THE AMERICAN PHYSIOLOGICAL SOCIETY

Founded in 1887 for the purpose of promoting the increase of physiological knowledge and its utilization.

OFFICERS

President
Ewald E. Selkurt, Indiana Univ. Med. Ctr., Indianapolis, Indiana

President-Elect
William F. Ganong, Univ. of California, San Francisco

Past President
Bodil M. Schmidt-Nielson, Mt. Desert Island Biological Lab., Salsbury, Maine

Council
Ewald E. Selkurt, William F. Ganong, Bodil M. Schmidt-Nielson, Jere Mead, James O. Davis, Walter C. Randall, Francis J. Haddy

Executive Secretary-Treasurer
Orr E. Reynolds, 9650 Rockville Pike, Bethesda, Maryland 20014

Publications

American Journal of Physiology: Cell Physiology
American Journal of Physiology: Endocrinology, Metabolism and Gastrointestinal Physiology
American Journal of Physiology: Heart and Circulatory Physiology
American Journal of Physiology: Regulatory, Integrative and Comparative Physiology
American Journal of Physiology: Renal, Fluid and Electrolyte Physiology
American Journal of Physiology (Consolidated)
Journal of Applied Physiology: Respiratory, Environmental and Exercise Physiology
Journal of Neurophysiology
Physiological Reviews
The Physiologist
Handbooks of Physiology
The Physiology Teacher

THE PHYSIOLOGIST is published bimonthly by the American Physiological Society at 9650 Rockville Pike, Bethesda, Maryland 20014. Address all correspondence to this address.

Subscriptions: Distributed with The Physiology Teacher to members as a part of their membership. Non-members and institutions, $12.00 per year in the United States; Canada, $12.50; Foreign and Postal Union, $13.00. The American Physiological Society assumes no responsibility for the statements and opinions advanced by contributors to THE PHYSIOLOGIST.

The Physiologist

A Publication for Physiologists and Physiology
Orr E. Reynolds, Editor

TABLE OF CONTENTS

Please Note: Society Business and Notices are on gray-edged paper.

INVITATION FOR SUBMISSION OF HISTORICAL ARTICLES

Publication of historical material has been an objective of The Physiologist since its inception and, from time to time, items ranging from short notes to full-length articles have been included.

With the imminence of the American Physiological Society's centennial year in 1987, it seems particularly appropriate to encourage submission of historical items. Such material will be of great value in compiling a special publication on the centennial history of the Society.

The accompanying article on L. J. Henderson by Bruce Dill will, we hope, stimulate other physiologists who have special knowledge of a historical nature to submit articles for publication.

Manuscripts submitted may be of any reasonable length from a few lines to 20 or more typed pages.

All manuscripts will be reviewed. Articles of several pages will be sent to at least two referees to assure accuracy, validity and relevance of content. The most important criterion for acceptability of historical material will be the archival value. The use of illustrations including photographs of people and equipment is encouraged.

Priority for acceptance will be given to items dealing with American physiology and the American Physiological Society. Articles of historical value concerning other parts of the world will also be accepted after favorable review, especially when the pathway of influence on American physiology is traced. If sufficient publishable material is received, a section of the History of Physiology will be a regular feature of each issue of The Physiologist.

L. J. HENDERSON, HIS TRANSITION FROM PHYSICAL CHEMIST TO PHYSIOLOGIST; HIS QUALITIES AS A MAN.

D. B. Dill

Applied Physiology Laboratory, Desert
Research Institute, University of Nevada System
Boulder City, Nevada 89005

INTRODUCTION

L. J. Henderson (LJ) played many parts; my account deals chiefly with the part I knew best, that of a physiologist. Baird Hastings, in his biographical memoirs of D. D. Van Slyke, prepared for the National Academy of Science in 1976, recalled that Van Slyke had a framed picture of LJ hanging in his office. In Hasting's opinion these two good friends "advanced physiology most" in their day.

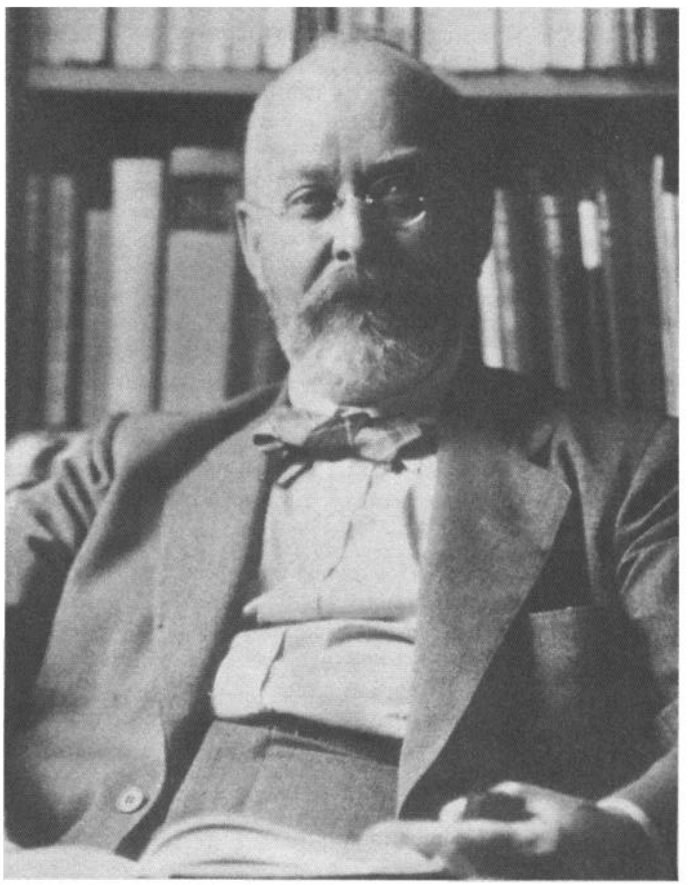
LJ left to the history of science a superlative legacy, his Memories. With the permission of his son, Lawrence J. Henderson (Larry), I borrowed a copy of the 265-page Typescript from Harvard University Archives; Larry has agreed to the selections that follow. Included are LJ's recollections of events as school boy, college and medical school student, post-doctoral student in Europe, and as a young professor, all as they molded his life as a physiologist and developed his qualities as a man. The Memories end with 1925, the year I met him. My account of subsequent events deals chiefly with my own observations and what I have learned from others, especially from his son Larry and his secretary, Hilda Richardson (now Hilda Carter Fletcher) to whom he dictated his Memories during three happy summers, 1937-39 at his summer home on Lake Seymour, Morgan Center, Vermont.

MEMORIES

In LJ's recollections of school days one that was deeply implanted revolved around an algebra problem that he solved by his own method, but his answer was rejected by the teacher because he had not followed the rule the teacher had taught. This aroused a strong feeling of injustice on his part, and convinced him of the incompetence of his teacher but he guessed that the experience helped develop the habit of thinking things out for himself. His teacher of physics, Irving Palmer, was the only one of his teachers who stood out in his memory as not only good, but very good. Palmer, he believed, permanently modified his intellectual life. Perhaps because of Palmer's influence he entered Harvard with a taste for natural science and mathematics, but with no suspicion that he had the ability to do scientific work. He reports surprise at ending his first year at Harvard with "fairly good grades."

The periodic table of Mendeleev delighted LJ. His reflections on it in his sophomore year at Harvard gave him the feeling there were many such undiscovered uniformities; such thoughts led to his later interest in the order of nature and the fitness of the environment that bore fruit 20 years later.

During this year he also spent much time thinking about Gibbs' Phase Rule as "a scientific instrument useful in attaining a generalized description." While an udergraduate he discussed the Phase Rule with one of his teachers and came away with the impression that he understood it better than his teacher did. His thinking about such matters became reveries to which he continued to return persistently. He remarks on his reveries being concerned predominantly with external and intellectual problems, not with personal affairs and emotions.



L. J. Henderson taken in his office in Morgan Hall in the Harvard Business School in the late 1930's.

The development of LJ's linguistic ability began at about age 10 when his father took him to the French colony of Saint Pierre Miquelon. He returned there at least three times in the next six years, living in the home of his father's French business representative. Although there was no formal instruction in French, the several months there greatly widened his outlook. When he was 17 he spent several weeks in a pastor's family near Göttingen. Here as before he had no formal instruction in German and was too shy to try to speak the language. But this shyness disappeared so that after graduation he had no inhibitions in his attempts to speak the languages. Later he remarks that as soon as he could speak the most rudimentary French or German he talked in these languages to himself. This practice played a role in his becoming at home with these languages including the ability to think in them. In the same way he frequently found himself delivering a lecture (without forming any sounds) on some topic of current interest such as the physiological aspects of osmotic pressure or the equilibrium of acids and bases in the body.

At age eighteen he had his first experience as a teacher. His younger brother, Harry, had lost a year because of sickness; LJ

resolved to prepare him for the college board examination. They equipped a primitive laboratory and Harry and a schoolmate were LJ's first students. Instruction included some theories new to him. He thought this was one of the most useful eipsodes in his education; what he learned in that way he thoroughly understood and never forgot.

In the summer after his Junior year LJ wrote an essay for the Bowdoin prize on the Arrhenius theory of electrolytic dissociation. This required reading the original paper in German. This experience taught him that there may be much difference between what one reads in a textbook and what the investigator actually reported. In another instructive experience at this time, he and a friend departed from instruction in the laboratory manual for determining the boiling point elevation by single salts. They combined two salts, sodium chloride and cadmium sulfate; to their delight the boiling point did not rise as much as with sodium chloride alone. The discovery led the professor, T. W. Richards, to allow them to depart from the usual laboratory course and to follow this lead opened by their chance discovery. This experience obliged them to look at the then primitive theory of ionization from a fresh point of view, and with the clear understanding that what they could find in books did not explain the facts they were observing. He remarks that this led him to pay more and more attention to the problem of the interaction of many variables.

His decision to apply for admission to the Harvard Medical School came about because of a feeling he would like to study biological chemistry. This was taught at that time chiefly in medical schools. Further, since there were few opportunities in America in that day to work at biological chemistry, it seemed "a reasonable precaution to go through Medical School." At this time, 1898, he "felt something like ardent curiosity about literature and music and about human behavior." He formed a life-long friendship with the poet E. A. Robinson, an accomplished observer of human behavior.

LJ entered Medical School in 1898 and for the first few months carried out required assignments. As time went on he paid less and less attention to classwork, one year attending lectures in chemistry in Harvard College. Before Xmas he moved to a room in Divinity Hall, Cambridge, and for the following two years shared a room with the landscape architect, Henry Hubbard in Stoughton Hall. In those three years he formed many lasting friendships with graduate students and instructors of the Faculty of Arts and Sciences. He also achieved "a clear understanding of some of the functions of a Univeristy" that he could hardly have learned about in Medical School. He did a great deal of thinking during those four years – not about imposed tasks but about topics that provided an escape from those tasks. Favorite topics for thought were application of the theory of solutions to physiology, acid-base equilibrium in the body and thermochemistry.

He graduated in 1902 cum laude – a surprise to him, to his classmates, and I dare say to his mother. She once told me in LJ's presence that her other boys gave her no worries, always got good grades, but Lawrence worried her. "He wouldn't study until time for an examination when he would stay up all night in preparation." To some extent he confirms this, he had "acquired skill in writing examination papers, which, I think, by this time was exceptionally great."

What LJ learned about clinical medicine was from books; his inability to become interested in patients he came to regret in later years. Within a few years he came to realize that the "mastery of clinical medicine is one of the most satisfactory and interesting achievements that any man can have." In retrospect he mentioned only two departments, anatomy and pathology. Anatomy was difficult because his visual memory was poor, but he got a good grade and appreciated the feeling of mastering a field with traditions and continuity back to the 17th century. He recalled from his course in pathology a discussion by George Magrath of the vicious circle of events in chronic nephritis. This was fixed in his memory because for the first time he saw time as a variable in the interaction of many other variables. His memories of clinical medicine, pathology and anatomy are in contrast to lack of mention of courses in biological chemistry (called medical chemistry at that time) and physiology.

Until 1902 his scientific thinking was spontaneous, not directed to improving his prospects for a career as a biochemist. It was not until two years later when he returned from Strasbourg to become instructor in biochemistry at the Harvard Medical School that he gave up thermochemical work for acid-base study, a problem related to physiology and medicine. He remarked on his indisposition to master methods, even to learn the rudiments of auscultation and percussion. This thought was inborn –– he was less disposed than most boys to do things with his hands. As time went on LJ gave up experimenting for he had skillful collaborators who were happy to provide him with data for study and interpretation. He recalled that during his four years in medical school he was thinking about scientific problems and trying to understand the more general aspects of science and to explore the bearings of modern chemistry upon physiology and medicine. This mental activity was at the expense of his routine assignments in the medical school and created a feeling of guilt.

In July, 1902, just before he left for Strasbourg for 4 semesters of work, he gave a report to his father, a sort of self-assessment. He believed he was able to do as well as the average of his professors. He recalls that he "finally realized that Godlike intelligence" is a rare quality.

At that time study in Europe was the thing to do for one interested in biological chemistry. Germany was then ahead of all other countries in most fields of chemistry. Hofmeister, biological chemist at Strasbourg was a leader in his field. When LJ expressed a wish to work on some problem involving blood. Hofmeister put him to work on the products of partial hydrolysis of hemoglobin. Nothing came of his two years' work. During the two years he remarked, "I was happy –– and never seriously disturbed at the failure of the investigation." The sources of his happiness were many: friendships among outstanding biochemists, Hofmeister's excellent lectures and work going on in other departments of the University. Further, he spent evenings studying systematically data on heats of combustion and in his second year wrote a paper on the topic, and submitted it to the Zeitschrift für physikalische Chemie. He was disappointed when Ostwald the editor turned it down, with the comment, nothing essentially new, but LJ was clear in his own mind that it was new; he took pleasure in it and that was important.

Apart from his scientific interests LJ gave increasing attention to politics. Within three months he became a strong

Francophile. He wrote to his mother in 1904 expressing the thought that a war involving Germany and England was not far away and that he hoped that US would join England. He indulged his "passion for travel," walking and cycling in the Vosges, the Black Forest and Switzerland, canoeing in England and making tours in the Alps and Italy. Looking back, he thinks those two years made him a European; he acquired a double personality, American at home, European when abroad. In those two years he gained greatly "in understanding, in knowledge, in judgement, in appreciation of the values of life, in tolerance, in taste." He commented at length on his experience in writing. He found it difficult to begin but once through the first draft he became "extremely industrious." Revision begins with interpolation ranging from phrases to whole sections. The time required increased as interruptions increased; he could do twice as much work in a month at his camp in Vermont as in a month at Cambridge.

LJ's first year after his return in 1904 was devoted to thermochemical studies with T. W. Richards and the preparation of lectures on biological chemistry (Chemistry 15) he was to present in the second semester at Harvard College. He could not bring himself to acquire an encyclopedic knowledge of biological chemistry. He concluded he would be more useful to students by "expressing opinions or explaining ideas that have resulted from my practice of doing as I please..." In his progress in understanding the general characteristics of science the topics that received much of his attention included "the history of science, the common features of the different sciences, the influence of one science on another, how to apply physical chemistry to physiology..." Specific questions included, "the history of our knowledge of osmotic pressure, its importance in physics, in chemistry and in physiology, the beginnings of electro-physiology, the divergence and convergence of organic chemistry and biological chemistry, the bearing of evolutionary theory on physiology, the physiological aspects of adaptation..."

When LJ returned to Cambridge in 1904, age 26 with about $1200, he was unconcerned about his future. During the year his former teacher of medical chemistry, Professor Wood, died. After much consideration it was decided to make him and Carl L. Alsberg (who had been assisting Wood for two years) responsible for modernizing biochemical instruction in the medical school. LJ continued with Chemistry 15 and was given a University appointment as instructor. He gave up his thermochemical studies with Richards and turned to the problem of acid-base regulation, a problem that had been in his mind since a first-year medical student. After some months he gained some understanding of the relation of a weak acid in the presence of a weak base. He went to two foremost physical chemists for help, Theodore Richards and G. N. Lewis but this topic was foreign to them. So he went on alone and he "worked out the equation $(H^+) = K' \frac{(HA)}{(BA)}$ which in its logarithmic form has come to be called the Henderson-Hasselbalch equation." He achieved this success by "turning things over and over in my mind, with the accompanying reveries." This achievement gave him great satisfaction: he had a field of his own. Shortly a student working under his direction demonstrated an increased production of lactic acid by the lactic acid bacillus in the presence of a phosphate buffer. At this time Folin was a biochemist at the McLean Hospital and was becoming well known by his papers on urine analysis published in the Journal of Biological Chemistry. Folin was present when LJ presented some of his ideas on acid-base relations at an evening meeting at the medical school. Folin discussed the paper and said that, "all this is very important, if true." This remark left a deep imprint; LJ recalled "my mixture of annoyance and amusement at his cautious, old-fashioned chemical scepticism."

There followed a productive period with studies of carbonic acid replacing his interest in phosphoric acid and of the role of carbonic acid-carbon dioxide in heterogeneous systems. Acidosis was then a topic considered by chemists. He found himself faced with problems more biological than chemical. At this stage he came to feel, "a physiologist rather than a biochemist." Thus, it came about that he presented his theory of acid-base regulation at the International Physiological Congress at Heidelberg in the summer of 1907. Bayliss was present and subsequently in his General Physiology included excellent summaries of LJ's acid-base equilibrium theory and also his discussion of the physiology of water as presented in The Fitness of the Environment. Bayliss' book eventually became recommended reading for students of Chemistry 15. In fact these lectures were in the field that became known as general physiology.

Up to 1907, no one besides Richards, Alsberg and one or two others at Harvard and T. B. Osborne at Yale had shown serious interest in his research. Osborne, a brilliant biochemist, was an authority on proteins and was the first to appreciate the significance of LJ's research to protein chemistry. They became life-long friends. After the Physiological Congress in 1907, he went for a few days to Strasbourg where he had a long talk with Spiro, editor of Ergebnisse der Physiologie. Spiro was greatly interested in LJ's theory and asked him to prepare a comprehensive account of it. This LJ agreed to do. He was busy with it for the next 12 months except for teaching. This became a classic of which LJ himself was inclined to boast after 30 years (15). The paper was a decisive step into the field of physiology. He had no doubt that it would give birth to "a great deal of work very fruitful for physiology and medicine, if not for biological chemistry." Later in his Memories he remarked that the results of his work were intensely interesting to him partly because "they were transforming him into a biologist."

About 1908 LJ began attending Josiah Royce's seminar on logic. The discussions led by Royce were entertaining. LJ felt the seminar was repairing some defects in his education and providing raw material that might prove useful. Further, the seminar gave him a better understanding of the history of science.

It was during this period that Bohr, Hasselbalch and Krogh announced the discovery of the influence of carbon dioxide pressure on the affinity of hemoglobin for oxygen. It was several years before Christiansen, Douglas and Haldane announced their discovery that oxygenation of blood affects the equilibrium between carbon dioxide and blood. As LJ comments, this had to be true: if x depends on y then y must depend on x. But the application of this simple principle had escaped all the eminent physiologists of the day. This was mildly frustrating to LJ who reported "a slightly amused consciousness of my stupidity."

In the summer of 1908 he spent a month in Strasbourg with Spiro finishing the Ergebnisse paper. Between them they

carried out a study of the movement of chloride between red cells and plasma. A theoretical explanation was formulated based on the theory of acid-base equilibrium and osmotic pressure; a few simple experiments cleared up the problem.

In the same summer Folin was appointed to the new endowed chair of biological chemistry in the medical school. This will be discussed later on the basis of Alsberg's correspondence with President Eliot. Alsberg left but LJ stayed on for three years. All his teaching at the Medical School became irksome and in his view an intolerable waste of time. Years later he realized that the appointment of Folin over the heads of Alsberg and him, "was one of the luckiest things that every happened to me..." He had an opportunity on a streetcar to discuss his situation with President Lowell who responded favorably to the suggestion that LJ organize a course in the college on the History of Science. His acquaintance with Lowell antedated Lowell's inauguration and became a fast friendship. They sometimes walked together the four miles between Cambridge and the Medical School to attend faculty meetings.

LJ's interest in the history of science dated back to undergraduate days when he had attended lectures by Richards on the history of chemistry. The factors that gave him confidence in this new undertaking were: the great amount of general reading he had been doing since 1898; books he read on departments of science; his reading and thinking about the method of science; attention he had been giving to elementary mathematics in connection with his research program; his association with Royce and membership in his seminar. His course gave him satisfaction; it influenced his scientific thinking, in fact, his whole point of view, leading to greater satisfaction in his intellectual life.

LJ recalled vividly that in walking down the slopes of Mt. Monadnock on or about February 22, 1912, that, "I was aware of what I had been looking for in thinking about the fitness of the environment." Before that date he had concluded that water and carbon dioxide can be considered primary constituents of the environment. What came to mind on Monadnock was realization that, "fitness is a reciprocal relation; that adaptations must be adaptations to something and that complexity, stability and intensity and diversity of metabolism in organisms could not have resulted through adaptation unless there were some sort of pattern in the properties of the environment. . ." Having reached this stage in his thinking he set about studying the properties of water and of carbonic acid. He set to work assembling the facts and within a few days began writing The Fitness of the Environment. After some interruptions including a death in the family and his lectures he went to work on the manuscript, "under the influence of excitement and enthusiasm." He completed his writing in the summer at Seal Harbor, Maine, arising about nine o'clock. After a leisurely breakfast, a stroll, a leisurely luncheon, a short nap, another walk he returned to his manuscript until about midnight with an unwelcome interruption for dinner. In retrospect he thinks the quiet hours were in fact filled with reveries that bore fruit once he started his day's writing. He estimates that the total time spent was from 50 to 60 days.

Not long after the publication of The Fitness of the Environment (14), LJ game a popular Sunday lecture on Water at the Medical School. The topic was clearly in mind: he spoke without notes. A stenographer took down his remarks for the Harvard Bulletin. The resulting manuscript with only minor corrections seemed to him to be, "better in style than anything I had ever written." There is no need here to comment on this remarkable book.

With the book behind him he remarked that he no longer "felt guilty in doing things that are not clearly the conventional business of a biological chemist or physiologist." One sequel that pleased him was that Bayliss in his General Physiology gave much attention to LJ's treatment of Fitness, including a small chapter on the physiology of water.

A study of Gibbs' contributions to physical chemistry led LJ to pose the question: "What is the fitness of the primary constituents of the environment as components and as environment of physicochemical systems in general?" His thinking about this question led to The Order of Nature (15).

LJ had become well acquainted with Royce and often consulted him when writing The Fitness of the Environment (14). Between them they organized an informal discussion club that met at the Harvard Club about three times a year with LJ acting as secretary. After the death of Royce it became known as the Royce Club. Usually there was a paper and always a discussion. This continued for a decade and must have been great fun. It gave him an understanding of what his colleagues thought about the scientific method and of the philosophy of science. Best of all, he became intimately acquainted with the great William Morton Wheeler. Raymond Pearl of Johns Hopkins had written a favorable review of the Fitness of the Environment; soon after LJ became acquainted with Pearl when he came from Maine to deliver a paper to the Royce Club.

An event in 1914 had an important and lasting impact on LJ's life. Walter Palmer had completed his internship at the Massachusetts General Hospital and was appointed to a fellowship there. He sought LJ's advice, expressing a wish to undertake physiological research. This association continued for many years. While LJ thought their joint publications of no fundamental importance, and felt that "the thinking involved in planning this work led me to no significantly new points of view," he had for the first time become familiar with the clinician's point of view. He concluded that his "association with Palmer was the beginning of a change that in the end became great." He soon learned enough about clinical problems so that with the help of clinicians he no longer felt uncomfortable in their company. In fact, with the passage of time ordinary conversation with clinicians was more comfortable than at meetings of biochemists, physiologists, chemists, physicists, zoologists, philosophers and sociologists.

He quotes the only two instances in which his academic freedom was infringed. In the first case an eminent investigator at the Massachusetts General refused to allow his laboratory to be used for a study of correlations between a large number of kidney functions in healthy subjects with those observed in certain renal disorders. The man was forgiven although LJ thought his position reflected overconfidence in his judgment of what work was worthwhile. Years later the Division of Biology vetoed his suggestion that a course in general physiology be established. Apparently they wished to avoid relations with other departments that might follow such action. The fact is that the course in Chemistry 15 that LJ taught for about 40 years came to include much of Bayliss' General Physiology.

