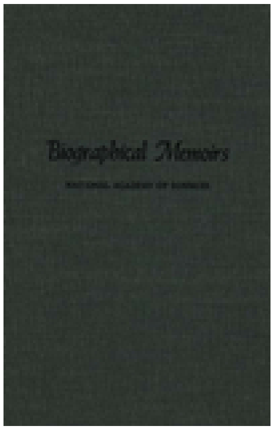

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Contents

Preface

The *Biographical Memoirs* is a series of volumes, beginning in 1877, containing the biographies of deceased members of the National Academy of Sciences and bibliographies of their published scientific contributions. The goal of the Academy is to have these memoirs serve as a contribution toward the history of American science. Each biographical essay is written by an individual familiar with the discipline and the scientific career of the deceased. These volumes, therefore, provide a record of the lives and works of some of the most distinguished leaders of American science as witnessed and interpreted by their colleagues and peers. Though the primary concern is the members' professional lives and contributions, these memoirs also include those aspects of their lives in their home, school, college, or later life that led them to their scientific career.

The National Academy of Sciences is a private, honorary organization of scientists and engineers elected on the basis of outstanding contributions to knowledge. Established by a Congressional Act of Incorporation on March 3, 1863, the Academy works to further science and its use for the general welfare by bringing together the most qualified individuals to deal with scientific and technological problems of broad significance.

BRYCE CRAWFORD, JR.
HOME SECRETARY
CAROLINE K. McEUEN
ASSOCIATE EDITOR

Biographical Memoirs

Volume 51

Abraham Adrian Albert

November 9, 1905-June 6, 1972

by Irving Kaplansky

Abraham Adrian Albert was an outstanding figure in the world of twentieth century algebra, and at the same time a statesman and leader in the American mathematical community. He was born in Chicago on November 9, 1905, the son of immigrant parents. His father, Elias Albert, had come to the United States from England and had established himself as a retail merchant. His mother, Fannie Fradkin Albert, had come from Russia. Adrian Albert was the second of three children, the others being a boy and a girl; in addition, he had a half-brother and a half-sister on his mother's side.

Albert attended elementary schools in Chicago from 1911 to 1914. From 1914 to 1916 the family lived in Iron Mountain, Michigan, where he continued his schooling. Back in Chicago, he attended Theodore Herzl Elementary School, graduating in 1919, and the John Marshall High School, graduating in 1922. In the fall of 1922 he entered the University of Chicago, the institution with which he was to be associated for virtually the rest of his life. He was awarded the Bachelor of Science, Master of Science, and Doctor of Philosophy in three successive years: 1926, 1927, and 1928.

On December 18, 1927, while completing his dissertation, he married Frieda Davis. Theirs was a happy marriage, and she was a stalwart help to him throughout his career. She remains active in the University of Chicago community and in the life of its Department of Mathematics. They had three children: Alan, Roy, and Nancy. Tragically, Roy died in 1958 at the early age of twenty-three. There are five grandchildren.

Leonard Eugene Dickson was at the time the dominant American mathematician in the fields of algebra and number theory. He had been on the Chicago faculty since almost its earliest days. He was a remarkably energetic and forceful man (as I can personally testify, having been a student in his number theory course years later). His influence on Albert was considerable and set the course for much of his subsequent research.

Dickson's important book, *Algebras and Their Arithmetics* (Chicago: Univ. of Chicago Press, 1923), had recently appeared in an expanded German translation (Zurich: Orell Füssli, 1927). The subject of algebras had advanced to the center of the stage. It continues to this day to play a vital role in many branches of mathematics and in other sciences as well.

An *algebra* is an abstract mathematical entity with elements and operations fulfilling the familiar laws of algebra, with one important qualification—the commutative law of multiplication is waived. (More carefully, I should have said that this is an associative algebra; non-associative algebras will play an important role later in this memoir.) Early in the twentieth century, fundamental results of J. H. M. Wedderburn had clarified the nature of algebras up to the classification of the ultimate building blocks, the *division algebras.* Advances were now needed on two fronts. One wanted theorems valid over any field (every algebra has an underlying field of coefficients—a number system of which the leading examples are the real numbers, the rational numbers, and the integers mod p). On the other front, one sought to classify division algebras over the field of rational numbers.

Albert at once became extraordinarily active on both battlefields. His first major publication was an improvement of the second half of his Ph.D. thesis; it appeared in 1929 under the title "A Determination of All Normal Division Algebras in Sixteen Units." The hallmarks of his mathematical personality were already visible. Here was a tough problem that had defeated his predecessors; he attacked it with tenacity till it yielded. One can imagine how delighted Dickson must have been. This work won Albert a prestigious postdoctoral National Research Council Fellowship, which he used in 1928 and 1929 at Princeton and Chicago.

I shall briefly explain the nature of Albert's accomplishment. The dimension of a division algebra over its center is necessarily a square, say n^2. The case $n = 2$ is easy. A good deal harder is the case $n = 3$, handled by Wedderburn. Now Albert cracked the still harder case, $n = 4$. One indication of the magnitude of the result is the fact that at this writing, nearly fifty years later, the next case ($n = 5$) remains mysterious.

In the hunt for rational division algebras, Albert had stiff competition. Three top German algebraists (Richard Brauer, Helmut Hasse, and Emmy Noether) were after the same big game. (Just a little later the advent of the Nazis brought two-thirds of this stellar team to the United States.) It was an unequal battle, and Albert was nosed out in a photo finish. In a joint paper with Hasse published in 1932 the full history of the matter was set out, and one can see how close Albert came to winning.

Let me return to 1928-1929, his first postdoctoral year. At Princeton University a fortunate contact took place. Solomon Lefschetz noted the presence of this promising youngster, and encouraged him to take a look at Riemann matrices. These are matrices that arise in the theory of complex manifolds; the main problems concerning them had remained unsolved for more than half a century. The project was perfect for Albert, for it connected closely with the theory of algebras he was so successfully developing. A series of papers ensued, culminating in complete solutions of the outstanding problems concerning Riemann matrices. For this work he received the American Mathematical Society's 1939 Cole prize in algebra.

From 1929 to 1931 he was an instructor at Columbia University. Then the young couple, accompanied by a baby boy less than a year old, happily returned to the University of Chicago. He rose steadily through the ranks: assistant professor in 1931, associate professor in 1937, professor in 1941, chairman of the Department of Mathematics from 1958 to 1962, and dean of the Division of Physical Sciences from 1962 to 1971. In 1960 he received a Distinguished Service Professorship, the highest honor that the University of Chicago can confer on a faculty member; appropriately it bore the name of E. H. Moore, chairman of the Department from its first day until 1927.

The decade of the 1930's saw a creative outburst. Approximately sixty papers flowed from his pen. They covered a wide range of topics in algebra and the theory of numbers beyond those I have mentioned. Somehow, he also found the time to write two important books. *Modern Higher Algebra* (1937) was a widely used textbook—but it is more than a textbook. It remains in print to this day, and on certain subjects it is an indispensable reference. *Structure of Algebras* (1939) was his definitive treatise on algebras and formed the basis for his 1939 Colloquium Lectures to the American Mathematical Society. There have been later books on algebras, but none has replaced *Structure of Algebras.*

The academic year 1933-1934 was again spent in Prince ton, this time at the newly founded Institute for Advanced Study. Again, there were fruitful contacts with other mathematicians. Albert has recorded that he found Hermann Weyl's lectures on Lie algebras stimulating. Another thing that happened was that Albert was introduced to Jordan algebras.

The physicist Pascual Jordan had suggested that a certain kind of algebra, inspired by using the operation $xy + yx$ in an associative algebra, might be useful in quantum mechanics. He enlisted von Neumann and Wigner in the enterprise, and in a joint paper they investigated the structure in question. But a crucial point was left unresolved; Albert supplied the missing theorem. The paper appeared in 1934 and was entitled "On a Certain Algebra of Quantum Mechanics." A seed had been planted that Albert was to harvest a decade later.

Let me jump ahead chronologically to finish the story of Jordan algebras. I can add a personal recollection. I arrived in Chicago in early October 1945. Perhaps on my very first day, perhaps a few days later, I was in Albert's office discussing some routine matter. His student Daniel Zelinsky entered. A torrent of words poured out, as Albert told him how he had just cracked the theory of special Jordan algebras. His enthusiasm was delightful and contagious. I got into the act and we had a spirited discussion. It resulted in arousing in me an enduring interest in Jordan algebras.

About a year later, in 1946, his paper appeared. It was followed by "A Structure Theory for Jordan Algebras" (1947) and "A Theory of Power-Associative Commutative Algebras" (1950). These three papers created a whole subject; it was an achievement comparable to his study of Riemann matrices.

World War II brought changes to the Chicago campus. The Manhattan Project took over Eckhart Hall, the mathematics building (the self-sustaining chain reaction of De cember 1942 took place a block away). Scientists in all disciplines, including mathematics, answered the call to aid the war effort against the Axis. A number of mathematicians assembled in an Applied Mathematics Group at Northwestern University, where Albert served as associate director during 1944 and 1945. At that time, I was a member of a similar group at Columbia, and our first scientific interchange took place. It concerned a mathematical question arising in aerial photography; he gently guided me over the pitfalls I was encountering.

Albert became interested in cryptography. On November 22, 1941, he gave an invited address at a meeting of the American Mathematical Society in Manhattan, Kansas, entitled "Some Mathematical Aspects of Cryptography."* After the war he continued to be active in the fields in which he had become an expert.

In 1942 he published a paper entitled "Non-Associative Algebras." The date of receipt was January 5, 1942, but he had already presented it to the American Mathematical Society on September 5, 1941, and he had lectured on the subject at Princeton and Harvard during March of 1941. It seems fair to name one of these presentations the birth date of the American school of non-associative algebras, which he singlehandedly founded. He was active in it himself for a quarter of a century, and the school continues to flourish.

Albert investigated just about every aspect of non-associative algebras. At times a particular line of attack failed to fulfill the promise it had shown; he would then exercise his sound instinct and good judgment by shifting the assault to a different area. In fact, he repeatedly displayed an uncanny knack for selecting projects which later turned out to be well conceived, as the following three cases illustrate.

* The twenty-nine-page manuscript of this talk was not published, but Chicago's Department of Mathematics has preserved a copy.

(1) In the 1942 paper he introduced the new concept of isotopy. Much later it was found to be exactly what was needed in studying collineations of projective planes.

(2) In a sequence of papers that began in 1952 with "On Non-Associative Division Algebras," he invented and studied *twisted fields.* At the time, one might have thought that this was merely an addition to the list of known non-associative division algebras, a list that was already large. Just a few days before this paragraph was written, Giampaolo Menichetti published a proof that every three-dimensional division algebra over a finite field is either associative or a twisted field, showing conclusively that Albert had hit on a key concept.

(3) In a paper that appeared in 1953, Erwin Kleinfeld classified all simple alternative rings. Vital use was made of two of Albert's papers: "Absolute-Valued Algebraic Algebras" (1949) and "On Simple Alternative Rings" (1952). I remember hearing Kleinfeld exclaim "It's amazing! He proved exactly the right things."

The postwar years were busy ones for the Alberts. Just the job to be done at the University would have absorbed all the energies of a lesser man. Marshall Harvey Stone was lured from Harvard in 1946 to assume the chairmanship of the Mathematics Department. Soon Eckhart Hall was humming, as such world famous mathematicians as Shiing-Shen Chern, Saunders Mac Lane, André Weil, and Antoine Zygmund joined Albert and Stone to make Chicago an exciting center. Albert taught courses at all levels, directed his stream of Ph.D.'s (see the list at the end of this memoir), maintained his own program of research, and helped to guide the Department and the University at large in making wise decisions. Eventually, in 1958, he accepted the challenge of the chairmanship. The main stamp he left on the Department was a project dear to his heart: maintaining a lively flow of visitors and research instructors, for whom he skillfully got support in the form of research grants. The University cooperated by making an apartment building available to house the visitors. Affectionately called "the compound," the modest building has been the birthplace of many a fine theorem. Especially memorable was the academic year 1960-1961, when Walter Feit and John Thompson, visiting for the entire year, made their big breakthrough in finite group theory by proving that all groups of odd order are solvable.

Early in his second three-year term as chairman, Albert was asked to assume the demanding post of dean of the Division of Physical Sciences. He accepted, and served for nine years. The new dean was able to keep his mathematics going. In 1965 he returned to his first love: associative division algebras. His retiring presidential address to the American Mathematical Society, "On Associative Division Algebras," presented the state of the art as of 1968.

Requests for his services from outside the University were widespread and frequent. A full tabulation would be long indeed. Here is a partial list: consultant, Rand Corporation; consultant, National Security Agency; trustee, Institute for Advanced Study; trustee, Institute for Defense Analyses, 1969-1972, and director of its Communications Research Division, 1961-1962; chairman, Division of Mathematics of the National Research Council, 1952-1955; chairman, Mathematics Section of the National Academy of Sciences, 1958-1961; chairman, Survey of Training and Research Potential in the Mathematical Sciences, 1955-1957 (widely known as the "Albert Survey"); president, American Mathematical Society, 1965-1966; participant and then director of Project SCAMP at the University of California at Los Angeles; director, Project ALP (nicknamed "Adrian's little project"); director, Summer 1957 Mathematical Conference at Bowdoin College, a project of the Air Force Cambridge Research Center; vice-president, International Mathematical Union; and delegate, IMU Moscow Symposium, 1971, honoring

Vinogradov's eightieth birthday (this was the last major meeting he attended).

Albert's election to the National Academy of Sciences came in 1943, when he was thirty-seven. Other honors followed. Honorary degrees were awarded by Notre Dame in 1965, by Yeshiva University in 1968, and by the University of Illinois Chicago Circle Campus in 1971. He was elected to membership in the Brazilian Academy of Sciences (1952) and the Argentine Academy of Sciences (1963).

In the fall of 1971, he was welcomed back to the third floor of Eckhart Hall (the dean's office was on the first floor). He resumed the role of a faculty member with a zest that suggested that it was 1931 all over again. But as the academic year 1971-1972 wore on, his colleagues and friends were saddened to see that his health was failing. Death came on June 6, 1972. A paper published posthumously in 1972 was a fitting coda to a life unselfishly devoted to the welfare of mathematics and mathematicians.

In 1976 the Department of Mathematics inaugurated an annual event entitled the Adrian Albert Memorial Lectures. The first lecturer was his long-time colleague Professor Nathan Jacobson of Yale University.

Mrs. Frieda Albert was generous in her advice concerning the preparation of this memoir. I was also fortunate to have available three previous biographical accounts. "Abraham Adrian Albert, 1905-1972," by Nathan Jacobson (*Bull. Am. Math. Soc.,* 80: 1075-1100), presented a detailed technical appraisal of Albert's mathematics, in addition to a biography and a comprehensive bibliography. I also wish to thank Daniel Zelinsky, author of "A. A. Albert" *(Am. Math. Mon.,* 80:661-65), and the contributors to volume 29 of *Scripta Mathematica,* originally planned as a collection of papers honoring Adrian Albert on his sixty-fifth birthday. By the time it appeared in 1973, the editors had the sad task of changing it into a memorial volume; the three-page biographical sketch was written by I. N. Herstein.

PH.D. STUDENTS OF A. A. ALBERT

1934 ANTOINETTE KILLEN: The integral bases of all quartic fields with a group of order eight. Oswald Sagen: The integers represented by sets of positive ternary quadratic non-classic forms.

1936 DANIEL DRIBIN: Representation of binary forms by sets of ternary forms.

1937 HARRIET REES: Ideals in cubic and certain quartic fields.

1938 FANNIE BOYCE: Certain types of nilpotent algebras. Sam Perlis: Maximal orders in rational cyclic algebras of composite degree.

LEONARD TORNHEIM: Integral sets of quaternion algebras over a function field.

1940 ALBERT NEUHAS: Products of normal semi-fields.

1941 FRANK MARTIN: Integral domains in quartic fields.

ANATOL RAPOPORT: Construction of non-Abelian fields with prescribed arithmetic.

1942 GERHARD KALISCH: On special Jordan algebras.

RICHARD SCHAFER: Alternative algebras over an arbitrary field.

1943 ROY DUBISCH: Composition of quadratic forms.

1946 DANIEL ZELINSKY: Integral sets of quasiquaternion algebras.

1950 NATHAN DIVINSKY: Power associativity and crossed extension algebras.

CHARLES PRICE: Jordan division algebras and their arithmetics.

1951 MURRAY GERSTENHABER: Rings of derivations.

DAVID MERRIEL: On almost alternative flexible algebras.

LOUIS WEINER: Lie admissible algebras.

1952 LOUIS KOKORIS: New results on power-associative algebras.

JOHN MOORE: Primary central division algebras.

1954 ROBERT OEHMKE: A class of non-commutative power-associative algebras.

EUGENE PAIGE: Jordan algebras of characteristic two.

1956 RICHARD BLOCK: New simple Lie algebras of prime characteristic.

1957 JAMES OSBORN: Commutative diassociative loops.

1959 LAURENCE HARPER: Some properties of partially Stable Algebras.

1961 REUBEN SANDLER: Autotopism groups of some finite non-associative algebras.

PETER STANEK: Two element generation of the symplectic group.

1964 ROBERT BROWN: Lie Algebras of types E_6 And E_7.

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Leonard Carmichael

Leonard Carmichael

November 9, 1898-September 16, 1973

by Carl Pfaffmann

Leonard Carmichael was born in the Germantown section of Philadelphia, Pennsylvania. He was the only child of Thomas Harrison Carmichael, a successful physician, and Emily Henrietta Leonard Carmichael, an active volunteer worker on many charitable boards. At the time of her death, she was chief of the Bureau of Recreation of Philadelphia. His maternal grandfather, Charles Hall Leonard, D.D., LL.D., was Dean of the Crane Theological School of Tufts University for many years.

Leonard attended the Germantown Friends School, although his parents were not Quakers. He further cemented the family traditions with Tufts when he entered the University in 1917. Not only was his grandfather a dean at Tufts, but his uncles attended college there. Leonard was elected to Phi Beta Kappa in his junior year, and received a B.S. degree *summa cum laude* in 1921. He was much influenced by his senior research project on the embryology of the eye muscles of the shark, which aroused his interest in the sense organs as directors of animal behavior. His interest in sensory psychology and physiology became a dominant theme in his later scientific career. As an undergraduate, he was much influenced by the books of Jacques Loeb, the biologist ultra-mechanist, and C. Lloyd Morgan, the proponent of emergent evolution. After reading Howard C. Warren's *Human Psychology,* however, Leonard decided that psychology (rather than anatomy or physiology) was the discipline in which he could best study the senses with a view to their functional, as well as biological, setting.

He entered Harvard as a graduate student on a fellowship provided by the educational psychologist, Professor Walter F. Dearborn, with whom he developed an especially close association. He was assigned a fine office and adjoining laboratory, and was able to work in the Harvard shop, rebuilding an improved model of the famous Dodge-Dearborn eye movement recording camera. Carmichael was encouraged to satisfy his interest in biology, as well as psychology, and he did so with a number of zoology courses. His first piece of graduate laboratory research was a quantitative study of the reaction of the meal worm (*Tenebrio molitor*) to light, under the direction of G. H. Parker, professor of zoology. Carmichael regarded Parker's lectures on the nervous system and the sense organs as models of clarity and scholarship. Among his psychology professors were E. G. Boring, L. T. Troland, and William McDougall.

Carmichael's continuing interest in the sensory control of, or release of, inborn patterns of behavior led Dearborn to recommend a theoretical and historical Ph.D. dissertation on the psychology and biology of human and animal instincts. A summary of the conclusions was published in an article entitled "Heredity and Environment: Are They Antithetical?" William Preyer's studies of signs of life in the fetus before birth pointed the way for Carmichael to investigate morphological growth of receptors and the nervous system in relation to behavior released at various stages of early ontogenetic development in mammals before learning begins, or is important. After receiving his Ph.D. degree, he was awarded a Sheldon Fellowship, which permitted travel and study abroad. "Report of a Sheldon Fellow," published in the *Harvard Alumni Bulletin* (1925), describes his visits to the University of Berlin and other German universities.

In 1924 he joined the faculty of Princeton to teach physiological psychology and the history and systems of psychology. There he began his research on the development of behavior with larval amblystoma and frog tadpoles. It had previously been shown that their physical development proceeded normally in laboratory Petri dishes when immobilized with a mild concentration of the anesthetic, chloretone. Carmichael focussed upon behavioral development when presumably all sensory input was reduced, and clearly all motor movement inhibited, so that no practice was possible. In the strongly antihereditarian point of view that dominated American behavioral psychology at that time, the outcome of this experiment aroused widespread interest. Carmichael found that when the anesthetic was removed, the experimentally treated organisms swam with vigor and coordination equal to that of the undrugged controls, who were allowed to move throughout development. As he stated in his autobiography:

> These studies supported a hereditary rather than an environmentalistic theory of the determination of the growth of organized behavior. At the time, the results of these experiments surprised me and almost shocked me. They did not support my then strongly held belief in the determining influence of the environment at every stage in the growth of behavior.*

Carmichael's reports of these experiments in *Psychological Review* (1926, 1927, and 1928) seemed to dodge the obvious conclusion. He continued to speak of the intimate interrelation of heredity and environment and the difficulties of disentangling their interaction.

* Leonard Carmichael, "Leonard Carmichael," in *A History of Psychology in Autobiography,* ed. E. G. Boring and Gardner Lindzey, vol. 5 (N.Y.: Appleton-Century Crofts, 1967), p. 37.

It was also at Princeton that he became interested in the history of research on reflex action, and published two papers, one on Robert Whytt and the second on Sir Charles Bell. Carmichael made frequent mention of Bell as an early contributor to physiological psychology. Indeed, Carmichael and his graduate students and colleagues formed the Sir Charles Bell Society and met together for dinner and general reports of one's doings during the Annual Meetings of the American Psychological Association.

Carmichael's paper on Bell (*Psychological Review,* 1926) was a careful review of Bell's contributions, such as his recognition in 1811 of many of the facts that Johann Müller later included in his 1838 *Handbook* under the doctrine of specific nerve energies. Bell clearly understood that the same stimulus will give two different sensations, depending upon the nerves affected. He noted that a sharp steel point applied to one type of papilla on the tongue would cause a feeling of sharpness by way of the sense of touch. When a taste papilla was touched, he perceived a metallic taste but no touch. Bell also gave a treatment of the five senses, reciprocal innervation of antagonistic muscles, and wrote on the expression of the emotions. On Bell's controversial priority for the demonstration of the separate functions of the dorsal and ventral roots of the spinal cord, Carmichael supported Bell's priority on the law that bears his name. Carmichael noted: "Magendie perhaps independently gave the principle a more exact formulation and a clear physiological proof."* More recent historical documentary evidence has become available and is interpreted by Cranefield (1974) to give the priority to Magendie.†

* Leonard Carmichael, "Sir Charles Bell: A Contribution to the History of Physiological Psychology," *Psychological Review,* 33: 196.

† Paul Frederic Cranefield, *The Way In and the Way Out, François Magendie, Charles Bell and the Roots of the Spinal Nerve* (Mount Kisco, N.Y.: Futura, 1974).

Carmichael moved to Brown University in 1927 as one of the youngest full professors on the Brown faculty, still in his twenties at the time of his appointment. He had been recruited to build a new laboratory and graduate department and to strengthen the undergraduate program in psychology. Carmichael was an excellent and popular lecturer. His elementary psychology lecture sections filled the largest lecture hall on campus. He personally gave all the lectures in the three successive sections every Monday and Friday morning. He enlivened his lectures with dramatic, but clear, demonstrative material, slides, and film strips. Junior faculty and graduate student teaching assistants conducted the quiz sections during the week. Leonard was voted the most popular teacher at the University a number of times by the students.

I was an undergraduate student at Brown when I first met Leonard. He was then a young bachelor, whose dashing campus image was reinforced by a bright red Buick roadster. The riddle of his numerous trips to Cambridge was solved by his marriage to Pearl L. Kidston of Hudson, Massachusetts, on June 30, 1932. After graduation from college, she worked at Harvard's Graduate School of Education. They had one child, Martha, born during Leonard's last year at Brown. Martha married S. Parker Oliphant, and their first child was named Leonard Carmichael Oliphant.

Although Carmichael was busily involved in organizing the new laboratory and department, equipping it for research and for graduate training in experimental and physiological psychology, and carrying out his own research, he personally taught undergraduate and graduate courses and guided the research of honor undergraduates and several graduate students. While I was an undergraduate at Brown, any doubts on my own career plans were settled after completion of Carmichael's elementary psychology course. In deed, Carmichael was my first and most important mentor and guided my honors and master's research in physiological sensory psychology. He urged me to apply for a Rhoads Scholarship to study physiology at Oxford. The Rhoads Scholarship was awarded to me, and following my studies at Oxford, I went on to Cambridge University. After two years of graduate work under the late Lord Adrian, I received my Ph.D. degree. Throughout the years, my strong personal ties with Leonard and Pearl Carmichael prospered.

At Brown University, Carmichael achieved his long-cherished goal of studying the development of behavior in fetal mammals. His study began with the fetal cat, and he developed an especially designed cradle in which the pregnant cat could be supported, so that after Cesarean section, the fetus, with fetal circulation intact, floated in a bath of warmed saline solution. A high cervical section of the maternal spinal cord permitted discontinuance of anesthetic, and thus the fetus could be studied in a normal physiological state, free of anesthetic.

James Coronius and Harold Schlosberg participated with Carmichael in the first study of the fetal cat. Verbal records of descriptions of the behavior were dictated, and motion pictures were taken. Interest was focussed on the responses to well-controlled sensory stimulation. In addition to fetal cats, Carmichael and his students subsequently made a prolonged series of studies on the development of behavior of the fetal guinea pig. More than 100 cutaneous pressure reflexogenous zones were studied throughout the entire active prenatal life of sixty-eight days. Carmichael noted in *The Experimental Embryology of Mind* (1941):

> Thus it is not the physical character of the stimulus, but rather that it shall be above the threshold of some of the complex of skin receptors and in a specific locus, that determines the response. Such typical patterns of behavior remain amazingly constant in an organism that is rapidly grow

> ing, and, conversely but similarly, growth may suddenly alter such responses, and such alterations of behavior may easily be confused with learned responses, especially in postnatal life.
>
> I have never seen any responses in the late fetus which, in their elements, have not appeared as a typical patterned reaction to isolated stimuli many times before. In the late guinea pig fetus the hair coat is well grown, the teeth are erupted, eyes and ears are functional, and adaptive integrated behavior is well established. At this time such an animal will, to use the language of teleology, attempt in a most effective and even ingenious way to deal with a tactual stimulus applied to its lip. First, it may be, it will attempt to remove the stimulus by curling the lip; then, if the stimulus remains, it is brushed by the forepaw on the stimulated side. If the stimulus still persists, the head is turned sharply. Finally, a general struggle is resorted to which involves movements of all four limbs and all trunk muscles. In a late fetus this final maneuver is sometimes so quick and effective that the experimenter is often thwarted and the offending stimulus is removed—by a guinea pig fetus that is having its own willful and annoying way in spite of anything the experimenter can do. Each of these special responses, however, may be seen as an old one to the person who has watched the growth of fetal behavior.
>
> Complex patterns of behavior emerge as a result of maturation. Such behavior is possibly as truly end-seeking and purposeful as is any behavior in the world which does not involve the use of language. I see no reason to believe that this emergent purposeful behavior is not as natural a result of the processes of growth as is the length of the fetal whiskers, and quite as independent of learning.
>
> The growing animal functions in a way that is in general adaptive at every stage. When I wrote my first papers in this field, dealing with the development of drugged amblystoma, I was so under the domination of a universal conditioned reflex theory of the development of adaptive responses that I denied categorically the truth of the statement just made. But every experiment that I have done in the field of the early growth of behavior has forced me to retreat from this environmentalist hypothesis. Now, literally almost nothing seems to me to be left of this hypothesis so far as the *very early* development of behavior is concerned.

The classical work of Preyer and Coghill on the sequence of motility in the developing amphibian larvae showed the first movement to be a C shaped or reversed C curvature. This was followed by an S or sigmoid form of reaction. The

S movement was fundamental to swimming, which consisted of a succession of sigmoid movements before the limbs developed. When they did appear, both sets of limbs moved only as part of the larger trunk movement. Independent limb action gradually began to individuate out of the dominant trunk movements. Movement of the trunk in walking was regarded as nothing more nor less than swimming movements at a generally reduced speed. Development, from the very beginning, was a progressive expansion of a perfectly integrated total pattern from which partial patterns individuated with various degrees of discreteness.

Carmichael saw something different in fetal mammals. He gave more importance to the early individuation of quite specific responses, which later became parts of integrated behaviors. Rather than debate the pros and cons of a wholistic versus specific development, Carmichael cautioned that the researcher would do better to record as unambiguously as possible the responses made by a fetus at any stage—rather than to fit all developmental changes into one formula. He agreed with William James's statement that: "Psychology must be writ both in synthetic and analytic terms."*

Carmichael's work began at a time when the advances in ethology documenting the release of species-specific behavior by patterned stimuli were not well known to the American biological and psychological communities. The regular occurrence of these species-specific behaviors, and their occurrence in vacuo, that is, where animals were reared in isolation so that postnatal experience did not occur, led Konrad Lorentz and Nikko Tinbergen to argue for the instinctive basis of much of animal behavior that occurred under natural circumstances. Such "releaser stimuli" were

* William James, *Principles of Psychology,* vol. 1 (N.Y.: Dover, 1890), p. 487.

often perceptually complex, for example, a sequence of movements by another animal, coloring and size of an egg, or particular location and size of a red bill spot.

Psychologists as a group even now tend to be cautious in attributing behavior patterns to genetically determined processes or propensities. Still, increasing interaction among students of animal behavior and psychology is leading to a sounder appreciation of the role of genetic determinants in behavior, both in their own right and as setting the stage upon which experience and learning can interact. Carmichael's influence on thought regarding the development of behavior and its sensory control was, in a sense, premonitory of such changing views on the heredity-environment issue. His two editions of the *Manual of Child Psychology* (1st ed., 1946; 2nd ed., 1954), and a more recent third edition (1970) of *Carmichael's Manual of Child Psychology,* under Paul Mussen's editorship, are witness to his never flagging interest in behavioral development.

Carmichael left Brown University in 1936 to become Dean of the Faculty of Arts and Sciences and professor of psychology at the University of Rochester. Two years later, he accepted the presidency of Tufts University with the understanding that he be allowed to continue his scientific work. However, he was less able to devote his energies to his past scientific interests, since World War II efforts overlapped with his Tufts years. The Laboratory of Sensory Physiology and Psychology at Tufts turned to war-related projects which included the improvement and application of new techniques to the study of eye movements and visual fatigue. Electronic, rather than ocular photography proved more suitable for long time reading fatigue studies, an old interest from his days with Dearborn.

