the number of skeptics perhaps declined, but they certainly did not disappear. From Meltzer through Rufus Cole and Alfred Cohn, in the 1920s and 1930s, to Frank McLean, in the 1960s, one can trace the theme that physiology—if it had once been useful for clinical medicine—was becoming less so every day. That, at any rate, was part of the justification these men offered for their efforts to establish and extend a new independent discipline of experimental clinical medicine. They hoped that this new
discipline would reduce some of the obvious distance between the laboratory and the ward.21

In a way, the frequency with which physiologists and academic clinicians continued to insist upon the need for closer interaction between the two fields is itself a striking indication of the persistence of their separation. Physiologist W.H. Sewall might insist, in 1923, that “today . . . every physician recognizes that he is likely to understand his sick man in proportion as he apprehends ‘clinical physiology,’”22 but that same year, one such “clinical physiologist,” A.B. Luckhardt, lamented the continuing split between the clinician and the laboratory worker. “Although both groups are intensely interested in the progress of medicine,” he wrote, “each group, curiously enough, views the work of the other either with a silent disregard or more often with disdain or openly expressed contempt.”23 Five years later, physiologist C. J. Wiggers thought he detected a few encouraging signs of increased cooperation between clinicians and physiologists, but he could still only hope that they would soon be “walking arm in arm,” rather than “making mere gestures of shaking hands across the street.”24

As a matter of fact, most such appeals for greater cooperation between the clinician and the laboratory worker probably understated the extent and depth of the split. The problem went beyond the mere indifference or skepticism of clinicians toward laboratory science. Some clinicians, including a few of the most celebrated, continued to echo D. W. Cathell’s concern that laboratory training might actually damage the practitioner’s ability to treat patients effectively. Although my first two examples of this sentiment will be drawn from statements by great English clinicians, there is no doubt that American counterparts could be found. Sir Archibald Garrod, renowned for his work on the “inborn errors of metabolism,” wrote in 1931 of his belief that “laboratory findings are little less fallible than clinical inferences, and . . . in some cases they actually mislead.”25 In 1919, the eminent cardiologist Sir James Mackenzie expressed the same reservation far more harshly in his book, The Future of Medicine. “Laboratory training,” he wrote, “unfits a man for his work as a physician, for the reason that, not only does the laboratory man fail to educate his senses, but he puts so much trust in his mechanical methods that he never recognizes their limitations and he fails to see that there are other methods which are essential to the interpretation of disease.”26 A decade later the American clinician, Alfred Cohn, who generally adopted a more nuanced and less hostile tone, did nonetheless insist that “the history of medicine since the Renaissance has shown plentifully that whenever the approach to an understanding of disease is made by scholars trained primarily in other pursuits of knowledge (including physiology) . . . the result, so far as understanding disease is concerned, is disappointing and sometimes grotesque.”27 As recently as 1967, in his book Clinical Judgment, American cardiologist Alvan Feinstein produced a perceptive and sometimes eloquent statement of the position that reliance upon “scientific” medicine—simple animal experiments, elegant physiological theories, and elaborate diagnostic instruments—can actually distort the clinician’s judgment when he encounters disease, in all its complexity, in real human beings. For Feinstein, not only incidentally, the persistence of the split between experimental biology and clinical medicine is reflected in the continued division of medical journals and conferences into separate scientific and clinical sections, which seem to have little to do with each other.28

To be sure, the major aim of Feinstein’s book is to lay the foundations for his own version of a truly scientific clinical medicine. But the “science” upon which he would have clinicians build is mathematical logic. More specifically, he advocates the use of Boolean algebra and Venn diagrams as the basis of a new and subtler form of disease “taxonomy.” Whatever its merits, Feinstein’s version of scientific medicine is clearly a world apart from Bernard’s (or Virchow’s) experimental “determinism,” and it is the skepticism of clinicians toward that experimental vision of medicine that we shall now seek to explain.

The Role of Economic and Temperamental Factors in the Division

We cannot begin to understand the persistence of the division between doctors and research physiologists unless we emphatically reject one of Virchow’s more exasperated admonitions. “Whether someone is or is not a practitioner ex professo has little to do with the matter,” he insisted. “If only people would finally stop finding points of disagreement in the personal characteristics and external circumstances of investigators.”29 What Virchow would have us do is to ignore an important part of reality—as if unanimity of opinion or convergence of interest could be expected from individuals and groups with deeply different “personal characteristics” and “external circumstances.”