Edwin Cohn came to work with LJ in the fall of 1915. He was put to work on acid-base equilibrium in sea water, a move in the direction from simple to complex systems, as was true

of Palmer's program. With U. S. entrance in World War I, Cohn was joined by Wallace Fenn in a study of bread making.

After many pages of memories devoted to the emotions aroused by the war he remarked that he, "had reached the point of diminishing returns in working along the old lines." In early June, 1919, he came to Morgan Center and in late August he remarks that he was as inactive mentally as he had been since an adolescent. Toward the end of August, he began studying the inter-relations in blood between oxygen, carbon dioxide and hemoglobin with Barcroft's (1) The Respiratory Function of the Blood at hand. He had a paper written within two or three weeks and it seemed to him that he had picked up where he had left off five years before, "as if there had been no interval." This marked the first step in a field of work he was to pursue for many years.

Franklin McLean came that fall to study with him for a year, saying he would like to investigate the behavior of chloride in the blood. He accepted LJ's suggestion that he study the influence of oxygen on the movement of chloride between red cells and plasma. By this time LJ had clearly in mind that variation in any one of the variables, oxygen, carbon dioxide, combined oxygen, combined carbon dioxide, red cell volume and chloride distribution could not occur without a variation in all the others. He remarked again that when the Bohr effect was discovered the reciprocal relation reported ten years later by Christiansen, Douglas and Haldane was a necessary corollary. His failure to predict this LJ had described as an instance of intellectual inertia.

McLean went to work in Boylston Hall and was joined by Harry Murray who had decided to become a physiologist. Harry's younger brother Cecil (or Mike) wished to work with LJ also; he told LJ that he had admired and had been greatly influenced by LJ's lectures in Chemistry 15. Franklin and Harry made progress on their problem under difficult conditions and presented their data to LJ for analysis. After many fruitless attacks on the problem he hit upon a solution that completely represented the data. He went for advice to E. B. Wilson then at M.I.T. Wilson remarked that what he had discovered was a nomogram. This was the first time LJ had heard the word: the nomogram played a major role in his work for the next ten years. McLean was concerned about the crudeness of the data but LJ insisted that it was good enough: "accuracy beyond a reasonable factor of safety is a mistake in so far as one is concerned with means to an end."

During the next six years LJ was almost completely absorbed in the research growing out of the McLean-Murray study and his nomographic description of it. He was in Europe from the summer of 1920 until September, 1921, on sabbatical leave. He felt quite at home in England that first summer. He was better known to English physiologists than to American in part because of the attention given to his work in Bayliss' General Physiology. He presented his nomographic description of blood both to the British Physiological Society and to the International Physiological Congress in Paris. The family settled down in Grenoble and young Larry was put in the Lycee. His plans for a quiet year were interrupted when President Lowell asked him to take over the exchange professorship at the Sorbonne for the spring semester. This kept him fully engaged; giving one course centered around fitness of the environment, the other on physical chemistry of the blood. At the same time plans for further studies were ripening in his mind.

In 1920, Roger Lee had introduced Arlie Bock to LJ and in that summer he saw something of him and Alfred Redfield in Barcroft's laboratory at Cambridge. A Peruvian high altitude study was being planned and he was able to provide some support from the DeLamar Fund. This began his close association with Arlie Bock that endured to the last.

The DeLamar Fund also was used to set up the Laboratory of Physical Chemistry on the floor above Cannon's Physiology Laboratory in the Harvard Medical School. This was done in 1921 with Edwin Cohn in charge. Later Ronald Ferry worked there. "I have had little to do with it . . ., my supervision was less close than I had expected it would be and, as my interest in the work waned, my relations became steadily less and finally ceased altogether." In explaining this development he remarked on his "tendency to go on with what interests me regardless of questions of expediency." In retrospect he stated that his three accomplishments that ranked first in his judgment were the theory of acid-base equilibrium, the theory of fitness of the environment and the synthetic treatment of blood as a physicochemical system.

During that period he read several books on nomograms and had several conversations with d'Ocagne out of which he decided to adapt that d'Ocagne nomogram as a general means of describing interrelations of components of blood. His third major field of research required collaboration with skilled investigators. He paid tribute to Arlie Bock who returned in 1922 from Peru to take charge of a small laboratory for clinical studies at the Massachusetts General Hospital. He characterized Arlie as "invaluable because he is ideally qualified by industry, initiative, enthusiasm, and immense power of work to do what had to be done." He then commented on the contributions of Van Slyke who "had long before become interested in the acid-base equilibrium and had done more than anyone else experimentally to develop the subject by his experimental researches. So we had come together and were already very intimately acquainted. At this point, we agreed that two laboratories were better than one for the present purposes and that we should both work on the problem. The first result of this was the important paper by Van Slyke, Wu, and McLean (30) reporting studies made in China while Van Slyke was there for a year." Their source of blood was a Manchurian pony.

LJ's only mention of the Silliman lectures in his Memories is his estimate that following the first application of the nomographic method between Van Slyke's group and his, 25 to 50 man-years were involved in the research on which his lectures were based. He dictated about 1500 words on July 19, 1939, and finished with about 500 words the next day. He decided the many papers published in the 1920's and his book Blood, a Study in General Physiology published in 1928 were record enough for that period. He decided to end the dictation, "with a feeling that it had better be done when I feel strongly curious about what I am doing."

DAYS WITH LJ AT MORGAN CENTER, VERMONT

My introduction to Morgan Center came during my first winter in Boston, 1925-26 when I visited LJ in his study on the third floor of 4 Willard Street not far from Harvard Square. On some of those evenings I was taught the techniques of constructing a nomogram using properties of blood. Arlie Bock was teaching me to acquire techniques for blood gas analyses at his laboratory in the Massachusetts General Hospital. One evening in early Spring I found LJ making plans for construction of another cabin at Morgan Center. The plan included detailed layout of the four walls of a one-room cabin. The 2 X 4's that formed the structure were arranged to form

niches for flowers, for books and an appearance that was home-like not barn-like. It may have been the same evening that he told me of his need for transportation at Morgan Center and proposed that I establish my family in a farmhouse he owned across the road from his camp. Olive and I and our two children, Betty age 10 and David age 7, at once accepted and began making plans for departure early in June. Some of Betty's recollections of Morgan Center have been recorded by Horvath and Horvath (22).

When we reached Morgan Center we made ourselves at home in a sparsely furnished farmhouse with a view of Lake Seymour and surrounded by a few acres of meadow with so many boulders that the hay was cut with scythes. There were woods above and across the gravelled road below which a path led through the woods to the Henderson cabins and beach. A short distance south the road crossed a brook fed by springs in the woods. A galvanized 3/4 inch pipe carried a never ending supply of water from a pool in the brook above the road down to a sink in the main cabin. One-half mile further was a superior (as LJ would say) farm family, Claud and Maybelle Peavey immortalized by Bernard de Voto in New England. There She Stands. (9).

Another half-mile beyond lived Charlie Burroughs. He and his wife and son were all good friends of LJ's. A principal source of income was maple syrup "boiled down" in a syrup house in the maple grove (sugar bush) using as fuel wood cut and hauled to the syrup house the previous fall. During the two summers at Morgan Center I came to know these families well and soon learned that they and LJ shared feelings of deep affection. He admired their Yankee thrift and independence, their ability to live comfortably on a cash income of a few hundred dollars a year. These families recognized no class barriers. Aside from always addressing LJ as "Professor," they spoke to him as man to man. I guess when he became acquainted with them about ten years before when he acquired the Morgan Center property, he discovered their character and came to appreciate their friendship. He occasionally expressed to me his regard for some of them. The Charlie Burroughs family were favorites; he once remarked that Mrs. Charlie Burroughs was queenly; I think he had in mind both posture and her behavior. Ten miles along this road was a railroad division town, Island Pond, to which we sometimes took the Henderson family. On such an occasion I visited the barber shop and learned from the barber that Larry had been there earlier. The barber was still angry. It seems that he had asked Larry if he didn't think Island Pond was a great little town. "Not at all," Larry replied. "On the contrary, I consider it a most inferior town."

In the other direction our road led to Morgan Center, one-half mile, with a post office in the only store. I became acquainted with the proprietor, Syl Blake, and learned much from him about ways to recover from the dietary deficiencies of winter. In June he was anxiously awaiting growth of sorrel so he could find enough for his wife to make a sorrel pie. "In the springtime," he said, "a sorrel pie is good for liver, kidneys, stomach, bowels." A mile beyond Morgan Center was the town of Morgan where there was a superior general store; it was about 10 miles further to Newport on the Canadian border and at the end of Lake Memphremagog. We frequently took the Hendersons to Newport where they did most of their shopping. A side road led to another crossing at Derby Line. We made some excursions from Derby Line a few miles to a town in Canada where a Quebec Liquor Commission store stocked excellent French wines that LJ required for special occasions.

Leon Burroughs, brother of Charlie, lived in Morgan Center. He made furniture of simple design that he sold to tourists and was the craftsman who built LJ's cabins. The original cabin had been followed prior to 1926 by a smaller cabin where guests slept and could wash hands and face and shave with cold water. They had a kerosene lamp and a lantern to light their way to and from the main cabin. That first summer Dr. Van Caulaert who had spent the year at the MGH in Arlie Bock's Laboratory, and his wife were invited to spend a few days with the Hendersons. LJ and I met them at Burlington: the last lap of their cross-country trip was by boat across Lake Champlain. At the dock they were less excited about finding us there to meet them than they were about the 98-year old skipper who had been captain of the ferry boat since he was mustered out at the end of the Civil War. The next day LJ told with hilarity how Mrs. Van Caulaert had come back to the main cabin soon after they had left with the lantern. "We find blankets," she said, "but we cannot find the sheets." "We don't use sheets," LJ explained. This may have happened before, but this occasion was implanted – I heard LJ tell the story more than once to newcomers in hilarious tones.

Within a few days after our arrival, Leon Burroughs was at work building another cabin following the plans I had seen LJ drawing up on coordinate paper during the Spring. This cabin had a fireplace, attractive red curtains and niches formed by the 2 x 4's for flowers and leaves. It became the living quarters for Mrs. Henderson. Edith Thayer Henderson was a member of an eminent family and a charming person, less enthusiastic perhaps than LJ about the community and the people. Years later when she was seriously afflicted with mental illness LJ told me he believed her illness antedated 1926 and that I had never known her in full mental health.

Larry, their only child, born in June, 1911, spent his days fishing, swimming, roaming the woods and visiting with the neighbors. He had a 22-gauge rifle and had a standing offer from his father of 25 cents per crow. I guess his father was confident that his offer would not cost him much nor upset the balance of nature.

The two summers blend together after 50 years. I recall that another cabin, the last, was built in 1927 by Leon Burroughs under LJ's watchful eyes with the coordinate paper in hand. He improved the style and conveniences with each cabin. The last one had drawers for storage under the built-in bunk with curtained windows looking out through the woods to the lake. LJ the planner and Leon the craftsman took great pride in their achievement.

There were not many visitors in those summers. On one occasion I took LJ to Lake Willoughby to bring his mother to the camp for a visit. She was a grand lady then about 80 years old and in good spirits and in good health except for poor hearing. She sat beside me with LJ behind and had fun reminiscing about Lawrence. She said her other two boys gave her no worries but Lawrence studied what he chose and neglected some subjects until the night before the final – "But he always passed" she added. LJ himself has related in his Memories his lack of interest in laboratory instruments and techniques and his mother gave evidence that this was demonstrated in his youth. "I am pleased that Lawrence never learned to drive," she said; "when the boys were young all had bicycles but not only was Lawrence's often broken, but sometimes he had accidents with one of the other boy's

bicycles." LJ didn't protest his mother's stories of his youth and we didn't discuss the incident later.

Another visitor was Mrs. Henderson's niece Elinor Gregory – a brilliant and charming woman who later became librarian of the select private library in Boston, the Atheneum. I became well acquainted with her, saw her occasionally in the next few years and then not until 1975 when I had the pleasure of reminiscing with her and her husband, Keyes Metcalf, famous and venerable librarian in their Belmont home near Boston.

Fig. 1. Reveries at Morgan Center. Seated in a chair made by Leon Burroughs, LJ could enjoy the fire or look through broad windows to wooded Vermont hills across Lake Seymour.

In my clearest memory of LJ during those summers he was sitting before the fireplace often reading French novels, sometimes smoking his pipe and as his Memories reveal, in reverie. It was here it seems to me that he spent many of his happiest hours and evolved some of his great conceptual schemes.

Mention of the fireplace brings to mind the first lesson I learned about that New England institution. In California in the few homes of that period with fireplaces orderly housewives cleared away all ashes every morning. An expensive home had a fireplace with a metal trap door through which ashes could be removed daily. One chilly morning early in my first summer at Morgan Center I undertook to build a fire. LJ was shocked when I asked for a match to ignite a pile of paper and kindling. I felt an ignorant Californian when he explained the coals buried in the six inches of ashes should be used to start the fire. I soon learned that a deep bed of ashes was as essential to the operation of a fireplace as a properly constructed flue.

Another enjoyable feature of their life was their meals. One ritual included preparation of Irish oatmeal for breakfast. The proper mixture of oatmeal, water and salt was cooked in a double boiler for a half-hour in the evening and heated in the morning by the maid (who taught during the school year) and was enriched with maple syrup and heavy cream. "It sticks to your ribs" LJ would say. The cans of oatmeal were imported from Drogheda, Ireland by S.S. Pierce in Boston and were purchased before departure together with coffee and many other first quality staples. I was able to add a local product to their menu. Betty and David helped me pick red raspberries that in mid-summer bore abundantly in the meadow near the woods. Cooking the crushed berries with the proper proportions of sugar and pectin yielded preserves that we shared with our hosts. I can recommend red raspberry preserves on toast as an excellent cholesterol-free breakfast.

I have sought further details of domestic affairs at the camp from Hilda Richardson Fletcher who, beginning in 1934, spent several summers there as LJ's secretary. During that period Robert E. Johnson (Bob) and his wife Margaret and later, Lucien Brouha and wife Elizabeth, were there as resident couples. Hilda recalls that there was no ice nor refrigerator. Perishable food was kept in a "cooler," known as a dumb waiter, below ground level. Meat was bought in large cuts. When a huge pot of lamb stew, including appropriate vegetables, had been prepared it was served for dinner each evening until consumed; LJ claimed it improved every day. Another favorite meat dish was Vermont smoked ham – this too would be served every day. There was no dessert: Hilda took with her a box of almond crunch which served for dessert; LJ liked it, calling it Hilda's specialté. This period followed LJ's hemorrhage from a gastric ulcer in 1931, so wine was not served except for special guests. On such an occasion, Elton

Mayo returned to Harvard from England by way of steamer to a port on the St. Lawrence, where he was picked up and brought to Morgan Center. A bottle of burgundy was opened in his honor. It is Hilda's recollection that guests were infrequent; most of LJ's summers at Morgan Center included many days with hours for reading and reveries. Hilda's recollection of Elinor Gregory's visits agree with mine: LJ had a warm feeling for his niece, Edith Henderson's sister's daughter.

I recall three visits to the camp after 1927. In 1929 my doctorate professor at Stanford, Carl L. Alsberg, visited Boston; I took him and a visiting physiologist from Oslo Asbjörn Fölling to visit LJ. Fölling had been a research fellow in Bock's laboratory in 1926 and returned in 1929 to join our high altitude party in Colorado and later to attend the International Physiological Congress in Boston. Alsberg and LJ reminisced about days at Strasbourg and their experiences teaching biochemistry together at the Harvard Medical School. I am fortunate in having taken a good picture of the three of them. (Fig 2).

Ferry, Baird Hastings and myself. We assembled for lunch at a golf club on the Canadian side of the border and had a great day after that at the camp. I recall that A. V. Hill was an enthusiastic early morning jogger. During the day he and I rowed across the lake; he was my passenger when members of the party were transported to Boston the following day. Alex Forbes, one of LJ's early students, came in his hydroplane: the lake was rarely disturbed by power boats then, so he could land and take off safely. I don't recall what help LJ had in the kitchen, but it must have been expert – there were no breakdowns.

My last visit was in 1938 when men in the Fatigue Laboratory were invited up for a weekend. Included were Sid Robinson, Bob Johnson, Frank Consolazio, Ross McFarland, Lucien Brouha, Ben Jones, Yule Bogue, research fellow from England, and Aste-Salazar, research fellow from Peru. Horvath and Horvath have a picture of the group. (22). That picture brings back feelings of respect, admiration and affection for LJ that we all shared.

Fig. 2. On the porch of the cabin at Morgan Center, Vermont, 1929. LJ, right, and Carl Alsberg, center, reminisced about their experiences reshaping biochemistry at the Harvard Medical School, 1905-1907. Alsberg then was Director of the Food Research Institute at Stanford. Fölling was a research fellow from Norway. This picture appeared in my article "The Harvard Fatigue Laboratory" in Circulation Research, Supplement 1, Vols. XX and XXI, March, 1967. It is reproduced by permission of the American Heart Association, Inc.

LJ had a major role in planning the Harvard Tercentenary celebration in 1936. The party he arranged at Morgan Center demonstrated great skill in planning, logistics, in feeding and sheltering a large group. Included were Prof. and Mrs. J. Barcroft, Prof. and Mrs. F. G. Hopkins, Prof. and Mrs. E. D. Adrian and Prof. A. V. Hill, all of whom were met when their boat docked at Quebec. For this and the trip to Boston LJ called on Arlie Bock, Alfred Redfield, Edwin Cohn, Ronald

ATTEMPT TO RE-SHAPE BIOCHEMISTRY AT THE HARVARD MEDICAL SCHOOL: JOINT UNDERTAKING WITH CARL L. ALSBERG

The biography of Carl L. Alsberg Scientist at Large (8) recounts many details of his experiences as a young faculty member at the Harvard Medical School. He became an M.D. at the College of Physicians and Surgeons, Columbia University,

at twenty-three in 1899, and left for Germany to do biochemical research with Schmiedeberg at Strasbourg and with Fischer in Berlin; both came to value him highly. In 1902, Alsberg returned to become assistant in the Department of Physiological and Pathological Chemistry at the Harvard Medical School with an initial annual salary of $400. In 1904 he was appointed instructor in biological chemistry; by 1907 his salary was $1,600.

During his first years at Harvard he became acquainted with Otto Folin who was then conducting research in the laboratory of the McLean Hospital, near Boston. In 1905 they were joint authors of a paper showing that contrary to a published report, the cystine of kidney stones was identical with the cystine obtained from protein hydrolysate.

Following the death of Edward S. Woods, department chairman in the summer of 1905, LJ who had returned from Strasbourg in 1904, was appointed a lecturer in the medical school and for the following three years he and Alsberg undertook with some success to bring new concepts into the teaching of biochemistry. They introduced the term Biological Chemistry and wrote a manual, Laboratory Practices in Biological Chemistry for the use of the First Year Course in The Harvard Medical School. For help in the laboratory they invited Otto Folin, who had published papers on blood and urine analysis, to help with instruction in analytical procedures. Folin soon became highly regarded by students and faculty; in 1907 he was appointed Associate Professor of Biological Chemistry. The next year he was appointed to the Hamilton Kuhn chair of biochemistry.

The appointment of Folin was a bitter disappointment both for LJ and for Alsberg. It brought to an end their dreams, for Folin had concepts of instruction in biochemistry very different from those of his two instructors. LJ put up with it for three years as recorded above, but Alsberg had a spirited, but fruitless exchange of letters with President Eliot. Previously, Alsberg had refused an offer from Columbia because of encouragement from President Eliot, then nearing the end of his long incumbency. In August, 1908, Alsberg wrote Eliot of an offer to take charge of the pharmacological work of the Bureau of Plant Industry in Washington; he wrote Eliot that this field and its opportunities strongly attracted him with the prospect of research assistance and freedom from administrative duties. He added that he had "no special desire to leave Harvard." To this phrase Eliot reacted vigorously: "The Corporation does not desire to retain in its service any man who does not distinctly prefer their service to any other which is open to him." Alsberg left shortly for Washington.

While their dreams didn't become reality at the time, years later, 1936, they were realized when Baird Hastings, a Van Slyke protégé greatly admired by LJ, was appointed to succeed Folin. In a recent letter Hastings writes that soon after his appointment he invited LJ over for a visit. LJ was pleased, on arriving he remarked, "This is the first time I have been in this department since 1911."

ARLIE BOCK'S LABORATORY AND THE HARVARD FATIGUE LABORATORY

The Harvard Fatigue Laboratory was an offspring of Arlie Bock's laboratory; after the latter was born, parent and offspring maintained close ties. In my first meeting with LJ in September, 1925, he suggested my joining Arlie Bock rather than Edwin Cohn. As recounted above, LJ's interest had shifted away from studies of the physical chemistry of isolated proteins, then Cohn's field.

Arlie had created an excellent laboratory. In ten years after graduation from the Harvard Medical School, Arlie had been a resident physician at the MGH, had served with the Harvard Hospital in France and had been a Moseley traveling fellow with Barcroft in Cambridge. There he learned how to derive oxygen and carbon dioxide dissociation curves of blood. LJ became better acquainted with him on a visit to Cambridge, learned Barcroft was enthusiastic about Arlie's talents and personality. LJ endorsed Barcroft's plan to include Arlie in his projected high altitude study; as revealed above, LJ provided some support from the DeLamar fund. When Arlie returned to Boston from Peru in 1922, he was assigned two rooms, each about 12' x 14' in the Bullfinch building of the MGH. Three years later, they were well equipped, one room for gas analysis and blood equilibriation, the other crowded with two gasometers, 120 liters and 500 liters, a hospital bed, a bicycle ergometer and the accessory equipment for respiratory and metabolic measurements. Arlie was moving up in his field of clinical investigation. In 1924 he had been appointed assistant professor; had published papers on carbon dioxide dissociation curves and gas equilibrium in the lungs and had provided data for LJ's Blood as a Physicochemical System II, of which Arlie was a joint author (16). The latter paper appeared in 1925; I had to digest it and become familiar with the Haldane, Van Slyke and all of the procedures used in the laboratory. At that time the first visiting scientist, G. S. Adair, had come and gone; during Arlie's year in Cambridge he had made Adair's acquaintance. Two others arrived in 1925, Van Caulaert from France, and Fölling from Norway; they have been mentioned in other connections. We all joined in a series of Studies in Muscular Exercise, the first four of which were published together in the Journal of Physiology (London) in 1928 (5). Marathoner Clarence DeMar, already a celebrated character in Boston, came as a subject both in running around the little park in front of the building and as a subject on the bicycle ergometer. Studies were carried out in those years under Bock's direction in which LJ was keenly interested. He spent much of his time prior to 1928 writing five papers in the Blood series and his book, Blood, A Study in General Physiology (17). Arlie had capable interns and residents during that period; Louis Hurxthal, John S. Lawrence and Tom Coolidge; John H. Talbott, who was to move to the Fatigue Laboratory stretched out years in the Medical School to five to work in Arlie's laboratory in 1926-27 and later in the Fatigue Laboratory.

It was LJ's belief that achievement of great accuracy could be a waste of time. However, Van Slyke, and collaborators had achieved great accuracy in blood gas analysis; we strove to emulate them. Thereby hangs a tale. In Blood II (16), the ratio of anion concentration in red cells to that in plasma was designated r. This principle, derived from the Gibbs-Donnan theory implied that $(Cl)_c/(Cl)_s = (HCO_3)_c/(HCO_3)_s = r$. In Blood IV (16) chloride concentrations determined in red cells and in plasma proved that r_{Cl} was significantly higher than r_{theory} so a line for each was shown in the nomograms of that paper. Blood V dealt with properties of blood in rest and exercise; the ratio for r_{HCO_3} was reported to be greater than r_{Cl}. Confidence in this finding was strengthened by a similar finding for horse blood by Van Slyke, Hastings, Murray and Sendroy (29). LJ discussed these discrepancies from theory in Blood p.109 (17). He suggested that "the most important unknown factor in this equilibrium may be the activity coefficients of the two ions in the two phases." Ferguson and

Roughton found the explanation in 1934 (12) the reversible combination of some CO_2 with hemoglobin as a carbamino compound.