To this method of registration could be added the simultaneous registration of brain waves, the electrical signs of oscillatory neural activity in different brain regions throughout the reading and other visual tasks. A book, *Reading and Visual Fatigue* (co-authored with Dearborn), appeared in 1947. He had pioneered with H. H. Jasper at Brown and the Bradley House some of the first EEG (electroencephalographic) registration of brain waves in humans and animals (1935).

He contributed in many other ways to the war effort. He was particularly proud of his role as director of the National Roster of Scientific and Specialized Personnel, which did invaluable work in the recruitment and assignment of scientists for the atomic energy and radar projects, among others. In the period from 1939 to 1945, he commuted between Tufts and Washington once or twice weekly, as he mentioned in his autobiography, "spending more than a year of nights on a sleeping car between Boston and Washington."* He also served on a number of advisory committees and boards at the national level. In 1947 and 1948, he was chairman of the American Council on Education.

Carmichael was elected to the American Academy of Arts and Sciences in 1932 and to the American Philosophical Society in 1942. He was elected to the National Academy of Sciences in 1943 and served as the chairman of its Section on Psychology from 1950 to 1953. He was president of the American Philosophical Society from 1970 to 1973. For almost a quarter of a century, he was a member, and for much of the time chairman, of the Board of Scientific Directors of the Yerkes Laboratories of Primate Biology. Later he served on a similar board for the Delta Regional Primate Research Center and for many years was on the

* Leonard Carmichael, "Leonard Carmichael," in *A History of Psychology in Autobiography,* ed. E. G. Boring and Gardner Lindzey, vol. 5 (N.Y.: Appleton-Century Crofts, 1967), p. 48.

Board of Scientific Overseers of the Jackson Memorial Laboratory at Bar Harbor.

Upon his call in 1953 to the Smithsonian Institution Secretaryship, Carmichael turned his considerable administrative talents to improving that Institution, to which was added, among other things, the new Museum of Sciences and Technology—the Smithsonian's first major new building in fifty years. Two wings were added to the Museum of Natural History, and the old Patent Office Building was acquired to serve as a home for the National Collections of Arts and the National Portrait Gallery. During his eleven years of tenure, the annual congressional appropriation rose from $2.5 million to over $13 million.

He found the opportunity to indulge, to some degree, his interest in behavioral development. He gave notice to the superintendent of the Washington Zoological Park that he wished to be called, no matter what the hour, when a birth was imminent among any of its numerous animal species. I remember his recounting how the newly born giraffe would struggle to its feet, and in relatively short order begin to display coordinated, though awkward, motor patterns. He became much interested in the developmental studies of primates, and indeed served as first president of the International Primatological Society.

Upon his retirement from the Smithsonian in 1964, he was elected Vice President for Research and Exploration of the National Geographic Society. He had been a trustee of the Society for many years and served for a time as chairman of its Committee for Research and Exploration. He was able to further his long-time interest in primate research, taking the opportunity to observe troops of wild temperate-zone monkeys in Japan, and to watch for some days over thirty wild chimpanzees deep in the forests of East Africa. He was proud of the Geographic's support of the original and epoch making field studies of Jane Goodall on chimpanzees in their natural habitat. The frontispiece in this article was one of his favorite photographs.

Throughout his busy career, he continued active work as editor of psychology books for Houghton Mifflin. At the request of Random House in 1957, he wrote *Basic Psychology,* which gave his general point of view about psychology for the educated reader. He was delightfully surprised by its wide and continuing acceptance over the years. In 1964 he wrote a chapter on "The Early Growth of Language Capacity in the Individual" in a book entitled *New Directions in the Study of Language,* edited by E. H. Lenneberg.

The photographically beautiful book, *The Marvels of Animal Behavior,* published in 1972 by the National Geographic Society, began with his introductory chapter, "Man and Animal, a New Understanding." In this, he covered a broad canvas of man's interest in animals, as manifested in the art of ancient and vastly different cultures, totemism, biblical and classical antiquity, and modern science, especially ethology. The book depicts not only behavior in the wild, much of it social behavior, but gives good accounts of field work and experimental studies. Peter Marler of The Rockefeller University worked with Carmichael as editorial consultant, aided by a distinguished group of animal behaviorists. Marler's own work provided subtle examples of how experience in bird song learning interacted with innate predispositions and provided another kind of documentation in support of Carmichael's view that learning itself always depended upon maturation or growth. Such recent work added to Carmichael's convictions that many psychologists during the last half century had given far too little weight to the role of inheritance in behavior change during individual development. It was a source of satisfaction to him that his lifetime study of receptor-initiated behavior had given him over the years a better and better understanding of the mechanisms of adaptive response and of mental life.

Leonard Carmichael as a person was formidable. He was taller than average and had an unusually resonant voice. For over half of his career, he was extremely formal in personal relations. He never called his graduate students by first names until some several years after their doctorate. He was similarly formal with his working associates. With years, however, he mellowed, as do most. Gatherings of his former students at meetings of the Sir Charles Bell Society became more relaxed, but still formal. Those meetings, hosted by Leonard and Pearl at their Georgetown home, with a superb buffet and ample libation, were a cordial exchange of academic reminiscences and family doings, and less the inquisitions on research done or not done that had characterized earlier meetings. The mood was one of affectionate loyalty to the "good doctor."

Much more could be said of Leonard Carmichael, his activities in national affairs and in the scientific and educational domains. His memberships, officerships, awards, and distinctions, too numerous to recount, include twenty-three honorary degrees, the Presidential Citation of Merit, the Public Service Medal of the National Academy of Sciences, orders of merit from four foreign countries, fellowships, trusteeships, and a legion of responsibilities and duties of distinction. His honorary degree citation from Harvard best sums it up: "A psychologist who combines distinction in his science and success in administration."

I wish to express my appreciation to Mrs. Leonard Carmichael for the wealth of bibliographic and other material provided and to Leonard Mead for information on the Tufts years.

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L. R. Cleveland

Lemuel Roscoe Cleveland

November 14, 1892-February 12, 1969

by William Trager

Professor Cleveland's scientific work was his life, or so it seemed to his students and colleagues. Always ready to talk science, he rarely revealed anything of his private life. This is unfortunate, since he had an unusual background. Born November 12, 1892 in Newton County, Mississippi, he grew up in this rural area with Indian children as his playmates. He worked on the farm and once remarked that in three years with the same mule he had gotten to know it better than most of his human friends. After two years of high school in Union, Mississippi, he entered the University of Mississippi where he received a B.S. in 1917 and then spent a year as a graduate student and Instructor in zoology. After a brief period of military service he taught at Emory University for two years and at Kansas State College for one year. He then entered Johns Hopkins University where he began the career of highly productive scientific research that terminated only shortly before his death in 1969. After receiving his Ph.D. in 1923 he stayed on at the Hopkins School of Hygiene and Public Health as a National Research Council Fellow until 1925 when he went to the Department of Tropical Medicine at the Harvard Medical School.

It was at Harvard that I had the good fortune to meet L. R. Cleveland and to become his first graduate student. In the fall of 1930 Cleveland's laboratory was an exciting place. He was just finishing his studies on cultivation of the human dysentery amoeba, *Entamoeba histolytica.* He and his associates had developed a much improved medium in which the entire life cycle of this important parasite could be propagated. They had also made initial attempts at bacteria-free cultivation of amoebae. With characteristic insight, Cleveland recognized the importance of what we now call axenic cultivation, but his efforts toward this end with *E. histolytica* were not successful. (Success in this was not achieved until many years later, with L. S. Diamond's work in 1961.)

Most exciting, however, was the new material Cleveland had brought back from the mountains of Virginia. He had discovered that the large wood-dwelling roach *Cryptocercus punctulatus* contains a seething mass of protozoa in an enlarged portion of its hindgut. All were new species, many representing new genera and new families. But what was especially significant and particularly fascinating to Cleveland was the fact that these protozoa were obviously closely related to the symbiotic intestinal flagellates of termites. The protozoa of termites had been known for many years, but it was Cleveland who first discovered their symbiotic nature. In a series of elegant experiments, done while he was a fellow of the National Research Council at the Johns Hopkins University School of Hygiene, Cleveland showed that the ability of termites to live on a diet of wood or cellulose depends on the digestive capacities of their intestinal flagellates. Termites deprived of these protozoa, but still infected with intestinal bacteria and spirochetes, would die of starvation if fed only wood or cellulose, but they could be saved if reinfected with the protozoa. This was the first instance in which a mutualistic relationship between internal microorganisms and their metazoan host was clearly proved. It was pioneering work (published in 1923-1928) that paved the way for many later studies on symbiotic microorganisms.

One can easily imagine Cleveland's delight at finding in a different kind of insect, the wood-feeding roach, the same types of protozoa with which he had already been so successful in termites. It did not take him long to establish that the roach *Cryptocercus,* like termites, depends on its intestinal flagellates for its ability to utilize cellulose as its principal food. He then embarked on a detailed study of all the new species of protozoa living in the hindgut of *Cryptocercus.* With the aid of a devoted research assistant, Miss Jane Collier, and of two postdoctoral fellows, Dr. Elizabeth Sanders Hobbs and Dr. S. R. Hall, he soon published a classic monograph (see bibliography, 1934).

Cleveland worked for the rest of his life mainly on taxonomic and experimental studies with the protozoa of *Cryptocercus.* Early in these studies he was the first to see and photograph in a living cell the fibers of the mitotic apparatus. In part because of their large size, certain of the flagellates of *Cryptocercus* provided exceptionally favorable material, but it was Cleveland's exacting microscopy and his application of the then newly available phase contrast methods that led to his beautiful results. He also gave much thought to the role of the centriole and its attendant organelles in cellular division. He produced two monographs (*Trans. Am. Philos. Soc.,* 1949 and 1953) on behavior and structure of chromosomes.

Far exceeding all these observations in general biological importance was Cleveland's discovery of the effect of molting of the host insect on sexual reproduction in its intestinal protozoa. Soon after he began working with *Cryptocercus* he noted cyst formation and various anomalous reproductive stages among the protozoa. There was a period of over five years during which Cleveland published hardly any papers while he was trying to determine what was really going on. Then came a long series of papers on the sexual cycles of the flagellates of *Cryptocercus* (summarized in a paper in J. *Protozool.,* 3[1956]:161-80). Sex had been unknown in these families of protozoa. Cleveland now showed that the sexual cycle was in all cases related to the molting cycle of the host insect. Some of the protozoa underwent only autogamy, others formed male and female gametes which fused and then underwent zygotic meiosis. Though the timing of these events also differed from species to species, it could always be correlated with molting in the roach. Furthermore, Cleveland showed that injection of an adult roach with a dose of the molting hormone ecdysone too small to induce molting in the insect, nevertheless did induce the sexual cycles of the protozoa. The effect seems to be a direct one by the host hormone on the symbiotic protozoa. In the later stages of his scientific career Dr. Cleveland prepared several excellent cinemicrographs dealing with the sexual cycles of flagellates of *Cryptocercus* and with the structure and movement of these protozoa as well as of protozoa in termites.

Since *Cryptocercus* occurs, in the eastern United States, only in the Blue Ridge Mountains, Cleveland regularly spent his summers at the Biological Station at Mountain Lake, Virginia, and, late in his career, at the Biological Station at Highlands, North Carolina. Here his rural youth surely served him well as he swung his ax to break up logs in the search for colonies of *Cryptocercus.*

Cleveland was elected to the National Academy of Sciences in 1952. He was President of the Society of Protozoologists in 1955 and was an honorary member of the Society. At Harvard, he moved in 1936 from the Medical School to the Biology Department where he was advanced to Professor of Biology in 1946. He became Emeritus Professor in 1959 and, at the invitation of R. B. McGhee, went to the University of

Georgia at Athens. Here he continued his active research. In 1965 he was among the protozoologists who were particularly honored at the Second International Congress on Protozoology in London.

Unlike his triumphant scientific career, Cleveland's personal life was marked with sadness. His first wife, Mabel Bush, whom he married in 1925, died of cancer in 1936, when their daughter, Margaret Elaine, was three years old. Margaret died when only twenty-five.

Cleveland's second marriage, to Dorothy Eleanor Colby, was more fortunate. She, and their son, Bruce Taylor Cleveland, a physicist, survive.

Cleve, as we called him, was dearly loved by his friends and family. He was sometimes difficult, but only because completely honest men are likely to be difficult. He will be remembered mainly, however, for his great body of scientific achievement, and this is surely the way he would want it to be.

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Lester Reynold Dragstedt

October 2, 1893-July 16, 1975

by Owen H. Wangensteen and Sarah D. Wangensteen

Lester Reynold Dragstedt, one of America's great surgical scientists, approaching his eighty-second birthday, died suddenly of an unexpected heart attack on July 16, 1975 at his summer home at Wabigama, a colony he and other University of Chicago scientists had founded on Elk Lake, Michigan, in 1951. Dragstedt had been active and apparently well up until the very end.

Dragstedt was born in Anaconda, Montana, on October 2, 1893, of Swedish immigrant parents. In his early life, Lester was encouraged by his father to memorize poetry with a special appeal to him, as well as Biblical passages and fragments of famous speeches. These he frequently recited from memory at various gatherings in Anaconda, a talent which found ready favor with many audiences and served him well in informal presentations throughout his professional life. Young Dragstedt graduated valedictorian of his high school class and was offered scholarships at the University of Chicago and other institutions. At this juncture, A. J. Carlson, a long-time friend of the Dragstedt's who had defected from the ministry to become an internationally renowned professor of physiology at the University of Chicago, intervened and wisely advised the senior Dragstedt, "Send the boy to Chicago. They will find out in three months if he has any brains and, if he does not, you can bring him back to Anaconda and put him to work in the copper smelter."*

In the beginning, Dragstedt. entertained the idea of becoming a physicist, having enjoyed the privilege of hearing lectures by Professor Robert Millikan. He was greatly influenced, however, by the inquiring and critical mind of A. J. Carlson, and upon graduation with a Bachelor of Science degree in 1915, enrolled in the graduate program at the University of Chicago as a student in physiology. Lester acquired a Master of Science degree in 1916 and a Doctor of Philosophy degree in physiology in 1920. An M.D. degree from Rush Medical College followed in 1921.

During his graduate studies, Dragstedt became a talented operating surgeon, having acquired skills operating upon animals in pursuit of physiological experiments. Though attracted to surgery, he was convinced that a career in physiology held out greater promise for innovative accomplishments.

Lester's first academic appointment was instructor of pharmacology at the State University of Iowa in 1916; the following year he became assistant professor of physiology there, a position to which he returned in 1919 after military service in World War I. It was at Iowa that Lester met Gladys Shoesmith, then a student at the University. Four years later, in 1922, they were married, by which time this talented young lady was not only a teacher of English, but principal of a school. She gave up her own career for another for which she was eminently suited—becoming Lester's constant companion and devoted supporter, in fair and stormy weather, throughout his illustrious life.

Dragstedt returned to the University of Chicago in 1920 as assistant professor of physiology and in 1923 became pro

* John H. Landor, "L.R.D.—Recollections and Reminiscences," *Surgery,* 8 (1977): 443.

fessor and chairman of the Department of Pharmacology and Physiology at Northwestern University. He maintained throughout his career a very close association with Carlson, his loyal mentor and advisor.

Dragstedt's second career began in 1925 when Dallas Phemister was appointed the first full-time professor and chairman of the new Department of Surgery at the University of Chicago. Prior thereto, Phemister had been in active surgical practice but had exhibited strong academic leanings. Before taking up his new duties, Phemister went to London and Europe to work and observe in preclinical science departments and to ready himself for the new opportunities and responsibilities at the University. Phemister appointed Dragstedt consultant to the architect to design suitable research facilities for members of the Department of Surgery. At the conclusion of this service, Phemister remarked to Dragstedt, "I am interested in teaching physiology to surgeons."* Phemister was convinced that Dragstedt, with his strong background in physiology and pharmacology, could make an important contribution to the new Department of Surgery, and he persuaded Lester in 1925 to abandon a promising career in physiology to become a physiologist-bsurgeon. Already skilled in the performance of technically difficult operations upon dogs, Dragstedt emerged as one of the great surgeons of the alimentary tract of his generation.

As a Rockefeller Fellow, Dragstedt went abroad in 1925 to gain experience in surgical pathology and clinical surgery. This was a dozen years before the development of the American Board of Surgery, which undoubtedly would not have lent its seal of approval to Dragstedt's unorthodox scheme of acquiring training in clinical surgery for the academic arena. Lester was accompanied by his mother; his wife, Gladys; and

* E. R. Woodward, "Lester R. Dragstedt, M.D., Ph.D.," *Gastroenterology,* 70 (1976): 3.

their daughter, Charlotte. Carol was born during the two-year stay in Europe.

Following temporary stops in Paris, at de Quervain's surgical clinic in Bern, Switzerland, and in Vienna with Eiselsberg, Dragstedt spent several months performing postmortem examinations at the Allgemeines Krankenhaus under the tutelage of Jakob Erdheim, whom Dragstedt came to admire greatly. He then proceeded to Budapest and worked under the direction of the famed gastric surgeon, Eugen Polya, and later with Professor Hümer Hültl at St. Rochus Hospital. Lester fell heir to a rich experience in operative surgery under these teachers for a fee of $150 a month. When Dragstedt returned, Phemister gave him an appointment as associate professor of surgery at the University of Chicago.

Phemister was unquestionably correct in his belief that Dragstedt could be persuaded to become a clinician. In fact, the titles of Dragstedt's papers—from his first publication in 1916 up to the time he accepted Phemister's proposal in 1925—suggest that here was a clinician in spirit, employing physiologic approaches in the resolution of clinical problems, a practice that Dragstedt continued throughout his great career.

Concerning Dragstedt's unusual training for clinical surgery, it may be recalled that Harvey Cushing remarked, concerning his own years in the laboratory with Hugo Kronecker in Bern and with Charles Sherrington at Liverpool, "I acquired more of real value for my surgical work than in my previous six years' service as a hospital intern."* Apart from native talent, it was Dragstedt's prior training in physiology and consistent use of scientific methods that accounted for his unusual success as a clinical surgeon.

* Harvey Cushing, "Instruction in Operative Medicine," *Yale Medical Journal,* 12 (1906): 879.

DRAGSTEDT'S VIEWS OF SURGICAL TRAINING AND HOW HE BECAME A SURGEON

Brief reference has been made to Dragstedt's preparation to become a surgeon, but who can speak better to the point than Dragstedt himself? In response to a letter of October 20, 1971, complaining of the rigidity of the training program of the American Board of Surgery, Dragstedt replied with a long letter on December 29, in which he outlined his own unconventional scheme of surgical training. His letter is so unique and tells so much about Dragstedt that it deserves to be quoted as written:

> I enjoyed reading your letter of October 20 very much indeed. Like you, I believe there should be more than one road to Rome. I have an idea that there is actually more than one road to Rome, but at present there seems to be only one road to certification by the American Board of Surgery. I have long felt that the rigid program of the Board tends to stifle creative work. When I was in charge of surgery at Chicago I required that the applicants for residency in general surgery spend a full year in laboratory research before entering upon the clinical part of their training in the residency. We maintained this full-time research year as an integral part of the residency training all during my tenure. I am not certain, however, that it is being maintained at the present time. During this year of research many of our prospective residents worked with me in my laboratory. I endeavored to get them to start thinking about research problems that were both important and practical for the limited time period. For the most part, however, when they began their research they worked with me on problems that I had already started, but not finished. On the way they learned the method of research and thought about problems of their own. I believe this method valuable for most of the young men who enter upon a research career. A few men had original ideas and some notion as to how to go about solving them. Usually after a year of work each of the residents has a fair concept of the method of research and how to go about it.
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> Now you are interested in my own training and experience. Here is a brief rundown of my medical career. At the end of my second year in the medical school at the University of Chicago I received a B.S. in Science. I then entered upon the training for a Ph.D. in Physiology with Dr. A. J. Carlson. At the end of one year of this training, I secured a Masters Degree

in Physiology with a minor degree in Pathology. I then went on to the University of Iowa as Instructor in Pharmacology. After one year there I was promoted to Assistant Professor of Physiology at the University of Iowa. While at Iowa I introduced mammalian work in both physiology and pharmacology and continued my research on intestinal obstruction and succeeded in keeping dogs alive after complete removal of the duodenum. I was gratified many years later to get a letter from Dr. [Allen] Whipple telling me that it was this paper that suggested to him his radical operation for cancer of the pancreas. While I was in Iowa City the United States got into World War I and I joined the Army. I went first to Washington, D.C. to the Army Medical School and was assigned to work on typhoid vaccine with Colonel Vedder. I was a private <u>second class</u> at this time. After several months I got tired of this activity and requested a transfer. I was thereupon sent out to Fort Leavenworth, Kansas to get training in the Army Medical corps. When they found out that I had training in Pathology I was made a <u>second</u> lieutenant and sent to Yale. While I worked at Yale under Colonel Winternitz in the toxicity laboratory I was assigned to teach toxicology to the officers of the medical corps stationed at Yale. The Spanish influenza became epidemic at that time and I was transferred to Camp Merritt, New Jersey as the camp pathologist. This was my best experience in the Army as I had to do autopsies from morning until night for about eight months. When the Armistice was signed I got the Dean at the Medical School in Iowa to request my return to teaching. After about half of a year of teaching at Iowa I decided to return to Rush Medical College and get my M.D. degree. While taking my last two years of medicine at Rush I also finished up the requirements for the Ph.D. degree at the University of Chicago. During this time I presented several papers on intestinal obstruction, removal of the duodenum and parathyroid tetany for the Chicago Surgical Society. Dr. Phemister was one of my teachers at Rush and was apparently impressed by the papers that I gave at the Chicago Surgical Society. When I finished my medicine Dr. Phemister urged me to take an internship in the Presbyterian Hospital with a view to becoming a surgeon. At that time there was no regular residency of the present type available and one became a surgeon by becoming an apprentice to an operating surgeon. I was reluctant to give up research, and in this frame of mind Dr. Carlson persuaded me to come back to his Department of Physiology as an Associate Professor. I stayed there for two years and then became Professor of Physiology and Pharmacology and Chairman of the Department at Northwestern University Medical School. Mrs. Montgomery Ward gave a large amount of money for the erection of a new medical school and this activity kept me quite busy. Along about this time the Rockefeller Foundation became interested in establishing a new type of medical school on the campus at the University of Chicago. They chose Dr. Phemister to be the first chairman of the department of surgery. Dr. Phemister wanted a department of surgery characterized by research activity. He prevailed upon me to give up my appointment at Northwestern and join the Department of Surgery as an associate professor of surgery. He said he thought it would be easier for a scientist to learn to be a surgeon than for a surgeon to learn to be a scientist. I was very happy at the appointment and taking his advice went to Europe for clinical training. I had no luck in Paris and then went on to Berne, Switzerland. I served as a voluntary assistant to Professor DeQuervain for three months. The work there was mostly thyroid surgery. I wanted training in abdominal work and so went on to Vienna. While in Vienna I took advantage of the opportunity to work with Jacob Erdheim, one of the greatest teachers that I have ever met. I worked all morning in pathology with Erdheim and in the afternoon with a young surgeon named Goldsmith at the Rothschild Hospital. While I was working at the Rothschild Hospital with Goldsmith I got acquainted with Fritz Silverstein, Head of the Department of Experimental Pathology at the University of Vienna. Silverstein knew of my work on the duodenum and asked me if the dogs from whom I'd removed the duodenum developed pernicious anemia. I had to admit to my chagrin that I had not made any measurements of the blood to see if this was the case so I embarked on a program of taking out the duodenum for Fritz Silverstein in Paltaufs old laboratory. While there I got acquainted with a number of the active research men at the University of Vienna—Pineles, Frölich, Winternitz, and many others. I was urged to go over and work with Professor von Eiselsberg which I did as a voluntary assistant for a short period. I was anxious to get to do some operating myself by this time and so took advantage of the economic conditions in this post-war period to go on to Budapest. I went immediately to Polya and told him that we knew about his fine work in America, that I would like to be his assistant and that I could pay him $150 a month for the privilege. All this was said in one breath. He readily assented and took me on as his first assistant. After he did a gastric resection for a duodenal ulcer he invited me to do the next one. I had done a lot of these, of course, in dogs, but had never done a gastric resection in man. I did the resection in the way that I customarily did in the dogs and he was apparently very pleased. I had been taught to close the duodenal stump by an ingenious method that I believe originated with Halsted. I had been taught this during my student period in the physiology laboratory in

Chicago by Dr. James J. Morehead [Moorhead], a local surgeon. Morehead had taught me how to do gastroenterostomies, gastric resections, Pavlov pouches and so on during the course of our collaboration on the problem of intestinal obstruction. Of course I didn't say anything to Polya about this work on the dogs. He apparently thought I was a safe operator and told me to go ahead and do all the operating I wanted. After a short period with Polya, however, I heard that Professor Hümer Hültl at the St. Rochus Hospital was a much better surgeon. Accordingly I went on to see Professor Hültl and used the same formula that had gotten me a place with Polya. Hültl accepted and again took me on as his first assistant. Again I helped him with one operation and the next one was a partial gastrectomy for duodenal ulcer. He asked me if I would like to do that operation. I agreed, did the operation the way I had done it on the dog, Hültl was pleased and told me to go on and do all the operating I wanted. I realized, however, that his assistants were there for that kind of work so I assured them I would not do all the operating but that I would like to assist each one of them so that I would learn the methods that they used. This proved to be a good formula and I had a happy time in this hospital for a period of about eight months.

I then returned to Chicago and became an assistant to Dr. Phemister in the Presbyterian Hospital in the mornings and a volunteer resident in the Cook County Hospital in the afternoons. After about six months of this work Billings Hospital was completed and we moved over there. When the hospital was opened I started by serving as Dr. Phemister's assistant and began my research work in the laboratory. As soon as patients began to come in sufficient numbers Dr. Phemister wanted me to take my own service with a resident and an intern. At first I tried to send my big cases to Dr. Phemister, but he refused to take them and insisted that I do them. He was in the operating room next door so I was comforted by the thought that I could always call on him if I should get in a tight spot. Well, Owen, this is the way I became a surgeon. It is a road to Rome that I do not believe is practical anymore. It was made possible by the economic conditions in Vienna and Budapest and by the desire of the Rockefeller Foundation to build a department of surgery where some of the surgeons were investigators. However, I think some sort of modification of this road to Rome might be possible in our modern world.

DRAGSTEDT'S GASTRIC SECRETORY STUDIES

It is not the intent of the authors to examine every publication by the subject of this memoir, but rather to look briefly at his main works. Dragstedt's most important work in a long and productive career concerned aspects of gastric secretion and digestion. It is entirely appropriate, therefore, to trace briefly the long story of theories and early experiments concerning the stomach's behavior. Theories of the nature of gastric secretion are as old as Hippocrates (460-370 B.C.), who thought of that process as cooking—which he termed "pepsis." Theodor Schwann in 1936 confirmed the presence of William Beaumont's "chemical principle" in gastric juice, which he observed was destroyed by heating; being a student of Hippocratic writings, Schwann named the proteolytic enzyme "pepsin." In an essay brought before Landon's Royal Society in 1686, Edward Tyson had established that the gastric juice contained a corrosive menstruum. In studies on the ostrich, the Italian Antonio Vallisnieri (1713) had ascertained the presence of an active digestive agent in the juice. In 1752, Réné Réamur had birds, dogs, and a sheep swallow sponges placed in perforated spheres, permitting direct contact with the gastric juice. He definitely established the solvent power of juice, as did Edward Stevens of Edinburgh in 1777 in similar studies on man, dogs, and sheep. Lazaro Spallanzani, in 1780, experimented on fish, cats, dogs, and man, also affirming the presence of an active digestive agent within the gastric juice. In 1786, John Hunter performed experiments on fish, lizards, and frogs, confirming the findings of prior investigators. He established the idea of the "living principle," concluding that gastric juice does not digest living things, a thesis that Claude Bernard disproved in 1844.

In his significant monograph of 1833, following studies extending back to 1825, Beaumont, a pioneer American military surgeon far removed from academic halls, concluded his extended studies on Alexis St. Martin, who had suffered a shotgun shell injury at close range (1822) to the lower left thorax and upper abdomen. Beaumont was able to close the thoracic wound, but throughout St. Martin's long life the gastric fistula persisted. Beaumont completed his studies on the secretory behavior of St. Martin's stomach with fifty-one observations, two of which were original and fundamental. In conclusion #24, he established the presence of a "chemical principle," antedating by three years the observations of Johannes Müller and Schwann. In conclusion #25, Beaumont demonstrated that the empty stomach contains no hydrochloric acid; that it takes the stimulus of ingested food to provoke secretion—an observation with which many distinguished physiologists disagreed. They failed to recognize that only patients with duodenal ulcer, one of the strongest manifestations of the ulcer diathesis, actually did have free hydrochloric acid in their stomachs, devoid of food.*

GASTRIC FISTULA STUDIES

With James Ellis, Dragstedt (1930) observed that total loss of gastric juice through a gastric fistula or total pyloric obstruction was uniformly fatal, and as the New York surgeons J. A. Hartwell and J. P. Hoguet (1912) had demonstrated in duodenojejunal obstructions in dogs, responded well to liberal intravenous administration of saline solution. Loss of the other gastrointestinal ion, potassium (K), W. B. O'Shaughnessy (1831) had recognized and successfully remedied by intravenous infusions of both the K and Na ions for the severe diarrhea of cholera patients, deficits he replaced in the amounts lost. James Gamble (1925) confirmed the importance of replacement of the K ion in high intestinal obstructions in animals and man. Dragstedt's gastric secretory studies began in 1924, investigations he continued throughout the remainder of a long and creative career.

* O.H. Wangensteen, "Claude Bernard's Work on Digestion," in *Claude Bernard and Experimental Medicine,* ed. Francisco Grande and M. B. Visscher (Cambridge, Mass.: Schenkman, 1967), pp. 45-74.

VAGOTOMY STUDIES

Dragstedt pursued the problem of the pathogenesis of gastric and duodenal ulcer over many years, studies that culminated in Dragstedt's reintroduction (1943) of transthoracic truncal section of both vagi nerves for duodenal ulcer, which Dragstedt and Frederick M. Owens announced they had performed upon patients with duodenal ulcer, the most refractory to treatment of all common manifestations of the peptic ulcer diathesis. The operation was not an innovation; Ivan Pavlov, the great Russian physiologist, and his associates had established the significant influence of the vagi nerves upon canine gastric secretion in the early 1890's. André Latarjet, of Lyons, France, had performed both supra- and infradia- phragmatic vagotomy for gastric states in man, including several instances of duodenal ulcer. Latarjet (1922-1924) learned from his own experience that bilateral vagal section necessitated a drainage procedure because of impaired emptying. Latarjet performed supplemental pyloroplasty to correct the situation. Dragstedt, too, learned the need for a complemental gastrojejunostomy to provide adequate gastric emptying. The effect of vagal section on gastric secretion was to occupy a major share of Dragstedt's attention throughout the remainder of his professional life.