In the case at hand, differences in “personal characteristics” were decidedly reinforced by wide differences in “external circumstances”—both in the nature of the tasks performed and in the structure of the respective reward systems. It is one thing to publish papers; it is quite another to treat patients. And surely no reader of this volume need be told that eminent American doctors (and, increasingly since the 1930s, ordinary doctors too)
have always enjoyed higher social status and higher incomes than eminent research scientists. Even in the first decade of this century, the difference was already obvious. In 1909, when S. J. Meltzer sought to entice a group of graduating physicians into his proposed new discipline of experimental clinical medicine, he felt the need to appeal to the example of the German university system. There, he claimed, scientific research was more highly valued than medical practice, and the character of youth was not formed by “sport and the habits of millionaires’ sons.” There, unlike the United States, “the worth of the individual is not measured exclusively by a gold standard.” Should anyone in his audience remain unclear, Meltzer warned them that medical practice was “a bewitching graveyard in which many a brain has been buried alive with no other compensation than a gilded tombstone.”

Meltzer’s ally, that ubiquitous layman Abraham Flexner, also had good reason to respect the power of the “gold standard.” Throughout the 1910s and 1920s, his efforts to create “full-time” salaried clinical chairs ran into serious difficulties, initially even at John Hopkins—perhaps partly because American physicians rather oddly found the very concept of a salary in some way unprofessional, but surely also because the proposed amount of the salary (initially, $10,000) was so much less than the income to which leading clinicians had become accustomed from their practices.

From the mid-1940s on, the income differential between research physiologists and practicing doctors became so striking that the American Physiological Society considered it a serious obstacle to the recruitment of high quality personnel. According to the data collected in the society’s remarkable self-survey of the 1950s, the average net income of physicians in 1940 was $4,400, compared to $3700 for physiologists. A mere five years later this modest gap had widened to $11,000 for physicians and $4625 for physiologists, while by 1952 it was $14,080 for physicians and $6560 for physiologists. The absolute differential is surely much larger now—with physicians earning a tremendous range of incomes, with a median of perhaps $65,000, while the salaries of employed physiologists cluster within a much narrower range, around $30,000.32 Already in the 1950s, the American Physiological Society was more concerned about the loss of “brainless people” to medical practice than Meltzer had once been about the “loss to clinical medicine of the brainy men who now devote their energies to the pure sciences.”

In the academic market of the 1970s, a cynical (or honest) physiologist might warn potential aspirants to the field that “research is a bewitching graveyard in which many a body has been buried alive with no other compensation than an occasional published paper.”

Yet throughout the period of widening income differentials, an apprecia-

ble (if declining) number of students proved willing to make economic sacrifices for the sake of physiological research. And as late as the 1950s, at least, most physiologists expressed general satisfaction with their chosen career.34 Once captured by research, few physiologists with M.D.s (from Claude Bernard on) forsook it for the medical practice they could have entered. For most of them, the obvious and growing financial appeal of a medical career could not divert them from their personal inclination; for them, an unfavorable difference in “external circumstances” could not overwhelm a more profound difference in “personal characteristics.” It did, however, help to increase the distance separating them from practicing doctors.

By now the available sociological and psychological data seem sufficient to establish the existence of significant differences in background, personality, and values between those intending to be physicians and other members of their age cohort, including those who undertake careers in scientific research.35 Indeed, the so-called “Two Cultures” gap between humanists and scientists may be as nothing compared to that which separates pragmatic, action-oriented, client-dependent professionals (including physicians) from those with essentially scholarly sensibilities (including research physiologists).36

Long before any data were systematically compiled, experimental scientists and practicing physicians were aware of deep differences between them. Such major nineteenth-century physiologists as Claude Bernard, Carl Ludwig, and Emil du Bois Reymond, although sometimes at odds over issues within science itself, were united in that mixture of disdain and grudging envy with which they regarded practicing doctors. Vallery-Radot, in his Life of Pasteur, captures some of this ambivalence in a presumably apocryphal, but nonetheless revealing exchange said to have been initiated by Bernard, who, unlike Pasteur, did at least possess an M.D. degree. Vallery-Radot has Claude Bernard ask Pasteur, “with a smile under which many feelings were hidden, ‘Have you ever noticed that when a doctor enters a room, he always looks as if he was going to say, ‘I have just been saving a fellow man?’”