One incident in that period was notable: the visit of A. V. Hill. He had been a runner in his college days and one of his areas of research at that time was the biochemical events in muscles during strenuous exercise. His paper with Long and Lupton (21) published in 1924 had drawn our attention. Arlie and I were delighted when he asked us to write a third edition of what was then the authoritative book in its field, Bainbridge's The Physiology of Muscular Exercise. We undertook the task, the third edition appeared in 1931.

have an office and secretary. Early in his thinking, he consulted his friend, Wallace Donham, dean of the Harvard Business School and the recently appointed brilliant professor of Industrial Research, Elton Mayo. LJ saw the possibility of a bond between Mayo and his associates concerned with sociological aspects of business management and his own interest in what he later described as the study of man in his every day life. Donham was enthusiastic about the possibility of Henderson and Mayo joining forces in his school. He agreed to set aside three rooms in Morgan Hall for offices for them and a secretary and a space in the semi-basement for a laboratory.

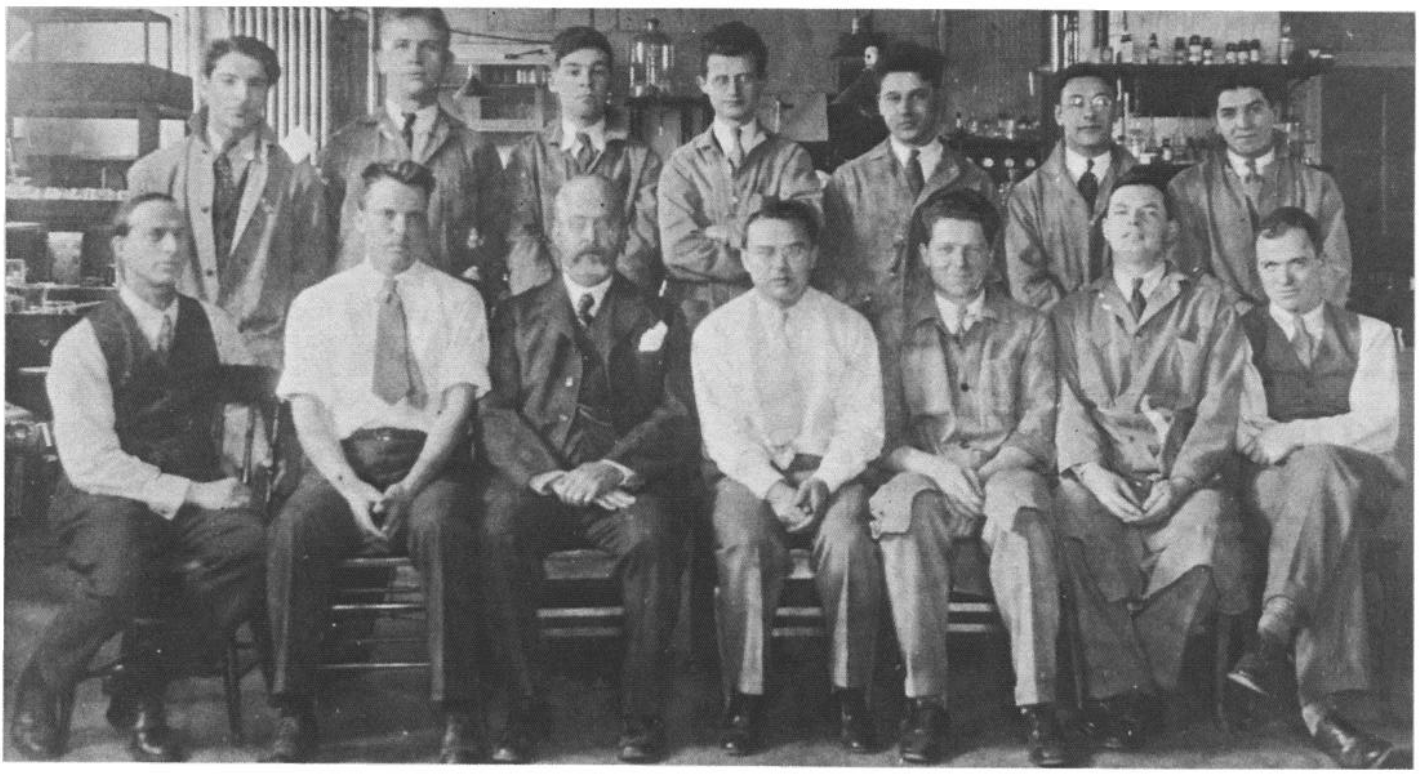


Fig. 3. Fatigue Laboratory group in 1934. Back row, left to right, William V. Consolazio, Blair V. Jager, John Maier, Vincent Morgan, E. J. Levenson, Vincent Gatto, C. Frank Consolazio. Seated, left to right, Frank S. Cotton, D. B. Dill, Professor Henderson, Harold T. Edwards, Cornelius Daly, Ben F. Jones and Will H. Forbes.

Jager, Maier and Levenson were medical students, Gatto was a college student, Cotton was a visiting research fellow from Australia.

LJ remarked in his Memories of how he acquired respect for the practice of medicine and affection for clinical investigators, notably for Arlie Bock. Several other such young investigators were in the same building. Next door was Fuller Albright and nearby were Chester Jones, Walter Baner, Howard Means, and at the end of the hall Paul White. They became giants in their fields.

The concept of the Fatigue Laboratory took form in LJ's mind probably in 1925. My guess is that his aim was to provide a place for his protégés to work – Cecil Murray (Mike), W.O.P. Morgan (WOP) and myself as well as a place for Arlie Bock to expand his experimental program. LJ never had had an office at Harvard except for a nook in Widener Library. He did his calculations and writing in his study on the third floor of his home at 4 Willard Street. It was high time for him to

With space committed LJ and David T. Edsall, dean of the Medical School, submitted a proposal to the Rockefeller Foundation for research support and to the Laura Spellman Rockefeller Foundation for funds to equip the laboratory. The two proposals were approved, $500,000 for ten years, and $25,000 for equipping the laboratory. With these commitments made, LJ and Edsall, who was also dean of the School of Public Health, obtained for me an appointment as Assistant Professor of biochemistry in the School of Public Health. All of these steps were taken with the approval of LJ's close friend, President Lowell. When I returned with my family from Morgan Center in 1927, laboratory construction was under way and before long was completed. The story of the laboratory has been covered in excellent style, I am proud to say, by my son-in-law and daughter, Steve and Betty Horvath.

(22). By way of a supplement to their account, Figure 3 pictures the laboratory staff with LJ, circa 1934.

Our ties to LJ became closer with opportunities for daily consultation. John Talbott benefited by this. He was given a room in bachelor quarters where he became a friend and medical advisor to young members of the Business School faculty. These friendships grew closer with the years. After his residency at the Presbyterian Hospital in New York, he had a major role in laboratory and field studies until 1935 when he was given responsibility for Arlie's laboratory, Arlie having been appointed Professor of Hygiene responsible for student health. W. V. Consolazio (Bill) moved with John to the MGH; they organized an active research program, part of their support coming from Fatigue Laboratory grant. When the war came John was commissioned in the Medical Corps, and Bill in the Navy to help organize the research program at the Naval Medical Research Institute. Further details of Talbott's war-time role have been related by Horvath & Horvath (22). Throughout this period Talbott's relations with LJ grew closer; he has written an account of LJ's career with excerpts from his medical writings (19). The war-time roles of the Fatigue Laboratory in the field of nutrition have been mentioned by Mayer in his biographical sketch of LJ (24).

There are events and conversations in that period that merit recording since they illuminate some facets of LJ's character. He admired beauty. Once he remarked that Quebec was the most priest-ridden country in the world. I was sharply rebuked when I made some comment on the dogmas of Catholicism and on poor people being made poorer to fund construction of cathedrals. His comment, "You have not seen the Basilica of St. Peter," ended the conversation.

There was a related quality, LJ was sensitive to the charm of women, provided it was coupled with intelligence. Edith Henderson had these qualities, she was a witty conversationalist and a linguist of ability. Elinor Gregory, her niece, had similar qualities. After Edith's hospitalization, Margaret Johnson, until her first baby was in prospect, and then Elizabeth Brouha with husbands Bob and Lucien were resident couples appreciated by LJ. In the late 1930's, he depended heavily on his secretary, Hilda Richardson, an attractive and talented young woman. She left in 1940 with her husband for Eau Claire, Wisconsin, but retained memories of LJ. Recently as Hilda Carter Richardson, Director of Public Relations, University of Wisconsin, Eau Claire, she was senior author of a history of the University. Also as part of the requirements for a master's degree in history she is writing an account of LJ's activities as Foreign Secretary of the National Academy of Sciences.

LJ's wit was keen. He once told me of a discussion with Professor Baxter who shared with LJ responsibility for planning a new chemical laboratory for Radcliffe. On one point they disagreed, Baxter urged adopting the Harvard plan – no stools. When LJ held for stools, Baxter remarked that so far as laboratory instruction in chemistry is concerned, he recognized no difference between the sexes. "I recognize one important difference," LJ replied, "women menstruate." There was no further argument.

LJ enlivened our luncheons in the Faculty Dining Room of the Harvard Business School. On one occasion the professor of marketing was unsuccessfully defending progress in marketing; the word progress used in that connotation was offensive to LJ. The professor hit upon an illustration of progress that he thought would appeal to LJ as a graduate of the Harvard Medical School. "Consider the progress in marketing crackers," he said: "they no longer are shipped in barrels, but reach the consumer without touching human hands." "Did you ever hear of a case of cracker poisoning?" was LJ's comment. On another noon asparagus was served: LJ expressed his delight and wondered why one of our group was not eating his asparagus. The explanation was, "I don't like the odor it gives my urine." To this explanation LJ asked, "Why bother to smell your urine?"

DISAPPOINTMENTS AND TRAGEDIES

While LJ cherished many happy memories of his first two years in Europe, including friendships formed at Strasbourg, his research was a disappointment. He told Professor Hofmeister that he was interested in a problem involving blood. He was put to work studying products of partial hydrolysis of hemoglobin but nothing came from two years' work on the problem. Evenings he spent studying data on heats of combustion but the paper he wrote was returned by Ostwald marked "Nichts wesentlich neues" (Nothing essentially new.)

In the summer of 1907 Folin was brought to the Harvard Medical School as associate professor of biological chemistry and a year later he became Hamilton Kuhn professor of biochemistry and chairman of the department. This greatly disappointed both Alsberg and LJ who had envisioned advancement in rank in recognition of their endeavors to strengthen the department they had already transformed from medical chemistry to biochemistry. Alsberg left, but LJ stayed on for three years. While he was promoted to assistant professor "he found the three years' experience irksome and an intolerable waste of time." His move to the college is described elsewhere.

One of the authors of the two papers of Blood III (16) was Cecil D. Murray, generally known as Mike. He had been an admiring student in LJ's Chemistry 15. In 1924 he was invited to spend some weeks with LJ in the alpine village of Nevache near Courmayeur, France. They completed the first paper of Blood III, and since the name of a laboratory of origin was required they created a "Claude Bernard Laboratory." The Murrays lived in Cambridge, Mike was one of LJ's protégés scheduled to work with him in the Fatigue Laboratory. He was profoundly shocked when Mike committed suicide in 1927.

The Yandell Henderson affair was a bitter disappointment – a blow to LJ's pride. Yandell and LJ were contemporaries, unrelated although on one occasion when I met Yandell he asked, "How is cousin Larry?" An experience I had in 1926 sheds light on Yandell's thirst for glory. There were two methods in common use for estimating cardiac output, the CO_2 rebreathing procedure and the acetylene method. Yandell described a third in the American Journal of Physiology that depended on rebreathing from a rubber anesthesia bag a mixture of oxygen and ethyl iodide. The final protocol closed with "Eureka!" I was given the task of evaluating the method. First, the method used for estimating ethyl iodide concentration gave too low results. When that was corrected ethyl iodide was found to disappear from the anesthesia bag either by diffusion through the rubber or adsorption by the rubber. Our findings were not published; the method didn't catch on, evidently others found it defective. A year later LJ gave the celebrated Silliman lectures at Yale with Yandell in the audience. The book on "Blood, A Study in General Physiology" was published by Yale University Press. Not long

afterwards LJ arrived at Morgan Hall with a copy of Science (Jan. 11, 1929) containing Yandell Henderson's review entitled "Is this science or metaphysics?" I never before or after saw LJ more furious. I think he was most angered by the closing sentence of the first paragraph in which Yandell wrote that the lectures, "are seriously infected with that most insidious disease of scientific thought, metaphysics." There were other insulting remarks including the charge of using "weasel words." A reply by Van Slyke appeared in Science a month later, February 8; he described Yandell's charges as "savage" and "absurd." There was an exchange of letters between the Hendersons which was wholly unproductive so far as LJ was concerned.* W. B. Cannon took up the attack but he gained no satisfaction from Yandell. In LJ's first letter near the close he opened a paragraph with, "Finally, your remarks are emotional and in many respects exclusively so." My guess is that Yandell, distinguished as a pragmatic physiologist who had been a colleague of Haldane in the celebrated Pike's Peak expedition, was bitter at finding in the book only two references to his research. He may have been especially resentful of a contemporary physiologist from Harvard receiving an invitation from Yale to deliver a series of lectures that he did not comprehend.

Another major disappointment was failure to obtain a professorship at Harvard for Raymond Pearl. Raymond Pearl was one of LJ's good friends, highly regarded as a distinguished biologist with broad interests in science. His affection and regard for Pearl is revealed in the obituary LJ wrote (20). Pearl was a New Englander and shared with LJ affection for Yankees. I met him in the summer of 1926 or 1927 when our family took LJ and Edith across Vermont hills one Sunday morning to join the Pearls for lunch in the village where they were visiting. We were informed that we must arrive well before eleven o'clock. It developed that Pearl had made a commitment to the pastor of the village church to attend the morning service. This illustrates Raymond Pearl's curiosity about people and their points of view. I was seated beside Pearl and enjoyed listening to his booming voice join in the hymn, Onward Christian Soldiers.

Pearl had two other good friends at Harvard, William Morton Wheeler and Thomas Barbour, Director of the Museum of Comparative Zoology. Between Wheeler, Henderson and Barbour a plan was formed in 1929 to have Pearl appointed a professor and dean of the Bussey Institution, succeeding Wheeler in the latter post. Many details of this affair can be found in the biography by Evans & Evans (11). The appointment was agreeable to President Lowell and to the President of the Board of Overseers. I have been told that Pearl who was enthusiastic about moving to Harvard spread the news to his friends at a meeting of the National Academy of Sciences. Word of the plan then reached E. B. Wilson, Professor of Vital Statistics, Harvard School of Public Health. Although LJ and Wilson were good friends, Wilson opposed the plan so vigorously that Wheeler, Henderson and Barbour gave up. Pearl was bitterly disappointed and was ill through the fall of that year. Henderson and Wilson didn't speak for years; a letter from Baird Hastings recounts how they were brought together:

"I had a hand in healing the rift between LJ and E. B. Wilson which had existed since the Raymond Pearl affair. I deplored that they were not on speaking terms, so I enlisted the help of Walter Cannon. We asked both LJ and E.B.W. to have lunch with us at Vanderbilt Hall and they accepted. I arranged for a table for four and LJ and E.B.W. sat opposite each other. For a while they talked individually only to Cannon or me. Then quite naturally, as if nothing had ever happened, they started conversing with each other. It was comforting to me to see these old friends on good relations again."

The year 1931 brought honor but also two tragedies. The Department of Philosophy, University of California, invited LJ to spend a semester at Berkeley as lecturer. "An approximate Definition of Fact," LJ's topic, was published by the University (18). One of the two tragedies was LJ's ulcer. I had received handwritten letters during January, then Arlie Bock told me about LJ's hemorrhage. After LJ's return in the fall he told me of the shock he experienced when he saw fresh blood along with his feces one morning. He took his pulse and found it was 140. I believe he phoned Arlie Bock first, who with Roger Lee, got in touch with medical friends at the University of California Medical Center in San Francisco. There was a period of hospitalization during which it was decided not to operate, but to prescribe a bland diet that excluded wine. When sufficiently recovered, LJ and Edith went by rail to Santa Barbara where he had a long period of convalescence at the Miramar Hotel. In the fall LJ seemed well recovered; on his bland diet there was no recurrence of the ulcer; so far as I could see he returned to his duties with full vigor.

The second tragedy involved them both, the mental illness of Edith Henderson. This was particularly tragic in view of her background. She had been a distinguished woman in character and appearance. After attending Radcliffe, Edith Thayer was admitted to P and S Medical School, Columbia, University. But her plans for a medical career were shattered when her sister, Grace, died suddenly and Edith had to come home to care for her mother. LJ and Edith were married in 1910 when he was 32 and she was 38. They went to Europe on their honeymoon, both were fluent in French and German and conversant with European civilization and culture. The birth of their son the next year was difficult, there were no other children. Senile dementia was well advanced when they were in California in 1931. Larry Henderson feels that LJ's ulcer may have depended on the distressing situations he had to face every day. Elinor Gregory visited them in Seattle on their return via Banff and Morgan Center. She has described to me how sad she was at meal times, remembering as she did the sparkling mealtime conversations of past years. Soon after their return on Arlie's advice Edith was committed to the McLean Hospital. Larry described the painful visits he and his father made. After two or three years Edith no longer recognized them.

A plan that went awry and threatened tragedy for the Fatigue Laboratory was put back on the track by LJ's capacity for quick and decisive action. Ancel Keys spent some years with Krogh and Barcroft. He had discussed plans for a high altitude study with each of them, and after his arrival to be an assistant in the Fatigue Laboratory in 1933 he found LJ receptive. Ancel then began planning an ambitious expedition that combined mountaineering and research. He sought permission from the USSR embassy for staging the expedition in the Pamirs. Months passed with no reply to his first, nor to his second inquiry. He then turned to the Karakorum in the Himalayas and obtained what he understood to be a firm

*The correspondence is filed in the archives of Baker Library, Harvard Business School.

approval from British authorities. The plan then took form with glassware packed in rugged cases built so as to open to form a functional apparatus, e.g., for lactic acid determination and for blood gas analysis. The portable Haldane apparatus was borrowed from Bock's laboratory. Several sources of support were found. The party was to include Keys, John Talbott, Will Forbes, Harold Edwards, Ross McFarland (then of Columbia) Greg Hall (Duke University,) Bryan Matthews (Cambridge University) and Hohwu Christensen of Denmark then a research fellow in the Fatigue Laboratory.

In late December numerous boxes were shipped to a dock in New York. Will Forbes went to New York to see them loaded on a freighter about to sail to India. After Christmas Ancel left by air for San Francisco to proceed to India to make advance arrangements. Then the bombshell! LJ received a cable from Barcroft to the effect that authorities had withdrawn permission. LJ immediately recalled Ancel from San Francisco and requested Will to unload the shipment. Carrying out that request on the day before New Year's was a challenge that Will met. LJ agreed that the reputation of the laboratory was at stake. He and I then hit on a possible solution. As soon as Ancel arrived we presented the plan to him and he accepted it without hesitation. The study would be in northern Chile. I would be involved, Hastings would be invited to join, and Ancel would be responsible for administration. Ancel agreed and soon had agreements from Chile and from the mining companies, one of which we knew had set up a village at 17,500 feet altitude. Plans for frequent news reports were cancelled; sources of support were notified and agreed to the change of plan. Hastings was about to move from the University of Chicago to Harvard; in his place he recommended Guzman-Barron, (Achito) his assistant professor of biochemistry and a brilliant native of Peru. The new plan went forward with no breakdown thanks to the administrative skill of Keys, to harmonious relations between the ten of us, and especially to the excellent relations with the people of Chile; they regarded Achito as muy simpatico. On the last evening the Chilean engineers entertained us at Ollague's only restaurant, an event never to be forgotten.

The death of LJ on February 10, 1942, was a tragedy for science, for a host of friends and for his son and many other relatives. He was about to be discharged after removal of a tumor of the bladder when an embolism blocked a major vessel and death came within minutes. It was a tragedy for science because of the loss of many years of productive thought by a brilliant man. There is recompense however, in that his achievements live after him; I agree with Hastings' comment mentioned above that Henderson and Van Slyke advanced physiology most in their day.

GREAT ACHIEVEMENTS

LJ's first great achievement was formulating the concept of regulation of acid-base balance in the body. LJ himself rated this one of his three greatest accomplishments (See Memories). He and Spiro started it together; it took LJ 12 months to finish their classic paper (13). The concept has been discussed by Parascandola (25).

In 1911, with the endorsement of President Lowell, he began lecturing in the college on the history of science. His lectures led to organizing a great department; Harvard became a pioneer in this field. Much relevant discussion is in a section on Memories.

During a climb on Mt. Monadnock, February 22, 1912, basic concepts of The Fitness of the Environment took form in his mind. A summer's writing in Maine produced this great book (14). It, and a later volume, The Order of Nature (15), have been discussed at length in many places – notably in Cannon's Biographical Memoir (7). LJ in his Memories rated this his second greatest accomplishment.

Another achievement, accomplished with the cooperation of Dean David T. Edsall of the Harvard Medical School, and with help from the DeLamar fund, was establishing in 1920 a Laboratory of Physical Chemistry in the same building, and closely affiliated with the Department of Physiology. It made a place for Edwin J. Cohn, JL's protégé of the World War I period. Cohn was a brilliant investigator who within a few years became a dominant authority on the physical chemistry of proteins. Many eminent biochemists and physical chemists were trained there. One, John T. Edsall, still active as an emeritus professor at Harvard has written a brief biography of LJ (10).

The concept of Blood as a Physiochemical System began to take form before 1920. LJ was intrigued by the facts developed by Krogh and associates in Denmark, by the Haldane group at Oxford, and by Barcroft and his students at Cambridge presented in The Respiratory Function of the Blood (1). The first paper on Blood as a Physiochemical System, developed from their investigations, particularly Barcroft's, was submitted to the Journal of Biological Chemistry, January 1921, and appeared later that year (16). It contained his first nomogram described on p. 415 of that paper as follows: "this nomogram expresses at once the results of Barcroft upon the oxygen dissociation curve of blood, and of Christiansen, Douglas, and Haldane on the carbon dioxide dissociation curve, as well as the peculiarities of the acid-base equilibrium, and of the distribution of chlorides. Obviously it has the property that if values are assigned to any two of the variables, all six are determined." The next paper of the series was published in 1924 with junior authors Bock, Field and Stoddard (16). Their many studies of blood, including oxygen and carbon dioxide dissociation curves, provided LJ with data from which he derived interrelations of pairs of variables illustrated by more than 100 figures. In addition, the paper included a large scale alignment chart and the first d'Ocagne nomogram; it greatly simplified representation of changes in the respiratory cycle. Blood III with Cecil (Mike) Murray as junior author was written in 1924 during a happy summer they spent in Nevache, France. It included deductions concerning capillary exchanges of carbon dioxide and oxygen, the area of the capillary bed and circulation of blood.

During the period 1925-27, five more papers in the Blood series were completed or being written (16) in time for LJ's use in preparing the Silliman lectures given at Yale. He was very proud of this honor: the book based on the lectures was entitled Blood, A Study in General Physiology. LJ rated the synthetic treatment of blood as described in that book his third greatest achievement (17).

LJ's transition from physiology to the social sciences began before publication of the Silliman lectures. His Memories dictated in 1937-39 included nothing of his achievements after publication of Blood, A Study in General Physiology, suggesting that he thought of that as the end of an era.

One of LJ's most enduring achievements after 1928 was establishing the Society of Fellows, fully described by Brinton (6). It enabled brilliant young men to pursue their intellectual

interests without registering for courses: all doors of the University were open to them. LJ's dream became reality when President Lowell not only endorsed the plan enthusiastically, but provided the endowment. President Lowell often sought advice from LJ, this he valued highly. In Lowell's book, What a University President Has Learned (23), the only mention of anyone who helped him is the following tribute to LJ in the introduction, "To Professor Lawrence J. Henderson, that most stimulating of comrades, earnest gratitude is due . . ."