The American Gastroenterological Association, dominated primarily by clinicians, lent Dragstedt's contribution on vagotomy faint praise in the early 1950's. A. V. Pollock of Leeds saved the day for vagotomy (*Lancet* 2: 785-800, 1952) in a report upon 1,524 vagotomies performed for peptic ulcer from scattered large metropolitan areas in Britain. Recurrent neostomal ulcer occurred in 6 percent when done for duodenal ulcer. When complemental gastric drainage was added, the recurrence rate was only 1 percent. Summarized Pollock, "this procedure [vagotomy] will stand or fall by the proved recurrence-rate" (p. 800).

In studies of carefully gathered physiological data, extending over many years, Dragstedt persuaded the surgical profession throughout the world that vagal section was an important component of any and all surgical procedures directed at overcoming the strong acid-peptic ulcer diathesis of duodenal ulcer. It came as a great surprise to many of his fellow surgeons and admirers to learn that Dragstedt (1936), the keenest and most ardent student of that problem, advised changing the name of peptic ulcer to "acid ulcer." Allusion has already been made to the important work of Beaumont (1833), more than a century earlier, and his conclusion that the gastric juice of his experimental subject, St. Martin, contained a "chemical principle" in addition to hydrochloric acid, a thesis arrived at by Beaumont in noting the influence of temperature upon the proteolytic quality of the gastric juice. He confirmed this finding by observing consistently the far greater digestive potential of gastric juice over HCl with a similar pH. Dragstedt's usual care in testing all premises had failed him in this instance.

THE ANTRAL EXCLUSION OPERATION

In 1895, the Vienna surgeon Anton Eiselsberg devised the antral exclusion operation to facilitate removal of difficult pyloric cancers and duodenal ulcers. Two decades later, his protégé Hans Haberer (1914) began to suspect that when gastric resection was accompanied by antral exclusion for duodenal ulcer, neostomal ulcer frequently occurred; by 1921, Haberer had given up the antral exclusion operation. It remained, however, for the London surgeon Heneage Ogilvie (1936, 1938) to demonstrate in two series of gastric resections for duodenal ulcer, one with antral exclusion and the other without, that Haberer had been correct in abandoning the antral exclusion operation for duodenal ulcer. Hans Smidt of Jena (1924) had shown in canine experiments that antral exclusion pyramided the occurrence of neostomal ulcer attending gastric resection for duodenal ulcer; in dogs with an isolated gastric pouch, fed on a meat diet, Smidt observed that, following antral exclusion, such dogs secreted 80 percent more HCl than did his controls. It remained for Dragstedt and his associates to confirm, in a succession of papers with solid data, Haberer's suspicion that antral exclusion was not a physiologic operation.

DRAGSTEDT REJECTED AUTOINTOXICATION AS THE LETHAL FACTOR IN OBSTRUCTION

J. Z. Amussat (1839), a young Paris surgeon, operated upon his famous teacher, François Broussais, for an obstructing rectal cancer, attempting to destroy the cancer and relieve the obstruction with the cautery. Unfortunately, he perforated the bowel, and peritonitis ensued. Appreciating that his reputation as a surgeon was at stake in this unhappy occurrence, Amussat formulated the theory of autointoxication as the responsible agent in his teacher's demise. For almost a century, this thesis dominated medical and surgical thinking as the chief lethal factor in death from bowel obstruction.

In a series of experiments, George Whipple (1913-1917), Nobel Laureate, appeared to have confirmed the autointoxication theory, finding a "toxic proteose" within the distended lumen of the obstructed bowel. Dragstedt was the first to cast serious doubt upon this thesis (1919-20). He observed that isolated closed canine duodenojejunal loops perforated because of the secretory dominance of that segment of the bowel. However, when he left the ends of the isolated loop open to drain freely into the peritoneal cavity, such loops in time became sterile; when the ends of such isolated loops were closed, the dogs tolerated the situation without incident. Through the work of Gamble, of Harvard's Children's Hos pital, and others, the autointoxication theory as the lethal factor in bowel obstruction has been permanently set aside.

CANINE LIVER AUTOLYSIS

It should be observed that Dragstedt, though not trained as a microbiologist, made another significant discovery concerning the lethal factor attendant on leaving a small fragment of adult canine liver free in the peritoneal cavity, detached from its source of blood supply. Frank Mann (1923), of the Mayo Clinic research laboratories, had first made this observation in his classic studies on experimental canine hepatectomy. A number of experimentalists addressed themselves unsuccessfully to its solution. Dragstedt and Ellis (1930) then did a novel and telling experiment. They performed cesarean section on female dogs about to deliver their young. The livers of these unfed puppies, which had not had the opportunity to enjoy their mothers' milk, were placed in the peritoneal cavities of adult dogs. Unlike situations in which liver segments of equal weight from adult dogs were introduced intraperitoneally—which quite uniformly terminated in death of the recipient within 24 hours—liver implants from newborn unnourished pups were well tolerated. When the abdomens of these canine recipients were explored a few weeks later, it was noted that the implants had been completely absorbed. An overlooked and forgotten observation of S. B. Wolbach (1909), Harvard pathologist, served to resolve the mystery: the adult canine liver contains very pathogenic anaerobic bacteria of the *Bacillus Welchii* type. Dragstedt's surmise proved correct and confirmed Mann's observation that a small fragment of adult canine liver, unattached to its normal source of blood supply, was lethal.

DRAGSTEDT AND THE PARATHYROIDS

F. D. Recklinghausen (1891) described osteitis cystica. Max Askanazy (1904), another distinguished German pathol ogist, first identified that condition with tumors of the parathyroid gland. It remained for Felix Mandl (1925), a young surgeon at the Allgemeines Krankenhaus in Vienna, to excise a tumor of the left inferior parathyroid gland and relieve the pain and stop spontaneous fractures in a patient with advanced osteitis fibrosa cystica who had a milky-white urine, owing to the inordinately high excretion of calcium in the urine.

This was about the time that Dragstedt began addressing himself to the problem of parathyroid tetany. The parathyroid glands had been discovered by Ivar Sandström, a young Swedish medical student (1880), an observation he encountered difficulty in getting published. When thyroidectomy was undertaken by Theodor Kocher of Bern, the first surgeon Nobel Laureate (1909), he performed excision of both lobes of the thyroid gland and its isthmus, and unwittingly excised the parathyroids too, the existence of which he was unaware. This sequel of thyroidectomy he subsequently described as "cachexia strumipriva" (1883). W. G. MacCallum, of The Hopkins, related (1909) that post parathyroidectomy tetany could be controlled by the administration of calcium, demonstrating that the parathyroids had an important role in calcium metabolism.

Dragstedt had formulated the thesis that loss of the parathyroids impaired the resistance of the body to toxins, a situation that he combatted with some success in dogs with a milk diet complemented with lactose. The diet factor again! Meanwhile, Adolph Hanson (1923), a general surgeon of Faribault, Minnesota, isolated a potent bovine parathyroid extract with the helpful advice of Arthur Hirschfelder, professor of pharmacology at the University of Minnesota. J. B. Collip (1925) isolated parathormone (which he initially called parathyrin), confirming MacCallum's (1909) thesis that the parathyroids control calcium metabolism. Had Dragstedt tested MacCallum's 1909 premise, his own address to the likely function of the parathyroids undoubtedly would have followed a different pattern. To be thrust into an unexplored field of investigation, almost devoid of guidelines, is always a risky undertaking.

PANCREATIC STUDIES

One of Dragstedt's broadest investigative efforts concerned the pancreas. He demonstrated that a complete external pancreatic fistula was compatible with life; that a complete biliary fistula was not lethal, but did diminish pancreatic juice recovered through an inlying pancreatic duct cannula. He and his associates reported that the adult canine pancreas, like the liver, contained anaerobic bacilli of the *Welchii* pattern, responsible for the high lethal factor of acute hemorrhagic pancreatitis or necrosis, occasioned by retrojection of bile into the duct of Wirsung, or other factors predisposing to pancreatitis. Dragstedt began his historic studies on canine pancreatectomy in 1936, observing, as had Frank N. Allan and co-workers at the University of Toronto in 1924, that total pancreatectomy in the dog was lethal, even though supported by insulin. Dragstedt observed that the addition of a liberal amount of raw bovine pancreas to the diet offered considerable protection against fatty infiltration of the liver, prolonging the lives of the dogs. Dragstedt went on to develop an alcoholic extract of the raw pancreas that also prolonged the lives of depancreatized dogs, concluding the protective agent to be another pancreatic hormone. Dragstedt called the raw pancreatic extract lipocaic ("burns fat") (1938). The Toronto biochemist I.L. Chaikoff observed (*J. Biol. Chem.,* 160: 489, 1945) that a liberal oral intake of methionine, added to a low-fat, high-protein diet, offered the same protection against fatty liver, with prolongation of life, as did raw pancreas or the alcoholic extract of Dragstedt. Chaikoff and his associates believed that the amino acid methionine enhanced the synthesis of choline, which they believed to be the primary lipotropic factor, but which Dragstedt denied. The exact mechanism of the protection provided by raw pancreas, an alcoholic extract thereof (lipocaic), or methionine has not been completely resolved. Most investigators believe that Dragstedt's alcoholic pancreatic extract is not a true pancreatic hormone.

Dragstedt and several associates worked very assiduously and persistently upon this complicated and intricate problem over the 1936-1946 decade, writing a succession of twenty journal articles upon the subject. The term lipocaic is still to be found in medical dictionaries, but that it meets the criteria of a hormone has not been definitely established. It is quite clear that Dragstedt's impact upon the importance of the liver-pancreas relationship has been very significant, even though his conclusion concerning the exact operative nature of that relationship has been challenged.

The Council on Pharmacy and Chemistry of the AMA assessed the status of lipocaic (*JAMA,* 114: 1454-55, 1940). The Council concluded: "In view of the experimental status of lipocaic, the Council postponed consideration to await development of further critical evidence and expressed the view that the preparation should not be recognized for routine practice."

Dragstedt and his co-workers demonstrated that the external secretion of the pancreas did not preclude the deposition of fat in the liver attending canine pancreatectomy, as did feeding raw pancreas. At this time, Dragstedt had become completely absorbed in the clinical role of vagotomy as an effective anti-peptic ulcer remedy and did not pursue the lipotropic action of lipocaic further.

In his many and broad addresses on problems of the pancreas (1933-46), Dragstedt did not become involved in the controversy over the hyperglycemic effect occasionally observed following insulin administration. This was long believed to have been an insulin contamination, but after more than a quarter of a century of consecutive work in competent hands, it has turned out to be another powerful pancreatic hormone, glucagon, a discovery which emerged also from experimental pancreatectomy.

The recent interesting work of Thomas E. Starzl of Denver on the liver-pancreas axis has shown that the problem is far more complicated than the initial workers had contemplated. Recognizing that protocaval shunts predispose to fatty infiltration of the liver, Starzl assessed the role of splanchnic blood flow upon isolated liver segments. He demonstrated that the pancreatico-gastric segment of the splanchnic blood flow had a far greater influence in supporting portal insulin than did the intestinal segment of the splanchnic blood supply and also retarded fatty infiltration of the liver more effectively (*Lancet* 2:1241-42, 1975).

DRAGSTEDT SUCCEEDS PHEMISTER AS SURGICAL CHAIRMAN

In 1947, Cornelius P. Rhoads, known to his friends as Dusty, and I (O.W.) spent a long evening at my hotel in New York, discussing surgery's role in the management of cancer. The eminent pathologist James Ewing (1855:1943), who had long been in command at the Memorial Cancer Hospital in New York, had suffered from painful conditions for which surgery in very competent hands had offered incomplete relief. Ewing was not, therefore, partial to surgery or surgeons. The coming of Rhoads to the Memorial was a great boon for surgery in cancer management. We talked well into the wee hours of the morning, and Dusty urged me to come on as their chief surgeon, a generous offer which I declined. I advised Rhoads to offer the opportunity to Alexander Brunschwig at the University of Chicago, a far better qualified surgeon than I for the task. Brunschwig had published a nice monograph on pancreatico-duodenectomy. He was an aggressive cancer surgeon and well suited for the task.

Little did I then know that this suggestion was to have an important repercussion on Dragstedt's career. It had come to his attention that Phemister, chairman of the Department of Surgery, who had encouraged Lester in 1925 to enter surgery, favored Brunschwig as his own successor; Dragstedt apparently was being overlooked in the selection process. Phemister was powerful enough in the University to make his recommendation stick. In a conversation many years later, Lester affirmed this circumstance, subsequently confirmed in a letter exchange with Loyal Davis (December 2, 1975), then chairman of the Department of Surgery at Northwestern University. Davis told Lester he would have a place for him and would welcome him in Northwestern's Department of Surgery should he be bypassed in the selection process. Davis added that he had no funds for the position, and Lester would have to earn his salary by practice—an arrangement to which Dragstedt assented. Indirectly, therefore, and unwittingly, this surgeon had a hand in making Dragstedt Phemister's successor at the University of Chicago. The migration of Brunschwig to New York served the careers of both men in a very satisfying manner.

DRAGSTEDT'S 1954 APPRAISAL OF HIS SCIENTIFIC ACHIEVEMENTS

In a letter to Academy Home Secretary A. Wetmore on March 22, 1954, Dragstedt indicated the following as his most significant contributions to medical literature: 1) that removal of the duodenum is compatible with life (1918); 2) that dogs undergoing total parathyroidectomy can be kept alive indefinitely when maintained on a milk diet fortified with lactose; 3) that an alcoholic extract of raw pancreas constitutes the hormone lipocaic, which with complementary insulin will sustain dogs in good health following total pancreatectomy, obviating fatty infiltration of the liver; 4) the pathogenesis of gastric and duodenal ulcer and the demonstration of complete vagal section as an effectual method of treating refractory duodenal ulcers.

Indeed, this was a very modest appraisal of his life's work five years prior to retirement from the University of Chicago. Dragstedt was to survive yet another twenty-one years and was to continue the diligent pursuit of his scientific endeavors through that entire period.

Dragstedt's research upon the parathyroids began in 1922, three years before their function had been clarified by Mandl (1925). Lester's work on the control of tetany attending complete parathyroidectomy had persuaded him that the parathyroids had an important role in complementing the liver in detoxifying alimentary tract toxins, a thesis that died when the work of Hanson (1923) and Collip (1925) provided strong support for MacCallum's thesis (1929) that the parathyroids controlled calcium metabolism.

RETIREMENT FROM CHAIRMANSHIP AT THE UNIVERSITY OF CHICAGO

At a dinner at the University of Chicago, May 28, 1959, honoring Lester Dragstedt's long years of successful scientific endeavor, in reply to brief speeches by the Toastmaster Percival Bailey, Paul Cannon, Dean Lowell Coggeshall, and this surgeon (O.W.), Lester rose to make his response. He had notes, but did not use them. He spoke warmly of his gratitude to the University, said he, a truly great University, "one of the best in the world." He spoke of the loyalty of his family, children, and grandchildren; of his debt to his University colleagues, both current and departed; of his gratitude to his many surgeon-friends in the Chicago area; and finally of his wife, Gladys, concerning whom he said, "To her I owe everything; my success and my friends." Lester obviously greatly appreciated the splendid opportunity the University had provided him and the loyalty of his friends and family.

PERSONAL HEALTH CHALLENGES

Dragstedt was a man of great vigor and indomitable spirit who enjoyed good health throughout most of his long life. He did, however, come to know the threat to a career that illness can bring. Perhaps few, if any, surgeons have had so large an experience in the performance of autopsies as Lester Dragstedt. It is not surprising, therefore, to learn that he contracted tuberculosis, which necessitated spending nine months in a tuberculosis sanatorium in Arizona. Subsequently, he underwent examination of his urinary tract at the hands of Chicago's well-known urologic surgeon, Herman Kretschmer, who discovered that Lester had a unilateral tuberculous kidney, demanding nephrectomy. Lester's brother Carl, a distinguished pharmacologist, was on hand for the operation and relates that the right renal artery was so short that Kretschmer was forced to leave a clamp on it. After the first wound dressing the next morning, Carl gradually released the clamp at intervals, latch by latch, until it was completely free, a somewhat risky practice that, with advances in vascular surgery, surgeons today are happy to forego.

Lester fortunately survived a severe bout with typhoid fever in 1927, during which he lost fifty pounds. Throughout most of his adult life, Lester was hard of hearing, a handicap he bore without complaint.

DRAGSTEDT JOINS HIS PROTÉGÉ WOODWARD AT THE UNIVERSITY OF FLORIDA

When Dragstedt retired from his professorship at the University of Chicago in 1959 at age sixty-six, his protégé Edward Woodward invited him to come to the newly formed medical school at the University of Florida at Gainesville. The Dragstedt's adapted happily to the Florida environment, delighted with the lush tropical vegetation, the bright birds that came to their feeders, and the delicate camelias that bloomed in the yard so abundantly under their care. As in Chicago, their home became a mecca for visiting scientists from America and abroad. Colleagues, students, children, and grandchildren recall the warmth and gaiety of gatherings in the Gainesville house, Lester's jovial and cheery hospitality, and Gladys' charm as hostess. They continued to travel widely; it was never a surprise to meet Lester in some remote airport, hospital, or art gallery—always with Gladys, his steadfast and perceptive companion, by his side. Summertimes they returned to their beloved Wabigama for family vacations.

A feature of Dragstedt's training program, pursued throughout his career at Chicago and in Florida, was the four o'clock afternoon tea in the laboratory, to which all Dragstedt protégés allude with nostalgic memories. These gatherings were always informal, with no prearranged agenda. The discussions were frank and open, permitting participation by all with a special interest in the subject matter. At Gainesville, Dragstedt continued his productive career as an experimental physiologist of the secretory behavior of the stomach, pancreas, and liver.

Woodward (1976) related that Dragstedt, without a note or lantern slide, presented a brilliant and lucid review of his life's work in a commencement address in June 1975, to which his audience of students, faculty, and friends responded with a prolonged standing ovation, affirming Dragstedt's ability to instruct, hold the attention of, and charm his audience with any subject upon which he chose to speak. That parting commencement address, no doubt, took exhaustive and exhausting preparation. Upon its completion, Dragstedt departed with his family for his favorite and customary vacation spot on Elk Lake, Michigan—where he died, shortly after arrival, from a sudden, massive coronary occlusion. All efforts directed at resuscitation by his son, Dr. Lester Dragstedt II, proved unsuccessful.

Gladys Dragstedt died two years after Lester. They left four children: Charlotte (Mrs. Thomas Jeffrey), of Gainesville; Carol (Mrs. Robert N. Stauffer), of Atlanta; Lester R. II, surgeon of the Veterans Hospital, Des Moines, Iowa; and John Albert, of St. Mary's College, Oakland, California. The grandchildren number thirteen.

So passed into memory and history one of the great surgical physiologists of this century, who left an indelible and durable imprint upon every area in which he worked; an eminent surgical teacher who enlarged notably upon Phemister's training school for surgical academicians at the University of Chicago. All privileged to have worked with Lester Dragstedt recognized that here was an extraordinarily gifted individual, compassionate and friendly, sympathetically interested in all the problems of his associates. Is it any wonder that his memory is cherished with great pride and warm affection?

The authors wish to express their gratitude to Mrs. Jeffrey and to Dr. Lester R. Dragstedt II for helpful suggestions concerning various facets of the lives of their parents. Dr. Carl A. Dragstedt also supplied valuable data about his brother's life and career.

Many of Dragstedt's protégés were extremely helpful in providing information concerning his professional and scientific career, especially Drs. John H. Landor and Edward R. Woodward. To Dr. Charles F. Klinger, the authors acknowledge their gratitude for aid in the collection of many of Dragstedt's scientific papers and the arrangement of the bibliographic references.

HONORS AND DISTINCTIONS

Degrees

1915 B.S., University of Chicago
1916 M.S., University of Chicago
1920 Ph.D., University of Chicago
1921 M.D., Rush Medical College, Chicago

Honorary Degrees

1953 Doctor *Honoris Causa,* University of Guadalajara, Mexico
1959 *Docteur Honoris Causa,* University of Lyons, France
1969 Sc.D., University of Florida, Gainesville
1973 Doctor *Honoris Causa,* University of Uppsala, Uppsala, Sweden

University Appointments

1916 Assistant, Department of Physiology, University of Chicago
1916-1917 Instructor, Pharmacology, State University of Iowa
1917-1919 Assistant Professor of Physiology, State University of Iowa
1920-1923 Assistant Professor of Physiology, University of Chicago
1923-1925 Professor and Head, Departments of Physiology and Pharmacology,

Northwestern University

1925-1930 Associate Professor of Surgery, University of Chicago
1930-1948 Professor of Surgery, University of Chicago
1948-1959 Thomas D. Jones Distinguished Service Professor of Surgery and Chairman of the Department of Surgery, University of Chicago
1959-1975 Research Professor of Surgery, University of Florida, Gainesville

Memberships in American Organizations and Societies

National Academy of Sciences
Phi Beta Kappa
Sigma Xi
Alpha Omega Alpha
American Association for the Advancement of Science
American Physiological Society

Society for Experimental Biology and Medicine
American Surgical Association
American Society for Clinical Surgery
American Gastroenterological Association
American College of Physicians
American College of Surgeons
American Medical Association
Central Surgical Society
Institute of Medicine of Chicago
American Academy of Arts and Sciences
Honorary Member of the Surgical Societies of Seattle, Los Angeles, Detroit, Minneapolis, Southern California, Graduate Surgeons of Los Angeles, and Boston

Honorary Memberships in Foreign Organizations and Societies

Surgical Society of Lyons
Surgical Society of Paris
Swedish Surgical Society
Argentine Society of Gastroenterology
Fellow of the Royal College of Physicians and Surgeons of Canada
Fellow of the Royal College of Surgeons of England
National Academy of Medicine of Mexico
Royal Academy of Arts and Sciences of Uppsala, Sweden (Foreign Corresponding Member)
Academy of Surgery of France
Association of Mexican Gastroenterologists

American Honors and Awards

1945 Silver Medal of the American Medical Association for original investigation

1946 Gold Medal of the Illinois State Medical Society for original investigation

1950 Gold Medal of the American Medical Association for original investigation

1961 Samuel D. Gross Prize of the Philadelphia Academy of Surgery

1963 Distinguished Service Award of the American Medical Association for research, teaching, and surgical practice

1964 Julius Friedenwald Medal of the American Gastroenterological Association for "Outstanding Achievement in Gastroenterology"

1964 Golden Plate from the Academy of Achievement

1964 Henry Jacob Bigelow Medal of the Boston Surgical Society for "Contributions to the Advancement of Surgery"

1965 Annual Award of the Gastrointestinal Research Foundation

1969 Distinguished Service Award (the first) and Gold Medal of the American Surgical Association

FOREIGN HONORS AND AWARDS

1953 Honorary Professor of Surgery at the University of Guadalajara, Mexico

1965 Gold Medal of the Surgical Society of Malmö, Sweden

1967 Royal Order of the North Star of Sweden, bestowed by the King of Sweden, for "Outstanding Contributions to the Science of Surgery"

1969 Silver Plaque of the Institute of Digestive Diseases and Nutrition of Mexico City

1969 Silver Plaque of the Association of Mexican Gastroenterologists

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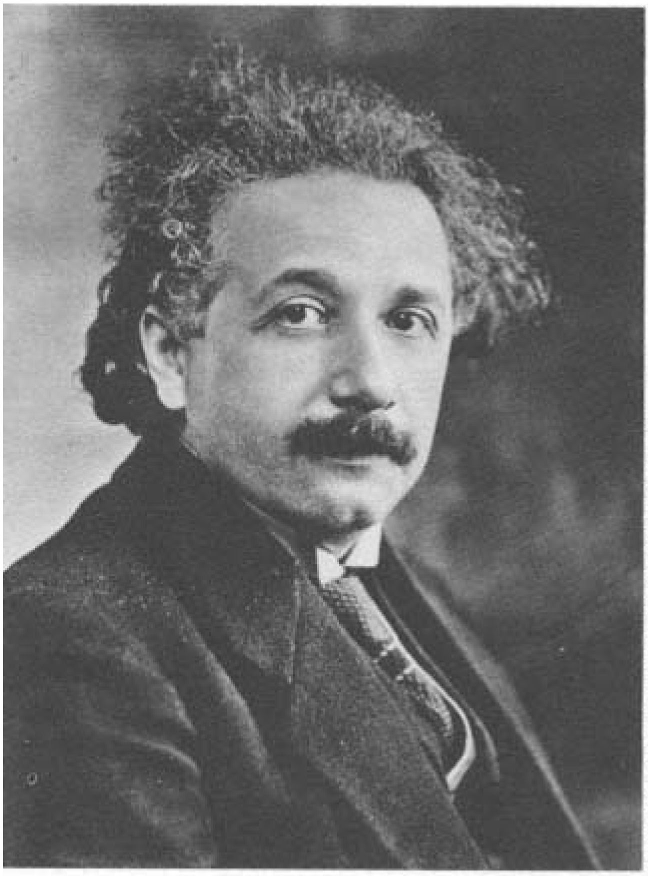

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A. Einstein.

Albert Einstein

March 14, 1879-April 18, 1955

by John Archibald Wheeler*

Albert Einstein was born in Ulm, Germany on March 14, 1879. After education in Germany, Italy, and Switzerland, and professorships in Bern, Zurich, and Prague, he was appointed Director of Kaiser Wilhelm Institute for Physics in Berlin in 1914. He became a professor in the School of Mathematics at the Institute for Advanced Study in Princeton beginning the fall of 1933, became an American citizen in the summer of 1936, and died in Princeton, New Jersey on April 18, 1955. In the Berlin where in 1900 Max Planck discovered the quantum, Einstein fifteen years later explained to us that gravitation is not something foreign and mysterious acting through space, but a manifestation of space geometry itself. He came to understand that the universe does not go on from everlasting to everlasting, but begins with a big bang. Of all the questions with which the great thinkers have occupied themselves in all lands and all centuries, none has ever claimed greater primacy than the origin of the universe, and no contributions to this issue ever made by any man anytime have proved themselves richer in illuminating power than those that Einstein made.

Einstein's 1915 geometrical and still standard theory of

* © February 15, 1979.

gravity provides a prototype unsurpassed even today for what a physical theory should be and do, but for him it was only an outlying ridge in the arduous climb to a greater goal that he never achieved. Scale the greatest Everest that there is or ever can be, uncover the secret of existence—that was what Einstein struggled for with all the force of his life.

How the mountain peak magnetized his attention he told us over and over. "Out yonder," he wrote, "lies this huge world, which exists independently of us human beings and which stands before us like a great, eternal riddle. . . . "* And again, "The most incomprehensible thing about the world is that it is comprehensible."† And yet again, "All of these endeavors are based on the belief that existence should have a completely harmonious structure. Today we have less ground than ever before for allowing ourselves to be forced away from this wonderful belief."‡

When the climber laboring toward the Everest peak comes to the summit of an intermediate ridge, he stops at the new panorama of beauty for a new fix on the goal of his life and a new charting of the road ahead; but he knows that he is at the beginning, not at the end of his travail. What Einstein did in spacetime physics, in statistical mechanics, and in quantum physics, he viewed as such intermediate ridges, such way stations, such panoramic points for planning further advance, not as achievements in themselves. Those way stations were not his goals. They were not even preplanned means to his goal. They were catch-as-catch-can means to his goal.

Those who know physicists and mountaineers know the traits they have in common: a "dream-and-drive" spirit, a

* A. Einstein, "Autobiographical Notes," in *Einstein: Philosopher-Scientist,* ed. P. A. Schilpp (Evanston, Ill.: Library of Living Philosophers, 1949), p. 4.

† B. Hoffmann, *Albert Einstein: Creator and Rebel* (New York: Viking, 1972), p. 18.

‡ A. Einstein, *Essays in Science* (New York: Philosophical Library, 1934), p. 114.

bulldog tenacity of purpose, and an openness to try any route to the summit. Who does not know Einstein's definition of a scientist as "an unscrupulous opportunist;"* or his words on another occasion, "But the years of anxious searching in the dark, with their intense longing, their alternations of confidence and exhaustion, and the final emergence into the light—only those who have experienced it can understand that."† For such a man there are not goals. There is only *the* goal, that distant peak.

Who was this climber? How did he come to be bewitched by the mountain? Where did he learn to climb so well? Who were his companions? What were some of his adventures? And how far did he get?

I first saw and heard Einstein in the fall of 1933, shortly after he had come to Princeton to take up his long-term residence there. It was a small, quiet, unpublicized seminar. Unified field theory was to be the topic, it became clear, when Einstein entered the room and began to speak. His English, though a little accented, was beautifully clear and slow. His delivery was spontaneous and serious, with every now and then a touch of humor. I was not familiar with his subject at that time, but I could sense that he had his doubts about the particular version of unified field theory he was then discussing. It was clear on this first encounter that Einstein was following very much his own line, independent of the interest in nuclear physics then at high tide in the United States.

There was one extraordinary feature of Einstein the man I glimpsed that day, and came to see ever more clearly each time I visited his house, climbed to his upstairs study, and we explained to each other what we did not understand. Over

* A. Einstein, "Reply to Criticisms," in *Einstein: Philosopher-Scientist,* ed. P. A. Schilpp (Evanston, Ill.: Library of Living Philosophers, 1949), p. 648.

† M. J. Klein, *Einstein, The Life and Times,* R. W. Clark, book review, *Science,* 174: 1315.

and above his warmth and considerateness, over and above his deep thoughtfulness, I came to see, he had a unique sense of the world of man and nature as one harmonious and someday understandable whole, with all of us feeling our way forward through the darkness together.

Our last time together came twenty-one years later, on April 14, 1954, when Einstein kindly accepted an invitation to speak at my relativity seminar. It was the last talk he ever gave, almost exactly a year before his death. He not only reviewed how he looked at general relativity and how he had come to general relativity, he also spoke as strongly as ever of his discomfort with the probabilistic features that the quantum had brought into the description of nature. "When a person such as a mouse observes the universe," he asked feelingly, "does that change the state of the universe?"* He also commented in the course of the seminar that the laws of physics should be simple. One of us asked, "But what if they are not simple?" "Then I would not be interested in them,"† he replied.

How Einstein the boy became Einstein the man is a story told in more than one biography, but nowhere better than in Einstein's own sketch of his life, so well known as to preclude repetition here. Who does not remember him in difficulty in secondary school, antagonized by his teacher's determination to stuff knowledge down his throat, and in turn antagonizing the teacher? Who that takes the fast train from Bern to Zürich does not feel a lift of the heart as he flashes through the little town of Aarau? There, we recall, Einstein was sent to a special school because he could not get along in the ordinary school. There, guided by a wise and kind teacher,

* J. A. Wheeler, "Mercer Street and Other Memories," in *Albert Einstein, His Influence on Physics, Philosophy, and Politics,* ed. P. C. Aichelburg and R. U. Sexl (Braunscheig: Vieweg, 1979), p. 202.