The physiologists of more recent times have continued to feel, and occasionally to express, that sense of ambivalence. In 1945, for example, the Harvard physiologist Walter Cannon wrote as follows:

My father's wish that I might become a physician was... never realized. Instead of engaging in practice I engaged in teaching medical students. This was what my predecessor, Dr. [H. P.] Bowditch had done. He told the tale of a conversation between one of his children and a little companion. The companion asked,
"Has your father many patients?" and the answer was, "He has no patients."
"What! A doctor and no patients?" Thereupon the apologetic answer, "Oh, no, he is one of those doctors who don’t know anything!" Possibly the children of other physiologists suffer from the same sense of inferiority. One of my daughters, on being informed proudly by a little friend that her father was a doctor, remarked somewhat sadly, "My father is only a father."38

If that is a joke, it is a deeply symbolic one, and we may wonder whether Cannon (like Rowditch before him) is not indulging the familiar-parental habit of speaking through their children.

That is not to say that Cannon himself actually felt an outright "sense of inferiority" vis-à-vis doctors; the rest of his autobiography suggests otherwise. Rather, it is as if he felt the need to call attention to the high (but ephemeral) social status of individual doctors in order to distinguish the very different and basically intellectual motivations that lay behind his calling. That self-identification with intellectual goals, which also permeates most other autobiographical accounts by research physiologists, helps us to understand the existence and persistence of the split between them and practicing doctors. If physicians have often looked upon research physiologists as remote "dreamers," or worse, the physiologists have tended to regard doctors as mere "technicians" who cannot or will not appreciate the value of basic scientific research or of scholarship in general. The result, quite obviously, has been to increase whatever distance between them might have been accounted for by any actual disjunction between physiological knowledge and medical practice.

In the following section, these psychological (or temperamental) differences between physiologists and physicians are invoked to help us understand the response of physiologists to the fragmentation of physiology as a discipline. Because of this fragmentation, physiologists faced the charge that their discipline had become both intellectually sterile and progressively less relevant to medical science. The physiologists, it seems, found the former charge more worrisome than the latter.

THE FRAGMENTATION OF PHYSIOLOGY: THE PRIORITY OF THE INTELLECTUAL CHALLENGE

Quite steadily, from about 1900 onward, physiology lost its privileged place in the world of experimental biology. Just as physiology had once declared its independence from medicine and medical anatomy, so new fields and specialties now seemed to declare their own independence from physiology. Actually, we know very little as yet about the emergence of these new disciplines. It is certainly premature, and probably misleading, to claim that the new disciplines evolved directly and simply out of physiology. But that was the way many physiologists saw it. For them, a sense of fragmentation resulted from the creation of independent societies by specialists who had begun to feel some dissatisfaction with the American Physiological Society, founded in 1887. In the early years of the twentieth century, there appeared in rapid order the Society for Experimental Biology and Medicine (1903), the American Society for Biological Chemists (1906), the American Society for Pharmacology and Experimental Therapeutics (1908), and the American Society of Experimental Pathology (1913).39 As early as 1911, the increasing fragmentation of what had once seemed to be physiology inspired the following remark from physiologist Henry Sewall: Ever ready to deploy (or mix) metaphors, Sewall voiced his impression that:

the course of evolution has ordered it that whereas physiology was then [in the 1870s] the dependent runt of the medical family, it is today the eldest son in a stable system of primogeniture.40 As with a noble jewel, whose beauty depends upon the cutting, we may name one facet Pathology, another Pharmacology, another Bio-chemistry, another Psychology, and so on, the jewel remains and ever will be Physiology.41

Other physiologists, then and later, expressed similar sentiments with remarkable frequency, and their common use of biological metaphors in which physiology remains the "trunk" or "mother-stem" of the vigorous new disciplinary branches does not entirely mask an underlying concern. Along with a growing sense of estrangement from the increasingly intricate methods, techniques, and instruments of modern experimental biology or "biophysics," some "classical" medical physiologists began to wonder whether physiology any longer existed as a discrete intellectual entity at all. The conceptual doubt was reinforced by recruiting difficulties, for the number of new Ph.D.s in physiology actually declined in the 1940s—not only absolutely, but also (and more importantly) in proportion to new Ph.D.s in other biological disciplines.41