Pareto's treatise on Sociology was brought to LJ's attention by William Morton Wheeler about 1926. He became greatly interested in the concepts of this Italian engineer who undertook to bring to sociologists' attention a systematic treatment by a physical scientist of man's emotions, thoughts and actions. Organismic and holistic concepts in the thoughts of L. J. Henderson have been discussed by Parascondola (26). Beginning in 1932, LJ conducted seminars on Pareto in the Sociology Department and in 1938 initiated a course, Concrete Sociology, Sociology 23. Bernard Barber, now professor of sociology at Barnard College, as a Harvard undergraduate, took that course when it was first given. He was deeply impressed. Recently, he obtained authority to publish the lectures as revised by LJ not long before his death. His book (3) also includes an assessment of LJ as a sociologist. He does not rate Pareto as having made a lasting contribution to sociology but he has no doubt that LJ's lectures and his concepts, derived in part from Pareto, constitute a great achievement having had great impact on the lives of many sociologists who have become eminent.

As revealed in Memories LJ gained an understanding of the human problems that arise in the practice of medicine. This was derived in part from Pareto and in part from his colleague at the Harvard Business School, Elton Mayo. It was related to his affection and admiration for Arlie Bock, Roger Lee, John Talbott and other young clinicians. One of the latter, Nobelist Dickinson Richards, has written an appreciation of LJ (27) and donated a bust of Hippocrates to the Harvard Medical School in his memory. The lecture LJ gave each year to medical students and one year to a physicians' meeting at Atlantic City, The Practice of Medicine as Applied Sociology (19), was an achievement enthusiastically recognized. Talbott in his essay on Henderson (28) quotes from that lecture: "I now state a proposition. According to this definition, a physician and a patient taken together make up a social system. They do so because they are two and because they have relations of mutual dependence. Also they are heterogeneous, they manifest sentiments, they have economic interests, they talk, reason, pretend to reason, and rationalize.

"If physician and patient constitute a social system, it is almost a trivial one compared with the larger social system of which the patient is a permanent member and in which he lives. This system, indeed, makes up the greater part of the environment in which he feels that he lives. I suggest that it is impossible to understand any man as a person without knowledge of this environment and especially of what he thinks and feels it is; which may be a very different thing."

His last great achievement was his success as Foreign Secretary of the National Academy of Sciences from 1936 until his final illness in 1942. In 1937 he visited Germany, France and England finding in England prospects for setting up worthwhile scientific relations between the Academy and the Royal Society. He met with two friends, distinguished physiologists A. V. Hill and Sir Henry H. Dale. His sessions with them and with Sir Albert Seward, Foreign Secretary of the Society and President Bragg led to an agreement whereby members of each organization would be welcomed at the other's meetings. It was arranged also to exchange lecturers in alternate years. The Pilgrim Trust in London underwrote the cost with an annual grant of 250 pounds, the title Pilgrim Lectureship was adopted and LJ was invited to be the lecturer in 1940. Unfortunately, illness prevented his doing so. Hence, the relationship was not initiated until Sir William Bragg, President, represented the Royal Society at an Academy meeting in Washington in 1941. His role as Foreign Secretary has been emphasized by Barcroft (2) who commented on LJ's "intense desire to knit more closely British and American science . . ." Barcroft added that Sir Charles Sherrington, eminent British physiologist, in passing through Barcroft's laboratory noted LJ's photograph and remarked, "A great man, that."

In summary, it is evident that LJ's wide range of accomplishments demonstrated his great intellectual capacity and wide range of knowledge making it possible for him to master one field after another, leaving each field in capable hands for further cultivation. Throughout his career LJ took an interest in young men who came to work with him. It was my good fortune, after spending two years with Carl Alsberg at Stanford, to obtain a post-doctoral fellowship to work with Alsberg's friend and former colleague, LJ. Those were great years. My feelings of admiration and affection for LJ have not dimmed with the years.

Acknowledgements

I am greatly indebted to Professor Henderson's son, Lawrence J. Henderson (Larry) for authority to use his father's Memories kept in Harvard's University Archives. Robert W. Lovett, Curator, Manuscripts and Archives Department, Baker Library, Harvard Business School, gave me access to Professor Henderson's correspondence. Larry has been helpful in many ways; he and LJ's niece, Elinor Gregory Metcalf, have provided information about Mrs. Henderson's background and illness. I have had the invaluable assistance of my daughter, Elizabeth C. Horvath, and of many colleagues: Arlie V. Bock, John H. Talbott, A. Baird Hastings and A. Clifford Barger. Memories was dictated to Hilda Richardson, now Hilda Carter Fletcher. It has been pleasant to renew acquaintance with her after more than a third of a century and to gain from her further insight into the life at LJ's camp. Acknowledgement also is made of support of NIH Grant AG00437-04A1.

References:

1. Barcroft, J. The Respiratory Function of the Blood. Cambridge University Press, 1914.
2. Barcroft, J. Obituaries: Prof. L. J. Henderson, Nature 149:374-375, 1942.
3. Barber, B., Ed. L. J. Henderson on the Social System. Selected Writings. University of Chicago Press, Chicago, 1970.
4. Bock, A. V. and D. B. Dill. The Physiology of Muscular Exercise, 3rd. Ed. Longmans Green & Co., London, 1931.
5. Bock, A. V., D. B. Dill, and J. H. Talbott, C. Van Caulaert, A. Folling and L.M. Huxthall. Studies in muscular activity. J. Physiol. (London) 66:121-174, 1928.
6. Brinton, Crane, Ed. The Society of Fellows. Harvard University Press, Cambridge, Mass., 1959.
7. Cannon, W.B. Biographical Memoir of Lawrence Joseph Henderson

1878-1942. Nat. Acad. Sci. 23, 2nd Memoir 31-58, 1943. Includes a complete bibliography.
8. Davis, J. S., Ed. Carl Alsberg, Scientist at Large. Stanford University Press, Stanford, California, 1948.
9. DeVoto, Bernard. New England, There She Stands. Harper's Monthly, March, 1932.
10. Edsall, J. T. Lawrence Joseph Henderson. Dict. Am. Geog. 349-352, 1973.
11. Evans, M. E., and H. E. Evans. William Morton Wheeler, Harvard University Press, Cambridge, Mass., 1970.
12. Ferguson, J.K.W. and F.J.W. Roughton. Chemical relationship and physiology of carbimo compounds of carbon dioxide and hemoglobin. J. Physiol. (London) 83:87-102, 1934.
13. Henderson, L. J. and K. Spiro. Das Gleichgewicht zwischen Basen und Saüren im tierischen Organismus. Ergeb. Physiol. 8:254-325, 1909.
14. Henderson, L. J. Fitness of the Environment. An Inquiry into the Biological Significance of the Properties of Matter. Macmillan, New York, 1913.
15. Henderson, L. J. The Order of Nature. Harvard University Press, Cambridge, Mass., 1917.
16. Henderson, L. J. Blood as physicochemical system. J. Biol. Chem. 45:411-419, 1921. Reference is made in the text to seven later papers in this series, all in the same journal. They are designated as Blood II, III, etc. Corresponding journal citations in J. Biol. Chem. are, II, 59:379-431, III, 65:407-444, 1925; IV, 73:251-262, 1927; V, 73:749-766, 1927; VI, 75:305-313, 1927; VII, 78:191-214, 1928; VIII, 81:551-574, 1929.
17. Henderson, L. J. Blood. A Study in General Physiology. Yale University Press, 1928.
18. Henderson, L. J. An approximate definition of fact. Publications in Philosophy of the University of California 14:179, 1932.
19. Henderson, L. J. The practice of medicine as applied sociology. Trans. Assn. Am. Physicians 51:8-32, 1936.
20. Henderson, L. J., Obituary of Raymond Pearl. Yearbook Am. Phil. Soc. 1940, p. 431-433.
21. Hill, A. V., C.N.H. Long and H. Lupton. Muscular exercise, lactic acid and the supply and utilization of oxygen. Proc. Roy. Soc. 96B, 438; 97B, 184, 1924.
22. Horvath, S. M. and E. C. Horvath. The Harvard Fatigue Laboratory. Its History and Contributions. Prentiss Hall, Englewood Cliffs, N.J., 1973.
23. Lowell, A. Lawrence. What a University President Has Learned. Macmillan, New York, 1938.
24. Mayer, J. Lawrence J. Henderson — A biographical sketch. J. Nutr. 94:1-5, 1968.
25. Parascondola, J. L. J. Henderson and the theory of buffer action. Med. Hist. J. 6:297-309, 1971.
26. Parascondola, J. Organic and holistic concepts in the thought of L. J. Henderson. J. Hist. Biol. 4:63-118, 1971.
27. Richards, D. W. Lawrence Joseph Henderson. The Physiologist 1:32-37, 1958. Sympathetic remarks made at the Respiration Dinner, Federation Meeting, Chicago, April 17, 1957.
28. Talbott, J.H. A Biographical History of Medicine. Excerpts and Essays on the Men and Their Work. Lawrence Joseph Henderson (1878-1942), Grune & Stratton, New York, 1970. pps. 970-974.
29. Van Slyke, D.D., A. B. Hastings, C. D. Murray and J. Sendroy, Jr. Studies of gas and electrolyte equilibrium in the blood VIII. The distribution of hydrogen, bicarbonate and chloride in oxygenated and reduced blood. J. Biol. Chem. 65:701-728, 1925.
30. Van Slyke, D.D., H. Wu and F. C. McLean. Studies of gas and electrolyte equilibrium in the blood V. Factors controlling the gas and electrolyte equilibrium in the blood. J. Biol. Chem. 56:765-849, 1923.

NEW AWARD ESTABLISHED

The first of the Theodore Weicker Memorial Awards in Pharmacology and Therapeutics will be presented at the 1978 Spring Meeting. The award consists of $10,000 and an appropriate certificate and is to be given for sustained, distinguished contributions in pharmacology. There are no restrictions on the institutional affiliation or age of a candidate. The award will be administered by the American Society for Pharmacology and Experimental Therapeutics, but membership in that organization is not a prerequisite for nomination. Additional details are available from the ASPET headquarters office in Bethesda.

The Theodore Weicker Memorial Award in Pharmacology and Therapeutics was established by the trustees of the Theodore and Elizabeth Weicker Foundation to honor Theodore Weicker for his advancement of materia medica and the practice of medicine.

A sum of $10,000 and a certificate will be awarded to an active investigator who has made sustained, distinguished contributions in pharmacology. The research may involve work leading to new concepts or knowledge in pharmacology or the development of drugs useful in the treatment of human disease. Studies concerned with means of improving drug therapy or decreasing toxicity of drugs or other chemical substances would also qualify.

There are no restrictions on the institutional affiliation or age of a candidate and membership in ASPET is not a prerequisite for nomination. However, nomination must be by a member of ASPET and no member can nominate more than one candidate. The award will be made annually but need not be given if the Selection Committee deems that no outstanding candidate has been nominated. The contribution for which the award is being made should not have been previously recognized by a major award.

The American Society for Pharmacology and Experimental Therapeutics would make the selection. A committee of five appointed by the President and approved by Council of ASPET would judge the merits of the candidates who are nominated.

Nominations shall be accompanied by:

1. Six copies of a summary which describe the importance of the candidate's work.
2. Five copies of articles published or ready for publication by the candidate that have direct bearing on the Award.
3. Six copies of a brief biographical sketch of the candidate.
4. Six copies of the candidate's curriculum vitae.

Nominations for this award shall be sent by September 15, 1977 to the Executive Officer, American Society for Pharmacology and Experimental Therapeutics, Inc., 9650 Rockville Pike, Bethesda, Maryland 20014.

The Award will be made at the Spring Meeting of ASPET and the recipient would be invited to address the membership on the research which led to the award. Travel expenses including hotel costs will be paid for the awardee and spouse by the donor.

COSTS AND BENEFITS OF APS MEMBERSHIP

In response to some correspondence from members concerned about the level of membership dues in the Society, the following analysis was prepared. On recommendation of Council it is published here for the information of other members who may have the same concern but have not expressed it in correspondence.

APS MEMBERSHIP – COST/BENEFIT ANALYSIS

APPORTIONMENT OF REGULAR MEMBER DUES INCOME –$45.00			NONMEMBER COST	NET SAVINGS
FASEB Assessment		$12.00[1]		
Federation Proceedings	$ 6.00		$38.00	
Membership Directory	3.00		20.00	
Public Affairs/Newsletter	2.00		10.00	
Placement Service	1.00			
	$12.00			
Council and Committee Support		3.00		
Meeting Program Development (Costs of Symposia)		3.00		
Education Office		8.00		
The Physiologist		8.00	12.00	
The Physiology Teacher		2.00	5.00	
Business Office		4.50		
Exec. Secy's Office (net)		3.00		
Dues to other organizations		1.00		
Awards (Bowditch and Daggs)		.50		
		$45.00[2]	$85.00	$40.00
OTHER BENEFITS:			NONMEMBER COST	NET SAVINGS
Discount on Meeting Registration @ $10.00 (2/year) Attending 1/year		$35.00	$45.00	$10.00
Discount on Journals (Average $20.00) 8 Journals and Sections Purchase of One Section (Example: Heart & Circulatory Physiology)		27.50	55.00	27.50
Discount on Handbooks (Average $20.00) Purchase of One Volume/Year (Example: Renal Physiology)		58.00	72.50	14.50
Reasonable Potential Cost/Savings		$165.50	$257.50	$92.00

[1] If a new method of financing FASEB operations is adopted, this assessment will increase to approximately twice this amount, to be compensated by increased society receipts from the annual FASEB meeting.

[2] Dues to be increased to $50.00 for regular members July 1, 1977.

STATISTICS ON APS MEMBERSHIP
(As of February 1977)
DISTRIBUTION BY EMPLOYMENT

		%
MEDICAL SCHOOLS	2,391	60.4
Physiology Departments	(1,088)	(27.5)
Other Preclinical Departments	(420)	(10.6)
Clinical	(825)	(20.8)
Administration	(58)	(01.5)
HOSPITALS AND CLINICS	359	09.1
VETERINARY SCHOOLS	40	01.0
DENTAL SCHOOLS	22	00.6
PUBLIC HEALTH AND GRADUATE SCHOOLS	111	02.8
UNDERGRADUATE SCHOOLS	245	06.2
COMMERCIAL COMPANIES	70	01.8
GOVERNMENT	255	06.4
INSTITUTES AND FOUNDATIONS	276	07.0
PRIVATE PRACTICE	47	01.2
EMERITUS OR INACTIVE	142	03.5
	3,958*	100%

*Number of survey questionnaires in the data bank that answered the appropriate question.

DISTRIBUTION BY "PRIMARY" DEGREE

	%
Ph.D.	49.8
M.D.	46.1
D.Sc	01.2
D.V.M.	01.7
D.D.S.	00.3
Other degrees	00.9

DISTRIBUTION BY SPECIALTY

	%
Cardiovascular	18.5
Neurophysiology	15.8
Endocrines and Reproduction	13.9
Electrolyte and Water Balance	05.7
Respiration	06.9
Renal	04.5
Muscle and Exercise	04.7
Gastrointestinal, Food and Nutrition	04.6
Environmental	04.2
Cellular and Tissue	04.7
Blood	03.3
Comparative	02.2
Energy, Metabolism and Temperature Regulation	02.0
All other categories (none above 1.2%)	09.0

CHARTER FOR RENAL SECTION APPROVED

Through the efforts of a Steering Committee elected at the Renal Dinner at Anaheim in April 1976, a Statement of Organization and Procedures was developed for the Renal Section of the American Physiological Society. This "charter" was reviewed by the APS Council at its recent March 1 and 2 meeting in Bethesda. Council approved the "charter" as written by the Steering Committee, the second such document to be approved, the first being that for the GI Section. (The Physiologist, Vol. 20, No. 1, Feb. 1977, p. 5).

Members of the Steering Committee for the Renal Section of APS are:

Franklyn G. Knox, Chairman
Sidney Solomon, Secretary
Reinier Beeuwkes III, Treasurer

The "charter" as approved by Council reads as follows:

ARTICLE I. Name

The name of this organization is THE RENAL SECTION of the AMERICAN PHYSIOLOGICAL SOCIETY.

ARTICLE II. Purpose

The purpose of this organization is: (1) to advise the American Physiological Society on matters of interest to renal physiologists, and (2) to assist the American Physiological Society in organizing, and presenting scientific sessions, symposia, and other programs of interest to renal physiologists. These activities include sponsoring an annual dinner and organizing an annual symposium for the spring meeting of the Federation of American Societies for Experimental Biology.

ARTICLE III. Membership

Membership is open to any member of the American Physiological Society who wishes to be a member. Membership in the renal section is automatic upon attendance at the renal dinner(s).

ARTICLE IV. Officers

Section 1. Steering Committee. The responsibility for management and supervision of the affairs of the Renal Section shall be vested in the Steering Committee. The members of the Steering Committee shall be the Chairman, the Secretary, Treasurer and the representative of the Renal Section to the Program Committee of the American Physiological Society.

A quorum for conducting official business of the Renal Section shall be three of the members of the Steering Committee. Such business may be conducted at meetings of the society or by conference phone call.

Section 2. Election of Officers. Officers shall be elected each year at the annual meeting. Nominations shall come from the floor.

Election of officers shall be by ballot if there is more than one nomination per office.

ARTICLE V.

Section 1. The Chairman may appoint committees that are necessary for the proper conduct of the affairs of the Section.

ARTICLE VI.

Dues will not be assessed.

ARTICLE VII.

Nothing in this Statement of Organizational Procedures shall be construed as contradictory to the Constitution and Bylaws or Operational Guidelines of the American Physiological Society.

APS AS A NONPROFIT, ASSOCIATION PUBLISHER

What follows is the introduction from the Annual Report for 1976 on the Publications of the American Physiological Society. It is included in this issue of The Physiologist to remind the members of the Society that publications are not a casual undertaking for the Society, rather it operates a sizeable, quality publications program and shares with other nonprofit publishers varied responsibilities.

To: The Publications Committee
From: Stephen R. Geiger, Publications Manager and Executive Editor

In this introduction I emphasize how the American Physiological Society operates as an association publisher, using illustrations from events that occurred in 1976. I hope this information will give those who read it a better understanding of this most important function of the Society. As a publisher, the American Physiological Society has the same complex responsibilities of other nonprofit publishers. The Publications Committee, with the approval of the Finance Committee and Council, is responsible for the management of an extensive program of journal and book publishing.

CONTENT:

The Publications Committee receives input on the scientific quality of the publications from colleagues and Editors. It recommends new ventures, revisions of previously published works, and modifications of existing publications.

In 1976 the book publishing part of the operation included the completion of the final volume, Parathyroid Gland, in the seven volume set of the Handbook of Physiology on endocrinology. It also included progress on two other volumes: Reactions to Environmental Agents and the first volume, Cellular Biology of Neurons, in the revision of the section on neurophysiology. Toward the end of the year, manuscripts began to arrive on the Editors' desks for revision of the section on circulation.

A new book publishing venture was approved and work was begun on a volume composed of peer reviewed articles written by the speakers at a symposium held at the 1976 Anaheim meeting of the Society. The volume, which will be entitled Disturbances in Body Fluid Osmolality, should be available by the time of the 1977 spring meeting.

The decision was made to prepare cumulative indexes for the American Journal of Physiology (1952 – 1975) and the Journal of Applied Physiology (1948 – 1975) in order to increase the usefulness of the journals. After examining several methods of preparation it was decided to produce the subject indexes by merging the existing volume indexes and to produce the author indexes by combining the tables of contents. A computer and phototypesetter were used to achieve economy and quality printing. The indexes were distributed to the 1976 subscribers of each journal to achieve maximum distribution and spread the cost equitably; however, they may also be purchased separately.

The most dramatic changes in the publications program involve the reorganization of the American Journal of Physiology and the Journal of Applied Physiology. Dr. Fishman, the Chairman of the Publications Committee, described that reorganization as consisting of the following essential elements:

1. The American Journal of Physiology and the Journal of Applied Physiology will continue to exist and to be available as separate publications of the American Physiological Society. This continuing arrangement will satisfy both the generalist in physiology and the librarian who will be able to preserve unbroken series of these two distinguished journals.
2. The Journal of Applied Physiology, under its own editorial board, will restrict itself to papers in respiration, exercise, and environmental physiology.
3. The American Journal of Physiology will henceforth be an umbrella for separate journals, each with its own editorial board and each devoted to specialized subject matter. Each component will be issued separately as well as part of the American Journal of Physiology. This arrangement will satisfy the specialist who is interested in having on hand outstanding papers in his own particular field of interest.

PROMOTION:

As a publisher, the Society is also responsible for the promotion of its products.

In choosing a company to promote the reorganization of the American Journal of Physiology and Journal of Applied Physiology, the Publications Committee decided on a young, imaginative company, Morna Conway, Inc. The general promotional theme that evolved was "times are changing and so are the journals of the American Physiological Society, but our commitment to excellence remains unchanged." A direct mail campaign reached about 20,000 libraries and about 60,000 individuals. Advertising in Society journals included a single page and later a four page, two-color insert. Special emphasis was given to the new journals, the American Journal of Physiology: Cell Physiology and the American Journal of Physiology: Regulatory, Integrative and Comparative Physiology in separate advertisements that not only appeared in the Society journals, but in related specialty journals. Fliers were distributed at various scientific meetings.

The Handbook of Physiology continued to be distributed under an agreement with Williams & Wilkins. The Publications Committee considered several alternatives for the sales promotion and distribution of Disturbances in Body Fluid Osmolality, before entering into an agreement with Williams & Wilkins to market the volume on a basis similar to that used for the Handbooks. A prime factor in reaching this decision was Williams & Wilkins access to foreign markets.

COSTS:

The Society, as other publishers, finance books differently from journals. With books, money must be invested to pay for the editorial and printing costs before a volume is published, but after such costs are calculated. With journals, most income is received from subscriptions at the beginning of the year. The money must then be carefully handled by the Business Manager, Walter A. Sonnenberg, and be available to pay costs that are incurred throughout the year. Prices are based on estimated costs for the year.

Publishers continually monitor costs and recommend changes in subscription prices and changes in printers if their prices no longer are competitive. Each of these changes was

considered this year for the Journal of Neurophysiology. In 1974 three hundred more pages and in 1975 four hundred more pages were published than in 1973 in this journal. At a cost of about $100 per page for producing this journal, the deficit for these two years was sizeable. After examining the prices of competing journals in the neurosciences and being reassured by the Editor that a price increase was long overdue, the nonmember subscription price was doubled. As a result, a net income rather than a loss is shown for this journal for the first time in several years. (Similar considerations led to increases in the subscription prices for all of the journals in 1976. Although this produced the desired effect, all journals finished the year in the black, it was undoubtedly part of the reason for a decrease in the number of subscribers to each of the Society's journals.)

Reduction in printing costs was also examined. Two quality companies were asked to submit estimates for printing the Journal of Neurophysiology. Their estimates were considerably below the actual charges by our present printer. We were pleased that when informed of the difference, Heffernan Press was able to offer reduced rates for 1977.

POSTAL REGULATIONS:

Second-class mailing privileges allow nonprofit publishers to mail journals at special rates, and therefore are of great concern to them.

In 1976 two items involving second-class mail required action. The first involved securing second-class mailing permits for the new journals. It was decided that this would be handled best by the Society's attorney. The other item involved the reinterpretation of the postal regulations in such a way that manuscripts on which page charges are collected would need to be labeled as advertising. If more than 75% of the pages are advertising, then the journals would no longer qualify for the lower rates (Science 194: 502-503, 1976). Three of the Society journals received such notices (American Journal of Physiology, Journal of Applied Physiology, and Journal of Neurophysiology). It was decided to consult the Society attorney on this matter. A letter has been sent to the Postal Service objecting to the ruling. We have kept abreast of developments on this issue by contact with fellow publishers and by attending a meeting called by the Federal Council on Science and Technology, which has set policy for the payment of page charges by government agencies.

LIAISON:

It is important for a publisher to maintain contact with other editorial and publishing organizations. This was primarily accomplished in 1976 through my active involvement in the Council of Biology Editors as a member of its Board of Directors and member of its Style Manual Committee. I also attended the annual meeting of the Council of Biology Editors, a meeting of the Association of American Publishers on the economics of journal publishing, and a workshop held by the American Association of University Presses on the promotion, circulation, and production of scholarly journals.