† *Ibid., p.* 204.

he could work with mechanical devices and magnets as well as books and paper. Einstein was fascinated. He grew. He succeeded in entering the Züricher Polytechnikum. One who was a rector there not long ago told me that during his period of rectorship he had taken the record book from Einstein's year off the shelf. He discovered that Einstein had not been the bottom student, but next to the bottom student. And how had he done in the laboratory? Always behind. He still did not hit it off with his teachers, excellent teachers as he himself said. His professor, Minkowski, later to be one of the warmest defenders of Einstein's ideas, was nevertheless turned off by Einstein the student. Einstein frankly said he disliked lectures and examinations. He liked to read. If one thinks of him as lonesome, one makes a great mistake. He had close colleagues. He talked and walked and walked and talked.

To Einstein's development, his few close student colleagues meant much; but even more important were the older colleagues he met in books. Among them were Leibniz and Newton, Hume and Kant, Faraday and Helmholtz, Hertz and Maxwell, Kirchhoff and Mach, Boltzmann and Planck. Through their influence, he turned from mathematics to physics, from a subject where there are dismayingly multitudinous directions for dizzy man to choose between, to a subject where this one and only physical world directs our endeavors.

Of all heroes, Spinoza was Einstein's greatest. No one expressed more strongly than he a belief in the harmony, the beauty, and—most of all—the ultimate comprehensibility of nature. In a letter to his old and close friend, Maurice Solovine, Einstein wrote, "I can understand your aversion to the use of the term 'religion' to describe an emotional and psychological attitude which shows itself most clearly in Spinoza. [But] I have not found a better expression than 'religious' for the trust in the rational nature of reality that is, at least to a certain extent, accessible to human reason."* In later years, Einstein was asked to do a life on Spinoza. He excused himself from writing the biography itself on the ground that it required "exceptional purity, imagination and modesty,"† but he did write the introduction. If it is true, as Thomas Mann tells us, that each one of us models his or her life consciously or unconsciously on someone who has gone before, then who was closer to being role-creator for Einstein than Spinoza?

Search out the simple central principles of this physical world—that was becoming Einstein's goal. But how? Many a man in the street thinks of Einstein as a man who could only make headway in his work by dint of pages of complicated mathematics; the truth is the direct opposite. As Hilbert put it, "Every boy in the streets of our mathematical Göttingen understands more about four-dimensional geometry than Einstein. Yet, despite that, Einstein did the work and not the mathematicians."† Time and again, in the photoelectric effect, in relativity, in gravitation, the amateur grasped the simple point that had eluded the expert. Where did Einstein acquire this ability to sift the essential from the non-essential?

The management consultant firm of Booz, Allen & Hamilton, which does so much today to select leaders of great enterprises, has a word of advice: What a young man does and who he works with in his first job has more effect on his future than anything else one can easily analyze. What was Einstein's first job? In the view of many, the position of clerk in the Swiss patent office was no proper job at all, but it was the best job available to anyone with his unpromising university record. He served in the Bern office for seven years, from

* A. Einstein, *Lettres à Maurice Solovine* (Paris: Gauthier-Villars, 1956), p. 102 (January 1, 1956).

† B. Hoffmann, *Albert Einstein: Creator and Rebel* (New York: Viking, 1972), p. 95.

† P. Frank, *Einstein, Sein Leben und seine Zeit* (München: Paul List Verlag, 1949), p. 335.

June 23, 1902 to July 6, 1909. Every morning he faced his quota of patent applications. Those were the days when a patent application had to be accompanied by a working model. Over and above the applications and the models was the boss, a kind man, a strict man, and a wise man. He gave strict instructions: explain very briefly, if possible in a single sentence, why the device will work or why it won't; why the application should be granted or why it should be denied. Day after day Einstein had to distill the central lesson out of objects of the greatest variety that man has power to invent. Who knows a more marvelous way to acquire a sense of what physics is and how it works? It is no wonder that Einstein always delighted in the machinery of the physical world—from the action of a compass needle to the meandering of a river, and from the perversities of a gyroscope to the drive of Flettner's rotor ship.

Whoever asks how Einstein won his unsurpassed power of expression, let him turn back to the days in the patent office and the boss who, "More severe than my father . . . taught me to express myself correctly."* The writings of Galileo are studied in secondary schools in Italy today, not for their physics, but for their clarity and power of expression. Let the secondary school student of our day take up the writings of Einstein if he would see how to make in the pithiest way a telling point.

From Bern, fate took Einstein to Zurich, to Prague, and then to the Berlin where his genius flowered. Colleagueship never meant more in his life than it did during his 19 years there, and never did he have greater colleagues: Max Planck, James Franck, Walter Nernst, Max von Laue, and others. Colleagueship did not mean chat; it meant serious consulta

* "Errinerungen an Albert Einstein, 1902-1909," Bureau Fédéral de la Propriété Intellectuelle (Berne, Switzerland), as quoted in: R. W. Clark, *Einstein, The Life and Times* (New York: The World Publishing Co., 1971), p. 75.

tion on troubling issues. No tool of colleagueship was more useful than the seminar. James Franck explained to me the democracy of this trial by jury. The professor, he emphasized, stood on no pinnacle, beyond question by any student. On the contrary, the student had both the right and the obligation to question and to speak up.

If the writing of letters is a test of colleagueship, let no one question Einstein's power to give and to receive. Consider his enormous correspondence. Look at the postcards he sent over the years to the closest in spirit of all his colleagues, Paul Ehrenfest in Leyden. They deal with the issues nearest to his heart at the moment, whether the direction of time in statistical mechanics, or quantum fluctuations in radiation, or a problem of general relativity. Or examine his correspondence with Max Born, or Maurice Solovine, or with everyday people. To a schoolgirl who mentioned among many other things her problems with mathematics, he replied, "Do not worry about your difficulties in mathematics; I can assure you that mine are greater."* Why did Einstein correspond so much with people that you and I would call outsiders? Did he not feel that the amateur brings a freshness of outlook unmatched by the specialist with his narrow view?

The benefits of colleagueship with Einstein I experienced more than once, but never with greater immediate benefit than in statistical mechanics. In a discussion of radiation damping, he referred me to a published dialogue of 1909 between himself and Walter Ritz. The two men agreed to disagree and stated their opposing positions in this single clear sentence: "Ritz treats the limitations to retarded potentials as one of the foundations of the second law of thermodynamics, while Einstein believes that the irreversibility of radiation depends exclusively on considerations of probability."†

* H. Dukas and B. Hoffmann, eds., *Albert Einstein; The Human Side: New Glimpses from His Archives* (Princeton, N.J.: Princeton Univ. Press, 1979), p. 8.
† A. Einstein and W. Ritz, *Physikalisches Zeitschrift,* 10 (1909): 323-34.

In accord with the position of Einstein, Richard Feynman and I found that the one-sidedness in time of radiation reaction can be understood as originating in the one-sidedness in time of the conditions imposed on the far-away absorber particles, and not at all in the elementary law of interaction between particle and particle. I joined the ranks of what I can only call "the worriers"—those like Boltzmann, Ehrenfest, and Einstein himself, and many, many others—who ask, why initial conditions? Why not final conditions? Or why not some mixture of the two? And most of all, why thus and such initial conditions and no other? No one who knows of Einstein's lifelong concern with such issues can fail to have a new sense of appreciation on reading his great early papers on statistical mechanics, and not least among them the famous 1905 paper on the theory of the Brownian motion. Surely the perspective he won from these worries will someday help show us the way to Everest.

Best known of Einstein's great trio of 1905 papers, however, is that on special relativity. "Henceforth," as Minkowski put the lesson of Einstein, "space by itself and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality."* Historians of science can tell us that if Einstein had not come to this version of spacetime it would have been achieved by Lorentz, or Poincaré, or another, who would also have come eventually to that famous equation $E = mc^2$, with all its consequences. But it still comes to us as a miracle that the patent office clerk was the one to deduce this greatest of lessons about spacetime from clues on the surface so innocent as those afforded by electricity and magnetism. Miracle? Would it not have been a greater miracle if anyone but a patent office clerk had discovered relativity? Who else could have distilled this simple central point from all the clutter of

* C. *Reid, Hilbert* (Berlin: Springer, 1970), p. 12.

electromagnetism than someone whose job it was over and over each day to extract simplicity out of complexity?

If others could have given us special relativity, who else but Einstein, sixty-four years ago, could have given us general relativity? Who else knew out of the welter of facts to fasten on that which is absolutely central? Did the central point come to him, as legend has it, from talking to a housepainter who had fallen off a roof and reported feeling weightless during the fall? We all know that he called that 1908 insight the "happiest thought of my life"* —the idea that there is no such thing as gravitation, only free-fall. By thus giving up gravitation, Einstein won back gravitation as a manifestation of a warp in the geometry of space. His 1915 and still standard geometric theory of gravitation can be summarized, we know today, in a single, simple sentence: "Space tells matter how to move and matter tells space how to curve."† Through his insight that there is no such thing as gravity, he had had the creative imagination to bring together two great currents of thought out of the past. Riemann had stressed that geometry is not a God-given perfection, but a part of physics; and Mach had argued that acceleration makes no sense except with respect to frame determined by the other masses in the universe.

It is unnecessary to recall the three famous early tests of Einstein's geometric theory of gravitation: the bending of light by the sun, the red-shift of light from the sun, and the precession of the orbit of the planet Mercury going around the sun. Neither is it necessary to expound the important insights that have come and continue to come out of general relativity. Einstein showed that the law for the motion of a mass in space and time does not have to be made a separate

* A. Einstein, "The Fundamental Idea of General Relativity in Its Original Form," unpublished essay, 1919 (excerpts, *New York Times,* 28 March 1972), p. L.32.

† J. A. Wheeler, University of Texas, lecture of 2 March 1979.

item in the conceptual structure of physics. Instead, it comes straight out of geometric law as applied to the space immediately surrounding the mass in question. Moreover, the geometry that he had freed from slavery to Euclid, and that he had assigned to carry gravitation force, could throw off its chains, become a free agent, and, under the name of "gravitational radiation," carry energy from place to place over and above any energy carried by electromagnetic waves—an effect for which Joseph H. Taylor, L. A. Fowler, P. M. McCulloch, and their Arecibo Observatory colleagues in December 1978 announced impressive evidence.*

One does not need to go into the theory of gravitationally collapsed objects or the evidence we have today, some impressive, some less convincing, for black holes: one of some ten solar masses in the constellation Cygnus; others in the range of a hundred or a thousand solar masses at the centers of five of the star clusters in our galaxy; one about four million times as massive as the sun at the center of the Milky Way; and one with a mass of about five billion suns in the center of the galaxy M87.

The collapse at the center of a black hole marks a third "gate of time,"† additional to the big bang and the big crunch. Einstein tried to escape all three. Two years after general relativity, Einstein was already applying it to cosmology. He gave reasons to regard the universe as closed and qualitatively similar to a three sphere, the three-dimensional generalization of the surface of a rubber balloon. To his surprise, he found that the universe is dynamic and not static.

Einstein could not accept this result. First, he found fault

* L. A. Fowler, P. M. McCulloch, and J. H. Taylor, "Measurement of General Relativistic Effects in the Binary Pulsar PSR 1913 + 16," *Nature,* 277 (8 February 1979) 437-40.

† J. A. Wheeler, "Genesis and Observership," in *Foundational Problems in the Special Sciences,* ed. R. E. Butts and K. J. Hintikka (Reidel: Dordrecht, 1977), p. 11.

with Alexander Friedmann's mathematics. Then he retracted this criticism, and looked for the fault in his own theory of gravitation. It turned out there was no natural way to change that theory. The arguments of simplicity and correspondence in the appropriate limit with the Newtonian theory of gravitation left no alternative. There being no natural way to change the theory, he looked for the least unnatural way he could find to alter it. He introduced a so-called "cosmological term" with the sole point and purpose to hold the universe static. A decade later, Edwin Hubble, working at Mount Wilson Observatory, gave convincing evidence that the universe is actually expanding. Thereafter, Einstein remarked that the cosmological term "was the biggest blunder of my life."* Today, looking back, we can forgive him his blunder and give him the credit for the theory of gravitation that predicted the expansion. Of all the great predictions that science has ever made over the centuries, each of us has his own list of spectaculars, but among them all was there ever one greater than this, to predict, and predict correctly, and predict against all expectation, a phenomenon so fantastic as the expansion of the universe? When did nature ever grant man greater encouragement to believe he will someday understand the mystery of existence?

Why did Einstein in the beginning reject his own greatest discovery? Why did he feel that the universe should go on from everlasting to everlasting, when to all brought up in the Judeo-Christian tradition an original creation is the natural concept? I am indebted to Professor Hans Küng for suggesting an important influence on Einstein from his hero Spinoza. Why was twenty-four-year-old Spinoza excommunicated in 1656 from the synagogue in Amsterdam? Because he denied the doctrine of an original creation. What was the

* G. Gamow, *My World Line* (New York: Viking, 1970), p. 44.

difficulty with that doctrine? In all that nothingness before creation where could that clock sit that should tell the universe when to come into being!

Today we have a little less difficulty with this point. We do not escape by saying that the universe goes through cycle after cycle of big bang and collapse, world without end. There is not the slightest warrant in general relativity for such a way of speaking. On the contrary, it provides no place whatsoever for a before before the big bang or an after after the big crunch. Quantum theory goes further. It tells us that however permissible it is to speak about space, it is not permissible to speak in other than approximate terms of spacetime. To do so would violate the uncertainty principle—as that principle applies to the dynamics of geometry. No, when it comes to small distances either in the here and the now or in the most extreme stages of gravitational collapse, spacetime loses all meaning, and time itself is not an ultimate category in the description of nature. No one who wrestles with the three gates of time, our greatest heritage of paradox—and of promise—from general relativity can escape the all-pervasive influence of the quantum.

Spinoza's influence on his thinking about cosmology Einstein could shake off—but not Spinoza's deterministic outlook. Proposition XXIX in *The Ethics* of Spinoza states: "Nothing in the universe is contingent, but all things are conditioned to exist and operate in a particular manner by the necessity of divine nature."* Einstein accepted determinism in his mind, his heart, his very bones.

Who then was first clearly to recognize that the real world, and the world of the quantum, is a world of chance and unpredictability? Einstein himself!

Why did Einstein, who in the beginning with Max Planck

* B. Spinoza, *Die Ethik,* Part One, Proposition XXIX (Hamburg: F. Meiner, 1955).

and Niels Bohr had done so much to give quantum physics to the world, in the end stand out so strongly and so lonesomely against the central point? What other explanation is there than this "set" he had received from Spinoza?

The early quantum work of Bohr and Einstein is almost a duet. Einstein, 1905: The energy of light is carried from place to place as quanta of energy, accidental in time and space in their arrival. Bohr, 1913: The atom is characterized by stationary states, and the difference in energy between one and another is given off in a light quantum. Einstein, 1916: The processes of light emission and light absorption are governed by the laws of chance, but satisfy the principle of detailed balance. Bohr, 1927: Complementarity prevents a detailed description in space and time of what goes on in the act of emission. Here Bohr and Einstein parted company. Einstein spoke against Einstein. The Einstein who in 1915 said there was no escape from the laws of chance was insisting by 1916, as he did all the rest of his life, against the evidence and against the views of his greatest colleagues, that "God does not [play] dice."*

If an army is being defeated it can still, by a sufficiently skillful rear-guard action, have an important influence on the outcome. No one who in all the great history of the quantum contested with Niels Bohr did more to sharpen and strengthen Bohr's position than Einstein. Never in recent centuries was there a dialogue between two greater men over a longer period on a deeper issue at a higher level of colleagueship, nor a nobler theme for playwright, poet, or artist. From their earliest encounter, Einstein liked Bohr, writing him on May 2, 1920, "I am studying your great works—and when I get stuck anywhere—now have the pleasure of seeing your friendly young face before me smiling and explain

* A. Einstein, *Albert Einstein und Max Born, Briefwechsel, 1916-1955, Kommentiert von Max Born* (München: Nymphenburg, 1969), pp. 129-30.

ing."* Bohr viewed Einstein with admiration and warm regard. Let him who will read Bohr's account of the famous dialogue, even today unsurpassed for its comprehensive articulation of the central issues. Who knows what the quantum means who does not know the friendly but deadly serious battles fought and won on the double-slit experiment, on the possibilities for weighing a photon, on the Einstein-Podolsky-Rosen experiment, and on the danger associated with unguarded use of the word "reality"? To help to clarify the issues brought up in the later years of the great dialogue, Bohr found himself forced to introduce the word "phenomenon"† to describe an elementary quantum process "brought to a close by an irreversible act of amplification."‡ Thanks to that word, brought in to withstand the criticism of Einstein, we have learned in our own time to state the central lesson of the quantum in a single simple sentence, "No elementary phenomenon is a phenomenon until it is an observed phenomenon."§

How could the correctness of quantum theory be by now so widely accepted, and its decisive point so well perceived, if there had been no great figure, no Einstein, to draw the embers of unease together in a single flame and thereby drive Bohr to that fuller formulation of the central lesson which he at last achieved?

If the quantum and the gates of time are the strongest features of this strange universe, and if they shall prove in time to come the doorways to that deeper view for which

* Letter to N. Bohr, 2 May 1920.

† N. Bohr, "Discussion with Einstein on Epistemological Problems in Atomic Physics," in *Einstein: Philosopher-Scientist,* ed. P. A. Schilpp (Evanston, Ill.: Library of Living Philosophers, 1949), p. 238.

‡ N. *Bohr, Atomic Physics and Human Knowledge* (New York: Wiley, 1958), p. 73,88.

§ J. A. Wheeler, "Frontiers of Time," in *Rendicotti della Scuola Internazionale di Fisica "Enrico Fermi,"* LXXII Corso, Problems in the Foundations of Physics, ed. N. Toraldo di Francia and Bas van Fraassen (Amsterdam: North-Holland, 1979), pp. 395-497.

Einstein searched, mankind will forever remember with gratitude his absolutely decisive involvement with both.

No one who is a professor and receives his support from the larger community can rightly be unmindful of his obligations to it. He must speak to the higher values of all insofar as he is qualified and able to do so. Burden though it was for Einstein to take on this extra duty, he did it to the best of his ability. What he defended were no whims, no lightly held fancies, but goals he held and deeply desired for the world. If in this undertaking he had some of the character of an Old Testament prophet, he also had all of the eloquence. Statements from Einstein created an audience, and the audience created the pressure for more statements. What is long, Einstein felt, is lost. Pith and pungency were the points of his pronouncements. Who does not know the causes for which he stood! Whoever admires greatness, let him read Einstein's words about the goals and the greatness of recently departed colleagues, as well as heroes out of the deeper past. For social justice and social responsibility, Einstein spoke up time and again: "A hundred times every day I remind myself that my inner and outer life are based on the labors of other men, living and dead, and that I must exert myself in order to give in the same measure as I have received and am still receiving."* He stressed the necessity of a political system that does not rely on coercion if people are to contribute all that lies in them to achieve.

He expressed admiration for the system of social care, going back to Bismarck, that makes provision for the individual in case of illness or need. Living through the tragedy of two world wars, he protested many times about the wastefulness of war: lives lost, hatred engendered, and values per

* A. Einstein, *Mein Weltbild,* trans. A. Harris, in *The World As I See It* (New York: Philosophical Library, 1949), p. 90.

verted; but when it came to a choice between war or freedom and justice, he spoke for freedom and justice. He refused the invitation to become the first president of Israel, but he worked after that declination as effectively as before for the welfare of a unique community, remarking, "The pursuit of knowledge for its own sake, an almost fanatical love of justice and the desire for personal independence—these are the features of the Jewish tradition which make me thank my stars that I belong to it."*

Things did not go in the world as Einstein had hoped. Things did not go in physics as he had desired. Determinism stood in ruins. His search for a unified geometric theory of all the forces of nature came to nothing—though today, with a new and wider concept of what geometry is, in the sense of a so-called "gauge theory," marvelous new progress is now being made toward his dream of unification. He left us in general relativity with an ideal for a physical theory that has never been surpassed. He showed a unique talent for finding the central point in every subject to which his philosophical antecedents gave right of entry. He did as much as any man who ever lived to make us face up to the central mysteries of this strange world.

Einstein worked with all his force to the very end. In his last days he had a tired face. Everything that he had to give he had given for his causes, and among them that greatest of causes, the goal toward which he had climbed so high, that snowy peak whose light today shines brighter than ever: "A completely harmonious account of existence."†

As we look up at the distant intervening craggy slope, we are amazed suddenly to make out the faint sound of a high far-off violin. Then out of the valley behind and below us

* *Ibid.,* p. 1.
† A. Einstein, *Essays in Science* (New York: Philosophical Library, 1934), p. 114.

comes an answering burst of song, young voices all. They chorus of the loftiness of the peak, the danger of the climb, and the greatness of the climber, the man of peace with the white hair. He no longer belongs to any one country, any one group, any one age, we hear them singing, but to all friends of the future. Least of all, they tell us, does Einstein anymore belong to Einstein. He belongs to the world.

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William Maurice Ewing

May 12, 1906-May 4, 1974

by Edward C. Bullard*

CHILDHOOD, 1906-1922

William Maurice Ewing was born on May 12, 1906 in Lockney, a town of about 1,200 inhabitants in the Texas panhandle. He rarely used the name William and was always known as Maurice. His paternal great-grandparents moved from Kentucky to Livingston County, Missouri, at some date before 1850. Their son John Andrew Ewing, Maurice's grandfather, fought for the Confederacy in the Civil War; while in the army he met two brothers whose family had also come from Kentucky to Missouri before 1850 and were living in De Kalb County. Shortly after the war he married their sister Martha Ann Robinson. Their son Floyd Ford Ewing, Maurice's father, was born in Clarkdale, Missouri, in 1879. In 1889 the family followed the pattern of the times and moved west to Lockney, Texas.

Floyd Ewing was a gentle, handsome man with a liking for literature and music, whom fate had cast in the unsuitable roles of cowhand, dryland farmer, and dealer in hardware and farm implements. Since he kept his farm through the

* This memoir is a corrected and slightly amplified version of one published by the Royal Society in their *Biographical Memoirs* (21:269-311, 1975). The main changes are that more detail is given in the first section and that numerous trivial errors in the bibliography have been corrected. The Royal Society has given permission for the republication.

years of the depression, he must have been a farmer of persistence and ability. He is spoken of with great affection by all who knew him; he was a marvelous storyteller and an accomplished violinist who played the old hoedown pieces with enthusiasm. His daughter Rowena has "such vivid memories of him playing, always standing so straight and tall."

Ewing's mother, Hope Hamilton Ewing, was born at Breckenridge, Stephens County, Texas, in 1882. She was the daugher of Isaac Hamilton of Illinois and Martha Ann Carnahan of Arkansas, and she and her family moved to Lockney in 1892. The Ewings and the Hamiltons were among the earliest settlers along the edge of the high plains of northern Texas. In 1901 she married Floyd Ewing; she was nineteen and he was twenty-two. In 1902 they set out on a homesteading venture in eastern New Mexico near Portales. They traveled in the traditional way with a wagon, two mules, a horse and a cow; they dug a well, set up a windmill, and constructed a "half dug-out" with a sod roof. A few months later they returned to Texas and drove a herd of fifty cattle to their ranch. Unfortunately, they had moved into an arid area in the worst year of a five-year drought. The story of the ensuing disasters has been told with great skill and sympathy by Maurice's brother Floyd, who was a professor of history at Midwestern University, Wichita Falls, Texas (F. F. Ewing, 1963). In 1904 they returned to Texas.

Maurice was the fourth of ten children. The three oldest had died very young in New Mexico so that he grew up as the eldest of seven. Mrs. Ewing was determined that her children should receive a good education and should have a wider choice of careers than was to be found in a small west Texas town. All but one, the eldest daughter Ethel, went to a university and had professional or academic careers. Ethel married very young and for many years was a successful teacher of the piano in Tulsa, Texas. Bob became a naval captain and now works at the Marine Science Institute at Galveston, Texas. Rowena married J. A. Peoples, a geophysicist and an early colleague of Ewing's; Lucy married C. H. Clawson, a professor of psychology at Amarillo, Texas; John, the youngest, worked for many years with Ewing at Lamont and is now at the Woods Hole Oceanographic Institution where he was, for a while, Chairman of the Department of Geology and Geophysics. It is remarkable that so many of Maurice's brothers and sisters should have followed careers which intertwined, in different ways, with his own.

Maurice enjoyed telling stories of his father's farm at Lockney. No doubt the stories improved with the passage of time, as when he said that he spent much of each spring killing rattlesnakes with a hoe while chopping cotton. The family was not well off, but he remembered his childhood as a happy time and all his life kept the slow speech, the self-confidence, and the kindliness of the rural Texas of his youth.

At public school in Lockney he at first preferred grammar and languages to other subjects; later, in high school, he developed an interest in science and mathematics. He ascribed the change to the excellence of the teaching in the Lockney high school. In 1922, when he was sixteen, he was awarded the Hohenthal Scholarship to the Rice Institute in Houston, Texas.

A STUDENT AT THE RICE INSTITUTE, 1922-1929

The journey to Houston had to be done in the most economical way. On one occasion, probably in his sophomore year, he started off on a motorcycle which he bought for $12 from a man who had taken it to pieces and could not get it together again. He had a $10 bill in his pocket and a blanket roll strapped behind him. On the first day the chain of the motorcycle broke and he ran out of gasoline; he abandoned the machine and boarded a freight train where he shared a car with two hoboes. The brakeman found them and took Ewing's watch and money; he persuaded him to return them by explaining that he was on his way to college and needed them. Later he was attacked by the hoboes. He got away from the train by pretending to be a homicidal maniac, was hit with a blackjack, and after a long cross-country chase, hid in some brambles in a churchyard and escaped. He lost his blanket roll and most of his clothes, but still had his $10 and his watch. He felt he was too scantily clad to board a street car but persuaded the police to drive him to Rice.* The ingenuity, the persuasiveness, the physical toughness, and the courage are typical of the mature Ewing. Clearly the boy was the father to the man.

In his early days at Rice, Ewing earned money by working in an all-night drugstore; he used to say that his main duties were to take coffee and sandwiches to the call girls who lived in the hotels around the old Humble Building. Later he left the drug store and took part-time jobs assisting with classes and in the library. This brought in about $34 per month. It must have been a hard life, but at the beginning of his third year at Rice he was able to say, in a letter to his parents: "Well, because of the grades I made last year, I was invited to a banquet of the Houston Philosophical Society . . . and I sure aim to go."

It was, I suppose, at Rice that he acquired his lifelong habit of working most of the night as well as all day. He also showed his interest in teaching and gave much time to coaching fellow students. His sister Lucy has described how during vacation he would stand over her while she played the piano, insist that she do it right, and explain the background of the piece.

* This story is taken from a letter M. Ewing wrote to his parents just after the event.

At the Rice Institute he at first majored in electrical engineering but later changed to physics and mathematics. Not surprisingly, he found physics, then in the great formative period of quantum mechanics, more exciting than contemporary engineering. He also found physicists more congenial than engineers (the Rice professors of engineering he described as "sarcastic Yankees"). In physics he was greatly influenced by H. A. Wilson, an Englishman and a well-known but unorthodox physicist, who claimed that he was the only one of his contemporaries in the Cavendish Laboratory who did not get a Nobel Prize (it would be interesting to look again at his ideas on nuclear systematics and see if they still look as implausible as they did at the time). Wilson ran a weekly colloquium at which the papers on the "new physics" were discussed as they came out, and where occasionally there would be a talk by a distinguished visitor ("Men," said Ewing, "whom I would otherwise have thought hardly mortal").

At Rice in the 1920's Ewing became a physicist. He learned not only the subject but also the attitude of mind. All his life he preferred simple arguments; his theory was set out in detail, well understood, and carefully explained; his instruments were ingenious and often made by himself without regard for current fashions. He told me that when he was in his late forties he heard a graduate student complaining to another that "Doc" expected him to use a galvanometer: "Never mind," replied the other, "all these old men will soon be dead."

During the vacations at Rice he worked in a grain elevator and later with an oil prospecting crew in the shallow lakes of Louisiana; this was his first introduction to underwater geophysics. It was an exciting time, when gravity and seismic measurements were revealing the salt-domes against whose sides the oil of the Gulf Coast fields is trapped. While still an undergraduate he wrote his first scientific paper (1926), en titled "Dewbows by Moonlight," which describes a rainbow seen on the dew-covered grass of the campus.

While at Rice he played the trombone in the marching band. There he was seen by a fellow. student, Avarilla Hildenbrand; as she afterwards told it: "When he came striding down the street working his trombone slide in and out, my heart stood still. He was my man." They were married in 1928.

Ewing obtained his B.A. in 1926. It is curious that H. A. Wilson then advised him that he had no aptitude for experimental work and should stick to theoretical physics. Rarely has a professor given worse advice. Ewing started graduate work in the Physics Department at Rice in the fall of 1926 and obtained an M.A. in 1927 and a Ph.D. in 1931. His Ph.D. thesis, entitled "Calculation of Ray Paths from Seismic Travel-Time Curves," was reported in two papers with Don Leet (1930, 1932a). The topic is central to much of Ewing's later work. Refraction seismology was not, at that time, well understood; there was, for example, a curious controversy as to whether the refracted ray went straight up and down or was refracted along the interface at the critical angle. A sound and detailed knowledge of the ray theory of propagation in a layered medium was critical for the seismic investigations of the next twenty years, and it was a fortunate chance that led Ewing so early in this direction. Regrettably, the collaboration with Leet, who was Director of the Harvard seismological station, broke down with bitter feelings on both sides. Ewing regarded this quarrel as having an adverse effect on his career in the thirties. However, jobs and grants were scarce for everyone, and I was never convinced that Leet's disparagement had as much effect as Ewing believed.

THE 1930'S SEISMOLOGY AT SEA

In 1929 Ewing became an Instructor in Physics at the University of Pittsburgh, but a year later moved to a similar position at Lehigh where he remained till 1940. He had a heavy teaching load in elementary physics but at once started to develop research in geophysics. The work of the next few years is not of great interest; it consists of a variety of projects, some of them suggested by local industry; for example, the paper on prospecting for anthracite (1936a) and one on locating a buried power shovel (1938d). The main theme, however, is the understanding of the methods of small-scale seismology with explosive sources (1932b, 1934 a, b, c, d, 1935, 1936c).