Certainly, by then, if not before, the situation had provoked something of an internal crisis in the field. In the early 1950s, the American Physiological Society, funded by the new National Science Foundation, undertook a remarkably complete and revealing survey of the discipline. The survey addressed itself in part to the question of how physiology could be defined, and in one striking aside, survey director R. W. Gerard carried Sewall's
evolutionary metaphor to one of its possible conclusions. For if Sewall, in 1932, could still see physiology as the “eldest son in a stable system of primogeniture,” Gerard raised the possibility that its new disciplinary offshoots might be better adapted to the age, while physiology itself faced the prospect of extinction, of becoming “a fossil on musty library shelves.”

The threat of extinction came from at least two directions simultaneously. For the new disciplinary subspecies not only included fields which seemed intellectually more exciting than physiology—biochemistry, cytology, genetics, cellular physiology, and biophysics—but also others which seemed (at least on the surface) of more immediate utility to medicine—notably bacteriology or microbiology, pharmacology, nutrition, immunology, and endocrinology. And so, as the fragmentation became increasingly obvious, physiologists had to decide which of these two developments they considered more threatening.

On the whole, despite the once aggressively utilitarian rhetoric of spokesmen like Bernard, physiologists apparently found the charge of intellectual sterility more immediately damaging that that of medical irrelevance. They certainly noticed both aspects of the challenge, but they responded to the latter with rather considerable restraint. Often enough, they referred to the value that physiology might derive from a closer interaction with clinical medicine (rather than the other way around), and they rarely exaggerated the direct medical utility of their work—even on so profoundly practical a problem as wound shock, which occupied a truly staggering proportion of American physiologists and their Journal after the nation entered World War I.

Insofar as physiologists did confront the clinical challenge, they seemed most concerned about such “academic” clinicians as Meltzer, Cole, Cohn, and McLean. The challenge these critics posed was at once intellectual and institutional. For Meltzer and his three spiritual descendants, all of whom had significant ties to Abraham Flexner and the Rockefeller Foundation, took the almost paradoxical position that the best solution to the medical remoteness of physiology was to create yet another entirely new independent discipline, “experimental clinical medicine.” In the more or less typical language of biomedical reformers, Meltzer claimed that this new discipline would keep more firmly in touch with medical problems. Yet he also insisted that it absolutely required independence from actual medical practice—more so, even, than such “ancillary” sciences as physiology.

Physiologists, already concerned about the fragmentation of their subject into new academic fields, began to wonder aloud what would be left of the discipline if it now faced a frontal assault from the proposed new discipline of “experimental medicine.” When physiologist C. A. Lovatt Evans expressed open concern about these “Meltzerian” proposals, in 1928, Alfred Cohn probably did little to reduce that concern when he responded as follows:

Although physiology has made itself independent, Professor Evans still harbors fears. He fears to cut the guiding strings of the alma mater [medicine], lest physiology lack nourishment. And like many... children, he fears lest the ancient mother be too feeble intellectually and too powerless, having reared and weaned her children, to be able to continue to order and to develop her own house. But the situation is just this: having learned as it were and indicated to her many offspring how they might best set up houses of their own, medicine is at length free to cultivate her own garden.

At this point, it seems to me, the rank-and-file practitioner might have been excused for casting a smile at the medico-academic elite. So the long campaign of research physiologists to achieve independence from medicine was to end, after all, in nothing more than an opposing effort by “academic” clinicians to reclaim part of the lost territory! To the ordinary doctor, the situation was doubtless reminiscent of other comical but irrelevant disputes among academics. In addressing themselves above all to the challenge posed by fellow academics, the physiologists were responding to the intellectual inclinations that had attracted them to research in the first place. But ordinary doctors can only have felt still further estrangement from such physiologists as Nobel Laureate Otto Loewi, who actually suggested in 1954 that the discipline might never have entered its “crisis” if only its practitioners had pursued problems of even more “remote usefulness for medicine.”

