new medical students annually and giving them instruction. In my day his lectures had become routine, but they were admirably illustrated with well-prepared demonstrations. His manner with students was kindly though dignified, and his judgments were generous. For some years he had not been active in research. But when, in October, 1896, we started plans to use the recently discovered x-rays to study the process of swallowing, he paid close attention to them and gave the enterprise wholehearted support.

In the following years of association with him I became well acquainted with his rare qualities as a man. He was eminently single-minded. He seldom spoke of the past—the prospects ahead were more important. He was a natural leader, tempered by courtesy, fairness and good will. His friends were many and there was mutual devotion between him and them. His conversation was not witty, but he had a delightful sense of humor. I well recall his hearty laugh as he told of an overheard conversation between one of his little daughters and a neighbor's child. The visitor expressed surprise that Dr. Bowditch, a doctor, had no patients; the daughter explained, "Oh, my father isn't that kind of doctor; he is the kind of doctor who doesn't know anything!"

He had a life full of achievement. As a young man, in 1861, he left his studies to enter the Union Army. Though wounded while leading a charge he returned to the fighting forces and served to the end of the war. At the start of his professional career he brought back to the United States ideas and inspiration which he had received from Ludwig and the enthusiastic group in Leipzig. He established here the first physiological laboratory to which students were welcomed. In stimulating his students to carry on investigations he began a movement which has now spread almost everywhere in our land. He touched many aspects of research himself. He was intimately concerned with many new developments in modern physiology. He preserved extraordinary health and activity until he was well over sixty years of age; then a slowly progressive disease—paralysis agitans—crept upon him, and he had to endure the gradual loss of all his powers. As he waited for the end his friend from early manhood, William James, wrote to him. "I admit that the form of your tragedy beats that of most of us, but youth is a stuff that won't endure in any one, and to have had it, as you and I have had it, is a good deal gained." He had had it, to be sure, and had used it in admirable ways, leaving a lasting example of service to worthy causes as his legacy to American biologists.

SILAS WEIR MITCHELL, 1829–1914[1]

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DR. S. WEIR MITCHELL was born in Philadelphia on February 16, 1829. His parents were of Quaker stock. His father, Dr. John K. Mitchell, was a noted Philadelphia physician, a professor in the Jefferson Medical College, and ahead of his time in scientific and literary attainments.

Silas Weir was imaginative as a child, perhaps an early index of his prolific pen in the production of works of fiction in later years. Once he was put to bed on bread and water for 24 hours, for saying and sticking to it, that he had seen "pink elephants walking down Chestnut Street."

But his bent towards the more serious things of science also was early in evidence. He writes, in his fragmentary autobiography: "One of my greatest joys was to go with my father to his chemical laboratory in Locust Street, where he conducted experiments and gave lectures in a spring course."

In 1844 at the age of 15, Silas Weir entered the University of Pennsylvania, which he described as "a small affair with some good men." As a college freshman he was twice reprimanded for disorder, and once warned for deficiency in scholarship. He had not learned to work hard, his health was not the best, and he was much given to day dreaming. Young Weir's father urged his son to go into business, or at least cease dreaming and make up his mind as to his future work. Weir decided on medicine, to his father's disgust. His father said: "You have no appreciation of the life. You are wanting in nearly all the qualities that go to make success in medicine. You have brains enough, but no industry." It seems that even an able father may not know his own abler son.

Young Weir entered Jefferson Medical College. He writes thus concerning his first year as a freshman medic:

> I had to learn to work, to concentrate attention. It came hard. I used to go over and over some confounded bone, and fall asleep. The more abominable those dry bones became, the more I worked. After six months of this I began to hear Dunglinson's lectures on physiology. This was very interesting. Although neither he nor anyone else taught physiology with experiments or illustra-

[1] Address at the fiftieth anniversary meeting of the American Physiological Society, Baltimore, Md., April 1, 1938.

tions, *still it captured me.* I think I began, then, to develop the desire to leave no riddle unsolved, and this has made the laboratory a delight to me.

He graduated from Jefferson Medical College in 1850. The following year he spent in Paris for further medical training. Concerning this he writes:

I took courses designed for training in surgery. But I liked much better the lessons of Claude Bernard in physiology, and of Robin in microscopy. I recall one remark to me by Bernard. I said, "I think so and so must be the case." "Why think," Bernard said, "when you can experiment? Exhaust experiment, and then think."