The change came in November 1934, on the day on which he was visited in his seismic truck at Lehigh by Dick Field and William Bowie. They came to suggest that he might interest himself in applying the seismic method of prospecting to the study of the continental shelf. Bowie was Chief of the Division of Geodesy of the Coast and Geodetic Survey, very much a member of the Establishment and something of a southern gentleman. R. M. Field was a Harvard man and a professor of geology at Princeton. He was a major eccentric, but he was also the man whose vision and enthusiasm started the bandwagon of marine geology on its triumphant course (for brief accounts of his life see Hess 1962 and Bullard 1962). He had largely founded and was Chairman of the American Geophysical Union's "Committee on the Geophysical Study of the Ocean Basins." He had a pretty clear idea of what he wanted done and why, as can be seen from the first report of his committee (Field 1933). I can easily visualize the meeting with Ewing, since I was taken by Field to see Bowie on a similar errand in 1937. Field would have been persuasive, persistent, talkative, and irrepressible, while Bowie would have lent an air of solidity and charm; together they would have been irresistible, particularly when they offered funds and ships. I do not know what made Field approach Ewing; it is likely that Field had heard him talk at the American Geophysical Union (1931, 1934a). For Ewing it was what he wanted above all else, a problem worth tackling and the possibility of support and facilities.

It was decided that the first project would be to shoot as many refraction seismic lines as possible spaced out between Cape Henry on the east coast of Virginia and the edge of the continental shelf 120 km out to sea, where the depth of water was about 100 m. This line was to be extended inland by measurements on land between the coast and the outcrop of basement rocks 120 km inland. The start was not propitious; the Coast and Geodetic Survey allowed Ewing and his two assistants (A. Crary and H. M. Rutherford) to embark in their ship *Oceanographer* (the yacht *Corsair* given to the survey by H. P. Morgan). Immediately before sailing, the captain was injured in a motor car accident, and an assistant, who was to have helped Ewing, was killed. The ship was fully occupied with surveying, and Ewing's work had to be fitted in while she was anchored at night. Shots were fired with seismographs on the bottom; this gave experience in handling the gear at sea, but no geological information was obtained. In the time available only reflection shooting could be attempted, and not surprisingly, no identifiable reflections were received from the basement.

The work convinced Ewing that the job could be done. On 1 July 1935 he wrote home: "I got proof that the measurements can be made at sea . . . the people sponsoring the work . . . think they can get the ship of the Scripps Oceanographic Institute for our exclusive use. If so we can clean up an important job in a few months. This is by far the most important project with which I have yet been connected. It is so arranged that I see no possibility of anyone stealing the credit from me." The anxiety about the credit for the work is typical of one side of his character; he was having a hard struggle to get established and could hardly believe that something would not go wrong.

When *Oceanographer* returned to port, Ewing set about the observations on the land section of the line. This was a task that his previous experience had made familiar. Meanwhile Field exercised his persuasive powers on Henry Bigelow, the Director of the Woods Hole Oceanographic Institution. He obtained the use of the R. V. *Atlantis* for two weeks. She was a steel-hulled ketch, 43 m in length over all and with a displacement of about 380 tonnes. She had sails and a diesel engine; the sails were often used, not only for propulsion but also to reduce her tendency to roll. The crucial work was done in this vessel in October 1935. Her Master was Fred McMurray, a very skilled and experienced seaman. On the first day Field, Columbus Iselin, and Henry Stetson accompanied Ewing's party on a short trip to test the gear. Four days later Ewing, Crary, and Rutherford set off for a two-week cruise. At each station a seismograph measuring the vertical component of the motion was lowered to the sea floor from the anchored ship on an insulated electric cable. Signals from the instrument were transmitted up the cable to a recorder in the ship. Charges of explosive were lowered from the ship's boat at distances of up to 11 km from the ship. The instant of explosion was transmitted to the ship by radio; the time of transmission of the wave traveling through the water gave the distance. Four refraction lines were shot on the Cape Henry section and three on a line running south from Woods Hole.

The object of the investigation was to study the nature of the transition from the ocean to the continent. Is the "shelf break," where the sea floor suddenly turns down from the shallow water of the continental shelf to oceanic depths, a fault in the basement, or is it the edge of a rubbish tip of sediments built out from the land over sunken continent or, perhaps, over ocean floor? Where is the true edge of the continent? In what sense has it an edge? These questions are fundamental for geology, and it is remarkable that they had never seriously been approached before. There were, of course, speculations based on the results of drilling on the exposed part of the shelf, but no one had had the skill or the enterprise to attempt what Ewing did.

He discovered a pile of sediments 3800 m thick. The work is a classic example of a discovery of great practical importance made in searching for knowledge. All the oil obtained from the sea floor comes from sedimentary basins like that discovered by Ewing. He told me that about 1936 he had approached an executive of a large oil company and asked for support for the work. He was told that there was no shortage of oil and that the company was not in the least interested in looking for it at sea.

Ewing's reputation was made—he had done something new and of first rate importance. The work was, of course, preliminary. It was open to the criticism that too little shooting had been done; the time-distance curve at the outermost station had only two points on it through which two lines were drawn by using seismic velocities extrapolated from stations nearer shore. To most people these were details which time and further work would remedy. Ewing's own reply to enquiries about how he could be sure with so few data was: "That's how you tell the men from the boys." To Leet however it was not so; he published a slashing attack on the whole operation and its conclusions (1937).

Ewing had expected that Field and his geological friends would seize on the information and produce interpretations in terms of structure and history. It did not happen, though his first paper (1937) was followed by one by B. J. Miller (1937) which was supposed to discuss and explain the results; it is a rather dull piece of work which sets out possible views and leaves the main questions undecided. Ewing, whose own paper was strictly factual, was surprised and perhaps a little disappointed. He had not, I think, realized how complete was the gap in knowledge represented by the ocean floor. For generations the oceans had been a place where geologists could safely deposit many of their difficulties; almost nothing was known and almost anything could be assumed.

Ewing decided to ignore the criticisms and to use what ship time he could get for other projects but to continue the study of the shelf sediments on land (1939c, 1940b). Further work on the shelf at sea was done in 1940 and 1943 but was not published until 1946; the most striking result of this later work was the discovery of seven km of sediment beneath the delta of the Orinoco (1946c, 1948b).

Work at sea continued on a wide front. Even before the first seismic work was published he had started gravity measurements in the U.S.S. *Barracuda* in collaboration with Harry Hess of Princeton (another protégé of Field), who had made similar measurements in the U.S. submarines S21 and S48 in 1928 and 1932. They borrowed the pendulum apparatus devised by Vening Meinesz and used it to explore the gravity low that he had found over the Puerto Rico trench in 1926. They found that it ran around the island arc of the Lesser Antilles and was clearly analogous to the low found by Meinesz around Indonesia (1937a, 1938e). Ewing used a quartz oscillator designed and constructed by W. A. Marrison of the Bell Telephone Laboratories to time the pendulums; this was an important improvement on the use of a spring-controlled chronometer, which was liable to change its rate on diving.

Gravity measurement, at first in submarines, later in surface ships, was a life-long interest on which he published many papers, most of them in collaboration with Joe Worzel (1950h, 1952g, 1954b, 1956d, 1966i).

Work was started about 1939 on the design and construction of a deep-sea camera (1946a, 1967c). A quotation from

Ewing's paper (1946) well illustrates his attitude to such things. He describes the failure of previous rather halfhearted attempts at photography in deep water and concludes: "The principal problems in underwater photography are not optical. . . . The problems are to find an interesting subject, and to put the camera in focus with it, to provide proper illumination, to hold the camera reasonably steady while the exposure is made, and to get the camera back afterwards" (p. 308). In this work and in deep-sea seismology the problems of making watertight equipment for use in deep water were faced for the first time. This involved considerable difficulties and a good deal of development. The published photographs (1944, 1945, 1946 a, c, 1967c) are outstandingly clear; they were obtained in depths of up to 730 m. Ripple marks were found in a depth of 150 m in the Gulf of Maine, later they were found to be common in oceanic depths. This was of considerable interest, as most geologists had supposed that ripple marks in sediments were a sign of shallow water. Actually little harm was done by this assumption, since most sedimentary rocks found on land have been formed in relatively shallow water or, at any rate, not in oceanic depths. Ewing's camera was the prototype of all subsequent deep-sea cameras, the results from which have given a detailed view of the ocean floor which could have been attained in no other way; they have been of great assistance in understanding the results of dredging and coring.

After the initial success of the seismic work on the continental shelf Ewing decided that the most important thing to do was to extend the work to deep water. The prize was great: it should be possible to find how much sediment there is on the ocean floor (if the oceans have existed, much as now, through the whole of geological time there should be many kilometers of sediment). One might also hope to obtain an indication of the nature of the basement beneath the sediments, to estimate the thickness of the crust, and to find the depth to the Mohorovicic* discontinuity, if it exists beneath the oceans.

The methods employed on the shelf were hardly practicable, the ship could not be anchored and no electric cable was available to bring the signals from the sea floor to the ship. Ewing (1938b, 1946c) first tried stringing the gear along a steel cable. From the ship the cable led down to the sea floor where it carried a watertight pressure vessel containing a four-channel oscillograph; further along the cable were four geophones and three bombs fired by a clock and a battery in the pressure vessel. I was fortunate to be present at the trials of parts of this equipment in *Atlantis* in 1937. It was a somewhat hazardous and a very difficult undertaking. The ship frequently dragged the whole string along the sea floor which prevented any record from being obtained; if cable was paid out to prevent the dragging the whole thing would pile up on the sea floor instead of lying in a straight line. There were many other difficulties, one of the most troublesome was the failure of explosives to go off at depth. When I was with him, Ewing decided that cast TNT might be better than the flake TNT, which was all we had on board. I said: "Maurice, we haven't got any and there's nothing we can do about it." He looked at me and smiled and said: "Don't you think perhaps . . . ." We melted the flake TNT in an electric coffee pot and poured it into molds made by folding paper. Ewing was a wonderful improviser; he had a pressure vessel for the recorder made from an oxygen cylinder with the top cut off; it did not last long, after it had been lowered for a test of watertightness, the wire came up carrying only the eyebolt which had been welded to the cylinder. Some believed that a large fish had eaten the cylinder. In his laboratory at Lehigh he had a pressure vessel to test equipment; it was made from an old fourteen-inch naval shell which Al Vine had found in an army junk yard. On one occasion he was testing blocks of TNT which were supposed to be strong enough to protect detonators from the pressure; he noticed that the pressure was rising rather rapidly and decided that the TNT had caught fire in the press. I do not remember what he did, but when he told me about it he did not seem greatly concerned. As a young man he sometimes appeared rash but, in fact, he knew what he was doing and was quick in making a sensible decision. Behind the large, rather shambling bear there was a very acute mind and a tremendous drive and determination to get the job done.

The attempt to shoot seismic lines in deep water with this equipment was unsuccessful (1946c). In view of the difficulties the scheme was abandoned and a new method tried in 1939 and 1940 (1938b, 1946c). In this the instruments and the explosive charges were sent to the bottom attached to balloons filled with thirty gallons of gasoline and with no wire to connect them with the ship, an idea suggested by the work of Auguste Picard. At the conclusion of the experiment ballast was dropped and the recorders, geophones, and firing clocks were returned to the surface by the buoyancy of the balloons. Ewing also used such balloons to recover a free-falling camera (1946c). I do not know if he was aware of the long and largely unsuccessful history of such devices going back to the seventeenth century (Hook and Moray 1667, Deacon 1971).

Preliminary work was done in shallow water around Bermuda in 1939; this gave the thickness of the coral cap. In 1940, a record of one shot at one geophone was obtained at each of two stations in depths of 2600 and 4800 m. The velocity of P-waves in the sediment was determined, but, according to the published papers, no indication of the basement beneath the sediment was found (1946c, 1948b); George Woollard, however, tells me that one recording did show a wave refracted from beneath the sediments.

The difficulties that prevented geologically significant results from being obtained in deep water could have been overcome, and, when the work was stopped in 1940 by the development of the war, it was clear that a method of seismic shooting in deep water was available. The surprising thing is that in 1937 none of us realized that these heroic expedients were unnecessary. All that was needed was to put the instruments and the explosives near the surface of the sea and to treat the water as another layer in the problem. There are, however, circumstances in which it is desirable to have seismographs and other equipment on the sea floor without wires to a ship; for these Ewing's method has been widely used in very sophisticated forms in recent years (without the hazardous gasoline-filled balloons). One of these instruments was developed at Lamont; it was deployed on the sea floor and recorded on land through a cable. It could be left on the bottom for a month or more (1961m).

During the whole of the period up to the war both funds and ship-time had been meagre and difficult to get. It is perhaps inevitable that institutions such as the Coast and Geodetic Survey and Woods Hole Oceanographic Institution should have regarded work on quite new lines as a thing to be fitted in among their regular business. Ship-time for seismic shooting had to be taken from other projects which had already been planned and which were clearly worthwhile. It was, in a way, a generous gesture to let Ewing share *Atlantis* for short periods with other projects. In fact he got forty-five days of shared time in the five years from 1935 to 1939. Clearly the availability of ship-time was the limiting factor in what could be accomplished.

The work at sea and the preparations for it involved an enormous expenditure of effort. Ewing and his students regularly worked far into the night and disrupted their home lives to a degree which was, perhaps, tolerable for the students but was damaging for Ewing. The work at Lehigh has been described by Woollard:

> It was a tight little group, and although we worked most nights on instruments or data analysis, and spent most weekends in the field, one night a week was devoted to relaxation. We'd start with spareribs and beer in a cheap little German restaurant, migrate up to the University rifle range for a couple of hours' shooting, and then end up at either Ewing's house or my apartment for more beer, music, and discussions . . . followed by scrambled eggs and coffee in the wee hours before calling it a night.

No doubt it was all great fun but it is not a recipe for a happy married life, and, when the strains and absences of wartime were added, Maurice and Avarilla parted and were divorced in 1941. Their son Bill, who was born in 1932, lived with his mother; he became a captain in the Air Force and was killed in an aircraft crash while in his thirties. Shortly before he died he was stationed near Lamont and he and his father got to know each other again.

Overworking was probably inevitable if worthwhile results were to be obtained with so little ship-time and money, but it was also a marked trait in Ewing's character. He was driven by an inner urge to compulsive overwork. He believed that every opportunity must be seized and exploited to the full. He seemed to feel that the world was against him, but was always sure that he and his band of students and friends would overcome the difficulties and show that with small resources they could achieve the apparently impossible. His confidence in his own ability and in the effectiveness of his students was one of his most endearing characteristics, but was not always appreciated by others.

THE WAR, 1940-1946

In 1940 Ewing became convinced that the United States would become involved in the war with Germany and that the

Navy would need his kind of knowledge and skills. He obtained leave from Lehigh, who made him an Associate Professor when he left (he had been made an Assistant Professor in 1936). He went to the Woods Hole Oceanographic Institution where he was a Research Associate from 1940 to 1944. Allyn Vine and Joe Worzel, who had worked with him at Lehigh, moved with him. Woollard followed later.

He and his group got to work with great speed. Even before government finance had been found for the work, they and Columbus Iselin, the Director at Woods Hole, had written a manual for the Navy entitled *Sound Transmission in Sea Water* (there is a copy in the Woods Hole library) and had redesigned and greatly improved the bathythermograph, which had been devised some years before by Athelstan Spilhaus. After a month or two they were, as Worzel put it, rescued from starvation and "practical socialism" by contracts from the Bureau of Ordnance and the Bureau of Ships of the U.S. Navy.

Ewing's style of work had an electrifying effect on what had been a rather slow-moving marine biological station. In his unpublished memoirs Iselin wrote: "He had a profound effect on the success of this laboratory. He arrived here first as a very young professor. . . . He brought with him several Lehigh students and the place has never been the same since. They literally worked night and day, and seven days a week."

The wartime investigations of Ewing and his group are described in a paper published after the end of the war (1946c), and a list of some of his reports is in National Defense Research Committee (1946). For a few months they were able to continue the refraction shooting on the continental shelf and in deep water. Soon, however, more pressing matters needed all their attention. Among the things studied was the "bubble pulse" from explosions. It had been known since 1898 that multiple shocks were produced by the detona tion of a single charge underwater. The phenomenon had also been noticed by Ewing while doing seismic shooting in Louisiana during his vacations from Rice. The cause had been correctly stated by the discoverer (Blochmann 1898) to be the collapse and rebound of the gas bubble, which overshot its equilibrium size, collapsed to a small radius and expanded again to produce a shock wave of intensity comparable to that of the original explosion. The explanation had been lost sight of and to both the American and British navies the phenomenon was something of a mystery. It was of importance as the bubble pulses can substantially increase the damage from underwater explosions. Ewing obtained pressure-time curves and arranged for H. E. Edgerton of MIT to take underwater photographs of the bubble (1946c, 1948b). These showed that it performed nonlinear oscillations during which it collapsed to a very small volume. Ewing obtained an empirical relation between the time interval between pulses and the size and depth of the charge. The theory was worked out by Chaim Pekeris and provided a correction to the empirical formula. It is curious that the explosives group at Woods Hole led by E. Bright Wilson was doing closely similar work at the same time (Arons 1948). Dr. A. B. Arons tells me that security was so tight at Woods Hole that he had only a vague idea that Ewing's group was working on the same matters as his own. It is a salutary example of the dangers of an excessive regard for security between groups in an organization. The whole thing was done yet again at about the same time by H. F. Willis and G.I. Taylor in England.

Perhaps the best-known work by Ewing during the war was his discovery and exploitation of the low-velocity sound channel in the ocean, which occurs at depths of 700-1300 m and is known as the SOFAR channel (an acronym for Sound Fixing and Ranging). The channel may be looked on as a pipe along which sound is repeatedly reflected, or as a wave-guide in which sound waves are trapped. Thus, if an explosion is made near the depth of minimum sound velocity, the sound will spread in two dimensions instead of in three and can reach great distances before its intensity falls below that of the ambient noise. In one of Ewing's experiments a charge of a few pounds dropped off the west coast of Africa was heard off the Bahamas. The phenomenon has obvious applications, and a network of SOFAR stations has been in operation for many years. A related matter is the seismic "T phase" which Ewing showed to be propagated across the ocean in the SOFAR layer (1950 f, g, 1952d, 1953c, 1957e).

The propagation of sound in the sea is a more complicated phenomenon than might be expected. The gradients of temperature, pressure and salinity bend the rays and produce shadow zones and focusing effects. Such matters are of importance in submarine detection and in the operation of submarines. Some work had previously been done by the Coast and Geodetic Survey but it had not been published and its importance was not appreciated by the Navy. The work at Woods Hole greatly clarified the subject; much of the information relevant to geophysics was subsequently published as a book (1948b, c). This book also contains an important paper by Pekeris on the theory of sound transmission in the sea.

All through the war Ewing kept fairly closely to the subjects in which he was an expert; in these he made very substantial contributions. He seems not to have had any wish to enter into questions of more general policy concerning the conduct of the war, he certainly had no desire to set himself up as an *éminence grise* to any military or political figure. In this he differed from many of his contemporaries on both sides of the Atlantic.

In 1944 he married Margaret Kidder whom he had met at Woods Hole; they had four children: Jerome, Hope, Peter and Margaret.

THE LAMONT GEOLOGICAL OBSERVATORY AND VEMA

In 1944 Ewing was invited to join the Geology Department of Columbia University as an Associate Professor (he became a full Professor in 1947). He accepted and moved there in June 1946 bringing many of his group with him. At first they worked in great congestion in a few rooms in the Schermerhorn Building on the main campus in New York, but in 1948 the widow of Thomas Lamont, a well-known banker, offered the University his estate at Palisades, a few miles from Columbia across the Hudson River. A fund of $250,000 was included with the gift. The University offered the place and the money to Ewing. Just at this time he and his group had a similar offer of a country mansion and financial support from MIT. They were tempted but decided to stay at Columbia; in this Ewing was influenced by the presence of W. H. Bucher in the Geology Department and by the friendliness of General Eisenhower, then President of Columbia.

To get the Lamont estate was an opportunity that might never occur again. Ewing hastened to make the decision irreversible by moving in equipment (the move has been entertainingly described by William Wertenbaker [1974 a, b]). It was a lovely place with a fine house and 155 acres of grounds; in a few years the house was full and several new laboratories were built. It no longer looks like a gentleman's residence, but it is still a wonderful place, with open space and trees, set in a village away from the stresses of New York City.

The new institution was, rather oddly, named the Lamont Geological Observatory. At first it was part of the Geology Department of Columbia University. In the early sixties it became an independent institution within the University. Ewing had the title of Director from 1949. A description of Lamont in its early days has been given by George W. Gray

(1956). During the first few years Ewing and Paul Kerr, who was Chairman of the Geology Department, succeeded in raising substantial funds from mining companies, the Rockefeller Foundation, and others. By 1969 these amounted to about $1,000,000. In 1968 Ewing went to see Mr. A. C. Newlin of the Doherty Foundation in search of a grant of $700,000; much to his surprise he was offered $7,000,000. Columbia accepted and in 1969 the name of the institution was changed to the Lamont-Doherty Geological Observatory.

The space and the funds which were now available were the basis of the remarkable developments of the period after 1949. A large part of the running costs came from the Office of Naval Research (ONR). As in so many fields ONR showed an enterprise, responsiveness, and good sense in the allocation and management of research funds which have not always been characteristic of apparently more relevant bodies. I suppose that it is, in theory, undesirable for the Navy to handle the bulk of the funds for civil research, but things have never been the same since their activities were restricted, and they did a wonderful job for Ewing.

In 1953 Ewing and Worzel returned from the Royal Society's discussion on "The Floor of the Atlantic Ocean" to find that the Navy had cancelled an arrangement to supply Lamont with a ship. They hastily hired the iron-hulled schooner *Vema* (62 m overall, 1010 tonnes displacement, propelled by sails and a diesel engine). For a small extra payment they also got an option to purchase. It is not easy to persuade a university to pay $150,000 for a rather antiquated sailing ship, and Ewing and Worzel only succeeded a few hours before the option ran out.

The decision to purchase *Vema* was crucial; it provided Ewing with a ship of his own which he could use how and where he needed it. For many years *Vema* was the center of

Lamont's seagoing activities. She was larger than. *Atlantis* and could operate worldwide. She spent a very large proportion of her time at sea and did all kinds of marine geophysical work in the Atlantic, Indian, and Pacific Oceans. Having her made an enormous difference to the amount of work that could be done; from the end of the war until recently ships were harder to get than money, and if you had a ship you could usually get the operating expenses from ONR, the National Science Foundation, or some other agency. The possession of the ship was particularly important for the development of instruments and equipment where it is difficult to estimate the time that will be needed. A couple of failures to make a new instrument work will rapidly disenchant an institution that lends you ships, but if you have your own ship and some money, you need not be so embarrassed by the difficulties and delays of doing new things.

In 1962 *Vema* was joined by the *Robert D. Conrad* built by the ONR. She is 63.4 m overall and has a displacement of 1360 tonnes.

From the foundation of Lamont, Ewing was in a new position; he was now the Director of what rapidly became a large and diverse organization working in many fields, in some of which he would not have claimed to be expert. The wonder was the extent to which he was expert, did know what was going on, and was an all-pervading influence. It is not possible here to describe the work of Lamont as a whole and attention must be concentrated on the parts that were central to Ewing's interersts.

SEISMOLOGY AT SEA

Early in 1949 Ewing fulfilled a long-felt wish; he got two ships at once (*Atlantis* and *Caryn* from Woods Hole). Worzel went out in one and Hersey in the other in the hopes of finding the depth of the Mohorovicic* discontinuity (the

Moho) under the deep sea by shooting a long seismic refraction line. It had long been suspected from the results of gravity measurement that, if the Moho existed at sea, it would be much shallower than it was on the continents, but there were several other possibilities. A reversed line 56 km in length was shot and the Moho found 5 km beneath the ocean floor (1949d, 1950d). Ewing and the others were cautious since the seismic velocity beneath the Moho was a little lower than it usually was under the continents; it was, however, pretty clear that they had a result of first-rate importance. It was now almost certain that the familiar basement rocks of the continents were absent beneath the oceans, or at any rate beneath the place between New York and Bermuda where the line had been shot. In fact it has proved a universal rule that the ocean floor is quite different from the continents, the Moho is very shallow, and the sub-Moho seismic velocity is usually near the continental value (1955a). The exploitation of this method of studying the crust beneath the floor of the deep sea became one of the main tasks of Lamont during the 1950's and 1960's (e.g., 1952a, 1953b, 1954a, 1956m, 1959c). There was, however, another means to the same end.

The wartime work on sound in the ocean, and especially the cooperation with Chaim Pekeris on the theory of the propagation of waves in a layered medium, turned Ewing's thoughts to the propagation of surface waves across the oceans. This is more complicated than the interpretation of the records of P and S waves from explosions; since the wave length exceeds the vertical dimensions of the layers, "ray optics" is inapplicable, and the solution of the wave equation is essential. The waves are dispersive, and thus their phase and group velocities are different; they are of two main types, Rayleigh waves and Love waves, which have different dispersion relations. In return for this complexity it is possible to obtain average properties of large areas of the earth. Refrac tion shooting gives a picture of the layers under the line of shots, the dispersion of the surface waves from an earthquake will show whether the results from the refraction shots are typical of a whole ocean. The resolving power is poor but the area covered is large.

The study of surface waves had been a favorite topic with theoretical seismologists since the beginning of the century. It was known that the dispersion curve for waves crossing the Pacific was different from that across the continents. This implied an important difference of structure in the upper 10-50 km and was consistent with the shallow oceanic Moho suggested by gravity observations. The observations of surface waves traveling across the Atlantic had been interpreted by Beno Gutenberg and Charles Francis Richter (1936) as indicating a structure intermediate between those of the continents and of the Pacific. Ewing saw that here was a tool which needed development and which could provide a short cut to the study of some of the major features of ocean basins.

His attack on the problem was both experimental and theoretical. Seismographs were installed in a vault at Lamont; the instruments had to be capable of recording much slower oscillations than those used for recording the "first arrivals" of P waves. Such instruments had been neglected, since most designers were more interested in improving the sensitivity at periods of 0.1-3 s rather than in the more difficult and apparently less rewarding task set by periods above 10 s. This was an example of the parsimonious and lopsided development of seismology which became so apparent at the meeting of experts on bomb-test detection in Geneva in 1958. These difficulties caused the injection of many millions of dollars per year into the subject, via the Advanced Research Projects Agency of the Department of Defense project "Vela Uniform," and rapidly transformed it. The work done at Lamont on instrument design during the 1950's (1958a) was an important base for the improved facilities and for the World

Wide Seismic Network. Later, instruments were developed that were capable of recording the natural oscillations of the Earth at periods between 10 s and 1 h. These periods of oscillation are now a major source of information about the interior of the Earth.

A series of papers by Ewing and Frank Press, starting in 1949, took up the theory of surface waves using more realistic models of the ocean floor than had been used previously (e.g., 1950c, 1955b). It was shown conclusively that the Atlantic was similar to the Pacific and not halfway to being a continent (the previous, erroneous view had arisen from an underestimate of the effect of the water on Rayleigh waves and from the absence of observations of short period Love waves). Ewing's first deep-water refraction station was typical of the oceans; this was a result of great importance, not only in itself, but in encouraging the more realistic use of surface-wave dispersion in the study of the Earth's crust and upper mantle. The method was applied in a series of papers by Ewing, Oliver and Press to elucidate crustal structure in many parts of the world (1955k, 1956h, 1959b); this work confirmed the universal difference in crustal thickness between the oceans and the continents which is one of the basic facts of geology. The work was summarized in 1956a and the theory in a book (1957a).

The work with earthquake records gave Ewing great satisfaction; he sometimes said that he wished that he could give up being the director of an institute and spend his time reading seismic records. The seismic work at sea was also near the center of his interests. Here he was stimulated by keen competition from Russell Raitt and George Shor at Scripps and from Maurice Hill at Cambridge; Ewing got the first measurement of the depth of the Moho, but occasionally they had the pleasure of discovering something before he did; for example, he missed the layer with a P wave velocity of about 41/2 km/s which lies beneath the sediments almost everywhere in the oceans. The oversight was probably due to having the shots too far apart in a laudable desire to get a station finished and get on to the next and also to the wish to have enough explosive left for some more stations.

Neither the refraction lines nor the study of surface waves could give any detail about the stratification or structure of the sediments of the ocean floor. For this it was necessary to observe reflections from small discontinuities in the sedimentary column. What was needed was, in a sense, an improvement in the echo sounder with more power and a lower frequency to give penetration into the sediments beneath the ocean floor.

Ewing had tried to observe such reflections as early as 1935 but had not obtained any useful results. Work on the improvement of reflection shooting started at Lamont in 1949. At first the ship was hove to, and a single hydrophone was lowered to record the shots (1949a). Later observations were taken under way. Progress was slow (for an account of work up to 1960 see Hersey 1963), and it was over ten years before really good, continuous seismic profiling was achieved. The success was largely the work of John Ewing, Maurice's brother.

In the early 1960's a single hydrophone was towed behind the ship and 0.2 kg charges were thrown into the sea every two minutes. The charges were attached to balloons to prevent them sinking to a depth where the bubble pulse would occur. The operator had to tuck the balloon under his arm, light the fuse, and throw the charge and its balloon into the sea. To do this hour after hour on a rolling ship in the middle of the night is a tedious operation and not without its dangers. In 1961 a man was killed in *Vema* and about a year later the method fell into well-deserved disuse. By then several other types of sound sources such as the "sparker" and the "air-gun" were available. The sparker produces sound by an underwater spark, and the air-gun is a container filled with air at a pressure of 150 atm which is suddenly released through a valve.

Much new information was obtained during the 1960's about the sediment of the deep ocean (1962g, 1963f, 1964g, 1965f,1, 1966a,1, 19671, 1968c). A quite unexpected discovery was the widespread occurrence of a conspicuous reflector named Horizon A. From the results of drilling, Horizon A is now known to be an Eocene deposit of hard amorphous silica, similar to the chert or flint found on land (19701). One of Ewing's most spectacular seismic discoveries was that the Sigsbee Knolls in the Gulf of Mexico are salt-domes (1966e, 1968d). Drilling has given indications that there are hydrocarbons trapped in the surrounding sediments, but it would be imprudent to drill for oil in such depths till techniques of control have been developed. Not striking oil has become an important objective in the planning of deep-sea drilling.

The techniques of reflection shooting at sea are of great importance to the oil industry. During the 1950's and 1960's they developed methods for use on the continental shelf where a string of several hundred hydrophones is towed behind a ship. Because of the cost, the use of these methods by academic institutions did not become common until 1972. The technique is elaborate; it involves digital recording on as many as forty-eight channels and processing by computers at sea and on land. The results are spectacular. The use of such equipment in the Gulf of Mexico was Ewing's main scientific interest in the last few months of his life. The equipment could produce 30 million bits of information per kilometer, so that even he must, at last, have felt that he had as many data as he could use.