**AN HERETICAL SUGGESTION: COULD THE SKEPTICAL CLINICIANS HAVE A POINT?**

Medical historians have not entirely ignored the skepticism of practicing doctors toward experimental physiology (or experimental science in general), and doubtless most would agree that economic and temperamental factors contributed importantly to it. But perhaps the most common “explanation” for the phenomenon—essentially an echo of Claude Bernard’s position—is that one could not expect “short-sighted” clinicians to appreciate the benefits that medicine would “soon” reap from the triumph of experimental science. On this view, older clinicians, who had not been trained in the new scientific methods, would be especially unwilling to acknowledge their dependence on experimen-
tal science and especially likely to persist in the delusion that medicine was essentially an art. These older clinicians were also most likely to feel vulnerable to any economic threat posed by the rising breed of "scientific" and presumably more effective physicians. And so, out of ignorance and self-interest, they would naturally dispute the medical utility of laboratory science.47

At some point, however, this sort of explanation can no longer suffice. Surely by about 1920, when most doctors would have been exposed to laboratory science, its value should have been so obvious as to overwhelm their skepticism, except perhaps for a few quacks and cranks. From the evidence already presented, it should be clear that skeptics nonetheless remained, including at least a few leading clinicians. If such an eminent clinician as Feinstein can continue even today to express reservations about the medical value of laboratory science, medical historians ought to be willing at least to reconsider the matter. Insofar as the skeptics have focused on physiology per se, there is perhaps particular reason not to dismiss their views out of hand.

Twenty years ago, the Downstate Medical Center of the State University of New York sponsored an ambitious symposium on "the historical development of physiological thought," resulting in a book of that title. The book contains sixteen essays, many of which remain valuable studies, but only one directly confronts the relationship between physiology and medical practice. That essay, by Owsei Temkin, begins as follows: "If we were to awaken a man from sleep by shouting into his ear: 'medicine depends upon basic scientific thought,' he might unthinkingly say 'amen!' " With typical perspicacity, Temkin goes on to show how thoroughly complicated the issue looks when the immediate response is replaced by a thoughtful one. Indeed, the thoughtful response soon threatens to dissolve into utter confusion because of the difficulty of even defining such terms as "physiology," "science," "medicine," or "health." In the end, Temkin seems to suggest, medicine does indeed depend partly upon basic scientific thought, but often of a sort that research physiologists might fail to recognize as "scientific" at all.48 At the same conference (though his paper was published separately), Paul Crane filled briefly pondered the far more specific question of whether the preceding half-century of "infinitely delicate and beautiful studies of microscopic physiology" had influenced medical practice. His rather rueful answer: no, "by and large they have not."49

In other cases, too, in which it has seemed obvious that experimental physiology must have made vital contributions to medical practice, it may prove instructive and sometimes surprising to examine the situation critically. Consider, for much too brief a moment, the clinical field

in which the pragmatic value of physiology might seem to be most obvious—cardiology. This is, moreover, a field of direct concern to the average office-based practitioner, and one in which physiological research has been accorded a quite immediate and specific role. As C. J. Wiggers put it in 1951, referring to the "clinical applications" of modern circulatory physiology:

The physiological interpretations of graphic recordings of the pulse enabled [Sir James] Mackenzie to give physiological interpretations to many of the common [cardiological] irregularities. Additional experiments by [Joseph] Erlanger and [Arthur] Cushny elucidated the phenomena of heart block and atrial fibrillation, and their discoveries soon proved valuable in clinical diagnoses. The foremost step in the decade was the invention in 1903 of the string galvanometer by Einthoven and his prompt application of this tool to the study of physiological and clinical problems . . . while the discerning mind of Thomas Lewis immediately envisaged the great strides that could be made in the field of clinical cardiology through correlation of electrographic phenomena in patients and experimental animals.50

Upon closer inspection, however, the situation begins to look rather more problematic than Wiggers's summary suggests. To be sure, electrocardiography did emerge from an important tradition in purely "academic" electrophysiology. But can one trace any major innovations in therapeutic cardiology to this new physiological knowledge? At the turn of the century, physiological experiments on dogs led Arthur Cushny to the diagnosis of auricular fibrillation for the clinical condition previously known as "delerium cordis. In the course of this research, Cushny complained bitterly that clinicians had failed to keep abreast of the recent avalanche of knowledge in the physiology of the mammalian heart. Yet his own further research, and that of others, produced no dramatic departures in therapy, but mainly provided a new rationale for the use of the digitalis compounds that had already been introduced "empirically" for the treatment of such cases.51

Astonishingly, some leading clinicians even disputed the diagnostic value of the new electrophysiology and electrocardiography. Among them was Sir James Mackenzie himself, who figured prominently in the clinical developments to which Wiggers refers, not only through his own contributions, but also as sometime collaborator with Arthur Cushny and as mentor of Sir Thomas Lewis. It was Mackenzie, we should recall, who warned in 1919 that laboratory training could actually distort the clinician's judgment and "unfit" him for his work as a physician. Mackenzie, moreover, practiced what he preached, eventually abandoning even the primitive polygraph he had used in his early cardiological studies for the more direct and more
traditional methods of clinical observation. Recall, too, that Feinstein, who to some degree shares Mackenzie's skepticism toward laboratory science, is also a cardiologist.