TECHNOLOGICAL CHANGES:

Increased utilization of computers is affecting publishing in two main areas: printing and subscription fulfillment. All of the Society's journals and books are now photocomposed and printed offset, with ever increasing utilization of computers (e.g., for hyphenation and justification) and optical character readers. Before long cathode ray tubes will replace the photocomposition equipment. The change to photocomposition was completed in 1976 without great loss in the quality of the printing. The per page saving in printing costs compared to costs during the first half of 1975, before the new technology was introduced, was about 15%.

Subscription lists for the journals are being stored in a computer, with terminals at FASEB. In 1977, they will be used to maintain the lists and to produce mailing labels. We hope that the system will eventually be used to store information on editorial boards and help in the analysis of promotional efforts and subscription patterns.

EDITORIAL OFFICE:

The American Physiological Society continues to operate an editorial office of unsurpassed quality. Copy editors, staff artists, and secretaries have continued to add greatly to the quality of the journals and books. In 1976, an increasing amount of production responsibilities were taken over by Brenda B. Rauner, Managerial Assistant. With her supervision and the enthusiastic cooperation of all in the Bethesda office, not only was a larger than usual amount of material received and published, but the reorganization of the American Journal of Physiology and Journal of Applied Physiology was well underway.

SATELLITE SYMPOSIUM – APS FALL MEETING

The symposium described below was scheduled so that it could serve as a satellite to the APS Fall Meeting to be held at Hollywood Beach, Florida, October 9-14. A special "triangle routing" flight is available from England.

The IVth International Symposium on Bioelectrochemistry will take place at the Marine Biological Laboratory in Woods Hole, Massachusetts, during October 2-8, 1977. The co-chairmen, Martin Blank and Arthur A. Pilla, both from Columbia University, are arranging a broad interdisciplinary program of invited and contributed papers under the title "Electrochemistry and Biological Interfaces." The major subheadings are: I. Molecular Interactions, II. Surfaces and Membranes (including faradaic processes, excitable membranes and light sensitive systems) and III. Cell and Tissue Systems (including red cells, epithelial cells, tissue growth and repair).

DISTURBANCES IN BODY FLUID OSMOLALITY

Disturbances in Body Fluid Osmolality initiates a new kind of publication venture for the American Physiological Society. The content of the volume, edited by T. E. Andreoli, J. J. Grantham, and F. C. Rector, Jr., is described in its preface as follows:

Within the past decade, there has been an enormous increase in information on the role of antidiuretic hormone (ADH) in regulating the osmolality and volume of the body fluid compartments, in the mechanisms by which the hormone exerts these regulatory effects, and in the measurements of plasma ADH levels in health and disease. These data, coupled with new insights into the factors regulating cell volume and the pathophysiologic consequences of body fluid hyperosmolality and hyposmolality, particularly in the central nervous system, have provided clinicians with a more rational frame of reference for evaluating and treating disturbances in body fluid osmolality. This book, an outgrowth of a Symposium on Disturbances in Body Fluid Osmolality sponsored by the American Physiological Society, is intended to gather some of these results in a single readable package.

The chapters in this book are divided into four groups:

- Recent information on the chemistry and conformation of ADH and related compounds, data on the location and possible nature of hormone receptors in the renal collecting duct, and current views and hypotheses about the molecular mechanisms by which ADH increases water and solute transport across renal collecting tubules and related epithelia, such as frog skin and toad urinary bladder.
- Recent advances in the measurement of plasma ADH concentrations, in the interrelationships between osmotic and nonosmotic stimuli in regulating ADH release, and in the renal mechanisms for forming dilute or concentrated urine (i.e., the processes by which we excrete water in excess of solute or vice versa).
- Insights, derived from animal models, into conditions—occurring at either the hypothalamic level or within the renal tubule—that serve as excellent analogues for the various types of diabetes insipidus observed in clinical practice and into the cellular pathophysiology referable to osmotic dilution or concentration of body fluids.
- An approach to the classification of clinical disorders of body water homeostasis, with particular regard to the diagnosis and explicit management of these diseases.

This book is intended to serve a dual purpose: as a source of information to the physiologist concerned with those processes that contribute to the regulation of body fluid osmolality and as a useful reference for the clinician faced with the evaluation, diagnosis, and management of patients afflicted with derangements in body water homeostasis.

The Publications Committee is pleased to have this fine 357 page, hard covered volume of peer reviewed contributions ready within one year and available to members of the Society for the moderate price of $20 ($25 to nonmembers). Further, the Publications Committee believes that a series of three books need to be produced to adequately test the value and economic viability of this sort of publication by the Society. Therefore, the Committee recommended, and the Finance Committee and Council approved, the publication of a second volume in this series on clinical physiology. The second book will be developed from the symposium on the physiology of lipids and lipoproteins in health and in disease, which was held at the annual meeting in Chicago. The volume will be edited by J. M. Dietschy.

It is hoped that this new kind of publication by the Society will receive your support. Order a copy now from the Circulation Department, American Physiological Society, 9650 Rockville Pike, Bethesda, Maryland 20014. Price postpaid to members is $20.

FULBRIGHT-HAYS AWARDS IN BIOLOGY

Among the approximately 500 Fulbright-Hays awards available in about 90 countries for 1978-79, a number have been programmed in biology.

Africa: biological sciences; Australia: ecology of salt lakes; immunology for parasitic diseases; neurophysiological research on monkeys; virology in algae; Austria: permeability of plant cell membranes (German required); Cameroon: biology is one of several suggested fields (French required); Columbia: any of several specializations (Spanish required); Denmark: physiological research; plasmid and virus genetics; Fiji: marine science; India: any life science specialization; Ireland: estaurine ecology/pollution; zoology; bacteriology; Israel: psychobiology; Jordan: biology; Lesotho: biology; Near East/South Asia: short-term teaching; Nepal: microbiology and/or ecology; Netherlands: human genetics; neurophysics; Senegal: plant or animal biology (French required); Sudan: microbiology or pharmacology; Turkey: phytopathology; postharvest physiology; U.S.S.R.: plant physiology; electrical activity of brain cells.

Those desiring a copy of the 1978-79 announcement of Fulbright-Hays award opportunities should immediately send name, address, highest degree, specialization and country interest to the Council for International Exchange of Scholars, Eleven Dupont Circle, Washington, D.C. 20036. Applications are due for the American Republics, Australia and New Zealand by June 1, 1977, and for Africa, Asia and Europe by July 1, 1977.

In the 1977-78 program, there are still a few openings in biology. Inquiries are welcome.

CIES will also assist in the administration of about 500 awards in 1978-79 for Fulbright scholars visiting the U.S. for lecturing and research. In many cases host institutions are expected to assist the scholar with full or partial maintenance. A directory of scholars currently in the U.S. is available on request and inquiries are welcome at any time regarding scholars from abroad for 1978-79.

THE COMMITTEE ON PUBLIC AFFAIRS AND PUBLIC INFORMATION

Committee Members: Barbara Horwitz, Ping Lee, Marian Kafka, James Holloway and Brian Curtis, Chairman

The Committee has been engaged in a number of projects which we thought might be of interest.

Our annual duty of screening the abstracts to FASEB, for those of interest to "the people next door", is just finished. These will be reviewed by the FASEB office of Public Affairs and available in the Press Room at the annual meeting in Chicago. We hope to expand this service to include screening journal articles as well. Marian Kafka, our representative to the FASEB Public Information Committee, will lead us in this effort, It is very important for us to inform and educate the general public since they provide almost all of our research support through their taxes.

I was recently appointed by APS to the newly created Public Affairs Group of the AAMC Council of Academic Societies. I would like to share with you a few highlights of a workshop the AAMC held for the newly appointed representatives.

The session was attended by 30 public affairs representatives from member societies of the Council of Academic Societies. Attending were five members representing Basic Science disciplines, including physiology, anatomy, immunology, and microbiology. The other 25 attendees were from academically-oriented clinical societies. The session opened with the following quotation directed to the entire group, "If you like good laws and good sausage, it may be better not to investigate how either are made." Tom Morgan introduced to the rest of the AAMC staff, Gus Swanson, John Sherman, and Judy Braslow.

Ted Cooper, the Assistant Secretary for Health, spoke both on the importance of input into law making and its implementation. He was particularly concerned with the new budget process on Capitol Hill which sets an upper limit for the health budget. He was very much afraid that the research budget was going to be squashed between the service component, particularly Medicare, Medicaid and the budget ceiling. Ted Cooper was followed by Steve Lawton, counsel to Congressman Roger's Health and Environmental Sub-Committee. Steve Lawton, too, stressed the importance of input, particularly the point that the earlier you try to influence the legislative history of a bill the more successful you are likely to be, running in at the last minute usually does not work very well.

We broke up into three groups of ten, each led by a former congressional fellow. The leader of my group was Art Silverstein, who was a FASEB fellow last year. We began with a discussion of the appropriations process with Minority Staff Counsel to the Senate Health Subcommittee, Jay Cutler who relates specifically to Jacob Javits and with Terry Lierman, a professional staff member to Senator Magnason's Committee. Authorization legislation spells out the goals, methods and maximum amount of money which can be spent on projects. They stressed a number of points:

1. The new budget process, including the dates of submission of authorization and appropriation bills, seemed to be working very well and the process is being heavily defended.
2. Budget ceilings are not yet a problem, however, they could become so as Congress works more closely with the administration.
3. The authorization subcommittees have carved out a good deal more action for themselves by their insistence in recent years that all new authorizations come in three year blocks. There is a move-a-foot to reduce all of the continuing authorization to 3 year periods.
4. They stressed that testifying before Congressional Committees was only about 5% of the job we should be doing. In the House, the subcommittee chairman and the ranking minority members are usually experts in the area since this is their major subcommittee assignment. We should try to arrange discussions with them. Their staff is quite important because they write much of the legislation and do a great deal of the negotiation. Continuing discussion with them is very helpful to both parties.

   In the senate, the staff are crucial since every senator is a member of many committees and subcommittees. With rare exceptions they are not experts in the health area, and they will frequently leave even quite major decisions to their staff. The staff are easily accessible early in the legislative year, however, as time goes on and they become busier the tendency is to be less available.
5. The language of the Committee report (which is written by the staff) is almost as important as the law itself, and the staff said it was often easier to get report language changed or put in specific terms than it was to change the law. It would have much the same effect.

Our second session was with Ted Cooper and John Sherman of the AAMC on the role of the Executive Branch in introducing and influencing legislation and on creating the regulations which would then carry out the legislation. They described the pathway by which new legislation wound its way up from the bureaus, to the assistant secretary, to the secretary, to OMB (Office of Management and Budget), to the President, and then to the Domestic Council. The pathway for the budget for ongoing activities is pretty much the same.

Throughout Ted Cooper's discussion, there was a not very veiled hint of terrific stress between himself, the majority of the professional HEW and OMB. At one point, he commented that "sometimes you just have to be a good soldier" and go up and tesitfy on a bunch of stuff you don't believe in at all. He brought out that there was, however, a tremendous amount of cooperation and information transfer between the middle level staff at HEW and the Congressional staff, even though at the higher echelons there was, in theory, no cooperation.

In the regulation making process, the executive departments take not only the law itself but also the committee report and floor debate into account as they try to forge a workable set of regulations. It is a very slow process, partially because there is a lack of staff in many of the counsel's offices and partially the tremendous amount of interlocking between agencies. They attempt to make the regulations at least compatible between the various agencies and, hopefully, even between departments. Dr. Cooper estimated that one year is about the minimum period of time in which a totally new law can be turned into regulations that are utlimately published. He stressed that at every step of the process, there was input for written testimony and objections. Ultimately, each of

these written objections had to be answered in the prologue to the final regulations.

Our third session, which was probably our best because we were by then fairly comfortable with the legislative process, was led by Steve Lawton and Stan Jones. Jones has just become the Staff Director to Kennedy's Health Subcommittee. Both men noted that the authorization committees have become much more powerful since they will not authorize any new legislation for longer than a three year period. The purpose of this is to make certain that there is a re-evaluation of all programs after three years, and, also, to retain power in the authorization committee. Once again, they stressed that the role of the staff was tremendously crucial.

Major legislation up for a renewal this year is research and research training authority, particularly for the Cancer and Heart and Lung Institutes. Kennedy would like to extend the scope of these hearings to the entire NIH mission. Lawton and Jones discussed, at considerable length, the ideas which their "principals" are thinking about for new legislation in 1977. There seemed to be fair agreement between Kennedy and Rogers. Their new bill will begin by cleaning up the present procedures including cancelling the "sunshine" Act for peer review groups and also a number of other recommendations from the President's Commission on Biomedical Research. The new bill will include a number of new ideas. Jones referred to them by these catch words:

1. Too fast. Kennedy, in particular, is very worried that a number of ideas, technologies and drugs are being adopted long before they are proven useful. They are adding a tremendous cost burden to health care. One example of this is the whole body CAT scanners which have proliferated so rapidly.
2. Too slow. While these two may seem at odds with each other, the staff and apparently their principals are convinced that there are lots of ideas and information floating around as a result of previously paid for research which has not been put into proper prospective or introduced into medical practice. These two staff members seem to hold this idea very strongly and in several informal discussions they were not budging an inch. I think that if the biomedical community has any great barrier to jump in the coming year, it is going to be this one.
3. Old information. They are worried that there is a good deal of information which is currently available in the academic centers which is not being dispersed out into the general practice of medicine. They are worried about information retrieval and how one can make better use of gathering and disseminating information upon demand.
4. Institute of Preventive Medicine. This seems to be as much a hobbyhorse to Senator McGovern as anyone else, but they do feel that it is important to give some status or prestige to the whole area of preventive medicine. They see the fruitlessness of requiring every medical school to have a program, and they are still groping for some means of giving preventive medicine more status.
5. Transfer Process. More status for the persons involved in the transfer process between research and medical care. They cite the analogy of the discoverings of physics used by engineers. Once again, they do not have a good handle on what it is they are trying to get at but realize that there is a problem.
6. Disease of the Year. In recent years various disease oriented groups have mounted campaigns to have a big push mounted to cure "their" disease. Congress is very worried now that they have got themselves into the disease of the year mentality as to how to get out of it without alienating their constituencies. There was an attempt when the Diabetes group came through to shunt them aside, yet the emotional nature of the discussion made that impossible to do. The Diabetes group also convinced them that there was a great deal of information waiting in the wings and all it needed was the money to get it out on center stage.

In the final session the whole group came back together again and the AAMC briefly described what they can do to help us.

1. Judy Braslow's office can help us follow Federal legislation.
2. They can serve as a resource bureau to get the people on Capital Hill in touch with specific specialists.
3. Holding small policy-making seminars in which they would try to help the biomedical community produce some strong positions which have wide acceptance. They were very aware of the danger that we would come up only with the lowest common denominator, which is about as useful as nothing at all.

Their suggestions to the Societies, and particularly to the Society representatives, were:

1. Contact your local senators and representatives and make firm friendships, both with them and their staffs. Let them know what you are doing and how Federal support is flowing back to their districts.
2. Contact the staffs of the committees crucial to your interest. Keep in touch with them. We can probably be most successful by developing a degree of friendship so they will call upon us. It was very clear from the discussion during these three days that the staff have a very wide group of contacts throughout the country. They are not at all hesitant to pick up the telephone and call people. When they do call people, however, they want answers right then and there.

It was clear that one of the items on the hidden agenda was a friendly interaction between these very crucial congressional staff members and a good cross section of active biomedical faculty. Certainly, the surroundings and the fact that we had the place to ourselves helped enormously. I think we did a great deal of good. The AAMC staff did a great job of organizing the meeting.

A number of questions can be raised relating to policy of the Society. The first one is how much interaction with Congress should we try through the Society, how much should we try through FASEB? This is certainly related to how many dollars are we prepared to spend on bringing members to Washington and how frequently. It would certainly be my intention every time I come to Washington to spend an extra day or two making the rounds on the Hill.

Brian A. Curtis, Ph.D.
Peoria School of Medicine
University of Illinois College of Medicine

SURVEY OF DEPARTMENTS OF PHYSIOLOGY

William F. Ganong, M.D.
President
Association of Chairmen of
Departments of Physiology

The Association of Chairmen of Departments of Physiology conducts an annual survey of Departments of Physiology and makes the results available to its members. Since much of the information obtained is of general interest, the Council of the Association has decided to make the results more widely available, and the American Physiological Society has generously agreed to publish the survey data in The Physiologist. We would appreciate comments and suggestions for additional items to be included in the Survey in coming years.

ANALYSIS OF ACDP QUESTIONNAIRES – 1976

	1974	1975	1976
TOTAL QUESTIONNAIRES	96	91	87
A. Medical Schools	88	81	81
Arts and Sciences Colleges	4	7	4
Osteopathic Medicine	1	1	1
Veterinary Medicine	1	2	1
B. Public	65	62	56
Private	29	29	31
ACADEMIC POSITIONS			
Professors (Including chairmen)	406	425	424
Professors (Excluding chairmen)	312	334	340
Associate Professors	334	328	337
Assistant Professors	390	410	374
Instructors	71	62	44
(Salary information detailed on attached charts)			
UNFILLED POSITIONS			
Professors	12	17	7
Associate Professors	23	17	11
Assistant Professors	39	43	39
Instructors	2	4	4
FACULTY STATISTICS			
Full-time paid positions (Mean)			12.9
Part-time paid positions (Mean)			1.0
Joint Appointments (Mean)			1.2
Joint Appointments Paid by Other Sources (Mean)			.8
Non-paid Volunteer positions (Mean)			2.8
Total faculty positions (mean)			18.6
FRINGE BENEFITS			
Percentage rate of fringe benefits (Mean – based on 79 responses)			16.37
Highest percentage rate reported			34.00
Lowest percentage rate reported			6.00

GRANT AND CONTRACT OVERHEAD RETURNED TO DEPARTMENT?

YES – 11
NO – 76

If "yes," percent returned (Mean – based on 9 schools) – 18.8
Highest percentage rate reported – 50.0
Lowest percentage rate reported – 7.5
No set rate (4 schools)

If "yes," for a specific purpose?
YES – 3
NO – 7
NOT STATED – 1

BUDGET (Nearest Thousand) — Mean

	1972	1973	1974	1975	1976*
Institutional sources	218	219	290	351	378
Research grants	227	261	337	392	502
Training grants	36	41	41	38	44
Budget support — other than above	40	28	41	40	26
TOTAL	521	549	709	821	950

*Based on 85 departments

GRADUATE STUDENTS AND POSTDOCTORAL FELLOWS (Based on 85 questionnaires)

Ph.D. degrees granted	253	205	179	189	152
Mean	3.8	2.2	2.6	3.2	1.8
Ph.D. students enrolled	767	658	1046	1124	1046
Mean	11.6	11.2	12.1	14.4	12.6
Postdoctoral fellows (current)	211	205	275	299	404
Mean	3.2	3.5	6.5	5.4	4.8
Postdoctoral fellows (completed training 7/1/75-6/30/76)					132
Mean					1.6
Vacant postdoctoral positions					45

Do you have a training grant that supports predoctoral trainees?

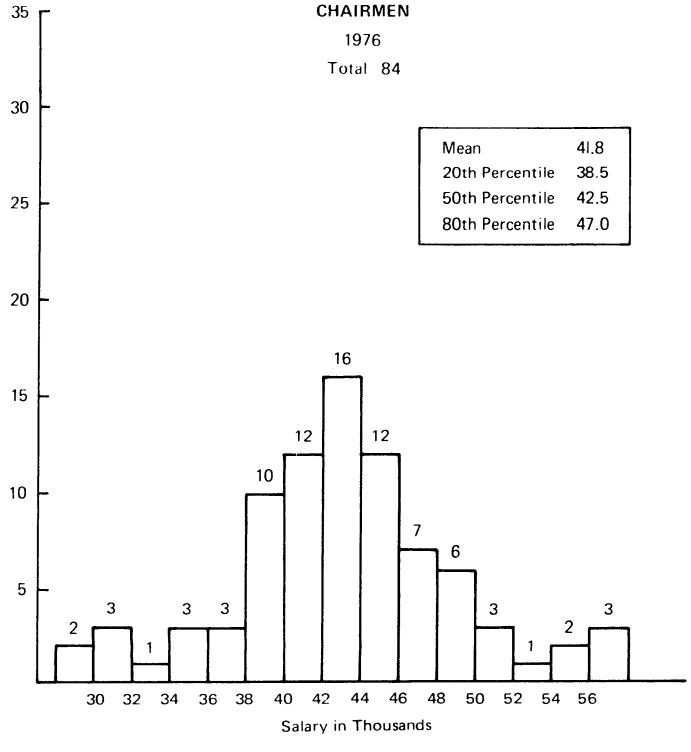
YES — 38

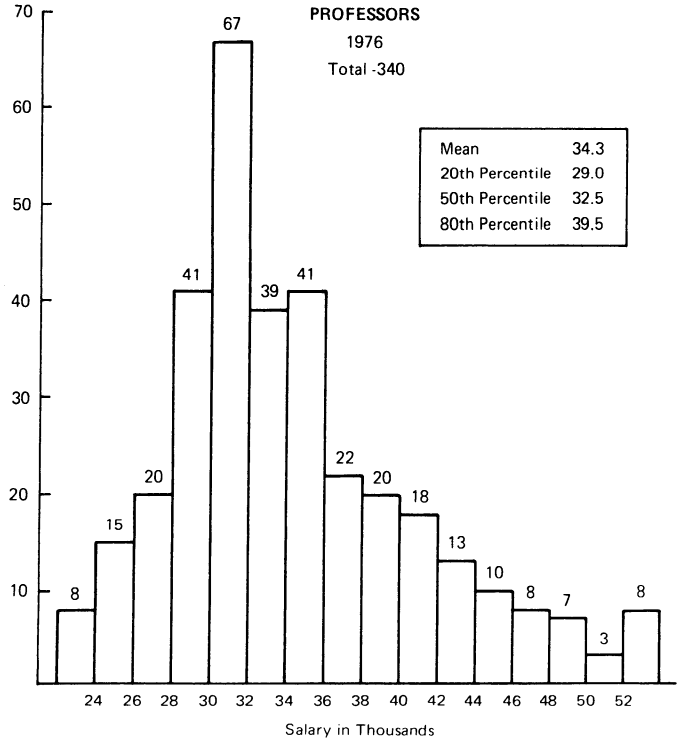
NO — 47

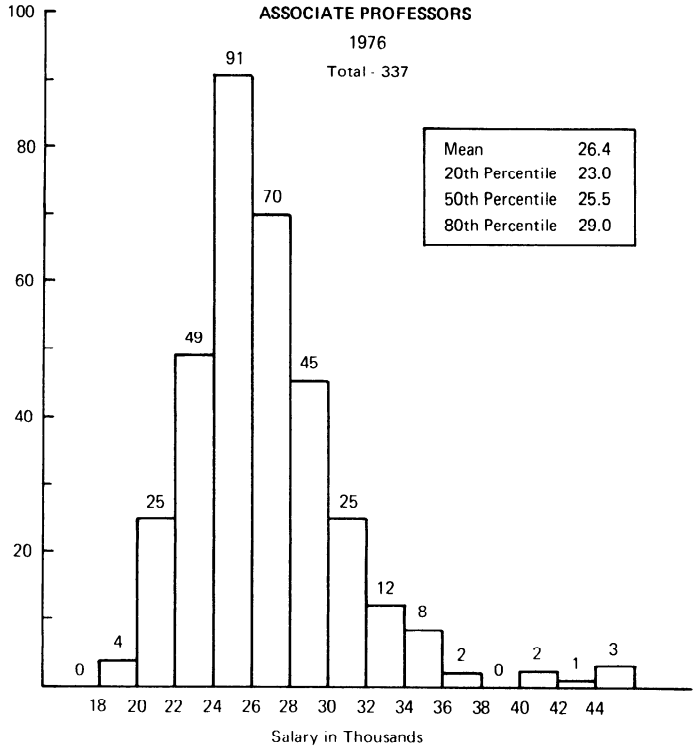
DEPARTMENTS OFFERING COURSES IN ALLIED HEALTH

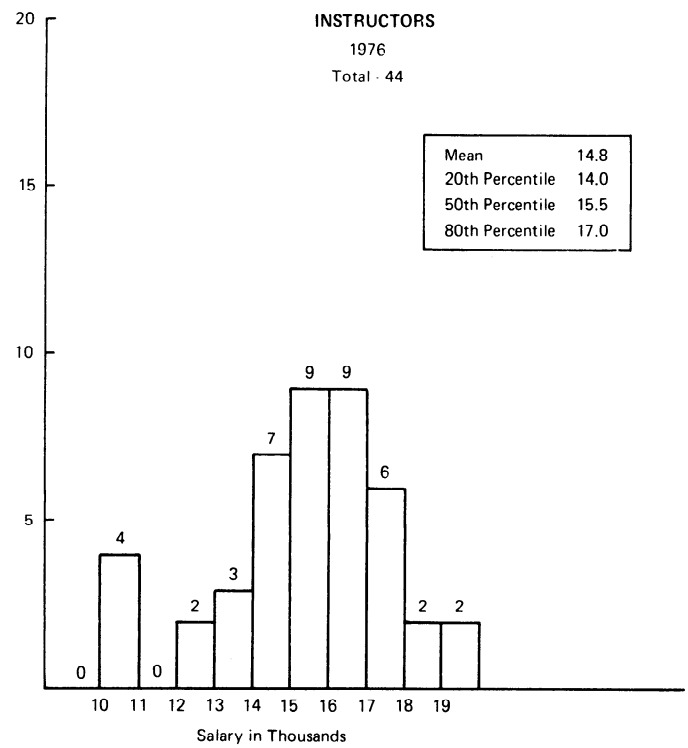
Dentistry	12
Pharmacy	14
Nursing	22
Other*	29
Undergraduate Arts and Sciences	3

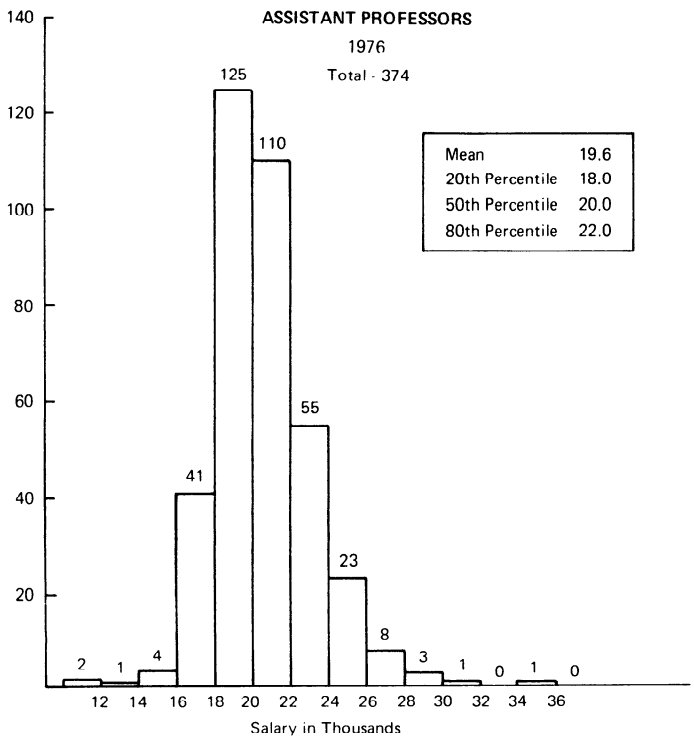
*Physical therapy, Occupational therapy, Medical technology, Dental hygiene, Radiation technology, Nuclear medicine, etc.