TOPOGRAPHY AND SEDIMENTS OF THE OCEAN FLOOR

Seismology was Ewing's first love, but he and his students pursued many other lines of investigation with an equal enthusiasm. The most basic tool of marine geology is the echo sounder. This was developed about 1914 by R. A. Fessenden and the Submarine Signal Company who used audio-frequency sources; in the 1920's ultrasonic instruments were produced which use frequencies of 10-30 kc/s. These instruments worked admirably, particularly after the introduction of a variable density recorder depending on the electrolysis of paper impregnated with potassium iodide. They had, however, unreliable timing arrangements depending on a centrifugal governor or on the frequency of the ship's electrical supply. The effect of this was catastrophic; a ship would sail across the Pacific with an echo sounder recording say 400 m too shallow, the soundings would then appear on charts as a ridge along the ship's track. At one time the charts of the North Pacific were crossed by several of these bogus ridges following ships' tracks in a roughly east-west direction. Ewing and his colleagues undertook the design of a Precision Depth Recorder, the PDR (1954e). This was based on the facsimile recorders used for transmitting photographs for newspapers. It had electronically controlled timing and paper that gave permanent records. Such instruments are now universally used in survey and oceanographic ships; they give a timing accuracy equivalent to about 1 m in depth.

The main stimulus to the development of the PDR was the desire to study the abyssal plains which stretch beyond the foot of the continental rise at depths of around 5000 m. The PDR showed how extremely flat they are; gradients of less than one in a thousand are common. Samples collected from the plains showed coarse sands, shallow water fossils, and bits of wood, which strongly suggested that the material was derived from the continental shelf. R. A. Daly (1936) had suggested that during the Pleistocene ice ages, sediment was stirred up by waves breaking on the exposed continental shelf and that the muddy water ran down the slope eroding the canyons. P. Kuenen, in Holland, had made laboratory exper iments which suggested that a cloud of sediment dispersed in water can indeed run rapidly and turbulently down the slope and spread sediment over the ocean floor. He called these clouds of sediment and water "turbidity currents." Ewing set out to investigate the abyssal plain off the eastern seaboard of the U.S. He suspected that the 1929 earthquake on the Grand Banks had set off a turbidity current and showed (1952i) that the failures of submarine cables suggested that some cable-breaking agency was set off by the earthquake and propagated down the slope at speeds of up to 90 km/h. When he showed that there was coarse and apparently recent sediment at the foot of the slope and pointed out that long lengths of some of the cables had been carried away and buried, most people were convinced of the reality of turbidity currents as the carriers of the sediments of the abyssal plains. Later similar phenomena were found off Sicily and in other places.

In the course of the work on the abyssal plains the canyons that cross the continental edge were traced far beyond the slope over the plains. They had levees on each side and were clearly formed by some process involving flow from the canyons. The discovery of the deep extensions to the canyons made it improbable that they had been cut by subaerial erosion at a time when the land stood higher or the sea lower. The process by which canyons are formed is still not clear, especially when they are cut in hard rock, as are some of those off southern California.

Going further out to sea Ewing naturally became interested in the mid-Atlantic ridge. Here he, Heezen, and Tharp found that the deep depression, which was known to occur on many echo sounder profiles near the crest of the ridge, was a continuous valley (1956k, 19601). It gradually became apparent that it was a worldwide feature of the mid-ocean ridges (except along the East Pacific Rise), that it always runs near the shallowest part of the ridge, that it is displaced where the ridge crest is displaced on what are now called "transform faults," and that it has steep sides which are pretty clearly fault scarps. This discovery was entirely unexpected and has proved central to the development of tectonic theory. It was a result of Ewing's policy of keeping any ships he could get going back and forth across the ocean measuring anything that could be measured, collecting anything that could be collected, and not worrying too much about anything except getting to know the ocean floor. It is remarkable that he was able to find a major topographic feature which all the hydrographic departments and research ships of the world had missed. They had all been across the central valley many times but had not seen that it differed from all the other valleys on the ridge in being continuous.

It had long been known that earthquakes occur on the mid-Atlantic ridge and the more recent studies of Gutenberg and Richter (1941) and of Rothé (1956) had shown that they were concentrated in a narrow belt near the crest. The uncertainty of location was perhaps 100 km, and Ewing suggested that they all actually occur in the central valley and that their distribution could be used to trace the course of the ridge and its central valley in the long sections where there were no adequate lines of soundings. These ideas and some additional lines of soundings (e.g., 1960i) enabled him to demonstrate the worldwide extent of the ridge and the valley (though on the East Pacific Rise there are earthquakes but no valley).

The topographic studies were accompanied by the collection of sediments in coring tubes. The art of coring had been revolutionized by B. Kullenberg's piston corer which used the hydrostatic pressure to prevent the core jamming in the barrel as it goes into the bottom. This machine had been used with great effect by Hans Pettersson during his *Albatross* expedition of 1947-1948; it increased the length of core that could be taken from about 3 to 30 m.

Ewing became an obsessive collector of cores. To examine a core in detail is a lengthy operation; for a core of deep-sea ooze it involves carbon-14 age determinations on the upper parts and separating, identifying, and counting foraminifera over the whole length. For a core of red clay it requires paleomagnetic studies, chemical analyses, and y-ray counts to determine uranium and its daughter products. Ewing collected cores at a rate greatly in excess of the rate at which they could be examined in detail. He split them lengthwise, looked at them all in a rough way, had a few studied in detail, and put them all into storage. Understandably the people who were paying for the operation became restive. Why did he collect so many? Ewing replied that when he found two that were alike he would consider slowing down the rate of coring. The real reason, I think, lay deeper. He once said to me: "I go on collecting because now I can get the money; in a few years it will not be there any more, then I shall have the material to keep my people busy for years" (I do not remember the exact words). In fact the Lamont collection of cores is an invaluable and almost inexhaustible mine of information about the floor of the deep sea.

A related investigation concerned the particles suspended in the ocean water which might be expected to throw light on the processes of sedimentation (1963e, 1965 d,h, 1967n, 1969 c,i,r, 1970g).

The picture of the western Atlantic which emerged during the 1950's from the work at Lamont was paralleled by work by others in the eastern Atlantic and in the eastern Pacific. Some sort of order and system gradually emerged, and it became clear that the geology of the oceans must be studied in its own terms and not as an appendage to continental geology. The way was now clear to extend the discoveries to the whole ocean; that is, to two-thirds of the Earth's surface. In this Ewing and Lamont played a leading part and made many important discoveries, particularly in the western part of the South Atlantic. The work in the North Atlantic was summarized in a masterly book by Heezen, Tharp, and Ewing (1959k).

In 1952 Ewing (1953e) made a technical advance which was of an importance comparable to that of the introduction of refraction seismic shooting at sea. He took the airborne, fluxgate magnetometer, which had been developed during the war by Victor Vacquier for submarine detection, and towed it behind a ship. This was the start of a great enterprise which is still in progress and whose results are the main basis of the recent development of ocean-floor tectonics. The instrument is troublesome to use; it drifts, it is cumbersome, it has moving parts: it has now been replaced by the proton magnetometer introduced in sea work by Maurice Hill. It was, however, Ewing who first got the bandwagon rolling and whose example led to the surveys of Mason and Raff off the coast of California which revealed the zebra-like pattern of magnetic lineations (for the pre-1960 history of magnetic measurements at sea see Bullard and Mason, 1963).

SEA-FLOOR SPREADING AND PLATE TECTONICS

By 1960 the general nature of the sea floor had, in large measure through the work of Ewing and his colleagues, become clear. The shelf, slope, rise, abyssal plains, abyssal hills, ridge, and central valley were all understood in a descriptive sense, as Ewing, Heezen, and Tharp had shown (1959k) and as was shown on a larger scale in the collective work edited by Hill (1963, but mostly written in 1960). These works take the features one at a time, describe them, and give what may be called their local history. Behind this, however, there were the most important questions. What was the history of the oceans? How had they been formed? Had they always been there? These questions are not seriously approached even in

Hill (1963). However, in the study of the sea floor and in other directions, particularly in paleomagnetism, a considerable head of steam was accumulating which, in the early 1960's, ripped apart what had become the established views of most geologists, at any rate in the northern hemisphere. The critical questions were: what is occurring along the central valley and why are the ocean floors so young (no sediments older than 150 Ma had been found but many samples of all younger ages)? The outcome is well-known and the route to it has been described by many authors. During the 1960's it was established beyond doubt that the oceans are young because they have been formed recently and that ocean floor is being formed today in the central valley of the mid-ocean ridge along the line of earthquakes that Ewing discovered there. The data from which all this was established came, in large part, from the work at Lamont, but the initial steps in the great synthesis did not (though Heezen was teetering on the edge of the ideas).

The course of Ewing's thoughts on these matters is not easy to trace in his papers. He was not given to sweeping generalizations about large-scale processes; he believed in the accumulation of information about the sea floor and that the major discoveries are made at sea. After it became clear that there were no buried continents beneath the oceans he believed that the oceans had always been where they are today. Gray (1956) quotes him as saying:

> We have every reason to believe that in that 2000 feet of unconsolidated sediment [on the ocean floor] the whole history of the Earth is better preserved than it is in the continental rocks. . . . As we punch deeper into the ocean sediments we may reach levels holding traces of the first animals that concentrated calcium carbonate, then evidence of atmospheric oxygen from the earliest green plants, and ultimately the primeval sediment of the earliest erosion, marking the advent of water in the sea. [This does not sound like Ewing's conversation and is, presumably, a summary.]

Just at this time the permanence of the relation between continents and oceans was being questioned by workers in paleomagnetism. There are occasional references to arguments against continental drift in Ewing's papers in the 1950's (e.g., in 1952e he said that if America had moved away from Europe there would not be time for isostasy to be reestablished).

In fact, Ewing took remarkably little part in the controversy that raged between 1955 and 1965. He probably thought that work at sea would make all such things clear, as in fact it did. At the first international oceanographic conference, held in the United Nations Building in New York in 1959, Ewing gave the first of the invited talks. He talked on "Shape and Structure of Ocean Basins." I waited, fascinated, for him to commit himself on these matters, but he said very little about them and in the published account (1961g) there is no reference whatever to the wider questions. In *The Sea* there is a review article by Heezen and Ewing (19631), in which it is said that there is tension beneath the central valley which may be accommodated either by compression of the continents or by expansion of the earth (the latter view was held by Heezen but not by Ewing).

About 1964, following the publication of papers by Hess and by Vine and Matthews, a number of the younger workers at Lamont began to examine their magnetic data from the new point of view and became convinced of the reality of seafloor spreading. It is remarkable that Ewing not only allowed but encouraged James R. Heirtzler, Neil Opdyke, Lynn Sykes, and others to pursue this investigation and to publish views that were basic to the subject on which he had spent his whole life but were contrary to his own beliefs. His open-mindedness led to what was, perhaps, Lamont's greatest success.

Ewing had always insisted that data and cores should be properly stored and catalogued and that all data from a given area should be available to anyone working on that area. In most other institutions data were regarded as the private property of the man who collected it or of the chief scientist of the cruise; whoever had it worked it up, published it, and kept it in ways and places of his own choosing. Lamont's policy of communal data storage gave them a two-year lead. They had it all available and in a very short time published a series of papers on magnetic lineations, the focal mechanisms of earthquakes, and the paleomagnetism of deep-sea cores which established the reality of plate tectonics.

A number of papers (1966c, d, m, n) written early in 1966 show Ewing deeply concerned about sea-floor spreading and impressed by the evidence but finding it unacceptable, at any rate for the Atlantic. He pointed out (1966d) that there were places in the northwest Pacific where Cretaceous sediments appeared at the surface and where the thickness was such that it could reasonably be supposed that sediments going back at least to the Triassic were present. There were also other difficulties, some specific, such as the discovery (1966c) of Miocene sediments in the central valley (they were probably from a transform fault and not from the central valley, without a detailed survey it is easy to confuse the two), some matters of general principle, such as the lack of variation of heat flow across the ridge (there is, in fact, a variation of the expected kind; Ewing [1966m] used a considerable body of Lamont data but, because of the high probability of damaging the equipment, had taken none in or close to the central valley; he ignored results from workers elsewhere).

I believe that he became convinced of the essential correctness of the "drifters and spreaders" views by the end of 1966. In November of that year a meeting was held at the Goddard Institute for Space Studies in New York. Just before the meeting started Ewing came up to me, looking, I thought, a little worried, and said: "You don't believe all this rubbish do you?" I admitted that I did, and I fancy that the following two days of systematic exposition, largely by his own students, convinced him (he did not contribute to the published proceedings of the meeting). He still found the ideas too simple and too uniformitarian. In this he was clearly right; quite complicated things have happened. Rates and directions of spreading have changed in the past, though the long intervals of no spreading that he later suggested in the Atlantic and Indian Oceans seem not to have occurred. I think his initial difficulties were due to knowing too much. If you have in your mind an enormous data bank, there is sure to be some fact that appears to contradict any general theory. You then become very wary of all general theories.

CAUSES OF ICE AGES

Starting in 1955 Ewing and W. L. Donn published a series of papers setting out a new theory of the causes of ice ages (1956g, 1958d, 1959d, 1961a, 1963g, 1964a, 1965a, 1966h, 19681, 1971d). The problem is of long standing and has two aspects: first, why has there been a series of ice ages and interglacials during the past two million years and at various earlier periods and, second, why are such groups of ice ages separated by intervals of perhaps 100 Ma with no ice ages? Ewing believed that the ice cover in the Arctic Ocean is unstable and subject to occasional melting (for the mechanism of the instability see 1956g). When the ice melts, absorption of the Sun's heat and evaporation are increased, precipitation on the Arctic land masses is greatly increased, the snow cover lasts through the summer, absorption of radiation is reduced, and an ice sheet builds up. This part of the theory is given an added interest by the recent thinning of the ice in the Arctic Ocean and the possibility that within one or two generations we may be faced by the beginnings of a crisis that, both politically and technically, we are in no state to face. It would seem prudent to put a substantial effort into the study of these matters by drilling in the Arctic seas.

The second half of Ewing and Donn's theory is that the occurrence of ice ages depends on the pole being situated in an ocean and that polar wandering and continental drift will cause this to occur intermittently at intervals of the order of 100 Ma. Here there is a difficulty in that the pole is at present 700 km from the nearest land and cannot have entered the Arctic Ocean as recently as 2 Ma ago. Such a shift would imply that the pole moved relative to the land at a speed of 35 cm/a which is too high to be credible. Again, what we need is a detailed climatic history of the late Tertiary in the Arctic which could be obtained by drilling and might show that the recent sequence of ice ages goes back further than is usually supposed.

LUNAR SEISMOLOGY

A major interest of Ewing's later years was lunar seismology. This was a joint project between a number of institutions, but the instrument development was done mainly at Lamont-Doherty. Ewing took a close interest in the instrumental work and also in the interpretation of the puzzling and unexpected records, which show oscillations continuing for tens of seconds instead of the sharp arrivals usual on terrestrial records (1969m, 1970 a, i, m, 19710,s, 1972 g, j, k, 1, m, o, 1973 g, i, j, k, 1974 d, e, h, 1975b). The propagation of these waves is, perhaps, somewhat similar to that of the SOFAR and T phase signals which Maurice had discovered long before.

OTHER INVESTIGATIONS

It is not possible here to describe the full range of subjects that, at one time or another, caught Ewing's interest. The titles of the papers will indicate them. In seismology there is a series of papers on the interaction of seismic and atmospheric waves (1951 b, e, 1952b, 1953a, 1967m, 1971x), another series on microseisms (1948a, 19521,m, 1953f, 19561, 1957c), three papers on the propagation of elastic waves in ice (1934 b, c, 1951 f). There are also five papers (1958g, 19601, 1962 c, d, 1963b) on the effects of nuclear explosions, five on petrology (1969 j, k, 1970 b, c, 1971p) and others on heat flow (1965c, 1966n) and paleontology (19591).

THE MOVE TO GALVESTON

The relation between an American research institute, such as the Lamont-Doherty Observatory, and the university of which it forms a part is a delicate symbiosis. The university gains prestige, a small amount of undergraduate teaching, and the supervision of a large number of graduate students. Financially it will usually come near to breaking even, the overheads on the outside contracts balancing the direct payments to the institute from the general income of the university. Once it is a going concern the institute needs the university, not primarily for financial reasons but to attract graduate students; students need Ph.D.'s and only a university can give them. It is easy to see how this relation can go wrong; the administration of the university feels that it has responsibility, but in practice little control over an organization which has its own finances and which will, if it comes to a fight, have wide support in the scientific community. On the other side, any encroachment from the central administration will be taken by the institute as interference by people who are contributing little and are activated by motives of self-aggrandisement.

Such a confrontation gradually developed at Lamont-Doherty and came to a head in 1972. After the student riots, Columbia found itself in a difficult financial situation; Ewing believed that the new President, William McGill, was not only trying to enforce a stricter control over his activities, but was also attempting to take a part of the Doherty money for general university purposes. The details are complicated and it is not necessary to go into them here. Such a dispute was difficult for Ewing who all his life had half felt that things would, somehow, sometime, go wrong. He retired from Columbia with a month's notice and left Lamont, as did Joe Worzel, James Dorman, and Gary Latham. He would have reached the retiring age in 1973 and would then have had to retire as Director, though he could, presumably, have stayed on as a professor.

In June 1972 Ewing moved back to his home state of Texas and became Cecil and Ida Green Professor at the Marine Biomedical Institute of the University of Texas (now the Marine Science Institute) at Galveston. He hoped to develop marine geophysics at the Institute and to keep a close collaboration with Columbia in scientific matters; to encourage this he became a Research Associate at Lamont-Doherty and went there for short visits every few months, staying in an apartment that had been made from his old office. In the words of his successor, Manik Talwani: "He probably did more scientific work here on those visits than he did during the last year before leaving for Texas."

In Galveston, Cecil Green, himself a distinguished geophysicist as well as an outstanding industrialist, and his wife not only provided a professorship but also part of the cost of a ship, the *Ida Green.* Their generosity was a great support to Ewing at a critical time; it enabled him to get his work going again with hardly a break. Green told me that, just after the move, he asked Ewing whether he would be happy in a small institution with a director who was a medical man and a biologist; Ewing replied: "Of course, look at all these smiling faces, that's what matters." The parting from Lamont had been a bitter and deeply disturbing experience for him, but once it was done I think he was genuinely glad to be clear of the troubles of Columbia and to be at sea again in a small ship with a group of friends and students working on a well-defined objective.

The objective was the study of the Gulf of Mexico by the methods of reflection seismology. For this purpose the *Ida Green* was fitted with the latest 24-channel seismic equipment with digital recording. He lived to see the first results (1975a), but on 28 April 1974 he suffered a cerebral hemorrhage and died, on 4 May, without regaining consciousness. He was within eight days of his sixty-eighth birthday.

PERSONALITY AND ACHIEVEMENT

Ewing was, in a sense, a devoted family man. His love for his family shows very clearly in a recording made immediately after an accident in *Vema* in January 1954. The ship was 300 km north of Bermuda in a gale with mountainous seas. Ewing, his brother John, and the first and second mates were securing some drums of lubricating oil which had broken loose. A freak wave caught them unawares and all four, with the oil drums, were washed overboard. The Captain of *Vema* directed the rescue operations from the masthead, and Captain McMurray, the old friend who had been Captain of *Atlantis* in the thirties, maneuvered the ship. Thanks to his skill and long experience all but the second mate were rescued, Ewing by a very narrow margin. He was left with a slight limp and minor effects of internal injuries for the rest of his life. Next day he had recovered sufficiently to record a message which was sent to his children and was afterwards published (1954r, publication was due to an error in the Lamont office). It is a message from a man who has come through a harrowing experience, is not sure if he is going to live or if he will be paralyzed, and wants to send a message to his family while he still can. Its theme is that he had only survived because of the feeling that he must get back to the family and children he loved and that it was only their love that had saved him. About the genuineness of his feelings there can be no doubt—everyone who knew him well has testified to it; yet, in practice, he was unable to spare sufficient time to keep his first two marriages afloat. His daughter Margaret has described how, to see something of him, she used to walk back to his office with him after dinner and then go home through the dark grounds when her bedtime came. In 1965 he and his wife parted and were divorced; shortly afterwards he married Harriett Green Bassett who had been his secretary at Lamont. She continued in her job after marriage; this must have had certain disadvantages, but, with a lessening of Ewing's habit of working through a large part of the night, it did at least enable her to see more of him than had her two predecessors.

Ewing was not a committee man, but he would devote substantial time to organizations and causes that he regarded as important. First among these was the Navy to whose wellbeing he was deeply attached. He was on the Board of Governors of Rice University (1969-1972); Vice-President (1953-1956) and President (1956-1959) of the American Geophysical Union; Vice-President (1952-1955) and President (1955-1957) of the Seismological Society of America; and Vice-President of the Philosophical Society of Texas (1973-1974). He was, for a time, on the National Academy of Sciences committee responsible for the ill-fated Mohole project and took a large part in its enormously productive successor the Deep Sea Drilling Project, of which Lamont Doherty was one of the five founding institutions. Ewing and Worzel were co-Chief Scientists on Leg 1.

Ewing had a passionate interest in the oceans; along with this went a desire to teach people about them. He was a great teacher, not in the formal sense of being skilled in classroom instruction, but in the way he could teach by example how to discover things. He spent much time at sea making things work, untangling greasy cables, looking at records, and deciding what to do next. He never asked anyone to do what he would not have done himself, and in fact he could and would do almost anything. He once told me that the pendulum apparatus he was taking to some island, perhaps Bermuda, was not quite finished, but that the ship had a lathe and he would finish it on the way. The long line of his distinguished students is testimony not only to his effectiveness as a teacher but also to his personal qualities which attracted them and kept so many of them at Lamont in a period when many superficially more attractive jobs were available. I knew him intermittently for thirty-seven years; to me he was uniformly friendly, welcoming, and amusing. He delighted in elaborating stories of the early days until no one knew what had really happened. I can imagine that, if you wanted something that he wanted for his own purposes, he could be a hard and difficult man, but I never saw it.

Ewing and his group discovered more new things about the Earth than any other group has ever done before. He himself was primarily interested in finding what was there. Lamont was set up for this purpose, "Observatory" was, perhaps, the right name; the emphasis was on data gathering and on its immediate interpretation and not on global theory. His success did not come merely from intelligence but from deeper gifts of character which enabled him to set up an effective organization of the kind he needed. As I was writing this a student from Cambridge came back from a month in *Vema.* I asked him how he found the ship, expecting complaints about her smallness and inadequacy. Instead he replied: "Superb, there's nothing like her anywhere. It's all so well run, you can get twice as much done as you can in any other ship." *Vema* was the center of Ewing's life and with her he discovered the nature of two-thirds of the Earth's surface. The last time I met him I asked him where he kept his ships. He replied: "I keep my ships at sea."

In writing this notice I have had unstinted help and advice from Ewing's family, friends, and colleagues; though it is only fair to say I have not, in all instances, taken the advice. It is impossible to mention all by name, but I am specially grateful to his widow, Harriett; to his ex-wife Margaret; to his sisters Mrs. Rowena Peoples and Mrs. Lucy Clawson; to his brother John; and to his early students, A. P. Crary, Allyn Vine, George Woollard, and Joe Worzel. I am conscious that I have done an injustice to Ewing's colleagues at Lamont in that I have ascribed to him discoveries that were the result of the joint efforts of many people. I hope that the names in the bibliography will indicate the extent to which Lamont was a scientific commune. To have made it so was one of Ewing's achievements.

I wrote the original version of this notice in 1975 while Hitchcock Professor at the University of California at Berkeley and while Doherty Professor at the Woods Hole Oceanographic Institution. I revised it for the National Academy of Sciences while working at the Scripps Institution of Oceanography and at the University of Alaska.

The letters and unpublished documents on which this memoir is based have been deposited in the archives of Columbia University. Ewing's own papers are in the scientific archives of the University of Texas at Austin.

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Rothé J.P. 1954. La zone séismique médiane Indo-Atlantique. *Proc. R. Soc. Lond.,* 222:387-97.
Wertenbaker, W. 1974. Profiles: explorer. *New Yorker,* 4 Nov., pp. 54-118 ; 11 Nov., pp. 52-100; 18 Nov., pp. 60-110.
Wertenbaker, W. 1974. *The Floor of the Sea: Maurice Ewing and the Search to Understand the Earth.* Boston: Little, Brown. 275 pp.

HONORS AND DISTINCTIONS

Awards and Memberships

Guggenheim Fellow, 1938
National Academy of Sciences, Member, 1948
Geological Society of America, Arthur L. Day Medallist, 1949
Philosophical Society of Texas, Member, 1953
Guggenheim Fellow, 1953, 1955
National Academy of Sciences, Agassiz Medal, 1955
U.S. Navy Distinguished Public Service Award, 1955
American Academy of Arts and Sciences, Member, 1951
Royal Netherlands Academy of Sciences and Letters, Foreign Member (Section for Sciences), 1956
American Geophysical Union, William Bowie Medal, 1957
Argentine Republic, Order of Naval Merit, Rank of Commander, 1957
Society of Exploration Geophysicists, Honorary Member, 1957
American Philosophical Society, Member, 1959
American Institute of Geonomy and Natural Resources, Inc., John Fleming Medal, 1960
Columbia University, Vetlesen Prize, 1960
American Geographical Society, Cullum Geographical Medal, 1961
Dickinson College, Joseph Priestley Award, 1961
Rice University, Medal of Honor, 1962
National Academy of Sciences, John J. Carty Medal, 1963
Geological Society of London, Foreign Member, 1964
Royal Astronomical Society (London), Gold Medal, 1964
Swedish Society for Anthropology and Geography, Vega Medal, 1965
Academia de Ciencias Exactas, Fisicas y Naturales (Buenos Aires), Corresponding Member, 1966
Third David Rivett Memorial Lecturer (C.S.I.R.O., Australia), 1967
Indian Geophysical Union, Honorary Fellow, 1967
American Association of Petroleum Geologists, Sidney Powers Memorial Medal, 1968 American
Association of Petroleum Geologists, Honorary Member, 1968 Saint Louis University Sesquicentennial Medal, 1969
Geological Society of London, Wollaston Medal, 1969
Sociedad Colombiana de Geologia, Honorary Member, 1969

Royal Society of New Zealand, Honorary Member, 1970

Royal Society (London), Foreign Member, 1972

Rice University, Alumni Gold Medal, 1972

Robert Earll McConnell Awards—American Institute of Mining, Metallurgical and Petroleum Engineers, 1973

Royal Astronomical Society (London) Associate, 1973

Houston Philosophical Society, Member, 1973

National Medal of Science, 1973

Canadian Society of Petroleum Geologists, Honorary Member, 1973

First Sproule Lecturer, University of Alberta, 1973

Distinguished Achievement Award for the Offshore Technology Conference, May 1974

American Geophysical Union, Walter H. Bucher Medal for 1974

Honorary Degrees

Sc.D., Washington and Lee University, 1949

Sc.D., University of Denver, 1953

Sc.D., Lehigh University, 1957

Sc.D., University of Utrecht, 1957

Sc.D., University of Rhode Island, 1960

Sc.D., University of Durham, 1963

Sc.D., University of Delaware, 1968

Sc.D., Long Island University, 1969

Sc.D., Universidad Nacional de Colombia, 1969

Sc.D., Centre College of Kentucky, 1971

LL.D., Dalhousie University, 1960

Bibliography

This list is based on one kept by Ewing, on the Lamont-Doherty list of publications, and on my own collection of his papers. It includes only material published in books, journals, and conference proceedings. Things not generally available, such as reports to government agencies and grant-giving bodies, are excluded, as are *NASA Preliminary Science Reports* and reports with a security classification, even if they are now declassified (a list of war-time reports will be found in the now declassified National Defense Research Committee [1946]). Regretfully I have had to exclude published abstracts from the list; they are often interesting and frequently are not followed by papers. As they sometimes precede the corresponding papers by as much as four years they are of importance to those interested in priority of discovery.

1926 Dewbows by moonlight. Science, 63:257-58.

1930 M. Ewing and L. D. Leet. Seismic propagation paths. Tech. Publ.
Am. Inst. Min. Metall. Eng., no. 267, 1-18. Also in: Trans.
Am. Inst. Min. Metall. Eng., 97(1932):245-62.

1931 L. D. Leet and M. Ewing. Velocity of explosion generated waves in a nepheline syenite. Trans. Am. Geophys. Union, 12th meeting: 61-65.

1932 a. M. Ewing and L. D. Leet. Comparison of two methods for interpretation of seismic time-distance graphs which are smooth curves. Trans. Am. Inst. Min. Metall. Eng., 97:263-70.

b. L. D. Leet and M. Ewing. Velocity of elastic waves in granite. Physics, 2:160-73.

1934 a. M. Ewing, A. P. Crary, and J. M. Lohse. Seismological observations on quarry blasting. Trans. Am. Geophys. Union, 15th meeting: 91-94.

b. M. Ewing, A. P. Crary, and A. M. Thorne. Propagation of elastic waves in ice, Part I. Physics, 5:165-68.

c. M. Ewing and A. P. Crary. Propagation of elastic waves in ice, Part II. Physics, 5:181-85.

d. M. Ewing and A. P. Crary. Study of emergence angles and propagation paths of seismic waves. Physics, 5:317-20. Also in: Bull. Am. Assoc. Petrol. Geol., 5(1935):154-60.

1935 M. Ewing and A. P. Crary. Propagation of elastic waves in limestone. Trans. Am. Geophys. Union, 16th meeting:100-103.

1936 a. M. Ewing, A. P. Crary, J. W. Peoples, and J. A. Peoples. Prospecting for anthracite by the earth-resistivity method. Tech. Publ. Am. Inst. Min. Metall. Eng., no. 683, 1-36. Also in: Trans. Am. Inst. Min. Metall. Eng., 119:443-83.

b. M. Ewing and H. H. Pentz. Magnetic survey in the Lehigh Valley. Trans. Am. Geophys. Union, 17th meeting: 186-91.

c. Seismic study of Lehigh Valley limestones. Proc. Pa. Acad. Sci., 10:72-76.

d. Frequency of water waves. Fld. Eng. Bull. U.S. Cst. Geod. Surg., 10:65.

1937 a. Gravity measurements on the U.S.S. *Barracuda*. Trans. Am. Geophys. Union, 18th meeting: 66-69.

b. M. Ewing, A. P. Crary, and H. M. Rutherford. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part I: Methods and results. Bull. Geol. Soc. Am., 48:753-802.

c. Science in the deep. Lehigh Alumni Bull., 24(5):8-9, 19.