Thus, even in the apparently straightforward case of cardiology, the medical utility of physiological research is open to some qualification, at the least. Surgery represents a second major clinical field in which experimental physiology has seemed to play an important and immediate role. Quite obviously, future surgeons can enhance their surgical skills by performing animal experiments, even if the immediate object is physiological. Moreover, some leading surgical mentors, notably Owen Wangensteen of the University of Minnesota, certainly did, and do, stress the value to surgeons of a thorough training in physiology. Indeed, one branch of modern "total surgery" even bears the name "physiologic surgery." In the 1964 edition of a leading American surgical textbook, this branch of surgery is said to have as its aim the alteration of "normal" but deleterious bodily function for the general good of the patient and to have its theoretical roots in Walter Cannon's concept of homeostasis. Cannon himself was considerably more restrained about the medical utility of his physiological work: "It is said," he wrote in 1945, "that our researches on the bodily effects of emotions have been helpful because they give the doctor pertinent information in explaining to his nervous patients the reasons for their functional disorders." Perhaps Cannon was being excessively modest here, and perhaps his work has had an immense impact on medical and surgical practice. Yet it is not immediately obvious exactly how and in what sense his physiological insights should have led to any significant reorientation of medical or surgical practice.

But let us not carry our own skepticism too far. Let it be clear that it would require infinitely more research to examine the wealth of possible contributions experimental physiology may in fact have made to clinical medicine or to health. At least on the surface, it seems obvious that experimental physiology must have made important contributions to the so-called "replacement therapies," of which insulin for diabetes, thyroid extract for myxedema, and liver extract for pernicious anemia serve as classic examples. It also seems clear that experimental physiologists must have contributed importantly to our understanding of such medically significant aspects of blood chemistry as pH levels, electrolyte balance, and incompatible blood types. The existing literature provides a host of other possible examples, thus far mostly in the form of schematic lists, which clearly deserve more extensive inquiry.

In the course of this more extensive research, a number of distinctions should be kept very firmly in mind. Different sorts of medical practitioners would doubtless come to very different sorts of conclusions about the utility of particular investigations in experimental physiology. What is useful to hospital-based specialists, for example, is liable to be very different from what is useful to office-based pediatricians, internists, and general practitioners. It is also vital to distinguish between the therapeutic, diagnostic, and preventive aspects of medicine with respect to the possible utility of experimental physiology. And whatever conclusions one may reach about the direct medical utility of laboratory physiology, one may well wish to consider separately its cultural value and its proper role in medical education. Let me conclude with some preliminary conclusions growing out of this last distinction.

REFLECTIONS ON THE GENERAL ROLE OF LABORATORY SCIENCE IN MEDICAL EDUCATION

Throughout much of this paper, I have insisted upon the persistent skepticism of practicing doctors toward the medical utility of experimental physiology. In the section immediately above, I have raised the question whether that skepticism may not have had some justification for at least some sorts of physicians. Yet Abraham Flexner, in his famous report of 1910 on American medical education, called physiology "the central discipline of the medical school." It has retained an importance place in the American medical curriculum ever since. For some readers, that will surely serve as prima facie evidence of its value to medical practice. Unless laboratory physiology is medically useful, why did it acquire and why does it retain such an important role in medical training?

To address this question, which might seem to dismiss out of hand any need to reexamine the relationship between experimental physiology and medical practice, we must give at least passing attention here to the more general role of laboratory science in medical education. What especially needs to be recognized is that doctors have gained at least one important benefit from the study of experimental science, quite apart from whatever direct medical utility it has, and quite apart from any indirect contribution it may make to medically effective modes of thinking. For the experimental sciences, like Latin in an earlier era, have given medicine a new and now culturally compelling basis for consolidating its status as an autonomous "learned profession," with all of the corporate and material advantages that such status implies.