BELTONE INSTITUTE FOR HEARING RESEARCH

The Beltone Institute for Hearing Research announces the forthcoming issue of an English translation of a monograph, No. 30 in their series. It is entitled "Sound Localization, Neurophysiological Mechanisms." It was written by Jacob A. Altman of the Pavlov Institute in Leningrad. The original monograph was published in Russian in 1972. The date of issue of the translation will probably be around the end of May, 1977. Interested readers should direct their requests to: The Circulation Manager, Translations of the Beltone Institute ofor Hearing Research, 4201 W. Victoria Street, Chicago, Illinois 60646. Translations are issued by the Institute free of charge.

PIO ISTITUTO DI S. SPIRITO ED OSPEDALI RIUNITI —ROMA
ANNOUNCEMENT OF COMPETITION
for the 1977"Giovanni Maria Lancisi" International Prize for Medical Science (Lit. 10,000,000)

Art. 1

The "Pio Istituto di S. Spirito" General Regional Hospital Agency, registered in Rome, in collaboration with the Accademia Lancisiana di Roma, hereby announces a competition for the 1977 "G. M. Lancisi" International Prize for Medical Science, of Lit. 10,000,000, to be assigned to the author or authors of an original, unpublished scientific work dealing with the following subject.

"METHODS OF REVASCULARIZATION OF ORGANS IN MAN"

Art. 2

Those intending to enter for the Prize should send the following to: "Premio Internazionale Scientifico 'G. M. Lancisi' presso la Presidenza del Pio Istituto di S. Spirito ed Ospedali Riuniti, Borgo S. Spirito, 3-00193 Roma (Italy)" by and not later than 12:00 hours of the 31st December 1977:

1) Application on plain paper to participate in the competition;
2) Nine copies of the work entered for the Prize, in Italian or in English, each copy bearing the signature of the author or authors. All works, accompanied by positive copies of any illustrations, should include a synopsis in Italian or English, of not more than one thousand words, as well as a full bibliography of the subject dealt with;
3) Certificates testifying to the birth, the residence and the citizenship of the competitor or competitors;
4) A copy of the curriculum vitae and of the scientific activity of the competitor or competitors, with a list of published works;
5) A declaration signed by the author or authors testifying to the original nature of the work, the institute or other place where the work has been carried out, that said work is unpublished and it has not been submitted, until such time as the "G. M. Lancisi" Prize has been assigned, to other prize competitions.

Failure to observe even one of the abovementioned rules will entail exclusion from the competition.

Art. 3

The Prize is assigned according to the decision of a scientific committee appointed by the Chairman of the Accademia Lancisiana, and said decision is final.

Art. 4

The Prize may not be assigned more than once to the same person, even when said person participates jointly with others.

Art. 5

Further information and clarification may be requested by applying to: "Premio Internazionale Scientifico 'G. M. Lancisi' presso la Presidenza del Pio Istituto di S. Spirito — Borgo S. Spirito, 3-00193 Roma (Italy)."

NSF CHAUTAUQUA-TYPE SHORT COURSES

Come October the 1977-78 series of NSF Chautauqua-Type Short Courses for College Teachers will offer the opportunity for some 3,000 college teachers to participate. Approximately fifty short courses will be offered in the 1977-78 academic year at field centers throughout the U.S. The program is designed to enable college teachers to keep abreast of advances in a variety of fields of science and to help them incorporate these advances in their teaching. The program is conducted by the AAAS with support from the National Science Foundation.

Typical short course topics during the 1976-77 series were: Origins of Life, Ethical Issues in the Life Sciences, Solar Energy, Psychology of Women, Biofeedback, Fundamental Particles: Quarks to Quasars, Genetics and Society, Plate Tectonics, Privacy, Population, and Microcomputers Applied to Science Education.

Participants meet in groups of 25 for two-day sessions in the fall and in early spring. During the interim participants are able to work individually or in teams on problems or projects related to the courses.

Further information is available from the AAAS. A preliminary announcement poster, available in May, lists the course titles and directors. A program announcement brochure, available in July, gives full details — course descriptions, directors, field center locations, schedules, eligibility and application forms. Please write to:

American Association for the Advancement of Science
Office of Science Education — Box C4
1776 Massachusetts Avenue, N.W.
Washington, D.C. 20036

APPEAL FOR BIOGRAPHY MATERIAL

For a biography of Dr. Alton Ochsner of Ochsner Clinic, New Orleans, opinions, evaluations, anecdotes, reminiscences, photos are needed. Photos will be carefully handled and returned. All material gratefully received by:

Ira Harkey, Ph.D.
401 Metairie Road, 706
Metairie, Louisiana 70005

NEWS FROM SENIOR PHYSIOLOGISTS

Hayden Nicholson to Bruce:

Those notes from Senior Physiologists are very gratifying to us old folks. I do enjoy reading the summaries published in the Physiologist. I make occasional visits to medical schools in connection with their accreditation surveys. My wife and I are in good health. Last winter I had a detached retina in Miami; it was repaired efficiently and promptly by friends in the Department of Opthalmology of the School of Medicine, University of Miami. Our chief recreation at home is gardening. I am enjoying my retirement very much.

Pharo Gagge to Bruce:

Like they did to Jim Hardy, Yale retired me last June since I had passed my 68th, so I am now Emeritus Professor of Environmental Health, which responsibility actually had taken a very little of my time. However, I am still working full time at Pierce as Deputy to Arthur DuBois, our new Director. The Pierce Foundation is kind to us "oldsters," who dislike to stop working, and hopefully will keep me on till 1 February 1978.

Other news from Pierce—Jim Hardy comes in nearly every day (is an ardent golfer). Arthur DuBois and Jan Stolwijk, our Associate Director, both have active programs in environmental health, air pollution, thermal physiology, exercise, as well as the modeling of human temperature regulation and the environment. Actually, the Laboratory is very fortunate in having a large group of bright and active young physiologists, psychologists, biophysicists, biochemists and engineers to support our multifield research.

My present effort for our Society is serving as an Associate Editor under Leon Fahri with the new "Journal of Applied Physiology." Both in this capacity and as a former section editor, I have enjoyed calling for editorial help from such people as yourself, Ed Adolph, Sid Robinson, Jim Hardy, Alan Burton and other senior physiologists. Their reviews have all been of great personal interest to me and hopefully of considerable insight and education to many new young authors in our Journals. I think back what a tremendous help were the detailed reviews by the late Cuthbert Bazett and Eugene DuBois of the first papers from C. E. A. Winslow and the Pierce on "Partitional Calorimetry" back in 1937, when we started eager but naive.

Bob Haupt to Bruce:

According to the University practice I can continue in my present capacity until I reach age 70. The only difference is that at age 67 I am on a 9-month appointment rather than 12.

My teaching duties for the past several years have been in the field of anatomy rather than in physiology. I still like to think of myself as a physiologist however. I just finished a revision of an elementary Physiology & Anatomy lab manual that is rather widely used throughout the U.S. and Canada. It is designed and utilized by courses particularly designed for students going into (or hoping to go into) the paramedical and health-related fields. Here it is taken by the Physical Education majors, Home Economic majors, and various and sundry others who are interested in pre-physical therapy, medical technology and occupational therapy, etc. It is a highly populated course in most colleges and universities as you know—and that makes for good sales and hence substantial royalty income for the author. It is published by MacMillan, and this is the fifth edition and about the 12th or 13th printing. I am also a coauthor of a number of other lab manuals in general zoology, kinesiology, comparative anatomy, and one on biological illustrating which is my specialty.

It has been 7 or 8 years since I was studying the mechanical aspects of muscle contraction. I was utilizing an ultra-high speed motion picture camera to study contraction and relaxation phenomena in vertebrate muscle preparations. The particular finding that intrigued me most was the extremely rapid relaxation that a muscle shows, as rapid, in fact as any phase of the contraction phase. This, it seemed to me, strongly suggested some truth to the rather old and somewhat controversial idea of active relaxation of muscle. If the Hansen-Huxley Model can account for the shortening of myofilaments it could also account for the rapidity of the early stages of muscle elongation (relaxation). Unfortunately this type of work is extremely expensive. When my N.S.F. grant ran out I was unable to find another means of support. Again with the help of a Government grant I secured the necessary equipment (camera, recording equipment, lights, tape, etc.) by which I prepare my own and other instructors' videotapes in full color. Needless to say this is a timely and very current approach and continues to the present time. Since I have been producing color video tapes on my own, without the usual aid of experienced television technicians and commercial equipment and personnel I have discovered that one has to become somewhat expert in camera and lighting techniques, in sound recording.

Lastly I will be so bold as to mention another type of endeavor which is now occupying more and more of my time. Quite a number of years ago I became aware that there are many talented zoology and biology students who would very much like to break into the professional field of biological or medical or scientific illustrating. Until the past few years there existed in the U.S. only a very few places where students with artistic ability could get advanced training along any of these lines and that was only in the field of medical illustrating. For a number of years I accepted a number of individual students each year as "special problems" students and helped them with the techniques and the knowledge needed to produce good quality anatomical or conceptual illustrations for publication. About 5 years ago I initiated a catalog course of one quarter duration, once a year. Every year since, I have had a class of 60 students (always 60 because I was forced to set that limit). They turn out some truly remarkable illustrations.

John Lawrence to Bruce:

I am continuing my scientific work and writing, going to medical meetings, taking part in seminars, and giving talks. In addition I am a member of the University of Califoria Board of Regents. I cannot take any other positions, as I keep pretty busy with all these activities.

Donner Laboratory is going strong. It has a fine new director, Dr. Edward Alpen, and I am still working in the lab with a research project and a couple of post-doctoral fellows training in nuclear medicine.

Doug Lee to Bruce from St. Thomas:

We moved into our new house here early in March, with no more than the usual number of minor problems that always await a happy homeowner. As in honor bound, I have been observing the principles of tropical housing, clothing and work that I have so often preached. The house is built ranch style, to conform to the hilly terrain and a large rocky outcrop, and particularly to secure a 240° view of ocean and islands. As

advocated, it is one room deep, with adjustable louvers above and below fixed windows, so that cross ventilation can be controlled without detriment to the view. Most of the time there is a steady trade wind, 10–20 knots, that makes it possible to do without air conditioning–and, of course, without heating. Energy conservation is not a problem. The ranch style, with the outcrop, provides a patio on the south side, and an 8 ft. veranda on the north side gives alternate outdoor space. There is no weather that cannot be enjoyed at some location. (Perhaps I should except the experience of a violent thunderstorm while seated in what is virtually a glass box.) We have ordered boards to bolt in place over the picture windows in case of a hurricane. Actually, the risk is small; a special Supplication Day each year has persuaded the Lord to keep hurricanes away since 1938.

My working regimen is something of a compromise: early breakfast, desk work or downtown business in the morning, 2-hour lunch break, gardening 2–5, cocktails, dinner 6–7, then relaxation and reading. The gardening might be better done in the morning, but then I would never get any desk work done in the somnolent afternoon. This is a chronic problem in the tropics, for which I have advocated arranging matters so that you go to check on how somebody else is working in the afternoon! I don't have anyone else to check on here!

The desk work at present consists of what I hope are the final stages of getting a definitive text on "Asbestos and Disease" to the publisher next month. As several of the details rely on compliance of the staff at Mt. Sinai Medical School, New York, I am not in complete control of the mechanics. The senior author, of course, is Dr. Irving J. Selikoff–Mr. Asbestos USA. But he is a very busy man not adapted to the trifling but critical details of publication. My New York visits involve a bit of head-beating, as well as serving in many other ways to remind me how fortunate we are to be out of it all in the Virgin Islands.

An ancillary hobby, in which we have just had our first exercise, is entertaining friends on a visit, and showing them some of the delights of the island. We have a nice guest room, done more or less in Japanese style, and accommodation for two children is available as well. We wil be happy to hear from those who would like to take us up on this. For more lengthy stays we can usually locate a condominium apartment, but the rates are pretty high in the season (15 Dec.–15 April). Actually any time of the year is good, with perhaps some hesitation about 15 September–15 November, when the wind can be erratic and the rain more frequent, producing hot and/or humid spells. Should anyone touch port here on a tour, we would be glad to pick them up and show them aspects of the island that official tours omit. We would like to have advance notice, of course.

I remember with affection and gratitude my introduction to American physiology at the Fatigue Lab. in 1935 and the wartime papers that were greeted with enthusiasm down under.

Pete Scholander wrote Bruce from Seattle:

Your letter reminds me of happy times I had in the Fatigue Lab when first I met you back in the 40's. Larry Irving and you somehow had gotten me in there to work out, with Jack Roughton, a micro test for CO in blood. One day I asked this great carbonic anhydrase transport guy: what chance Jack do you give me that a hemoglobin solution might accelerate oxygen diffusion? The answer was 2%!

Another highlight was this: in Oslo I had worked with students (now professors and what have you) on gas secretion in swimbladders of fish, of which the most famous one was the whitefish living on the bottom of Bodensee in Switzerland, secreting some 10 atm pure nitrogen. Telling your colleagues about it they spared no time in informing me that there was a much better whitefish in nearby Charles River. Sightings clearly demonstrated population explosions once in awhile, and particularly on Sunday mornings. Now, it so happened that Larry had been entrusted with an exposure suit developed by the Norwegian merchant marine. The war was on and it fell upon me to test the suit. So while submerged to my neck in that Blaue Donau of Boston: there they were, bobbing gently amongst icefloes–the Charles River whitefish, and only males at that! Ach so! not po but co pulation!

What am I doing now? Having recently finished an earth-shaking monograph on osmosis with colleague Ted Hammel, I have in company with Larry's daughter Susan been traveling all over the U.S. in our van camper on a self-invited lecture tour to numerous friends, incidentally quoting an old Alaska saying "a drop of oil in the machinery never hurt nobody." So, wining and dining our way through Texas, Florida, Carolina, Penn., Woods Hole, Maine, Toronto, Glacier Park, etc., our campaign or rather champagne lecture-trip finally brought us back here in Seattle, looking across Lake Washington to beautiful Mt. Rainier–only to find a paid-for invitation to the Centennial Physiological meetings at Cambridge. So off we went to England, and rented one of them left-sided cars we toured from Loch Ness to Plymouth. Back here again we bolted as fast as possible to our little fly-in cabin at Eliguk Lake in the midst of BC wilderness. The Hammels joined and we indulged in the most tremendous "idiot" fishing you can imagine, for three weeks! For hors d'oeuvres: light smoked trout au Dill* served with Hammel's Eliguk Special (half and half Olde English Cyder 7% and Ron Anejo 40%). Then for dinner "Truite a la Eliguk" with wild mushrooms**oh la la!, served with Spatlese smuggled from Max Planck Institute by our honored guests.–Kind of busy, hey?

Now then: am I interested in an administrative job? I simply don't have time, and you would never get a recommendation from Scripps!

In a hand written note you ask me about my "activities and affairs: in La Jolla. I am flattered by the latter, being a feeble 70, however, I may not know how hot it is in Boulder, but I tell you, such a question in societal La Jolla is an absolute no no!–Regarding religious affairs on the other hand, my denomination is best illustrated by what happened to Larry Irving one lovely winter day at Point Barrow. Walking to the lab he met one of his many great Eskimo friends who had just attended a lecture in the mission church, or rather shack. Said he: "Larry, I know airplanes, I know helicopters, I service them, what the hell is an angel?"

Finally: what about scientific activities? I hope to do some simple experiments on the "Hammel effect" which latter explains elegantly deviations from van't Hoff's law in solutions (dilute or not) as a direct function of molecular size.–Then, as a stark outsider, I am fascinated by the profound topological blackout which looms over DNA replication, such as: unwinding a million turns in a 10 minute cell division requires 100,000 RPM; how about torque accumulation in opened loops? One single twist in an otherwise parallel copying device interlocks the replica, etc. How would you like a helical zipper?

*Dill: a spicy herb.

**Wild mushrooms: a hobby from childhood.

Hasta la vista, your admiring Pete heading for the nearest black hole in alpha Centauri.

Hurley Motley to Bruce:

I have enjoyed very much reading the letters concerning the senior members in the Physiologist. Since becoming Emeritus Professor of Medicine at USC, I have kept fairly busy with a number of other activities. This year I am working as Director of the Pulmonary Function Laboratory at the new Hospital of the Good Samaritan in Los Angeles. I am also Director of Research at the Hospital of the Good Samaritan. From 1966 to 1976 I was a Trustee of the American Registry for Respiratory Therapy, later name changed to National Board for Respiratory Therapy. I found this very interesting in helping to improve the quality of respiratory care by technicians and therapists. There has been a tremendous improvement in the quality therapy as a result of the program for Certifying Respiratory Therapy Technicians and the more advanced grade of Registered Therapists. I received a Commendation in January 1976 from the Board of Supervisors of Los Angeles County for service on the Scientific Committee of the Los Angeles County Air Pollution Control District for the past twenty years. I was chairman of this committee for the last seven years. I am also a member of the Committee on Environmental Pollution of the Los Angeles County Medical Association. We still have an air pollution problem in Los Angeles. I was appointed for a four-year term in 1974 as a member on the Coal Mine Health Research Advisory Committee (NIOSH) of the Department of Health, Education and Welfare. I am also a consultant on pulmonary disease for the Bureau of Hearings and Appeals of Health, Education and Welfare here in Los Angeles County. I am in fairly good health and enjoy the activities listed above. I enjoy the climate in Southern California, even with the smog, and I would be very reluctant to move to another area in the future.

Will Forbes to Bruce:

I am still interested in problems of public health especially those related to longevity. I consider longevity one of the better (and perhaps the best) indicator of the success of medicine and public health and it therefore interests me that this country makes such a poor showing in longevity. Back in 1912 we were about 12th among the nations of the world in longevity (as measured by the average expectation of life at age 10) and have been slowly sinking since then so that now we are about 35th instead of 12th. Almost all of Europe except Albania is ahead of the U.S.A. We spend a great deal on medicine and get lots of medicine but get very little longevity out of it.

It is only our males who have a short expectation of life. Our females are up fairly near the leaders, i.e., we are about 10th in longevity for females surpassed only by the Scandanavian countries, New Zealand, Australia and Canada and one or two others.

Leland Wyman to Bruce:

My wife and I are still active in our garden and traveling occasionally after 56 years of marriage. I have not done anything about physiology since I retired in 1962 but I have been active in my other field, the ceremonialism, art, and sandpaintings of the Navajo Indians of the Southwest. We have spent several summers in Flagstaff, Arizona, where I am "curator of archives" at the Museum of Northern Arizona, and I have brought out six books and numerous papers in that field since I retired, and have two more in press.

Hubert Catchpole to Bruce:

Since my last report I have taken on a new lease of life as Professor of Histology at Illinois College of Dentistry and continue to publish and/or continue battles with editors. Why is it that the potboilers can be published with ease whereas anything with the glimmering of an idea spells a fight? Perhaps this is a question for oldtimers to ponder, as it's not the way things used to be.

Aldo Luisada to Bruce:

I have continued to expand and increase the scope of the Caraiology Department of Oak Forest Hospital. We have now 4 well-trained cardiologists and 4 cardiology residents on the staff plus an occasional senior student from one of the medical schools. We have 6 cardiac wards with a total of 180 beds. We have a good Electrocardiography and Vectorcardiography Laboratory and an excellent Laboratory for noninvasive studies. Among the latter, we now take routinely impedance cardiograms for measuring cardiac output. The hospital has now opened a Critical Care Unit and a Stress Laboratory which are partly under my direction.

Last year I selected some of the best tracings recorded in the last 35 years and I have prepared an Atlas of Non-Invasive Methods, which should appear next month.

An active and interesting life is the best remedy against the ills of old age.

Rob Clarke to Bruce:

I have remarried and am living happily and quietly among old familiar scenes and many friends old and young.

Ashton Graybiel sent Bruce some of his recent reprints dealing with vestibular side effects, especially motion sickness. He added that he is not free to move about because of the heavy investment here in large and expensive devices.

His wife Moira has bronchiectasis and emphysema but is able to carry out many chores that involve only light work. Much of her time is spent reading.

Their daughter Ann is associate professor in neuroanatomy at MIT and son Ashton is practicing rheumatology in Pensacola and has an appointment as visiting Clinical Professor of Medicine at LSU in New Orleans.

Al Behnke to Bruce:

My interest continues (1) in inert gas transport relative to quantification of body fat and water, (2) in reduction of football impact injuries by use of energy-absorbing materials as lightweight styrofoam or equivalents, and (3) in measures to maintain fitness of diving and submarine personnel.