1938 a. G. P. Woollard, M. Ewing, and M. Johnson. Geophysical investigations of the geological structures of the coastal plain. Trans. Am. Geophys. Union, 19th meeting: 98-107.

b. M. Ewing and A. C. Vine. Deep sea measurements without wires or cables. Trans, Am. Geophys. Union, 19th meeting: 248-51.

c. M. Ewing and H. H. Pentz. A proposed investigation of Vening Meinesz anomalies. Trans. Am. Geophys. Union, 19th meeting:90-91.

d. Locating a buried power shovel by magnetic measurements. Proc. Pa. Acad. Sci., 12:31-33.

e. Marine gravimetric methods and surveys. Proc. Am. Philos. Soc., 79:47-70.

1939 a. Sub-oceanic seismology. (2) Report on the application of reflection—and refraction—methods. Advance report of the commission on continental and oceanic structure, Part 5, to the Washington Assembly of IUGG, pp. 50-51.

b. G. P. Woollard and M. Ewing. Structural geology of the Bermuda Islands. Nature (Lond.), 143:898.

c. M. Ewing, G. P. Woollard, and A. C. Vine. Geophysical investigations in the emerged and submerged Atlantic coastal plain. III: Barnegat Bay, New Jersey section. Bull. Geol. Soc. Am., 50:257-96.

1940 a. Present position of the former topographic surface of Appalachia (from seismic evidence). Trans. Am. Geophys. Union, 21st meeting: 796-801.

b. M. Ewing, G. P. Woollard and A. C. Vine. Geophysical investigations in the emerged and submerged Atlantic coastal plain. IV: Cape May, New Jersey section . Geol. Soc. Am. Bull., 51:1821-40.

1941 Deep sea seismic methods and bottom photography. Rep. Woods Hole Oceanog. Inst., 1940:20.

1942 Submarine gravity measurements. Rep. Woods Hole Oceanog. Inst., 1941:19.

1944 Speaking of pictures. Life, 13(Nov. 17):12-14.

1945 Marine mysteries. Science Illustrated, 6(5):47-52.

1946 a. M. Ewing, A. C. Vine, and J. L. Worzel. Photography of the ocean bottom. J. Opt. Soc. Am., 36:307-21.

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d. F. Press and M. Ewing. Low speed layer in water-covered areas. Geophysics, 13:404-20.

1949 a. J. B. Hersey and M. Ewing. Seismic reflections from beneath the ocean floor. Trans. Am. Geophys. Union, 30:5-14.

b. M. Ewing and F. Press. Notes on surface waves. Ann. N.Y. Acad. Sci., 51:453-62.

c. I. Tolstoy and M. Ewing. North Atlantic hydrography and the mid-Atlantic ridge. Geol. Soc. Am. Bull., 60:1527-40.

d. M. Ewing, J. L. Worzel, J. B. Hersey, F. Press, and G. R. Hamilton. (no title.) Geol. Soc. Am. Bull., 60:1303.

1950 a. M. Ewing, J. L. Worzel, N. C. Steenland, and F. Press. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part V: Woods Hole, New York and Cape May sections. Geol. Soc. Am. Bull., 61:877-92.

b. F. Press, M. Ewing, and I. Tolstoy. The Airy phase of shallow focus submarine earthquakes. Bull. Seismol. Soc. Am., 40: 111-48.

c. M. Ewing and F. Press. Crustal structure and surface wave dispersion. Bull. Seismol. Soc. Am., 40:271-80.

d. M. Ewing, J. L. Worzel, J. B. Hersey, F. Press, and G. R. Hamilton. Seismic refraction measurements in the Atlantic Ocean basin, Part I. Bull. Seismol. Soc. Am., 40:233-42.

e. F. Press and M. Ewing. Propagation of explosive sound in a liquid layer overlying a semi-infinite solid. Geophysics, 15:426-46.

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i. Presentation of the Day medal to William Maurice Ewing (with his reply). Proc. Geol. Soc. Am., 1949:77-78.

1951 a. F. Press and M. Ewing. Theory of air coupled flexural waves. J. Appl. Phys., 22:892-99.

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c. D. B. Ericson, M. Ewing, and B. C. Heezen. Deep-sea sands and submarine canyons. Geol. Soc. Am. Bull., 62:961-65.

d. K. E. Burg, M. Ewing, F. Press, and E.J. Stulken. A seismic wave guide phenomenon. Geophysics, 16:594-612.

e. H. Benioff, M. Ewing, and F. Press. Sound waves in the atmosphere generated by a small earthquake. Proc. Natl. Acad. Sci. USA, 37:600-603.

f. F. Press and M. Ewing. Propagation of elastic waves in a floating ice sheet. Trans. Am. Geophys. Union, 32:673-78.

g. B. C. Heezen, M. Ewing, and D. B. Ericson. Submarine topography of the North Atlantic. Geol. Soc. Am. Bull., 62:1407-17.

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1952 a. C. B. Officer, M. Ewing, and P. C. Wuenschel. Seismic refraction measurements in the Atlantic Ocean. Part IV: Bermuda, Bermuda Rise, and Nares Basin. Geol. Soc. Am. Bull., 63:777-808.

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c. D. B. Ericson, M. Ewing, and B. C. Heezen. Turbidity currents and sediments in the North Atlantic. Bull. Am. Assoc. Petrol. Geol., 36:489-511.

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e. The Atlantic Ocean basin. Bull. Am. Mus. Nat. Hist., 99:87-91.

f. F. Press and M. Ewing. Magnetic anomalies over oceanic structures. Trans. Am. Geophys. Union, 33:349-55.

g. J. L. Worzel and M. Ewing. Gravity measurements at sea, 1948 and 1949. Trans. Am. Geophys. Union, 33:453-60.

h. F. Press and M. Ewing. Two slow surface waves across North America. Bull. Seismol. Soc. Am., 43:219-28.

i. B. C. Heezen and M. Ewing. Turbidity currents and submarine slumps, and the 1929 Grand Banks earthquake. Am. J. Sci., 250:849-73.

j. M. Ewing and F. Press. Crustal structure and surface wave dispersion. Part II: Solomon Islands earthquake of 29 July 1950. Bull. Seismol. Soc. Am., 42:315-25.

k. Seismic investigations in great ocean depths. P.-V. Assoc. Oceanog. Phys. UGGI, 5:135-36.

1. M. Ewing and F. Press. Propagation of elastic waves in the ocean with reference to microseisms. Pontif. Acad. Sci. Scr. Varia, 12:121-29.

m. M. Ewing and W. L. Donn. Studies of microseisms from selected areas. Pontif. Acad. Sci. Scr. Varia, 12:351-65.

1953 a. M. Ewing and F. Press. Further study of atmospheric pressure

fluctuations recorded on seismographs. Trans. Am. Geophys. Union, 34:95-100.
b. I. Tolstoy, R. S. Edwards, and M. Ewing. Seismic refraction measurements in the Atlantic Ocean (Part 3) . Bull. Seismol. Soc. Am., 43:35-48.
c. M. Ewing and F. Press. Mechanism of T wave propagation. Ann. Geophys., 9:248-49.
d. M. Ewing, B. C. Heezen, D. B. Ericson, J. Northrop, and J. Dorman. Exploration of the northwest Atlantic mid-ocean canyon. Geol. Soc. Am. Bull., 64:865-68.
e. B. C. Heezen, M. Ewing, and E. T. Miller. Trans-Atlantic profile of total magnetic intensity and topography, Dakar to Barbados. Deep-Sea Res., 1:25-33.
f. F. Press and M. Ewing. The ocean as an acoustic system. In: *Symposium on Microseisms,* Publ. Nat. Res. Counc. Wash., D.C., no. 306:109-11.
g. M. Ewing and F. Press. Propagation of earthquake waves along oceanic paths. Bur. Centr. Assoc. Seismol. Int. Trav. Sci. (Ser. A), 18:41-46.
1954 a. M. Ewing, G. H. Sutton, and C. B. Officer. Seismic refraction measurements in the Atlantic Ocean. Part VI: Typical deep stations, North America basin . Bull. Seismol. Soc. Am., 44:21-38.
b. M. Ewing and J. L. Worzel. Gravity anomalies and structure of the West Indies, Part I. Geol. Soc. Am. Bull., 65:165-74.
c. M. Ewing and J. L. Worzel. Gravity anomalies and structure of the West Indies, Part II. Geol. Soc. Am. Bull., 65:195-200.
d. J. Oliver, F. Press, and M. Ewing. Two-dimensional model seismology. Geophysics, 19:202-19.
e. B. Luskin, B. C. Heezen, M. Ewing, and M. Landisman. Precision measurement of ocean depth. Deep-Sea Res., 1:131-40.
f. R. M. Brilliant and M. Ewing. Dispersion of Rayleigh waves across the U.S. Bull. Seismol. Soc. Am., 44:149-58.
g. M. Ewing and F. Press. An investigation of mantle Rayleigh waves. Bull. Seismol. Soc. Am., 44:127-47.
h. B. C. Heezen, D. B. Ericson, and M. Ewing. Further evidence for a turbidity current following the 1929 Grand Banks earthquake. Deep-Sea Res., 1:193-202.

i. C. B. Officer and M. Ewing. Geophysical investigations in the emerged and submerged Atlantic coastal plain. Part VII: Continental shelf, continental slope, and continental rise south of Novia Scotia. Geol. Soc. Am. Bull., 65:653-70.
j. F. Press, J. Oliver, and M. Ewing. Seismic model study of refractions from a layer of finite thickness. Geophysics, 19:388-401.
k. M. Ewing and F. Press. Mantle Rayleigh waves from the Kamchatka earthquake of Nov. 4, 1952. Bull. Seismol. Soc. Am., 44:471-79.
l. W. L. Donn, R. Rommer, F. Press, and M. Ewing. Atmospheric oscillations and related synoptic patterns. Bull. Am. Meteorol. Soc., 35:301-9.
m. M. Ewing, D. B. Ericson, A. W. Bally, and G. Wollin. The deep sea and early man. Quaternaria, 1:17-28.
n. M. Ewing and D. B. Ericson. Exploration of the deep sea floor. Quaternaria, 1:145-68 (extensive summary in Italian, *ibid.,* pp. 193-202).
o. M. Ewing, F. Press, and W. L. Donn. An explanation of the Lake Michigan wave of June 26, 1954. Science, 120:684-86.
p. W. L. Donn, M. Ewing, and F. Press. Performance of resonant seismometers. Geophysics, 19:802-19.
q. B. C. Heezen, M. Ewing, and D. B. Ericson. Reconnaissance survey of the abyssal plain south of Newfoundland. Deep-Sea Res., 2:122-33.
r. A letter to my children. Reader's Digest, Oct.:5-8.
1955 a. M. Ewing and F. Press. Geophysical contrasts between continents and ocean basins. In: *The Crust of the Earth,* ed. A. Poldervaart. Geol. Soc. Am. Spec. Pap., 62:1-6.
b. F. Press and M. Ewing. Earthquake surface waves and crustal structure. In: *The Crust of the Earth,* ed. A. Poldervaart. Geol. Soc. Am. Spec. Pap., 62:51-60.
c. D. B. Ericson, M. Ewing, B. C. Heezen, and G. Wollin. Sediment deposition in the deep Atlantic. In: *The Crust of the Earth,* ed. A. Poldervaart. Geol. Soc. Am. Spec. Pap., 62:205-20.
d. M. Ewing and B. C. Heezen. Puerto Rico trench topographic and geophysical data. In: *The Crust of the Earth,* ed. A. Poldervaart. Geol. Soc. Am. Spec. Pap., 62:255-68.

e. F. Press and M. Ewing. Waves with Pn and Sn velocity at great distances. Proc. Natl. Acad. Sci. USA, 41:24-27.

f. M. Ewing, J. L. Worzel, D. B. Ericson, and B. C. Heezen. Geophysical and geological investigations in the Gulf of Mexico, Part I. Geophysics, 20:1-18.

g. M. Ewing and F. Press. Seismic measurements in ocean basins. J. Mar. Res., 14:417-22.

h. M. Ewing and F. Press. Tide-gauge disturbances from the great eruption of Krakatoa. Trans. Am. Geophys. Union, 36:53-60.

i. J. L. Worzel, G. L. Shurbet, and M. Ewing. Gravity measurements at sea, 1950-51. Trans. Am. Geophys. Union, 36:335-38.

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m. B. C. Heezen and M. Ewing. Orléansville earthquake and turbidity currents. Bull. Am. Assoc. Petrol. Geol., 39:2505-14.

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1956 a. M. Ewing and F. Press. Surface waves and guided waves. In: *Handbuch der Physik,* 47:119-39. Berlin: Springer-Verlag.

b. M. Ewing and F. Press. Seismic prospecting. In: *Handbuch der Physik,* 47:153-68. Berlin: Springer-Verlag.

c. M. Ewing and F. Press. Structure of the Earth's crust. In: *Handbuch der Physik,* 47:246-57. Berlin: Springer-Verlag.

d. G. L. Shurbet and M. Ewing. Gravity reconnaissance survey of Puerto Rico. Geol. Soc. Am. Bull., 67:511-34.

e. E. T. Miller and M. Ewing. Geomagnetic measurements in the Gulf of Mexico and in the vicinity of Caryn Peak. Geophysics, 21:406-32.

f. M. Ewing and F. Press. Rayleigh wave dispersion in the period range 10 to 500 seconds. Trans. Am. Geophys. Union, 37:213-15.

g. M. Ewing and W. L. Donn. A theory of ice ages. Science, 123: 1061-66.
h. F. Press, M. Ewing, and J. Oliver. Crustal structure and surface-wave dispersion in Africa. Bull. Seismol. Soc. Am., 46: 97-103.
i. W. L. Donn and M. Ewing. Stokes' edge waves in Lake Michigan. Science, 124:1238-42.
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k. M. Ewing and B. C. Heezen. Some problems of Antarctic submarine geology. In: *Antarctica in the International Geophysical Year,* ed. A. P. Crary et al., Geophys. Monogr. no. 1, 75-81. Wash., D.C.: American Geophysical Union.
l. D. H. Shurbet and M. Ewing. Microseisms with periods of seven to ten seconds recorded at Bermuda. Trans. Am. Geophys. Union, 37:619-27.
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1957 a. M. Ewing, W. S. Jardetzky, and F. Press. *Elastic Waves in Layered Media.* N.Y.: McGraw Hill. 380 pp.
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A. Irving Hallowell

Alfred Irving Hallowell

December 28, 1892-October 10, 1974

by Anthony F. C. Wallace

In the years immediately following World War II, the University of Pennsylvania committed itself to the expansion of its Department of Anthropology. At the war's end, Frank G. Speck remained as the sole senior professor, aided on a part-time basis by graduate student instructors and sundry curators from the University Museum. Speck called back his former student, Loren Eiseley, from Oberlin, as chairman, and Eiseley and Speck together persuaded their former colleague, A. Irving Hallowell, to return from Northwestern. Speck, Eiseley, and Hallowell then set out to create what has become one of the country's major departments of anthropology.

The few graduate students who were in residence at the time of Hallowell's arrival in 1948 knew him primarily as one of the founders of the new field of "culture and personality." He was particularly noted for his use of the Rorschach, or ink-blot, test to assess the personality structures of American Indian populations. This innovation in the use of projective techniques made him something of a controversial figure, for many anthropologists—including his own mentor, Speck—were not especially in favor of the kind of clinical approach to the study of human society that the use of such tools as the Rorschach seemed to imply. But as we came to know him as a teacher and advisor, we students realized that "the Rorschach" was only a single aspect of Hallowell's extraordinarily rich mode of approach to the study of man. He brought to bear on his chosen subjects—the Ojibwa Indians of the United States and Canada—not only the concepts and tools of clinical psychology, but also the traditional ethnographic and linguistic skills he had learned from Speck and other teachers in the school of Franz Boas, the techniques of functional analysis that were being introduced by the social anthropologists, and a trained capacity for historical and scholarly analysis. This variety of intellectual resources made his explorations of Ojibwa society at once precisely descriptive and richly evocative models for emulation by others working in other communities. Hallowell was, indeed, one of the principal figures in the development of modern ethnography, which is distinguished by its effort to combine detailed description in standardized categories of overt observable behavior (the "etic" approach) with careful attention to the need to infer the more-or-less covert cognitive and emotional structures of the people being observed (the "emic" approach).

EDUCATION

The diversity of professional abilities that Hallowell brought to bear in teaching and research was partly owing to an eclectic sort of educational career. His parents, Edgar Lloyd and Dorothy Edsall Hallowell, were (according to Hallowell) of a "conservative" inclination, and perceiving in their son "no outstanding talents" or "professional" interest, sent him to a three-year manual training high school and then to college at the Wharton School of Finance and Commerce at the University of Pennsylvania. The Wharton School at that time, before World War I, offered a broad curriculum not only in business courses but also in the social sciences, and furthermore required its students to take liberal arts courses outside the school itself. So Hallowell studied, in addition to technical subjects necessary to a business education, all the courses in economics and sociology that were offered and sampled such topics as chemistry, history, English literature, and Italian Renaissance painting. This exposure to the liberal arts and to the atmosphere of social reform fed the fires of rebellion against conservative family values and cultivated what he later characterized as his "socialistic inclinations." Plans to enter upon a business career were laid aside, and unable to find funds to finance a graduate education in sociology, Hallowell went into social work as a caseworker for the Family Society. This experience brought him into contact with poverty and into the houses of unfamiliar ethnic groups, black and white.

During his social work years Hallowell continued to take courses in sociology. He was also exposed to the new ideas of psychoanalysis through the lectures of the anthropologist Alexander Goldenweiser at the Pennsylvania School of Social Work. And he took some courses with an old friend and fellow fraternity member from undergraduate days, Frank G. Speck, who was now teaching anthropology at the University of Pennsylvania. Speck's lectures opened his eyes to a wide vista of cultures, "far beyond the ethnic groups in my own back yard," and he decided to leave social work for anthropology. He took an M.A. in anthropology in 1920 and a Ph.D. in 1924. He entered upon the stage as a full-fledged follower of the school of Franz Boas, who had briefly taught both Speck and Hallowell in his seminar at Columbia and whose abstract conception of anthropology as the Science of Man in all his aspects, physical, psychological, linguistic, and cultural seemed to provide that broad base that was required to transcend provincial American culture and address the basic problems of social reform.

PROFESSIONAL DEVELOPMENT AND CONTRIBUTIONS

Hallowell's doctoral dissertation was "Bear Ceremonialism in the Northern Hemisphere" and was published as a whole issue of the *American Anthropologist* in 1926. This work brought together data from northern Europe, Asia, and North America to reveal the existence of a complex system of beliefs and ceremonies about the bear which were, in varying local expressions, almost universally practiced among primitive peoples in the circumboreal culture area. He also drew attention to archaeological remains from the paleolithic which indicated an extraordinary antiquity, on the order of tens of thousands of years, for this widely-diffused culture complex. The work remains a classic contribution to culture history.

But Hallowell was not satisfied with the role of comparative ethnologist, which would require more work in the library than in the field, and so after some casting about, in the late 1920's he began that series of studies of northern Algonkian life and culture which he was to continue for the remainder of his professional career. His works on the Abenaki of Quebec, the Montagnais-Naskapi of Labrador, and particularly the Saulteaux or Ojibwa of the Lake Winnipeg region were significant not merely in providing a rich and intimate portrait of one of the few remaining hunting-and-gathering cultures in North America, but also because the Ojibwa papers and monographs revealed his theoretical and methodological innovations which, because they could be applied in any ethnographic setting, were of general interest to anthropologists. One of the tragedies of his professional life was the loss in the mails of the only copy of the final summary of the Ojibwa ethnology which Hallowell wrote during his emeritus years and which his deteriorating health prevented him from doing over again.

The Ojibwa series (which followed one paper on the Abenaki and one on the Montagnais-Naskapi) began in 1934 and by the time of Hallowell's death amounted to about forty individual papers, articles, chapters, and one monograph (*The Role of Conjuring in Saulteaux Society,* 1942). The works cover virtually all aspects of Ojibwa culture—kinship and social organization, economics and technology, ecological relationships (particularly as they affected land tenure), social control, values and morality, medicine, religion, folklore, temporal and spatial orientation, dreams, sexual behavior—and deal in addition with factors of personality structure, mental health, and culture change. Taken together, they constitute one of the most complete recordings of the changing way of life of a hunting-and-gathering population that is available in the ethnographic record.

The theoretical and methodological issues addressed in the Ojibwa series shifted, over the years, from strictly ethnological matters to those involving psychological considerations. The initial stimulus was a classic—but conventional—question concerning the relation between kinship terminology and rules of marriage. The older evolutionary theorists of the nineteenth century had postulated a close connection between the two, but more recent opinion, as advanced by Boas and his students, questioned the tightness of the coupling and suggested that changes in kin terms were linguistic rather than sociological phenomena. After discovering evidence in some old dictionaries to suggest that the northern Algonkians might in fact once have practiced cross-cousin marriage (as their surviving cousin terminology would have suggested to an evolutionist), Hallowell read a paper asking, "Was Cross-Cousin Marriage Practiced by the North-Central Algonkian?" This paper received no support and a very critical appraisal. But learning that another ethnologist had recently found Naskapi men actually marrying their mother's brother's daughters, Hallowell was spurred to visit a related Algonkian group, the Saulteaux (Ojibwa) of the Lake Winnipeg region to learn for himself. In his brief autobiography (1972), Hallowell recorded one of the classic native answers to a field worker's apparently naive question:

> I well remember an early conversation with William Berens, my closest collaborator. I hesitatingly asked him whether a man could marry a woman he called *ninam* (female cross-cousin). His reply was, "Who the hell else would he marry?"

And throughout the series occur reports on material culture, the size of hunting territories as a function of ecological adjustment, the role of conjuring and the decline of native ceremonies, folktales, and various other necessary, if conventional, parts of standard ethnography.

But increasingly the topics dealt with psychological questions. Hallowell's interests in this area included, but extended beyond, the field of "personality and culture," which concerns itself primarily with the relation between the motivational structure of individuals, couched more or less in the language of psychoanalysis and clinical psychology, and the culture of their group as the ethnographer describes it. In his view, the entire field of psychology was potentially relevant to the concerns of anthropology, and so he was eager to take advantage of the findings and methods of learning theory, of gestalt psychology, of the test-and-measurement field, and of the newer work in perception which emphasized the importance of the social and cultural characteristics of the perceiver in determining what is perceived and known. His writings on the phenomenology of perception of space and time among the Ojibwa were read and cited frequently by psychologists who were eager for confirmation of their findings in cross-cultural research. In a very real sense, Hallowell completed one of the tasks which Boas, with great prescience, had fore seen as theoretically central not only for anthropology but also for science as a whole. The early physicist Boas, trained in psychophysics to study how the observer's characteristics determine his perception of experimental phenomena, had generalized this Kantian view of epistemology to include a concern with the way in which the "genius of a people" determines their perception of the material world, of the cultural repertoire presented to them for acceptance by their neighbors, and even of themselves. In his work on the cultural determinants of perception, Hallowell thus carried forward the investigation of one of the great problems of epistemology and of the philosophy of science.

With his abiding interest in the subject of perception, it is perhaps not surprising that Hallowell should turn to tests of perception—the so-called projective techniques—and particularly to the Rorschach test as his favored technique of assessing individual Ojibwa personalities. He collected a series of 266 Rorschach records from various Ojibwa communities, and although he never prepared an over-all summary of the results in the form of a sketch of typical Ojibwa personality structures, he used the data in a number of papers, including both those descriptive of Ojibwa cases and those explicating the use of the Rorschach test in cross-cultural research. He encouraged his students to use the Rorschach for comparative studies; my own dissertation research involving the use of Rorschach protocols was conducted under his guidance.

Undoubtedly the best known and most controversial of Hallowell's works on Ojibwa personality were those in which he described an aboriginal personality type—an isolated, tightly controlled, atomistic individual well adapted to the hunter's life—failing to change, except pathologically, under the stress of acculturation, particularly in reservation circumstances. This notion of "psychological lag" is, in a formal way, not unlike the so-called "doctrine of survivals," which in more sophisticated form is sometimes invoked in analyzing the functional relations among changes in kinship terminology, marriage rule, and rules of residence and descent. Critical attacks on the idea of the aboriginality of the family hunting territory among the northern Algonkians (which Speck had originated and to which he had contributed) and a wholesale assault on the image of the northern hunters as "atomistic," resting in part on the claim that his views were based on a refusal to accept the Marx/Engels theory of cultural evolution, left him unmoved.

In his mature years, Hallowell developed further his ideas about the nature of the human personality and began to construct a theory of psychological evolution. Invoking again the theme of the self as perceiver, he posed the problem: at what point in human cultural evolution did man become an object to himself? Such a transformation he viewed as crucial, for only with this perceptual reflexivity is a moral, and therefore human, social order conceivable. Anthropology itself he finally came to view as one more step in the long evolutionary process of man becoming aware of man.

POSITIONS, SERVICES, AND HONORS

Hallowell's initial academic appointment was as instructor in anthropology at the University of Pennsylvania from 1923 to 1928. Successive promotions followed; he became full professor in 1939. Thereafter, apart from the years spent at Northwestern from 1944 to 1947, Hallowell remained in residence as Professor of Anthropology at the University of Pennsylvania until his retirement in 1963. And even after that he maintained a busy office in the department, where he conducted business for the National Academy of Sciences and counseled students and colleagues. During his emeritus years he was sought after as a teacher on a number of campuses, including Wisconsin and Chatham College, and formed a particularly strong connection at Bryn Mawr, where he taught regularly and helped to supervise dissertations until a few years before his death.

He also served various institutions in other capacities. At the University of Pennsylvania he held the positions of Curator of Social Anthropology in the University Museum and of Professor of Anthropology in Psychiatry in the Medical School. He served as Chairman of the Division of Anthropology and Psychology in the National Research Council from 1946 to 1949, as President of the American Anthropological Association in 1949, and as President of the American Folklore Society and the Society for Personality Assessment. He edited the Wenner-Gren Foundation's monographs series, the Viking Fund Publications in Anthropology, from 1950 to 1956.

Among his honors and awards may be mentioned his election to the National Academy of Sciences in 1961 and to the American Philosophical Society in 1963. He was awarded a Guggenheim Fellowship in 1940, received the Viking Medal for outstanding achievement in anthropology in 1956, and was accorded an honorary Doctor of Science degree from the University of Pennsylvania on his retirement in 1963. In 1965 a Festschrift was published in his honor, edited by Melford Spiro and entitled *Context and Meaning in Cultural Anthropology.*

PERSONAL STYLE

During the years when I knew him as student and colleague, Hallowell lived in a comfortable old frame house in a woodsy suburb of Philadelphia. There he and his wife, Maude, on occasion entertained students, faculty, and visitors to the area at small gatherings where the talk revolved around personality structure and its assessment, psychocultural evolution, and other psychologically oriented aspects of anthropology. Hallowell was enthusiastic in conversation and encouraged students to argue and debate him. On occasion he could be testy, however, and it was said by awed graduate students that he invariably took a negative position to any new proposal submitted to his judgment but that he generally worked his way around to approval of it two days later. In lecturing, as in writing, Hallowell liked to surround the points he made in clear academic prose with a thicket of allusions to the literature, so that lecture notes and published papers alike bristled with footnotes and bibliographical asides. The style of all this was, however, more sprightly than pedantic, and in personal conversation the apparatus of scholarship was replaced by a fund of humorous but illustrative anecdotes. Although he set high standards of scholarship for himself and his students, he regarded the machinery of examinations and dissertations more as a developmental process than as a series of hurdles to exclude the unworthy. I well recall his remark after I had completed my dissertation (under his supervision): "I'm going to tell you what Frank told me when I finished *my* dissertation. Now that you've got that out of the way, you can get to work."

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Herbert Spencer Harned

December 2, 1888—July 29, 1969

by Julian M. Sturtevant

Herbert Spencer Harned started graduate work in chemistry at the University of Pennsylvania shortly after the introduction into this country of the theoretical and mathematical approach to chemical problems embodied in physical chemistry. His doctoral research was in preparative inorganic chemistry under the direction of Edgar F. Smith, but before he started this work, he spent a brief period in the laboratory of Joel H. Hildebrand. This apprenticeship with Hildebrand appears to have played a major role in steering his interests away from classical chemistry, toward the newer physics-oriented discipline.

Hildebrand, after obtaining his Ph.D. at Pennsylvania in 1906, had studied with Walther Nernst in Berlin, and after returning to Philadelphia, had instituted a program of research and instruction in physical chemistry. In Hildebrand's laboratory, Harned worked on a titrimetric method for the determination of magnesia in limestone, and a joint publication, Harned's first, resulted from this work in 1912. This research involved the use of the hydrogen electrode, with measurements of potential to an accuracy of only 10^{-2} volt. Harned was confident that measurements with this electrode could be carried to much better levels of accuracy, and an important part of his later scientific work consisted in show ing that the hydrogen electrode could be utilized in a wide variety of electrochemical cells having potentials stable and reproducible to 10^{-4}, or even 10^{-5} volt. This drive toward experimental perfection and accuracy characterized Harned's long-continued studies of electrolytic solutions. His privately stated aim was to obtain thermodynamic data that could stand unchallenged for decades, and he certainly achieved this aim in abundant measure.

Herbie, as he was always known to his countless friends, was born on December 2, 1888, in Camden, New Jersey, the son of Augusta Anna Traubel Harned and Thomas Biggs Harned. Herbie was the youngest child in the family, with a sister, Anna, ten years older than he, and a brother, Thomas, six years his senior.

Herbie's mother and father, as well as other Harned's, had been very close to the poet Walt Whitman. Herbie's mother was hostess at the Whitman dinners which were held every Sunday night in the Harned home. She was the one who held the Whitman coterie together, and the hostess who sat next to the poet at his seventieth birthday party. Although Herbie was only four when Whitman died, and one of his few remembrances of Whitman was his being frightened by the poet's beard, he was always very proud of his parents' intimacy with the poet. Herbie's lifelong interest in literature was doubtless in large part due to the influence of parents and relatives who were intimate with literary figures such as Whitman.

When Herbie was five years old, the family moved to Germantown, an outlying section of Philadelphia. His father established a successful law business in downtown Philadelphia. The Germantown household was a lively place, with a succession of dinner parties and dances. Although Herbie, as the youngest member of the family, was for many years only a spectator of these events, the constant presence of guests, many of them distinguished individuals, made a deep impression on him.

Herbie was a boy of slight build, but under the tutelage of his brother Tom he developed early a knack for sports requiring sharp eyes and good coordination—to the point where he became a match for boys of larger physique, especially in tennis and cricket. The family lived close to the Germantown Cricket Club, where excellent instruction and facilities in a wide variety of sports were available. Among Herbie's playmates in Germantown were several boys, including William Tilden, who later became outstanding athletes. Herbie was fond of telling how he defeated Bill Tilden at tennis when he was fourteen years old, and Bill was eleven. At about this time, Herbie began to show a real talent for cricket, and he devoted much time to the sport until he was twenty-six. He played on various teams, in prep school and in college, and on first-class amateur teams organized at the Germantown Cricket Club. He played against numerous American teams, as well as against outstanding English, Canadian, Australian, and Bermudan teams, both in this country and abroad. Once in a match in Bermuda, he batted an entire inning, making 113 runs not out, his greatest achievement in the sport. Herbie felt that cricket taught him a great deal that carried over into his professional life. It emphasized fair play and good sportsmanship; it showed him that by hard practice with careful attention to form, he could overcome the disadvantage of his relatively small stature; and it required playing not only on the team, but also for the team.