It is probably not without significance in the outstanding adult achievements and character of Weir Mitchell that in his formative years as a medical student, he came under the influence of the two ablest teachers of physiology of that period both in America and in Europe. Dr. Mitchell's study in Europe was interrupted by illness (small pox), and cut short by his father's failing health. So in the autumn of 1851 he returned to Philadelphia, and started in the practice of medicine, a period of extraordinary achievements covering 63 years. His father urged him towards surgery, but young Weir found that he had neither the stomach nor the hand for surgery, so internal medicine became his bread and butter, physiology and experimental medicine his avocation. He writes:

"I was asked by several gentlemen to join in a summer school of medicine. I was to teach physiology, and this I was glad to do, as I always had some leaning in that direction, and had it not been for the fact that I failed later in my efforts to become a professor of that branch, physiology would have been my life long work. But I never was a good teacher, because of poor memory."

In the sixties Weir Mitchell became an applicant, in turn, for the professorships of physiology, both at Jefferson Medical College, and at the University of Pennsylvania Medical College. The appointive authorities in both institutions turned him down. I know not their reason, but I can not praise their perspicacity, for they rejected one of the ablest, if not the ablest man of their generation, a man that would have done honor to any faculty, in any school, in any country, at any age. Surgeon-General Dr. W. A. Hammond thus expressed his disgust and disapproval of Mitchell's rejection for the professorships in physiology: "It is an honor to be rejected by such a set of hairy apes." Oliver Wendell Holmes, commenting on Dr. Mitchell's rejection by the Jefferson School, said: "Perhaps it is hardly desirable that an active man of science obtain a professorial chair too early, for I have noticed that the wood in academic fauteuils has a narcotic quality which occasionally renders the occupants somnolent, lethargic, or even comatose." Louis Agassiz wrote to Dr. Mitchell, "I hope the selection of a professor of physiology in the most important medical school of the United States will be influenced chiefly by the opinion of the most competent men of the country, as I am satisfied that in that case your selection is assured, and being in position in which you can devote your abilities to further the advance of your science, there shall be another center of real progress in one of our institutions of learning."

Dr. Mitchell, who in national politics was a Republican, thought his rejection by the Jefferson Medical College and by the University of Pennsylvania was due to myopic democrats on the board and on the faculties of these institutions. Dr. Dalton, then professor of physiology in the College of Physicians and Surgeons in New York City, thought the reason was the then prevailing opposition of medical men to experiments in the medical sciences. Friends in Philadelphia told Dr. Mitchell that for every experiment he performed he would loose one patient. But these disappointments did not paralyze his hand, did not sour his soul. He worked, and sang, and ever urged his younger colleagues to fresh endeavors. In less than two decades Weir Mitchell had become the leading citizen of Philadelphia, and the leading man in clinical neurology in the United States, honored alike at home and abroad. But the reasonable procedure in appointment to university chairs, proposed by Agassiz eighty years ago has not yet been generally realized in this country. The "hairy ape" is still about, inside and outside our institutions of higher learning.

Weir Mitchell began his experimental and clinical publications in 1852, and during the following sixty years he published over two hundred and forty papers and monographs in these fields. His interest in the action of snake venoms started early and continued throughout his life, by personal work, by financial support, and by inducing younger investigators to lend a hand, such as Hammond, Keen, Reichert, Simon Flexner, Noguchi and Leo Loeb. Perhaps the outstanding contribution on this program was the discovery of the toxic albumins. In his work on the cerebellum (1869) he advanced, on the basis of experiments and clinical observations, the general theory that the cerebellum is an augmenting organ for the skeletal motor system, a theory later supported by the extensive work of Luciani.

The other main line of scientific pursuit of Weir Mitchell falls in the field of clinical neurology, evidently greatly conditioned by his army experience in our Civil War, 1860–64. He writes as follows concerning his service as an army surgeon:

My years in the United States hospitals were confined, except for a few days' work away from home, to the great hospitals surrounding Philadelphia, where in fact there were twenty-six thousand beds for the sick and

wounded. I declined the position of brigade surgeon. . . . My first appointment in October, 1862, was at the Filbert Street Hosptial. . . . There I began to be interested in cases of nervous diseases and wounds of nerves, about which little was then known. In consequence, other men who did not like these cases began to arrange transfers to my ward.