With reference to (1) I look forward to a visit to Loren Myhre at Brooks Air Force Base. His ethanol technique (with Ulrich Luft) for determination of TBW is not only appealing but remarkably reliable. Combined with nitrogen extinction measurements at 34,000 ft. simulated altitude and densitometric estimation of body fat, we are in a position to move forward since you reviewed our studies at Harvard in 1933. In regard to (2), I am trying to adapt an outline you provided in wartime for the various steps to develop dry rations. Chiefly the football problem, apart from elimination of the cutlass (clothesline) tackle is acceptability of radically new equipment materials by day in and day out tests on a football field. In the matter of fitness of diving and submarine personnel, apart from motivation, the problem is acceptability of innovations in the routine of diet, exercise, and watch-standing. Lundgren's incorporation of nicotine in chewing gum (if implemented) may diminish or eliminate smoking.

On the personal side, I am reducing to my low-level 1938 weight and keeping reasonably fit to apply recompression in the occasional diving case and to participate (if acceptable) as a subject in nitrogen transport studies.

Austin Henschel to Bruce:

My activities continue about as usual. I serve two days a week as a consultant for the Institute of Occupational Safety and Health. During the spring quarter I teach a graduate course in Occupational Ergonomics at the Institute of Environmental Health, University of Cincinnati Medical School. This fall I will go to the University of Alexandria as a World Health Organization consultant for two months. There I will introduce my course in Occupational Ergonomics into the curriculum of the High Institute of Public Health at the University of Alexandria.

Helen and I are in good health and remain physically and socially active. We seem not to have enough time to do all the things we are interested in. I still paint whenever there is time.

Charlie Hassett to Bruce:

The Medical Laboratory (at Edgewood) is now headed by a Col. Cadigan, who arrived in July. His assistants, Lt. Cols. Kyser and Chipman are able and interested in getting the laboratory back to work. Last year there was an almost complete stoppage of research because of the political fracas and the effects are still apparent. The laboratory appears to be headed for Class II status under the Surgeon General's Office. All work must now be reviewed by TSGO and the volunteer program is still under suspension.

Joe Holmes demonstrated the use of ultrasound when Bruce was in Denver in September. The movements of an 8-month fetus were displayed on a screen for the mother and us to watch. Joe described in a letter applications in medicine and potential for physiology.

I am glad you were able to see the ultrasonic lab since I had told you about it on many occasions. Now that diagnostic ultrasound has really assumed a definite role in the diagnostic services in many hospitals, it seems worthwhile in reviewing a bit of its development as a medical diagnostic technique.

We first started working with it here at the University of Colorado Medical Center back in 1950. After the demonstration by Howry here in Colorado, by Wild in Minnesota, and by Ludwig in Pennsylvania, that when ultrasound was sent into the human body reflected echoes came back from various anatomical structures within, the concept of using ultrasound diagnostically became a reality. Unfortunately, a great deal of time had to be devoted to devise and develop techniques for displaying this echo information in a way the physician could use diagnostically. We can attribute to Dr. Wild the discovery that pathological lesions such as tumor had a different echo pattern from the normal. Dr. Howry and our group here at Colorado developed the concept of compound scanning which made it possible to reproduce an anatomical-type picture of structures within the body. This was always displayed as a cross section since the sound went in through one plane. When pulsed at 400 times per second, the transducer became quiescent after each pulse and received the signal. This was amplified and displayed on an oscilloscope screen as a single bright dot. By moving the transducer around the body surface, the dots coalesced to give the outline of structures like the liver, the spine, the kidney, and so forth. When there were irregularities of the organ interface, then by rocking the transducer as it was passing across the body surface, one could get a more complete picture of the anatomical contour. This was called compound scanning.

The two areas of greatest clinical interest have been obstetrics and gynecology and echocardiography. Over 50 percent of our routine clinical load is in obstetrics, where the fluid filled uterus offers ideal contrast for examination of its contents. When the echo producing anatomical structure has motion, the dot moves back and forth on the oscilloscope screen. By applying appropriate sweeps, one can demonstrate in wave form the motion of various intracardiac structures, for example, the valves and ventricular walls.

Another area in which ultrasound has proved diagnostically useful is the abdomen. Because of its greater sensitivity to density differences, ultrasound in the form of image processing can display tissue structures not seen by x-ray except with use of contrast media. However, it should be remembered that the ultrasonic wave is a pressure wave and not an electromagnetic wave. Therefore, it behaves in tissue in a somewhat different fashion than x-ray and also to date has no demonstrated toxicity at power levels required for diagnostic studies.

Recent equipment has the capability of displaying differences in echo height either as eight shades of gray or as colors. This means one can observe differences in density within an organ such as the kidney and perhaps detect changes caused by a disease process such as glomerulonephritis.

It became apparent early in ultrasonic scanning that tumors had different echo patterns, depending upon the type of tumor present. Therefore, there has been a desire to achieve tissue characterization which means by measuring certain ultrasonic parameters within the tissue one can describe more effectively the nature of a pathological lesion. Thus, it has been possible to develop an ultrasonic microscope which makes it possible without staning to look at differences in density, microscopically in tissue slices. In addition, ultrasound is being developed for tissue characterization by making specific measurements within tissue such as attenuation, velocity of sound transmission, spectrum frequency analysis, echo scatter, and impedance coefficient, all of which has the potential for characterizing a pathological lesion in vivo without the need for biopsy.

The other area in which ultrasound seems to provide unique diagnostic information is that of real time scanning. By employing a multiple transducer with 64 to 120 elements combined in a single unit, one can rapidly and successively display an anatomical area to the point where one can obtain up to 40 frames/sec in depicting cardiac, aortic, or fetal movement. In obstetrics it is quite possible to watch the baby move, even to sucking its thumb, and, of course, to record fetal respiratory and cardiac movements. What is even more important is that with refinements in technique one is going to be able to examine the contractility, for example, of the myocardium directly following a coronary occlusion and thus determine how it is recovering. It is quite amazing to be able to look at the heart without need for contrast media and see the various elements moving in a coordinated pattern. This technique is also applicable to viewing the motion patterns of the aorta and other large vessels.

Whereas in image processing ultrasound may have some serious competition with the new CAT, in such areas as real time scanning and tissue characterization ultrasound promises to provide information not available by other techniques. In addition, its current, extensive use in the hospital is a revelation to someone who saw it start as a result of the development of materials testing and sonar during World War II into

an extensively used diagnostic tool in medicine. Furthermore, the equipment is becoming simple enough so that one can anticipate that within the next year or two there will be routine use of such equipment in the office of the cardiologist, the ophthalmologist, and the obstetrician.

It would seem to me because of its noninvasive character that ultrasound should be used much more than it is by physiologists and pharmacologists to study physiology in humans where its noninvasive character has an advantage over all other techniques. Furthermore, one has the capability of doing followup studies on the same patient from day to day when testing a new drug therapy.

Ed Van Liere to Bruce:

It was good to hear from you and to know as you put it "all goes well on the desert." We have had a very cool summer, and a miserable month of October—cold and rainy.

All goes well with the University here: an enrollment of over 20,000. The Medical Center is going along well too. So many of my old colleagues have either retired or are dead that it is rather depressing. I miss them.

W. Va. is going through a mild "boom," for as you know there is a lot of coal in the state and considerable oil.

Have stopped doing biological research, but do some writing and also do considerable work for the "W. Va. Med. J." and a little for the JAMA.

Hannes Benzinger to Bruce:

For samples of recent activities I enclose two reprints, Nature 229, 100 (1971) and a chapter of Zotterman's book, "Sensory Functions of the Skin," Stockholm 1976, and also copies of the front pages of two books for L.L. Langley's Human Physiology Benchmark Series.

The two volumes of the Benchmark Series Hannes edited were "Temperature, Part 1, Arts and Concepts" 1976 and "Temperature, Part 2, Thermal Homeostasis" due for publication in 1977. These are published by Dowdin, Hutchinson and Ross, Shroudsburg, PA, L.L. Langley Series editor. Earlier Hannes wrote a chapter for the first volume of the Benchmark Series entitled "Homeostasis: Origins of the Concept." L.L. Langley, Ed. 1964. This volume included papers by Claude Bernard, L.J. Henderson, W.B. Cannon and J.S. Haldane.

In 1963 Hannes was awarded the Sheele Gold Medal by the Swedish Chemical Society. He enclosed reprints for Society Archives.

Leslie Chambers to Bruce:

I continue in excellent health and fully active in a professional appointment, taking pride in having been requested to continue well beyond normal "mandatory" retirement. In addition to a normal load of teaching and student guidance, I have become increasingly involved in a consultative role in the development of community health programs in Latin America. Ione and I have had four periods of travel among the primitive cultural groups in the high Andes, in southeastern Mexico and Sinaloa in addition to participation in advisory capacity with Public Health agencies in Mexico. Apparently life begins at seventy.

I have no plans for retirement and will make none until I or the University of Texas, or the inexorable "one-hoss shay" syndrome change my perspective.

Otto Edholm to Bruce:

Many thanks for your letter in which you ask me to tell you about my activities. I retired from the Medical Research Council in 1974, and the Division of Human Physiology was dissolved. The work of the Division and a number of those who worked there have transferred to the Environment Physiology unit under Professor J. S. Weiner.

I have been fortunate enough to be appointed a Visiting Professor at the School of Environmental Studies at University College London. This used to be, some years ago, the Bartlett School of Architecture. The new name indicates that the interests of the school have broadened; specifically a department of environmental design and engineering was established; Ralph Hopkinson was appointed to the chair. He and I had become acquainted in the early days of the Ergonomics Research Society and have maintained contact, through our mutual interest in environmental problems. I now take part in teaching, describing the physiological features of thermal sensation and comfort, as well as the basic physiology of sensation. It is of course extremely interesting and stimulating to become involved in quite a new world of learning, and I am greatly enjoying my contacts with staff and students.

I also have a personal grant from the MRC, which enables me to take part in a research program, together with Ralph Hopkinson. We are studying physiological responses to quite mild stimuli and the way in which these may be modified by personality characteristics. Although we find very considerable unusual differences, personality characteristics apparently account for only a small proportion of these differences.

I have found great pleasure and satisfaction in the two years of retirement, due to my good fortune in being able to continue interesting research and teaching, without the worries and anxieties of administration. Of course, the example set by yourself and so many other senior members of the society is a great encouragement to those like myself who have just entered the ranks of the "retired."

Ancel Keys to Bruce:

We celebrate Christmas here with our three children and five grandchildren and leave for our "Minnelea" on December 29th. For the third year Margaret has a date to roast a turkey, being raised for the occasion by the mayor, on New Year's Day, the Jeremiah Stamlers, the Mayor's family and ourselves sitting down to table together to eat it, American-style stuffing, cranberry sauce and fruit cake (M has already made the cake here!).

Ancel's Xmas card taken at Minnelea (their villa in Italy) November 15th, shows them between the well and the outdoor fireplace back of their house; the gate to the driveway is in the background. In the opposite direction is the Mediterranean, 210 feet (vertical distance) below the house; Sardinia and Sicily are 150 miles to the west and south, respectively.

Robert Kroc, president of the Kroc Foundation, Santa Ynez CA wrote Edward Adolph:

The enclosed brochure summarizes much of what I've been doing since 1969 when I took early retirement from the Warner Lambert Research Institute.

This is for deposit in Society archives as you requested in your recent letter to all of us who are trying to slow or wishfully reverse the trend of accumulating years.

My activities with the Foundation keeps me fully and interestingly occupied.

Russell Huggins to Edward:

From 1971 to 1974 I was on leave from Baylor College of Medicine, and during that period served as Acting Chairman and Professor of Physiology at Mahidol University, Bangkok, Thailand, under the auspices of The Rockefeller Foundation. Returning to Baylor in 1974, I was invited back to Asia in

1975 and 1976 to serve as External Examiner in Physiology at the National University of Malaysia, Kuala Lumpur, Malaysia. The invitation has been extended again for 1977 and I plan to accept it. Regrettably, this means missing the Federated Meetings and that of the Circulation Society.

Nathan Lifson to Edward:

I am engaged in a combination of research, teaching and administration typical for a medical school professor of physiology. I just made your birth deadline year of 1912. I am 65 and do not plan to retire until 68, the compulsory age at this University.

Dick Riley to Edward:

I am now, as of October 1st, handing over the chairmanship of the Department of Environmental Medicine to Dr. Gareth Green. He will become Chairman of a combined department called Environmental Health Sciences. I will stay on in active scientific work until July 1, 1977. At that time Polly and I move to the little town of Petersham, Massachusetts. It so happens that the Harvard Forest is also located in Petersham, and there are fascinating studies of the circulation in plants going on there. With luck I will be given an opportunity to participate, but as yet nothing has been formalized. As of the Annual Meeting of the American Thoracic Society next May, I will become President and have more than enough administrative responsibilities. I would not be interested in moving away from Petersham, but note that both the Worcester and the Amherst-Northhampton areas are within commuting distance.

Rachmiel Levine, Executive Medical Director, City of Hope National Medical Center to Edward:

A Search Committee is engaged now in interviewing possible candidates for the Medical Directorship. As soon as my successor arrives on the scene, I will "retire" to the laboratory because I feel that I would like to become a Research Fellow again. It is my intention to pursue some further explorations into the mechanism of insulin action on membrane permeability.

I am in good health and very active. Since April 1st, I have taken over the editorship of the section on Metabolism, Endocrinology and Gastrointestional Physiology of the "American Journal of Physiology." For 40 years now I have attended to the clinical care of patients with metabolic disorders and have tried to pursue problems dealing with the regulation of carbohydrate metabolism by hormones, especially insulin. During the last 26 years I have added to that the administrative and teaching duties which go with chairmanships and directorships. I want to transfer the administrative tasks, in order to be able to enjoy hunting, the scientific snarks and to perform my editorial duties.

Rachmiel enclosed for the Archives his curriculum vitae and list of publications.

Lew Dexter to Edward:

I was very touched by your letter to us members of the American Physiological Society who were born before 1912. I have just become emeritus at the Harvard Medical School, but am still very active in teaching here at the Peter Bent Brigham. I am one of those members of the Physiological Society who is a clinician so that I can continue to function in a Harvard allied hospital.

What I am not doing is sitting on a whole variety of committees, and what I am doing is a lot of teaching. I have given up my cardiac catheterization laboratory and for the first time in many years I have had an opportunity to do some thinking. As a result of this, I am putting together a variety of studies which we carried out but never published because I had not had a good enough grasp of the subject to be able to put into meaningful articles. At this moment I am having all of the fun and practically none of the drudgery, so I am very content with my lot. I am not interested in a position that involves a lot of administration. I am really not free to move to another area.

I have had a long-time interest in the pulmonary vascular diseases of the lung, i.e., pulmonary hypertension. I have always been very puzzled by the pulmonary response to unilateral pulmonary artery occlusion with a balloon, particularly in patients with a lot of pulmonary hypertension. Although I do not have all the answers, I believe I have a feel for the subject which I never had before, have a pretty good concept of what is going on physiologically, and what needs to be done to prove or disprove the concepts.

A second problem has been the disappearance of pulmonary hypertension after mitral stenosis surgery and its persistance after surgery for congenital heart disease. This has been a subject which has plagued the surgeons for several decades—almost thirty years. I now think I have a pretty good idea as to why one disappears and the others don't. There are a few loose ends which need tightening up, but at least I can point out what these are and some of the younger eager beavers can do this.

As you see I have become emeritus but I am not really retired, and I hope to continue in this position for at least a few more years. My kindest regards.

Virginia Fiske to Edward:

When I retired from Wellesley College in June of 1976 the college offered me laboratory space in its new Science Center, so I am back in the laboratory and delighted to be able to continue my studies on the effect of light on endocrine secretion, more specifically, the role of the pineal gland on periodic endocrine secretion. This arrangement allows me to maintain contacts with students and former colleagues and to work on my research at my own pace and with few interruptions.

I am not free to move to another area, but I am interested in being actively involved in my field.

My husband, my sons and I continue to take part in a variety of community activities. We find that while this kind of volunteer commitment is not all joy, it does help us feel that we are, in a small way, part of the current scene and have some input. Fortunately I still enjoy very good health and a game of tennis—doubles!

George Maison to Edward:

Though it has been 30 years since I was entitled to call myself a physiologist and over 20 since my last research publication, the Physiological Society has continued to be a source of internal identity.

The theme of my life seems to have been variation. Seven years as physiologist, four of aeromedical research in what you aptly called "our war," seven years as pharmacologist, 15 as pharmaceutical business man, three as part-time marriage counselor, six as part-time hospital psychiatrist account for 42 years since my M.D.

If the results of my serendipitous life experiment are of interest, it has been joyful and personally rewarding, there have been few dull moments and none without challenge.

By way of discussion, it would seem that the state of all these arts in my lifetime has not precluded the do-it-yourselfer

as, clearly, I was not adequately trained for these tasks. I think experience has equipped me to face the questionable benefits of the prolonged old age and the waxing tremor promised by my familial genes. The conclusions are not yet available.

Paul Bucy to Edward:

I retired as Head of the Division of Neurological Surgery at Northwestern University Medical School in November 1972, and have been living in Tryon, North Carolina since. Although I am retired so far as teaching and research and the practice of medicine are concerned, I find myself very busy. I have accepted an appointment as Clinical Professor of Neurology and Neurological Surgery at the Bowman Gray School of Medicine at Winston-Salem, North Carolina. I have no regular duties there but do attend conferences occasionally, give lectures, and participate in other activities.

My time is fully occupied as Editor and Publisher of "Surgical Neurology." This is an international journal devoted to neurological surgery and to research and other activities of interest to neurological surgeons. It is a monthly publication which I started, the first issue having appeared in January 1973. The amount of material being submitted for publication and being published has steadily increased, as has our list of subscribers.

Stan Bennett to Edward from Chapel Hill:

It is gratifying to learn through your letter of your continuing interest in physiology and in science.

Though I reached the age of 65 in December, 1975 and anticipate handing over the responsibilities for the Chairmanship of the Department of Anatomy in the early summer of 1977, I am hoping to stay on actively as a Professor engaged in teaching and research till the age of 70. After a very active investigative period in Seattle, I found myself at the age of 50 giving up all research in order to assume the responsibilities of the Deanship of the Biological Sciences, including the School of Medicine, at the University of Chicago. I am happy to say that I have been able to reestablish myself in active investigations which are entirely new for me, devoting much of my attention to theoretical and computational approaches to the molecular structure of membranes. I have been doing all of my own programming and calculations and have been gratified that fruitful new lines of endeavor have opened up to me again after a number of years during which I had to give priority to administrative matters. I am beginning to publish once more and look forward with pleasure to continuing interaction with active scientists for a number of years to come.

It is very nice to see the names of some of my very good old friends and teachers on the Committee on Senior Physiologists. Philip Bard and Hallowell Davis were my teachers in Physiology when I was a first year medical student at Harvard. I remember their stimulating discourse and guidance with grateful pleasure. I have seen Maurice Visscher many times recently because of our common interest in the activities of the National Society for Medical Research. As for Hymen Mayerson, I can recall stimulating discourse with him in New Orleans about 15 years ago. How long is it since I have seen Bruce Dill? Maybe 30 years. With very affectionate regards to you and warmest good wishes to all my friends on the Committee on Senior Physiologists.

James Leathem to Edward:

Rutgers has mandatory retirement at 70 and retirement from administrative positions at 65. Thus, I have stepped down from the Directorship of the Bureau of Biological Research and returned to the Department. I continue to teach a full schedule giving 100 lectures/semester and participate in at least one laboratory. I have 10 graduate students and we have two NIH grants supporting our endocrine research of Reproductive Physiology and aging and factors influencing the development of the hypothalamic-hypophyseal adrenal axis. So I have maintained my laboratory in a full-time professorial job and hope to continue as active scientifically as possible.

Francis Saunders to Edward:

I retired in May of this year, after nearly 38 years on the Research Staff of G.D. Searle and Co. in Skokie, Ill. Since then, my wife and I have moved to Davis, California. Getting settled in our new home, gardening, woodworking, bicycling, bowling and California sightseeing have fully occupied my time during the past few months and scientific activities have been limited to reading a few journals. As time goes by, I hope to build up contacts at the University of California at Davis.

Eugene Still to Edward:

I recently passed my seventy-eighth birthday. Except slowly developing cataracts, occasional difficult micturition, and ciruclatory difficulty in one leg due to WWI injury—I am in good health. My appetite is excellent—digestion the same. My senses are good and I'm physically able to conduct my hobbies and meet my obligations.

I am working in a group to encourage and consult with high school students and to interest them in the sciences. We have an annual meeting and demonstration to show the public and parents the results of the students' investigations. Prizes, scholarships, etc., are given. This work is very rewarding.

I am an amateur orchidologist and am especially interested in the use of mericlones in the development of hybrids. It was necessary to give up my long time interest in sailing as I am no longer strong enough to meet the demands.

Samuel Leonard to Edward:

The wife and I travel a lot via small camper, but when home, consult with students, do a little advising but am not active at the bench.

In passing—on our way back from Kansas a couple of weeks ago, we stopped to visit Dr. Sydney Asdell (Frederick, Maryland). He seemed quite hearty even after a mild stroke several years ago.

It is pleasant to recall seeing you, Dr. Fenn and many others at meetings of the Soc. Exper. Biol. Med. in Rochester. They were excellent scientific gatherings—learned a lot besides one's own major field. I think the idea of gathering news of the "old timers" is great.

Manny Mendelson to Edward:

My heartiest good wishes to my many friends and former colleagues in the Society and particularly on your committee. Since my retirement some 7 years ago I have been busier than ever with a variety of indoor and outdoor activities. My laboratory interests ranged through sensory physiology, body temperature control, new anthelmintics, and aviation physiology from sensory-motor performance to bioacoustics. Until recently I tried to follow the bioacoustic literature, looking for work on speech recognition and human thought processes, which had glimmered faintly on the horizon about a decade ago—but to no avail. Since I have retained no connections with academic or research groups, I am sorry to say that some promising investigative areas will probably die in limbo.

I am not interested in administrative positions and it would require a very attractive opportunity elsewhere to lure me

away from a very happy, even though somewhat lonely situation here. With deep appreication and great affection.

Leon Churney to Edward:

Your letter arrived at the moment of my retirement. On that day I sent a publisher a 950 page ms titled "A Bibliographical History of Electrophysiology Chronologically Arranged." Plans for the future are not finalized, but I intend ultimately to write up the 5 abstracts that I published on the subject of automaticity in cardiac ventricles. Also I hope to finish my textbook, "Essentials of Electrophysiology"; the product of 20 years of graduate teaching. Finally, I have prepared a free communication for the forthcoming International Congress of Physiology, Paris, July 1977. The title of the paper is The Glitsch (≈ The Inverse Muscle Twitch)—a somewhat cryptic title with which to conclude this brief message.

N.T. Werthessen to Edward:

I'm still at work, albeit I had planned to retire this year. Having read for years the advice of a number of your correspondents about the importance of planning one's retirement, I very carefully did just that. But several things have intervened. All are due to my job as bioscientist at the Office of Naval Reasearch's Boston Branch Office. This post, I have often said, legally pays one a good income; but morally it is almost improper to accept the compensation.

It is, in my opinion, a position that should be held by someone 50 or over who has spent his life doing research intensively in a specialized field—but who has retained his curiosity as to what else has happened in biology and medicine. Given that condition, he is back in the fascinating phases of graduate school again. His mentors are holders of ONR contracts. They are experts in their field. They need his help in maintaining their research. They are anxious, let alone delighted, to delineate with care and precision the implications of their current studies.

ONR is essentially a problem-solving agency (through the wisdom of those who organized it). Thus we must maintain continuous contact with the scientists it supports. Our role is a very special and exciting one. We are part of the research effort. Tough as it is, we try to keep up with the specialized literature in half a dozen different biological and medical specialties. We also are asked to continue our own research activities and publish with a reasonable frequency so that we do not forget just how difficult it is to push back the boundaries of ignorance. Thus, if we function properly, we should stay a bit ahead of those trying to solve ONR's problems, perceive where the answers may lie, and stimulate research in those areas.

Before I really retire, I have a few tasks to see to that are approaching completion. The first of these concerns a derivative of research on prostaglandins that could prove to be the answer (therapeutically) to hemorrhagic and cardiogenic shock. The second is a clue as to the possibility of a metabolic derangement lying at the root of violence due to alcohol consumption. The third lies in my own research on the precise cause of the lesion that leads to atherosclerosis. This latter work was what took me to Tokyo to report, with my colleagues from Albany Medical College and Northeastern University, on the ability of cholesterol oxidation products, rather than cholesterol, per se, to produce the possible first lesion leading to atheroma. In the November issue of the "Archives of Pathology" our paper appeared on the concentrate of these substances found in U.S.P. cholesterol. My hunch is that when this study is completed, cholesterol, per se, will be exonerated from all the guilt that has been piled upon it.