It was Flexner, once again, who asserted that the "possessio of certain portions of many sciences arranged and organized with a distinct practical
ordinary vital statistics, we have even do not think in statistical terms and that medicine serves vital functions and apologists (but especially “academic” critics) to forget that sick people wonder whether such primary-care physicians as pediatricians and intern- 

general human support.62 But if we do not yet really know the extent to which physicians influence ordinary vital statistics, we have even less sense of whether or how much their scientific training enhances their ability to perform their less dramatic “supportive” functions. The slim available evidence does suggest—contrary to a segment of popular opinion—that scientific orientation and training are not inimical to the humanitarian aspects of the physician’s role.63 But it remains to be established that physicians so trained perform more effectively in their supportive tasks, and there is perhaps particular reason to wonder whether such primary-care physicians as pediatricians and internists are being appropriately trained to handle the medical problems that

they actually face.64 Especially in view of the high cost of training “scientific” doctors, we deserve rather better evidence that the expense is justified. For Flexner, who had graduated from Johns Hopkins during its golden early years as the American embodiment of “Germanic” research ideals, it was a self-evident proposition that an expansion of laboratory training in the medical curriculum would result in improved health for Americans. Thus he noted in passing—as if establishing a causal connection between two parallel phenomena—that “the century which has developed medical laboratories [roughly, 1810–1910] has seen the death-rate reduced by one-half and the average expectation of life increased by ten or twelve years.”65 It is precisely this alleged connection between laboratory training and health that requires critical scrutiny. And surely it will not do for medical historians to assume in advance the health benefits of experimental science or to ignore entirely the other functions that laboratory training plays in medical education.

NOTES

This is a considerably revised version of the paper I delivered at the conference, “Two Hundred Years of American Medicine,” in Philadelphia, 2–4 December 1976. As originally presented, the essay included a section that argued for the existence of an “Anglo-American” style of physiology. I now expect to develop that theme in a separate publication. Moreover, since giving my paper, I have profited from the criticisms of James Secord, Robert Bernstein, and an anonymous referee for the University of Pennsylvania Press, and (especially bibliographically) from reading an as yet unpublished essay on the “social and intellectual location of physiology in America” by David Bearman. In the footnotes that follow, I have tried to indicate my more specific debts to Mr. Bearman.


3. A perhaps typical, if not classic, example of the genre is Carl J. Wiggers, “The Interrelations of Physiology and Internal Medicine,” Journal of the American Medical Association 91 (1928): 770–74, where a long but utterly schematic list is given of the “physiologic researches . . . of immediate interest to the clinician.”


7. Ibid., p. 50.

8. Ibid., p. 91. This was almost certainly Franz Schuh, one of the most "scientific" surgeons in Europe. See Erna Lesky, *The Vienna Medical School of the 19th Century*, trans. L. Williams and I.S. Levi (Baltimore: Johns Hopkins University Press, 1976), pp. 168–73. If so, one can only imagine the response of less academic surgeons.


10. Ibid., p. 55.
15. See text below, section 6, and the sources cited in n. 60.
17. Ibid., p. 1466.
42. Ibid., p. 249.
43. See, e.g., Carl J. Wiggers, "Physiology from 1900–1920: Incidents, Accidents, and Advances," [1951], in *Excitement and Fascination of Science*, pp. 547–66, who says (on p. 558) that his hemodynamic studies "tended to confuse rather than clarify" the problems of shock. More generally, see the other essays in *Excitement*.
55. I am indebted to Robert Bernstein for emphasizing to me the examples from studies of blood chemistry. It may well be significant that Rufus Cole, who
was otherwise somewhat skeptical about the utilitarian claims of physiologists, did concede the medical value of their research in these areas. See Rufus Cole, "Progress of Medicine During the Past Twenty-Five Years as Exemplified by the Harvey Society Lectures," *Science* 71 (1930): 617–27.

56. Cole, ibid., offers a few other examples that may be all the more deserving of investigation since they come from a clinician and moderate critic of physiologists' claims for medical utility. For an expansive list of such claims, I refer the reader again to Wiggers, "Interrelations."


58. I mean to suggest here, though none of my sources develops the idea, that laboratory training may encourage a receptivity to novelty. That claim is sometimes made on behalf of liberal studies in general.