This so interested the Surgeon General that he created a small hospital for nervous diseases in Christian Street. . . . The hospital soon outgrew the building, and again the Surgeon General created a new special hospital of four hundred beds. . . . There came out of this a series of well known papers and one book which revolutionized knowledge as to wounds of nerves. . . .

We worked on at notetaking often as late as twelve or one at night and when we got through, walked home, talking over our cases. Usually the work took four or five hours and we did it all in person. The late hours came two or three times a week and usually followed an inflow of cases of injuries to nerves after some serious battle. I have worked with many men since, but never with men who took more delight to repay opportunity with labor . . . the opportunity was indeed unique and we knew it. The cases were of amazing interest. Here at the time were eighty epileptics and every kind of nerve wound, palsies, choreas, stump disorders. I sometimes wonder how we stood it.

Mitchell did not stand it. His health gave away and he had to quit work for a rest period in 1864.

In 1872 he published the significant monograph on "Injuries to Nerves and Their Consequences." In another monograph entitled "Fat and Blood," he outlined his once famous, but now nearly forgotten "rest cure" for functional nervous disorders. There is probably more sound physiology and rational therapeutics in the "rest cure" than even realized by Dr. Mitchell. But other days, other gods and other thrusts into the fog. Freud is now the oracle in this field.

There are several versions as to the intellectual or circumstantial steps that lead to the Mitchell "rest cure." Dr. Keen opined that Weir Mitchell's "yeasty mind" began to orient the matter, stimulated by two patients with functional nervous ailments who happened to break their legs. There seemed to be mental improvements in consequence of the enforced rest while the bones were healing. But Weir Mitchell is reported to have told Dr. Osler that the idea came to him purely as a trial and error, having a psychotic patient whom another physician had failed to help by exercise, he decided to put the patient to bed. But Weir Mitchell was a striking, kind, forceful person. The temporary or lasting effects of some therapeutic procedure in functional nervous disorders seem to depend, in part, on the personality of the doctor who prescribes the procedure.

Dr. Mitchell offered through the American Physiological Society out of his own pocket two prizes towards defraying the expenses of experimental research on the nervous system. The second of these Mitchell prizes was awarded in 1891 to W. H. Howell and G. C. Huber for their work on the degeneration and regeneration of peripheral nerves.

Eight years after his graduation from the Medical School, Dr. Mitchell published a digest of the "American Papers on Physiology" in the two previous decades. He called it aptly a "melancholy catalogue," when compared to the contributions to physiology in Europe during the same period.

Weir Mitchell was a man, like Beaumont, and (in his earlier years) Meltzer, whose character, intelligence and restless curiosity forced them to break new ground in the medical sciences, though institutional connections or aids were lacking. In a letter to Owen Wister (1908) Mitchell said: "I have been through a time in my life when I was working at scientific problems, earning my living, harassed by anxieties, had but slender means, and was yet aware that for great scientific research a man must have leisure—freedom from care."

I met Dr. Weir Mitchell but once, in the year 1904, in Philadelphia. He made a lasting impression on me, with his great dignity, his patrician bearing, his kind and thoughtful visage, something like the impressions made on me years later by Pavlov of Leningrad, and Schaefer of Edinburgh. Weir Mitchell knuckled down to no man, be it the King of England or the President of these United States. He was surely both kind and stimulating to his younger colleagues. Writes Dr. W. W. Keen:

> I was sitting in Dr. Brinton's office, with a skull in one hand and in the other a volume of Gray's Anatomy, the blinds opened and I looked out between them into a pair of brilliant eyes. A tall, fair, energetic man was standing on the sidewalk. Then and there he asked *me*, a doctor three days old, to help him in his snake-poison experiments. Thus, began the most powerful influence of my professional life.

He was a prodigious worker, with a very large medical practice. He wrote some fifteen stories or novels and some verse, "as a recreation." A good friend said of him: "Dr. Mitchell has no more sense of humor than a poker." That characterization hardly squares with his life and letters, and the opinion of others among his many distinguished and devoted friends. It is told that, confronting an hysterical lady patient, and argument and persuasion having failed to induce her to get out of bed, Dr. Mitchell said: "If you do not get out of that bed in five minutes, I am going to get into that bed with you." The five minutes being up, Dr. Mitchell with great deliberation took off his coat, and then his vest, but when he started to take off his pants the lady got out of the

bed in a hurry and in a fury. One may question the efficacy of this therapeusis in our country in 1938, at least outside the city of Philadelphia. There is, humor, banter, as well as elegy in this letter to his niece, Hester Hone.