Those of you who have tried to retire with such unresolved puzzles in hand will understand why I am still in harness. As soon as possible, however, I will quit and go fishing on my cruiser offshore when the weather is good—rather than swear on weekends when it is not.

Piero Foa to Phil Bard:

I retired a few months ago from the chairmanship of the Department of Research, Sinai Hospital of Detroit but I continue to serve as a consultant to the acting chairman and to the administration of the hospital. One of my jobs in this capacity is to help select a successor, preferably an M.D. with a strong background in physiology or related fields of medical research (suggestions and applications will be welcome).

Except for being relieved of administrative duties, my life has changed very little and I continue to do research, to direct the work of several graduate students and to serve as professor of physiology, full time, at Wayne State University School of Medicine.

T.N. Jahmisian to Phil:

I am continuing to work at Argonne National Laboratory as a consultant. An office space is available as well as the use of an electron microscope. I have been assisting Dr. Michael Fry in his cancer research. I have had over twenty years of experience in electron microscopy and would be willing to work as an administrator, or carry out research elsewhere.

Joe Hughes to Phil:

After being with Gasser, I took an appointment at The Institute of the Pennsylvania Hospital as Director of Laboratories and combined clinical interests with experimental laboratory work.

During the war I served as Commander in the Medical Corps of the Navy. Following the war my chief interest was Clinical Psychiatry and I was Professor of Psychiatry at The Women's Medical College from 1946 through 1964, when I resigned following a severe coronary. I continue full time practice as Consultant in Psychiatry at The Pennsylvania Hospital and The Institute of the Pennsylvania Hospital.

Anne and I have eight children—six sons and two daughters. Four of my sons are physicians. Anne was the daughter of Dr. Eugene Opie, who remained active at the Rockefeller Institute until age 97. He then came to Philadelphia and had an apartment close to our home. We saw a great deal of him in the last year of his life. I met Anne when I was working with Dr. Opie when he was the Director of the Henry Phipps Institute in Philadelphia following my medical internship.

J.W. Archdeacon to Phil:

I was on leave from the University of Kentucky to the University of Malaya when you visited there in 1965. I went back again for another sabbatical leave in 1973-74. There has been another leave in 1975 to the Physiology Department at the University of Benghazi (now Ghar Younis), Benghazi, Libya. And now I am trying to convince a Department of Physiology either in Africa or Asia to take me on after July 1, when I retire here at Kentucky. Prospects look good!

I have continued in research until just recently and now engage mostly in teaching activities, especially in Endocrinology. I do not believe I can yet tolerate inactivity very well, thus the desire to teach abroad again. There is no talk of these other continents of Physiology vanishing and no Research Grant Reporting and Application systems to drive you crazy!

Bob Johnson to Phil:

Although I left Urbana and the University of Illinois as emeritus professor in 1973, I am not yet retired, holding a professorship at Knox College until mandatory retirement in 1979.

Margaret and I are now well settled in this small community and are both happy and well. The town is well served by air, rail and interstate highway, and we are always glad to see old friends. Although we have made new ones, there is nothing to substitute for good old friends. I teach physiology to first year medical students, and coordinate a program in which 16 first-year students from Rush Medical College (Chicago) spend their first nine months here at Knox College, where they learn basic medical sciences, and at Galesburg Cottage Hospital (associated for teaching with Rush Medical College) where they are introduced to clinical medicine. I continue to do research on metabolism, currently doing direct calorimetry on the Eastern box turtle, and asking the question never answered as yet: does oxygen consumption correlate with heat production in chelonians? Also I publish on the history of science. My current effort is a biography of Sir John Richardsen, the father of Arctic biology. (London: Taylor and Francis, 1976).

Best wishes to all on the Committee on Senior Physiologists.

Walter Seegers to Phil:

Having been at it for 30 years, I expect to continue as department chairman on a year-to-year basis, and to proceed about it as usual. Last summer I lectured at several places in Japan, my research grant will help me until 1980, and blood coagulation research was never more interesting. Most puzzling of all is the question whether it's physiology, biochemistry, clinical medicine, or just something that interests me. Maybe my greatest discovery is that my life is about me, and so-called scientific orientation does not change the fact.

Louis Jaques to Phil:

The University of Saskatchewan still has the enlightened policy of not requiring retirement until June 30th following the 67th birthday and with a July birthday, this means I keep on going until June 30th, 1979. Further, I was made a named professor four years ago (Lindsay Professor) which carries with it the instructions to devote oneself to scholarship (no committees, administration, etc.).

With a small group of students, my research is going better than ever. This week, our article appeared in The Lancet on "Intrapulmonary Heparin—A New Method of Anticoagulant Therapy"— Heparin without the needle! Much of what I struggled with for so many years is now being clarified to reverse the opinion that has been expressed about this remarkable drug—"Heparin should be reserved for patients and never be a subject for research!"

Hal Wiggers to Phil:

It was truly a pleasant surprise to hear from you through the Committee on Senior Physiologists and equally interesting to realize that my somewhat senior but former colleagues—Bruce, Maurice, Hal Davis, Ed and Hy—are continuing active pursuits. I recall vividly our last meeting. You proclaimed your intention of casting aside the Dean's mantle for the pleasure of returning to the laboratory and teaching—which you did shortly thereafter. I credit you with great foresight, because in the succeeding years, the Dean's position became far more complex, less fun and certainly more demanding of his time, energy, patience and sense of humor. One found oneself increasingly involved in problems of business, management and finance at the expense of time for devotion to the philosophy and process of Medical Education. Nevertheless, I must confess that for most of my twenty-one and a half years as Dean and Executive Vice President of Albany Medical College, much pleasure and satisfaction has been derived from the recruitment of an exceptionally fine faculty and the construction of excellent facilities which enabled students and faculty to do their things. These activities led to the development of an outstanding Medical Center which provided the ultimate in professional health services to the 2 million residents within its regional service orbit. It was my privilege to observe and verify that the medical school is the Sine Qua Non of a teaching medical center.

If I was in any sense born with a "silver spoon in my mouth," it relates to the genes inherited from both parents and to the fantastic example of a dedicated scientist as portrayed by my dad, who, somehow, convinced his son, who graduated from college at the onset of the great depression of the early thirties, and who withdrew his acceptance to Harvard School of Business Administration, to enter a doctoral program in Physiology. It was on a "try it, you may like it" basis. Obviously, I liked it. It also provided me with my first opportunity to really know my dad and what made him tick. Those who made major contributions to my development as a doctoral candidate include Paul Quigley, Donald Gregg, Hall Green and Harvey Goldblatt. Upon receipt of the doctoral degree from Western Reserve University, the "silver spoon" again reappeared in the form of the W.T. Porter Fellowship of the American Physiological Society, which offered a fellowship year under the tutelage and friendly guidance of Hal Davis—one of the most remarkable scientists I have met in 42 years of professional endeavours. One could not avoid maturing as both scientist and person under his wing. Teaching and research opportunities were abundant during 5 years with Magnus Gregerson at Columbia, 2 years back at Western Reserve and 4 years with George Wakerlin at the University of Illinois in Chicago. The remarkable experiences at all of the above culminated in the choice of an offer as Chairman of Physiology and Pharmacology at Albany Medical College (AMC) of Union University in the fall of 1947 or Chairman of Physiology at Georgetown School of Medicine in Washington. Again, the "silver spoon" must have influenced my selection of AMC where I enjoyed 27 years of activity. In 1953, my 17 years in the field of physiological research and teaching came to a sudden halt when I accepted an invitation to become Dean—an offer which I thought I had wisely declined in 1951. Having had its complacency and extreme provinicialism profoundly shaken by my predecessor, Jim Campbell (currently President of Presbyterian—St. Lukes—Rush Medical Center), AMC was ripe for someone to assemble the shattered bits into a pattern for modern medical educational pursuits. Six years of experience as Chairman of Physiology and Pharmacology and participation on the Executive Faculty Council turned out to be a remarkable training ground for the Deanship—somewhat surprisingly, I learned that in addition to all other functions, the Dean was privileged to be the primary fund raiser for this minutely endowed institution (less than $900,000). Suffice it to say that 21½ years as Dean and Executive Vice-President (1966) was a stimulating and rewarding experience, even though it produced a true workaholic. Equally rewarding were the opportunities provided to see first hand many of the Medical Schools of this nation while serving

on program-project review committees for the National Heart and Lung Institute and the Veterans Administration Central Office as well as various HEW Construction Funding Agencies. This "silver spoon" of opportunity enabled me to work with or become acquainted with many of the nation's distinguished investigators and medical educators—a series of truly invigorating experiences. Membership on the Administrative Council of the Council of Deans and The Executive Council of the AAMC broadened the scope of my perspectives in medical education and health services.

As the date for compulsory retirement approached (6-30-75) I reflected upon the many sabbaticals which had been joyed by faculty colleagues—and finally requested early retirement (9-1-74) with a nine month "terminal" sabbatical, which was generously granted by the trustees.

My wife Ginny and I had long since decided that retirement should find us dwelling in a more moderate climate than Albany could provide—and had actually purchased a residence in Greenville, N.C. where our younger daughter and family reside. Greenville happens to be the home of East Carolina University, which, in November of 1974, was authorized to begin development of a medical education program leading to an M.D. degree. In December 1974, I was asked to be a consultant to the program and in February 1975 was requested to become "acting Dean" to provide leadership for the program and to assist in identifying and recruiting a Dean. Upon his appointment and arrival in August 1975, he urged me to continue as a Senior Consultant in the development of this medical school—and I am currently working in this capacity on a 2½ day/weak basis. This allows time for my major weaknesses, golf, and for chief of lawn care, gardens and house and car maintenance, from most of which puttering I derive pleasure and satisfaction. Life in this small North Carolina city is most delightful, aided by the cultural contributions of the Schools of Art, Drama and Music at the University. Except for occasional bouts with lower lumbar disc disease, my health has been excellent—although my capacity for recalling new and old names appears to be dwindling.

I hope to find time soon to put in writing some of my concerns re the inadequacies of undergraduate and post-doctoral education. There are serious flaws on the basis of my observations over many years. I am neither interested nor sufficienly au courant to re-enter scientific investigation nor am I looking for more administrative activity than I now entertain. As of the present, I am neither free to move to another area nor contemplating such.

Art Martin to Phil:

My long concern with excretory processes in cephalopods made it necessary for me to go to the South Pacific, beginning in 1970, to do some work on Nautilus. Its kidneys, all eight of them, are certainly homologous to those of the other cephalopods, and in turn quite recognizable as molluscan kidneys. But the plumbing just does not make sense and I felt I had to do something about it since few physiologists have spent the time on octo-pee that I have. So now I have made three trips and have some of the writing in hand. The problem grew like Topsy. I now have some pretty definite leads on how long it takes a Nautilus to grow up, and when its breeding season occurs, and even how many eggs it lays. But even yet there is no embryological series on this important forbear of all extant cephalopods, and I may be able to help obtain one. The processes involved in buoyancy control have been most ably set forth by Eric Denton, but even here we have been able to make some contributory measurements.

As for the kidneys the story is almost unbelievable. I now have reason to think that half of them have been devoted to the problem of calcium storage. This sounds like nonsense in a marine animal until one understands the necessity of calcifying each new septum in the shortest possible time. Air will accumulate behind the elastic septum within a few days of its formation, and the animal can move neither up nor down in the water without rupturing it. So it appears that almost half of the necessary calcium is accumulated in the kidneys in the form of calcium phosphate, and all I have yet to understand is how these urinary stones are mobilized and made available through the blood. You can see that the solution is potentially useful even in medicine.

Fred Hirsch to Phil:

I have retired twice (once from the U.S. Navy and once from the Lovelace Foundation for Medical Education and Research in Albuquerque, where I was the Program Director for Clinical Research for many years). However, I am still working full time as a Staff Physician in one of New Mexico's institutions for the mentally retarded. I still have a minor hand in research, although it is far removed from the discipline of radiobiology that occupied my attention for so many years. My two chief areas of current interest are in the antenatal diagnosis of conditions which are inevitably associated with mental retardation such as Down's Syndrome and the other various Trisomies. The other is trying to figure out how the profoundly retarded we have here who have associated disabling cerebral palsy and which has confined them to bed for as long as forty years, maintain a ventilation compatible with life when their tidal volumes are in the vicinity of 90-150 ml, and their thoraces virtually fixed by ankyloses.

I see Bruce Dill from time to time and always marvel at the hunk of robust physiology he still presents. I also am in frequent touch with Ulrich Luft, who was my colleague at Lovelace for so many years. He is still active and productive, even though the Lovelace Foundation is practically extinct. He in fact is the "last of the Mohicans" there. In response to your question as to my willingness to relocate, please be advised that the only time I will leave New Mexico is to join my ancestors, and that will be associated with substantial resistance on my part!

A. Albert to Phil:

I have just retired from the Mayo Clinic, and at the moment am training some younger consultants. I shall be free April 1, 1977 and plan to spend some time taking care of neglected personal affairs. I am not interested in a new job or leaving Rochester because we have everthing we want here.

Dietrich Smith to Phil:

Since my retirement eleven years ago I have had very little to do with the academic or scientific world. I did serve as Secretary to the Maryland Society for Medical Research for five years and I am still responsible for the appearance of its "Bulletin," a job I have been trying to pawn off on somebody else without any success so far. I am not at all happy with what I see in medical education and I am relieved I do not have any further responsibilities in this area. I am old-fashioned enough to think that basic sciences are still basic and that medical students can learn a lot of physiology from cats and dogs. And I heartily deplore what for want of a better word, is known as the "quota" system in medical school admissions.

On the personal side, I am still in good health, although arthritis limits my mobility. My wife and I have been spending

our winters in Hawaii where my son helps administer the Social Security System on the islands. It is a welcome change from the Baltimore winters, as I am sure you will agree.

Giuseppe Moruzzi to Dr. Bard:

Many thanks for your kind letter, which I was happy to receive. It was good to hear from my American colleagues. The retirement age in Italy is at 70 and I have to carry out administrative and teaching duties up to 1980. Therefore it would be difficult for me to reply to some of your questions.

I am happy to respond to the inquiries concerning scientific and writing activities. I am interested in the neurophysiological mechanisms underlying the levels of activity in the intact and thalamic pigeon, with particular regard to the problem of the sleep-waking cycle. The last experimental paper was published at the end of 1975 (Arch. Ital. Biol., 113:303-325, 1975). I am now completing a Handbook of Physiology for medical students. The first volume, "Fisiologia della vita di relazione" (Torino, UTET, 1975) has already appeared. It is mainly concerned with neurophysiology and sensory physiology. A monograph, "El messaggio nervoso," has been published in collaboration with Rita Levi-Montalcini and Angeletti (Milano, Rizzoli, 1975).

Best regards and many wishes for Christmas and the New Year.

Dr. Moruzzi sent biographical material summarized in an article from "Modern Men of Science."

Herb Ellis to Phil:

I reached the chronological status of a senior physiologist the end of November last. I haven't yet noticed any great change, physiological or otherwise, upon crossing the 64/65 ageline. Oh yes, I am now retired; Burroughs Wellcome has a policy of mandatory retirement at 65, but they also make provision for some of us to continue as consultants if they feel we still have a few creative ideas left. Under these circumstances, the somewhat greater amount of free time still seems more like vacation than retirement.

After 23 years of research as Senior Pharmacologist, chiefly in the cardiovascular field, I became more and more interested in applications of computer processing to my research problems and to those of my colleagues. The way opened for me to leave my position in the Pharmacology Department to become a liaison between the entire scientific staff of WRL and the computer facilities, under the coined title of Scientific Systems Analyst. As such I became part of the new Scientific Data Management Department. With the installation of an in-house computer facility a great deal of our time was required to make the transition from time-share to in-house service, but I did find a little opportunity to begin exploring some possibilities in computerized graphic procedures. It is in this area that I have been asked to continue as a consultant for the S.D.M. Department.

Would I be interested in a position enabling me to continue scientific activities? Obviously for the present I have such an opportunity, so for at least a year my desires are fulfilled, but I don't yet feel any desire to be put to pasture. As for being free to move to another area, the answer is also a "hedgy" no. When Burroughs Wellcome moved from New York to Research Triangle Park, N.C. in 1970, my wife and I faced up to the question of moving with them, and decided to come, so we built our house with retirement in mind, including a small darkroom, a modest workshop, and a small swimming pool. And we have made many friends here over the last six years so – it would take a pretty unbelievable offer to lure us away from Chapel Hill.

I have enjoyed the brief excerpts of letters from the senior physiologists, but can't quite get used to the idea yet that my name may belong in that list.

Julius Comroe to Phil:

The mandatory retirement age at the University of California is 67 rather than 65. However, as of January 1, 1977, I have requested half-time status and will continue half time until July 1978 when I will retire.

My main activities at the University of California, San Francisco are:

I am Director of our National Pulmonary Faculty Training Center, one of six designated by the National Heart, Lung and Blood Institute to relieve faculty shortages in certain schools of medicine by providing three years of training (both scientific and clinical) for pulmonary physicians who agree to return to faculty positions in the medical school that sponsored them. The positions can be in clinical departments of medicine, pediatrics, or surgery or in basic science departments that would like to include on their staff faculty members with their research and teaching interests in respiration and the lungs.

I am Associate Program Director of our Pulmonary Research Training Grant supported by the NHLBI (1977-1983) and which provides support for 16 or 17 full-time research trainees a year.

I am continuing to write a "column" (actually 4-6 pages) a month for the American Review of Respiratory Diseases. The column is entitled The Retrospectroscope and is intended to determine, by looking backwards, how the major advances in clinical medicine and surgery have actually come about. I have written 23 of these to date; when I've finished No. 30, I intend to rearrange them, edit them a bit, and collect them for separate publication so that they might reach a broader audience.

In my other half time I will continue to work on the major problem that has interested me for 4-5 years, namely, the contribution of basic science to clinical advance in medicine and surgery. I may get involved in a nationally televised program dealing with this subject in the next year and hope to write a book for the lay public to enlarge their understanding of how discoveries really do come about in medicine and surgery.

I have also decided that it would be fun to get together an anthology of wit and humor on biomedical science. Some of our colleagues have written some very clever and witty things over their lifetime and I think that they (the writings – not my colleagues) ought to be put in one place instead of scattered. I will be grateful for contributions of your own or others that might be suitable though, of course, I can't promise to publish them all.

Roberta Hafkesbring to Phil:

In 1964, after 34 years at Woman's Medical College (now Medical College of Pa.), I retired and left immediately for volunteer service in Korea. For six sessions, I served as Visiting Professor of Physiology in Ewha University Medical College. This was a difficult, challenging experience, but I thoroughly enjoyed it. The country and its people were excellent.

In 1975 (Dec.) I bought a cottage in Claremont Manor, CA – a very pleasant place in which to retire.

Best regards to you and greetings for a healthy and happy 1977.

Frank Johnson to Phil:

Thank you for your recent note. How well I remember you lecturing in your undergraduate physiology course here. I never dreamed at the time that I would some day be lecturing in an undergraudate course of my own in the same spot, in Room 10 Guyot. As it happens, the last lecture in that course was given just a week ago. The same course will not be offered again, as I am scheduled to become emeritus at the end of the present academic year.

As for news other than the above, I venture to forward herewith a recent copy of the Princeton Alumni Weekly containing an article I wrote concerning the status of research on bioluminescence. I realize that as a reply to the request for material for The Physiologist the article is preposterously long and for this I apologize and express the hope that some members of the editorial staff will have time to look through it for items of possible interest.

Frank's story will be added to the Archives; it is an excellent source of information on bioluminescence with reference to Boyle's observations in 1672 and to the eminent Princeton authorities including Conklin and Harvey. The concluding paragraphs were:

Research in bioluminescence is now greatly overshadowed by such areas as biochemical genetics, which has burgeoned so dramatically since the discovery of the structure of "DNA" (now practically a household word) and by investigation of the ultrastructure, or so-called molecular biology, of cells and tissues, as well as by the structure and synthesis of macromolecules, including even genes. Progress in these other areas during recent years has been little short of terrifying and has even led to serious, high-level discussions of the moral aspects of certain fields of biological research. But the study of bioluminescence needs no apology, or defense. Through recorded history, it has been a prime example of "pure science," for which the chief incentive to thoughtful inquiry has remained, in Robert Boyle's words, "the wonder and delight" it proffers.

The foregoing account can scarcely do more than intimate the scope of the contributions that research in bioluminescence has made to biology, yet Princeton can justly take pride in the fact that much of the progress stemming from work in this field has been accomplished by members of its Department of Biology. With regard to the chemistry of Biology. With regard to the chemistry of bioluminescence alone, it is noteworthy that three-fourths of the known chemical structures of specific luminescent systems have been established through the efforts of the Princeton group. What good the future might bring from continued basic research in bioluminescence is anybody's conjecture. As Benjamin Franklin once asked, "What good is a baby?"

Frank also enclosed their Xmas card, his protrait taken in the light of luminous bacteria (Bioscience, April, 1976).

R.W. Dougherty to Phil:

I am currently working on a book entitled, "Experimental Surgery in Farm Animals." This effort was encouraged by my colleagues at the National Animal Disease Center, Ames, Iowa. The book is being written from the viewpoint and experience of an animal physiologist with approximately 40 years' experience in the field and with the help of others in the same field of endeavor. The art work has been completed by the excellent medical artist, Miss Marion Newson. It will include a large number of experimental surgical preparations on horses, cattle, sheep, swine and deer.

My wife and I are living on a small farm near Ames, Iowa. Although I retired March 1, 1974, I am still listed as a collaborator at the National Animal Disease Center and am still listed on the graduate faculty of Iowa State University. I would be interested in any additional assignments, but would depend on the assignment and the place.

I do appreciate hearing of the activities of other retired physiologists.

S.C. Wang to Phil

As you know, our retirement age at P & S (Physicians and Surgeons) is 68, which will be July, 1978 for me; that is why I am still working, as usual.

George Koepf to Phil:

You probably don't remember the year or so that I collaborated in a research project with Dr. Gemmill in your department at The Johns Hopkins Medical School – 1938-39. I was primarily associated with George Thorn at that time. I do, however, remember you and your kindness in allowing me to work in your laboratories.

More recently, i.e., for the past 20 years, I have had the misfortune of having to forego the fun of actual research for time-consuming administrative affairs of the Medical Foundation of Buffalo, an independent health related research organization here in Buffalo, New York. I plan on staying on the job for a bit even though I am 65 years of age as the organization seems to want me.

Phil Bard to Hy:

I retired June 30, 1964 as Professor of Physiology and Director of the Department at the Johns Hopkins University School of Medicine and became Professor Emeritus. For the following twelve years I worked in my old laboratory provided me by my successor, Vernon Mountcastle, who has done such a splendid job in expanding and improving the Department. Under his guidance it has become one of the great Meccas for neurophysiologists, but it is also a well balanced department with many areas of physiology represented and the old standards of laboratory teaching of medical students has been splendidly maintained. During those twelve years I continued work on body temperature regulation and the central nervous mechanisms involved in producing endotoxin fever. There was also some work on the isolated hypothalamus, done with Jim Woods, especially the site of the locus of the area responsible for the regulation of the antidiuretic hormone. During that time I enjoyed association with old Baltimore friends and colleagues. In 1964, just before I retired I lost my first wife, Harriet, whom you and Caroline knew. The next year I married Janet Rioch, the sister of our colleague David Rioch and we were very happy. Janet died in 1974. Early last year I married Coleen Gillis, the widow of a very old friend of mine. She is Scotch and came to the U.S. in 1957 and met her first husband who died in 1971 – they lived in California. She wanted to stay in California and I wanted to return to the scenes of my youth. We moved out here last May and are enjoying life. We are only 25 miles from my birthplace where I was brought up. I left it in 1917 when I joined the U.S. Army. When I returned from France I entered Princeton and then went to Harvard and eventually, in 1933 to Hopkins. But I have many ties here, a sister, a daughter with four children and various nieces and nephews. I keep busy and have several papers to write.

Dr. Bard died April 5, 1977.