> We are just in from fishing and are sitting in our shirt sleeves, looking very lazy and as brown as King Alfred's cakes. I got one salmon but he was as troublesome as a young lady, and did require a good deal of attention before he said yes. And after all he weighed only 21 pounds. Oh, if I could have you in a canoe for an afternoon, and let you take a 30 pound salmon, and show you how beautiful the woods are, and the river when the sun is low, all a rolling surface with blue, and green and gold, with bits of brown and hanging purples.

Weir Mitchell saw an unusually full life in his labors, in his friendships and in later approbation of society. He also had his share of shadows. To be sure, his earlier frail frame changed into a magnificently robust and vigorous manhood. But despite his great industry and his great success, he could write in 1874: "I am meditating much work to fill the chinks of loneliness." When a doctor friend tried to cheer him after the death of his beloved twenty-two year old daughter, he replied: "Can you mend an old man's broken heart?" The following lines ("Ode to a Lycian Tomb") written by Weir Mitchell and published in 1899 seem to say that the pean of pessimism of the ages: "Vanitas vanitatis, omnis vanitas est," will now and then touch the spirit of any person whose life lingers and who achieves a significant understanding of man:

> Thou who has wept for many weep for me,
> For surely I, who deepest grief have known
> Share thy stilled sadness, which must ever be
> Too changeless and mending, like my own
> Since thine is woe that knows no time's release
> And sorrow that can never compass Peace.

As a final attempt to make a great colleague of the past walk and talk at this semicentenary permit me to quote the following paragraphs from Dr. W. H. Howell's History of the American Physiological Society—the first 25 years:

> Weir Mitchell was the outstanding physiologist in the United States during the two decades preceding the establishment of the physiological laboratories at the Harvard and the Johns Hopkins Universities. . . . He was probably the most distinguished and the most widely known member of the American Physiological Society at the time of its foundation. . . . The Society was fortunate, indeed, in having among its original members five men, Mitchell, Wood, Welch, Osler, and Vaughan who were or who soon became the acknowledged leaders of scientific medicine in this country.

With these glimpses of the past, and standing in the turbulent present, what about to-morrow? Is the output of significant contributions by American physiologists still going sharply up? Is it reaching a plateau? Is there a descent in quality? What are our desiderata and the ways to their approach?

(1) Confronting the physiologists of to-day and to-morrow is a complexity of unsolved scientific problems, more varied and seemingly more fundamental than could have been conceived even in the dreams of Mitchell, Bowditch and Martin, or tackled by the techniques of their generation. So we need not weep for worlds to conquer, or excuse sloth by lack of problems to solve.

(2) To-day, the physiological approach, that is the dynamic and experimental as over against the static, the structural, the primarily descriptive, has conquered practically all biology and medicine in this and other countries. Excellent physiological research is coming from laboratories of Zoology and Anatomy, from Medicine and Surgery, from Psychology and Bacteriology. The battle line of physiological attack on the unknown has then been greatly extended in the last fifty years by the biologists themselves, as well as by the pathfinders in the physical sciences. I look upon this as a great stimulus to the present generation of physiologists, to try to hold their own, to endeavor, if possible, to lead.

(3) Physiology is to-day even more important in the medical school, in the training of physicians, than it was fifty years ago, and I can see no indications of its diminishing importance is this field. Moreover, physiology has gained and is gaining status as university discipline, along with chemistry and physics, in our stronger universities and colleges. Fifty years ago physiology in the United States was scarcely more than a feeble handmaiden to medicine.

(4) The teaching of experimental physiology is in the process of significant extension to the levels of general college education, and even into the high school. Society is slowly but surely realizing that the rule of reason as regards health, happiness, and the farewell to fear, require a more thorough understanding of the machinery of our bodies than that contributed by the number of bones in our skeleton, and the action of whiskey on the gastric mucosa. The teaching of individual health, of public health—a desideratum for every citizen, old and young, white, yellow or black—becomes an exercise for parrots, for morons, unless based on our dynamic body machinery (that is, physiology) in health and in disease. Our more perspicacious colleagues in botany and zoology saw long ago that even such lovely human processes as colonic peristalsis may involve as much fundamental biology as ameboid movements, reproduction in the sporophytes or the sociology of the honey bee. In other words, teaching of physiology in the service of society

is going beyond and below the field of medicine as conceived and practiced 50 years ago. It has become co-extensive with the field of preventive medicine, which we talk about to-day, and dream about for to-morrow.

(5) A new problem now confronts the workers in physiology, or at least a different aspect of the problem than that facing our colleague of 50 years ago; that is; the publication of research. The steadily increasing number, as well as the number of annual volumes, of scientific journals is becoming an economic problem, if not a housing problem, both for the investigators and the libraries. The heat from the friction between editors and contributors is oxidizing some of our verbosity and redundancy. But when editors force elimination of experimental protocols and other essential data to the point, where independent evaluation of conclusions becomes difficult if not impossible, we have reached a crisis, and that crisis is here. What to do? The final answer is not more volumes or more journals. Nor is it less research, or publications of mere conclusions. Let us see to it that an answer is not essayed via the ancient torch of the modern Saracens. National depositories for experimental data, perhaps in the form of Leica films, available to investigators under government frank, might be the permanent solution, but this seems far away. And in the meantime we spawn more journals, waste much epinephrine, and there is no peace.

(6) Some of my colleagues, particularly those of advancing years, see clouds ahead on the score of the number and the caliber of men and women we train annually in our laboratories for service in physiology: Some think we train too many, or at least too many of inferior caliber. Some even propose a control of recruits on the principle of the labor unions and the guilds of the middle ages. I think this would be as wasteful and unfortunate as it is undemocratic. I am not so pessimistic, as our late colleague Meltzer, who said to me a few years ago that we were breeding cretins as the next generation of physiologists. In the first place, none of us can either pick out or train genius. Jefferson Medical College evidently did not think very much of Weir Mitchell when he graduated, or for a couple of decades afterwards, for that matter. But Weir Mitchell carried on. Provided the doors to opportunity are opened by the keys of proved achievements only, we should not worry so much about numbers. Of course, you perceive I am reasoning on the ancient, and formerly biologically sound and acceptable theory that we must create our own opportunities, that we must scratch for our own living. One thing seems fairly clear: The steep curve of establishment of new physiological laboratories in our educational institutions is rapidly reaching a plateau in the United States. But I foresee a marked increase in physiological laboratories and physiological research in industry and in governments. In view of the great extension of the physiologic battle line of to-day and its probable further extension to-morrow, we should probably see to it that our graduate students become as well grounded in chemistry, physics, pathology and medicine as they are, or should be, in the dynamics of the amoeba, the frog, the guinea pig, and the dog.

(7) So you will note I am fairly optimistic about science, and our branch of science, for to-morrow. And I hope that this qualified optimism is not the euphoria of senility. I have serious apprehension on one point only, namely, the danger to science and to the freedom of man in the cyclical psychosis of society. We hear voices saying: "Science is bankrupt," "Science is at the root of our present universal chaos and misery." "We must declare a moratorium on science." "Let us seek escape from the reality of the present in pre-scientific faiths," etc. As I read history, all great achievements in science have come through individual endeavors by relatively free men. In the land of the dictator the man of science becomes either a slave or a court jester. A regimented, a dictated science is science in eclipse. Weir Mitchell lived through the period of our Civil War. That local mental aberration was serious enough. And all its sequelae are not yet healed, after an elapse of more than 70 years. But now the psychosis of salvation through violence is pandemic. The dictator screams: "We think with our blood," and men cheer. New faiths, new formulae, new fetishes, new saviors via the sword, spring up over night from troubled soils, all with the same old clay in their feet, the same old sawdust in their skulls. Science and violence are incompatibles. But may we not gather some courage from the fact that chaos and violence do not seem to be permanent states, even in gaseous matter? Those of us who individually escape the pan-psychosis have to answer the question: Is the life of the termite and the bee worth the while for man? There is some courage in numbers, and to-day we—men of science—are a large company. Were we united it should be easier for us to say *no* to violence, than for Galileo, who stood alone. When pressed nigh onto despair, the offer of "collective security" in exchange for relative personal freedom is tempting, indeed. But look about you, and at the past. Is it not true that when men have bartered personal freedom for collective security, they have lost freedom without gaining a security worth having? Is it not a fact, that cave fishes have some security—but no vision, while the eagle has scant security but enjoys both vision and wings?