Herbie's father became seriously ill in 1910, and never fully recovered his health, with the result that the family circumstances became very straightened. It is a good indication of the calibre of this man that although he was going heavily into debt to keep his family together, he nevertheless donated his very valuable collection of "Whitmania" to the Library of Congress. In 1914, Herbie's mother, to whom Herbie was most deeply attached, died of cancer. The home in Germantown and other properties were sold to retire debts, and the family moved to an apartment in Germantown. Since Herbie's elder brother had gone to Chicago, the responsibility of maintaining the family fell primarily on Herbie. It was this responsibility, according to his own account, that stimulated him to adopt a very serious and determined approach to his preparation for a professional life.

In the fall of 1915, Herbie met Dorothy Foltz of Chestnut Hill. A year later they became engaged, and they were married on September 8, 1917. Dorothy, who survives Herbie, proved to be the ideal wife for a young man deeply involved in establishing a scientific career, with the long hours of extra-familial activity involved in that pursuit. Herbie's father, by then nearly seventy, lived with the young couple, and a close relationship soon developed between him and his daughter-in-law.

Herbie became a captain in the Chemical Warfare Service in June 1918. After two months at the CWS establishment at the American University in Washington, during which he wrote a long report on phosgene, he was sent to France. A period of field training was followed by duty at the central research laboratory of the American Expeditionary Force near Paris. A number of lasting friendships were made with chemists stationed there. A study of the kinetics of adsorption of gases on charcoal which Harned started at this laboratory was completed after his return to the States. This was a pioneering effort in this field and has been frequently cited.

Herbie's father died in September 1921. A very close father-son relation had developed, most particularly since Herbie's return from France, and this was another keenly felt tragedy.

To return now to Harned's educational and professional careers, it is evident that he received much preschool training at home. He has particularly singled out as of incalculable value to him the instruction his mother gave him in arithmetic before he went to school at the age of nearly seven. His success in learning arithmetic gave him confidence in understanding any kind of school work. After three years at a small school run by two Misses Knight, he was sent to the Penn Charter School, an excellent Quaker preparatory school in Philadelphia, where his course was strictly classical, with no hint of science. He has stated that his teachers there were all excellent, and that after his strict training there, he found his college courses to be quite easy.

In 1905 Herbie entered the University of Pennsylvania, where in his freshman year, although he continued with his classical studies including Greek and Latin, he had his first contact with science. He took the course in chemistry and was immensely impressed by the accuracy of the measurements which fixed the composition of air and water—to the extent that he decided to pursue a career in science. By the time he became an upperclassman, his two major interests were chemistry and literature, the latter from an entirely nonprofessional point of view.

Harned graduated from college in 1909, and, as noted above, stayed on at Pennsylvania for graduate work in chemistry. Three of his teachers in graduate school he has singled out as having had an especially great influence on him. Two of these, Edgar F. Smith and Joel H. Hildebrand, have already been mentioned. The third was a philosopher, Edgar Singer. His course on the history of modern philosophy was considered by Harned to be the best seminar course he ever had, and led him to take several additional courses with Singer. His experience in these courses gave him a viewpoint which significantly motivated his later professional career:

search for the most fundamental quantity you can find and then measure it with the highest accuracy you can achieve. Some years later he discovered this quantity, the chemical potential, in the work of Josiah Willard Gibbs, and the major portion of his research career involved the accurate measurement of this quantity.

In summarizing his studies in philosophy, Harned wrote that after being tossed this way and that by the conflicting views of nineteenth century philosophical thought, there was only one mode of thought and action to which he could subscribe. This was the quantitative method of science, as exemplified in the work of Copernicus, Galileo, Kepler and Newton. There was beautifully illustrated here the importance not only of fundamental laws and theories, but also of accurate observations and measurements.

Harned obtained his Ph.D. in 1913. In that same year, Hildebrand left Pennsylvania to join the faculty at the University of California at Berkeley, and Harned was made an instructor and head of the Physical Chemistry Division at Pennsylvania. As was more frequently the case in those days than now, he was saddled with an extremely heavy load of undergraduate and graduate teaching. There were approximately forty students in his undergraduate course in physical chemistry, and since the laboratory only accommodated ten students, he had to divide the group into four sections, each of which spent many hours a week in the laboratory. All of this, combined with graduate lectures, constituted a tough assignment, carried with far less help from teaching assistants than is customary today.

It is a clear measure of the strength of Harned's dedication to research that within two years, despite these formidable teaching duties, and working entirely on his own, he was able to publish a pioneering twenty-two page paper on the precise ($\pm 10^{-4}$ volt) utilization of the hydrogen and calomel electrodes in the determination of the activities of hydrogen and hydroxide ions in neutral salt solutions. This paper, in which he showed without any doubt that the law of mass action was not applicable for calculating ionic equilibria in solutions of strong electrolytes, attracted much attention and spurred visits by chemists from other universities to his laboratory. All of this served to bolster his self confidence at the threshold of his career, and to confirm his belief that he had initiated an important program of research.

It was evident to Harned that a definitive interpretation of the results obtained in this first work was hampered by the presence of small but unknown liquid junction potentials. In the following year, 1916, he published his first paper on electrolyte activities determined using cells without liquid junction, and he continued using such cells, in a steadily expanding diversity of applications, over the next forty-odd years.

Harned wished to avoid too narrow a specialization at this period in his career. His papers on conductimetric titrations (1917 and 1918) constituted the first utilizations in this country of a conductance bridge in chemical analyses. Also in 1918, just before entering the army, he published his first paper in the field of reaction kinetics in solution. He returned briefly to reaction kinetics several times in later years, but as one may infer from remarks he made, not as frequently as he would have liked.

Harned's remaining years at Pennsylvania became increasingly productive, and by the time he left there to go to Yale University in the fall of 1928, he had published thirty-four papers. After a long period of working alone, he finally began to have collaborators, both graduate students and people of more advanced standing. In 1924 Gösta Åkerlöf came from Sweden to his laboratory on the recommendation of Svante Arrhenius. He remained in close association with

Harned for more than twenty years at Pennsylvania and Yale, although he worked essentially independently after receiving his Ph.D. In 1927 Robert A. Robinson came as a Commonwealth Fellow from Birmingham, England. He and Harned maintained throughout the rest of Harned's career a close relation that culminated in the joint publication of a monograph on multicomponent electrolyte solutions in 1968.

During these years, he made systematic measurements of the activity coefficients of strong acids and bases, in both dilute and concentrated solutions, in the presence of neutral salts. He discovered useful regularities in these systems, one of which has come to be known as Harned's rule. This states that in solutions of constant total ionic strength, the logarithm of the activity coefficient of one solute is directly proportional to the concentration of the other. This was to be a matter of continuing interest to him, and three of his last papers, published between 1959 and 1963, are concerned with the effect of temperature on such systems. It should be added that Harned was well aware that this rule is not universal, and that caution must be exercised in its application.

In this period he initiated his work on the thermodynamics of electrolytes in mixed solvents with a study of hydrochloric acid in water-ethanol mixtures. This work was greatly extended in later years, culminating in a series of papers, published from 1936 to 1939, concerning hydrochloric acid in water-dioxane mixtures containing as much as 82 weight percent dioxane (dielectric constant about 10 at 25°).

Harned has written that he regarded the year 1927-28, his last year at Pennsylvania, as the most fruitful one in his scientific life. A graduate student, John M. Harris, Åkerlöf, Robinson, and he worked jointly on four separate topics: the use of amalgam electrodes in studying the thermodynamics of solutions of electrolytes; the thermodynamics of solutions of mixtures of electrolytes at high concentrations; the first application of cells without liquid junction to determine the ionization constants of weak electrolytes; and the investigation of neutral salt effects in homogeneous catalysis. During this year, ideas and methods were developed which were later widely employed not only in his laboratory, but in many others around the world.

Although his research was going very well, Harned decided to accept an offer from Yale. He felt that this move would significantly expand his research opportunities. There was available in the Sterling Chemistry Laboratory, then only five years old, what seemed like almost unlimited space for his laboratories; he was assured of initial financial support in his research which quite surpassed that to which he was accustomed; and it seemed probable that he could expect to have a good group of graduate students as colleagues.

Immediately on arrival at Yale, Harned, in harmonious cooperation with the other physical chemists on the staff, carried through revision of the graduate program in physical chemistry. He was determined that the orientation of this program should be exclusively toward pure research, with a firm basis in the mathematical and theoretical aspects of the subject. He eliminated everything in the nature of conventional undergraduate courses from the graduate program.

Harned's arrival at Yale coincided with the start of a vigorous university-wide expansion in plant and program. He has written very approvingly of what the president, James Rowland Angell, and the graduate dean, Wilbur Cross, accomplished for the university, particularly in bringing about a remarkable upgrading of the graduate school.

I shall take the liberty of inserting here some personal comments. I went to Yale as a graduate student in chemistry in 1927, and joined the staff in 1931. I therefore had frequent contact with Herbie until his retirement in 1957. There is no doubt that, despite the general growth of the University referred to above, his own initially promising situation at Yale began to deteriorate a few years after he arrived there as a result of decreasing financial and administrative support for his own research program and for physical chemistry in general. The Chemistry Department entered a period of decline relative to chemistry departments at other institutions and to other science departments at Yale. Fortunately, a goodly stream of graduate students continued in physical chemistry, most of whom worked with Harned. It is greatly to his credit that despite the difficulties inherent in the situation, he remained a very productive scientist.

This period of relative quiescence of the Chemistry Department was enlivened by a few very important events. The first of these Harned considered to be the single most important thing he accomplished at Yale. In 1931 he received a letter from Lars Onsager stating that due to financial exigencies, he was losing his post at Brown University. Within hours Harned had arranged the offer of a Sterling Fellowship to Onsager which, most fortunately for Yale, was accepted. Needless to say, this fellowship was soon converted to a permanent position on the faculty.

In 1945, largely through Harned's efforts, Raymond M. Fuoss was persuaded to leave the central research laboratory at General Electric to join the Yale faculty. His addition to the staff made certain the preeminence of Yale in the physical chemistry of electrolytes.

In 1951, after the retirement of Arthur J. Hill as chairman of the Chemistry Department, the university was most fortunate in persuading John G. Kirkwood to come from the California Institute of Technology to serve as chairman. There ensued a period of very healthy development of chemistry at Yale which markedly improved the atmosphere for Harned's last few years before retirement.

Shortly after setting up his laboratories at Yale, Harned, in collaboration with Benton B. Owen, undertook an extension of his earlier work on cells containing weak acids and bases, and developed a highly precise method for determining the dissociation constants of such substances. This method has been widely employed by many workers in other laboratories, particularly for obtaining dissociation data for weak electrolytes and ampholytes of interest in biochemistry. With various colleagues, notably Robert W. Ehlers, the method was adapted for measurements over a wide range of temperatures. This involved as the first step a careful determination of the standard potential of the silver-silver chloride electrode over the temperature range 0-60°C. The cell used in this work, composed of a hydrogen electrode, a silver-silver chloride electrode, and a solution containing a weak acid (in the Brönsted sense) in protonated and unprotonated forms, together with an appropriate neutral salt, has come to to known as the Harned-Ehlers cell. This cell has found important additional application in the establishment of a practical pH scale by Roger G. Bates and others, and it has been shown in several of Harned's papers to be well suited for the determination of dissociation constants in mixtures of organic solvents with water.

With the potential of the silver-silver chloride electrode well established over a wide temperature range, it became possible to use precise electromotive force measurements to determine the dissociation constant of water, also over a wide temperature range, and from these data to evaluate the enthalpy of dissociation of water. The cell measurements gave the value of 13.52 kcal mol^{-1} at 25°C, whereas several concordant direct calorimetric measurements gave 13.35 kcal mol^{-1}. The difference in these values, which is well above the apparent accuracy of the two methods, remains unexplained. It is an indication of the objectivity of Harned's approach to his research that, in view of the fact that enthalpy values are obtained directly by the calorimetric method but only indirectly by differentation with respect to temperature from electromotive force data, he was prepared to accept the calorimetric value in a situation such as this.

In 1932 Harned decided to write a comprehensive treatise on electrolytes, and asked Benton Owen, his first graduate student at Yale, to be his coauthor. Harned has emphasized in his "Memoirs"* that this was a most felicitous choice; his and Owens approaches and talents complemented each other well, and the coauthorship produced a better work than either of them could possibly have achieved individually. The preparation of this book, which came to be known as the "Opus" around the laboratory, proved to be a very difficult task, and it was not until nine years later that a lengthy manuscript was finally completed. *The Physical Chemistry of Electrolytic Solutions* was published late in 1942 in the American Chemical Society Monograph Series, and was immediately accepted as the standard treatise in the field. The "Opus" appeared in a second edition in 1950, and a third edition in 1958, with an increase in length from 612 pages to 803 pages; these new editions were obviously more than routine revisions of the original.

In January 1942, Harned was named official investigator for a project on isotope separation supported by the Office of Scientific Research and Development. His assignment was to investigate the possibility of separating uranium isotopes by an electrophoretic procedure. Most of the physical chemists in the department were drafted into this enterprise, a large area of the laboratory was walled off and blacked out to meet security requirements, and a round-the-clock seven-days-a week program was instituted with a large number of moving boundary cells under constant surveillance. Unfortunately, it

* Unpublished three-volume set.

eventually turned out that this method was impracticable. The group then investigated one or two other methods of separation which also turned out to be unsuccessful. The final task assigned the group was to devise a procedure by which very pure uranyl nitrate could be obtained from ore from the Belgian Congo. In this effort, success was achieved and the process was carried to pilot plant scale. All of this work was carried out under circumstances involving heavier than usual teaching loads because of the presence on campus of a Navy V12 educational program, and the continuation of graduate instruction and research in physical chemistry.

Shortly after the conclusion of this war work, Harned developed an ingenious conductimetric method for the determination of the diffusion coefficients of electrolytes. The first report on this method was published in 1945; from then until 1958 some nineteen papers detailed its broad application. This method gave sufficiently precise results to lead to the first quantitative experimental verification of the limiting Nernst equation relating diffusion coefficients to ionic conductances. Of even greater importance, striking support for the Onsager-Fuoss theory of conductivity was obtained, in particular with respect to the magnitude and sign of the electrophoretic term involved in this treatment.

Harned was closely associated with several important research efforts which do not appear in publications carrying his name. He persuaded Andrew Patterson to try to verify Onsager's theory of the Wien effect. In a classic experiment, Patterson and J. A. Gledhill established the validity of the theory, which relates the change in conductivity of electrolyte solutions to changing field strengths. Their method utilized pulsed fields, and in the case of weak electrolytes permitted the measurement of the rate of recombination of ions at the termination of the pulses, thus initiating the study of chemical reaction rates on a hitherto inaccessible time scale.

Another example of his wide influence is the following. L. G. Gosting was interested in investigating the equivalence of the Onsager coefficients L_{ij} and L_{ji} in multicomponent diffusion. Harned pointed out to Gosting that the information concerning the variation of ionic activity coefficients that is needed in obtaining the Onsager coefficients from diffusion coefficients is readily available on the basis of Harned's rule if the diffusion experiments are carried out at constant total molarity. Gosting's elegant work was the basis for the definitive test of the Onsager principle in diffusive processes.

From time to time during his career, Harned published review papers which summarized progress in fields to which he had contributed and discussed the the significance of his results and those of other workers. This very useful type of publication saw its greatest development in the "Opus" mentioned earlier.

In the course of his thesis work at Pennsylvania, Harned prepared the mixed chloride of niobium $(Nb_6Cl_{12})Cl_2 \cdot 7H_2O$ and showed that the complex ion $(Nb_6Cl_{12})^{++}$ carries two positive charges. This synthesis was published in 1913. Many years later, Linus Pauling saw this publication and became interested in the structure of the ion; he also thought that it might be useful as a heavy metal replacement in a protein single crystal for X-ray crystallographic study. He and his colleagues apparently had difficulty in preparing the compound in amounts large enough for the structural and protein studies, and in 1956 Pauling invited Harned to come to the California Institute of Technology to repeat the synthesis. At this time, Harned found that by using elevated temperatures and cadmium in place of sodium amalgam as reducing agent he could increase the yield from 6 percent to 60 percent.

Harned served as a consultant at the Oak Ridge National Laboratory for the period from 1950 to 1965. He felt that his long association with that laboratory was very profitable to him as well as to the people there with whom he consulted.

Yale had at that time a policy of mandatory retirement at the end of the academic year during which one reaches the age of sixty-eight, and therefore Harned had to retire on June 30, 1957. He published sixteen papers after that, including one giving a second important Harned's rule concerning activity coefficients of electrolytes in mixed solvents. He also published a monograph (with R. A. Robinson), and remained active in a consultative capacity not only at Oak Ridge, as mentioned above, but also with an Atomic Energy Commission contract at Yale.

Professor Harned's long and active life came to an end on July 29, 1969, at the age of nearly eighty-one. He is survived by his wife Dorothy and three daughters and a son.

I am greatly indebted to Mrs. Herbert S. Harned (Dorothy) for the opportunity to study various documents of Professor Harned's, in particular his three-volume handwritten "Memoirs of H. S. Harned." The task of preparing this biographical sketch was immeasurably facilitated by the availability of these memoirs.

Professor R. A. Robinson, of the University of Newcastle upon Tyne, delivered the opening address at a session dedicated to Harned at a meeting of the American Chemical Society in Chicago in 1970. Professor Robinson very kindly made available to me the manuscript of this unpublished address, and I have made much use of it.

I have received important help in the preparation of this sketch from several colleagues, including Philip A. Lyons, Andrew Patterson and Arthur M. Ross.

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Walter Abraham Jacobs

December 24, 1883-July 12, 1967

by Robert C. Elderfield

Walter Abraham Jacobs died in Los Angeles after a long and impressive career. He left behind some 273 publications which record important contributions in the field of the chemistry of natural products of biological importance, as well as the development of chemotherapeutic agents—the significance of which was not recognized until some years had passed.

Jacobs was born in New York City on December 24, 1883 and attended local elementary schools. He received an A.B. degree in 1904 and an A.M. in 1905 from Columbia University, after which he enrolled at the University of Berlin for study under Emil Fischer, earning a Ph.D. degree in 1907.

On his return to New York, Jacobs received an appointment as a fellow in chemistry in the laboratory of Phoebus A. Levene at the newly established Rockefeller Institute for Medical Research. In 1908 he became an assistant, and in 1910 an associate in Levene's laboratory. During these years with Levene, he was closely associated with the latter's work on the chemistry of the nucleic acids.

The studies, first with the nucleotide inosinic acid from beef extract, disclosed its essential chemistry by hydrolysis to the nucleoside, inosine, and by subsequent cleavage from the latter of its crystalline sugar component which was identified as D-ribose. This became the pattern for similar studies with the nucleotide guanylic acid, and with yeast nucleic acid (ribonucleic acid), from which the various nucleosides were then isolated and interpreted. The attempted extension of this procedure to similar studies with thymus nucleic acid (subsequently shown to be desoxyribosenucleic acid) was interrupted by Jacobs' promotion in 1912 to associate member of the Institute with independent status.

Dr. Simon Flexner, Director of the Institute, felt that the developing field of chemotherapy warranted a division of its own, and Jacobs was placed in charge of it. In collaboration with Michael Heidelberger, he began an investigation of the possible chemotherapy of polio. It was known that hexamethylenetetramine apparently exerted a slight therapeutic effect, and an extended series of quaternary salts was prepared by reaction with aromatic and alipathic halogen compounds. Some of the salts displayed bactericidal properties, and a few appeared to prolong the life of polio-infected monkeys. Unfortunately, this was due to a loss of virulence of the virus strain.

After the disappointing outcome of the chemotherapeutic studies concerning polio, the Jacobs-Heidelberger team turned its attention to African sleeping sickness, for which no effective and non-toxic drugs were available. Ehrlich had produced a powerful synthetic agent against trypanosomiasis in *para*-arsenophenylglycine, in which the arsenic was trivalent. The analogous pentavalent substance, *para*-phenylglycine arsonic acid, was considerably less toxic, but devoid of activity against the disease. Jacobs reasoned that the lack of activity could be due to the free carboxyl group which conceivably could react with many centers of the tissue proteins before reaching the parasites. He therefore proposed masking the carboxyl group by conversion to the amide. The resulting substance, sodium *para*-phenylglycine amide ar sonate, was the first, simplest, and best arsenical of a series subsequently synthesized.

The new arsenical, named Tryparsamide by Simon Flexner, was found to be extremely effective in trypanosome infected animals by Drs. Wade H. Brown and Louise Pearce, who now formed part of the chemotherapeutic team. Several patents were awarded for control of the drug and several of its analogs—although none of the latter proved to be superior to Tryparsamide. At the conclusion of World War I, Louise Pearce made an extensive study of the drug in the Belgian Congo, which showed Tryparsamide to be more effective than previously used drugs. Further tests in the United States demonstrated some utility in the treatment of tertiary syphilis.

Some years later (1953), Belgium recognized the successes of Tryparsamide by making Drs. Jacobs, Heidelberger, Brown and Pearce officers of the Order of Leopold II.

During World War I, a portion of the Institute was designated as U.S. Laboratory #1, and served as a training facility in laboratory techniques for army physicians. Jacobs and Heidelberger investigated possible synthetic substitutes for Salvarsan, a drug in scant supply and of undesirable toxicity. One analog (arsenophenyl-glycine-*bism*-hydroxyanilide), which appeared to be less toxic and at least as active against syphilis when studied by Brown and Pearce in animals, showed promising results in some one hundred human syphilitics. Unfortunately, a second batch, for no apparent reason, caused severe, dangerous dermatitis and was abandoned.

At the conclusion of the work on arsenicals, Jacobs and Heidelberger desired to turn to fields other than synthetic organic chemistry, but Flexner's faith in this approach prevailed and attention was turned to pneumococcal and streptococcal infections. The drug, Optochin, had been used with partial success in the treatment of pneumococcal infections, so further modification of the cinchona alkaloids was investigated, and a long series of papers resulted. Unfortunately, most of these substances killed infected mice faster than drug or infection alone. The state of this area of chemistry in the United States at the time is reflected in the refusal of the *Journal of the American Chemical Society* to publish the work on the cinchona alkaloids—on the grounds that no one in America was interested in alkaloids. Only after long arguments were the manuscripts accepted.

One of the intermediates used in modification of the alkaloids and in other syntheses was *p*-aminobenzenesulfonamide, or sulfanilamide, which had been prepared in 1908 by Gelmo in Germany. This was shown by Trefouel, Trefouel, Nitti, and Bovet to be one of the metabolic products of the azo dye Prontosil, the antibiotic action of which was demonstrated in the same year (1935) by Gerhardt Domagk for which he was awarded the Nobel Prize. Actually, it developed that the antibiotic action of Prontosil was due to the sufanilamide liberated on metabolism of Prontosil. It apparently never occurred to Jacobs and Heidelberger in 1920 that such a simple substance could control bacterial infections by other than direct antibacterial action. If the antibiotic action had been recognized, many thousands of lives could have been saved in the intervening years.

After some nine and one-half years, the team of Jacobs and Heidelberger separated. Flexner agreed that chemotherapeutic research through synthetic methods could be abandoned, and Heidelberger was transferred to the new laboratory of Donald D. Van Slyke to become familiar with biochemistry. Jacobs became a full member of the Rockefeller Institute in 1923, and turned his attention to the elucidation of the structures of substances of natural origin which displayed powerful physiological actions in order to correlate structure with such activity. The name of the laboratory was changed to that of chemical pharmacology.

The first group explored was that known as the cardiac glycosides, noted for their specific and powerful action on the myocardium, and which are unrivaled in value for the treatment of congestive heart failure. Of the group, the glycosides from digitalis species are probably the best known. Extracts of other plants containing members of the group such as strophanthus species were used as arrow poisons by African tribes. The ancient Egyptians were familiar with the properties of squill, and the Romans used it as an emetic, heart tonic, diuretic and rat poison. Strophanthus was introduced into modern medicine in 1890, and the modern use of digitalis dates from 1785, when William Withering published his famous book entitled, *An Account of the Foxglove and Some of Its Medicinal Uses: With Practical Remarks on Dropsy and Other Diseases.*

At the time Jacobs began his investigations on the structures of these important compounds, little was known of them or their chemistry. It was known that they were glycosides consisting of a rather complicated aglycone moiety, which was responsible for their major pharmacological properties, joined with one or more sugar molecules. The digitalis glycosides are present in the plant in extremely small amounts, whereas the seeds of *Strophanthus kombé* are relatively rich in the glycosides of strophanthidin. Jacobs' plan of attack on the structures of the aglycones was to place major emphasis on the structure determination of the more accessible strophanthidin and to attempt a correlation of this structure with the digitalis aglycones and others by chemical interconversions of appropriate derivatives. This plan proved to be completely successful and the structures of a half dozen or so of the aglycones were demonstrated.

It should be noted that when the study of the cardiac aglycones was begun (1923), similar studies on cholesterol and the bile acids by Windaus (1903), Diels (1903) and Wieland (1912) in Germany were also under way. There was no reason to believe that the three groups of substances would ultimately be found to be closely related. Also, with the possible exception of ultraviolet spectroscopy, none of the instrumental methods, such as infrared spectroscopy, X-ray spectroscopy and nuclear magnetic resonance, were available. Chemical transformations and degradation with subsequent interpretation formed the basis for structural elucidation—a long and tedious process at best.

Conversion of a strophanthidin derivative to a periplogenin derivative, and correlation of the latter with derivatives of digitoxigenin and gitoxigenin from digitalis followed. Although many structural features of the four aglycones were known, the problem of the carbon skeleton remained unsolved. In 1927, Diels and his co-workers heated cholesterol with selenium, thereby dehydrogenating it to its basic carbon ring system, cyclopentanophenanthrene. Similar dehydrogenation of strophanthidin also yielded Diels' hydrocarbon, thus providing conclusive evidence that the basic ring system of the cardiac aglycones was, indeed, identical with that of cholesterol and the bile acids. With the aid of X-ray data, the latter basic ring system was shown to be that of the Diels' hydrocarbon, a perhydrocyclopentanophenanthrene, by British workers.

The problem of the location of the unsaturated lactone side chain on the nucleus of the aglycones was resolved by application of the Barbier-Wieland degradation, used successfully in degradation of the side chain of cholanic acid, to a derivative of digitoxigenin with the formation of etiocholanic acid as the final product. Almost simultaneously, R. Tschesche in Germany accomplished a similar degradation of another aglycone, uzarigenin, to allo-etiocholanic, the difference being in the stereo configuration of the lactone side chain at the 17-position of the nucleus.

Although all available evidence up to the actual Barbier-Wieland degradation appeared to involve the 17-position as the point of attachment of the side chain, Jacobs, a most meticulous planner of experiments, was loath to attempt the degradation of the side chain. A suitable derivative of the available strophanthidin was not available, and only a few hundred milligrams of an appropriate digitoxigenin derivative were available for the three-step degradation and characterization of the product. Jacobs was hesitant to commit this hard to obtain substance to a degradation—the outcome of which, despite its logical prediction, was not certain.

It so happened that the necessity for this decision arose early in the fall of 1934. Jacobs and his wife habitually took long Columbus Day weekends to admire the fall foliage of the Adirondack Mountains—and this author took the opportunity to commit the entire supply of digitoxigenin derivative to the degradation, with considerable trepidation. After three days and nights, pure samples of etiocholanic acid and its methyl and ethyl esters awaited Jacobs' return. Although the physical constants of all these agreed with the published data, Jacobs, after some hesitation, agreed to request authentic samples from Wieland in Munich for comparison. All compounds were identical with the samples, and this author heaved a sigh of relief.

With one minor revision, involving the position of the double bond in the unsaturated lactone side chain, the structure of the cardiac aglycones was established.

During his investigations of the cardiac aglycones, Jacobs began structural studies of the saponin group. These are plant glycosides that possess the distinctive property of form ing a soapy lather in water. The plant heart drugs also display this property, but are classified separately because of their distinctive physiological heart action.

Early emphasis was placed on the readily available sarsasapogenin from *Smilax ornata* Hooker. The aglycone occurs as a glycoside with two rhamnose and one glucose units. The empirical formula of the aglycone was revised to the now accepted $C_{27}H_{44}O_3$. Reinvestigation of the selenium dehydrogenation of sarsasapogenin resulted in the isolation of Diels' hydrocarbon, thus establishing that the sapogenins possess the steroid ring system. The presence of a C_8 side chain was indicated by the isolation of a ketone $C_8H_{16}O$, which was not identical with methyl isohexyl ketone from cholesterol. A partial structure for sarsasapogenin was suggested in 1935.

By this time, great emphasis had been placed on the cortical steroid hormones and their possible therapeutic applications. This resulted in a hectic search for accessible plant steroids which could be converted to cortical hormones. Several investigators, amply financed by the pharmaceutical industry, entered the field. Jacobs, with his small staff, was literally snowed under.

However, one further correlation was accomplished—that of sarsasapogenin with the representative steroid alkaloid, solanidine. By this correlation, the structure of solanidine was established and another member of the steroid group was recognized.

Beginning in 1932, an intensive study of the structures of the ergot alkaloids was undertaken in cooperation with the late Dr. Lyman C. Craig. Ergot is the product of a fungus which grows on grain, particularly on rye. Its effect on pregnancy has been known for 2,000 years and it was first used by physicians as an oxytocic agent some 400 years ago. Consumption of edible grain contaminated by the fungus has resulted in death and destruction for centuries, but it was not recognized as the agent responsible for destructive epidemics until 1670. It was first employed by a physician about 1815, although it had been used by midwives long before.

At the time this investigation was undertaken, the chemistry of the ergot group was almost completely unknown. By 1934, on hydrolysis of the alkaloids, a substance named lysergic acid, which proved to be the characteristic building block of the ergot alkaloids, was isolated. The other products of hydrolysis of the alkaloids were amino acid derivatives joined by peptide linkages to themselves and to lysergic acid.

The structure of lysergic acid was then shown by degradation and substantiated by subsequent synthesis of the dihydro derivative and of lysergic acid itself. In this work an unnatural amino acid was encountered for the first time. Subsequently, the now famous L.S.D., the diethyl amide of lysergic acid, was synthesized by A. Hoffmann and Arthur Stoll in Switzerland.

The suspicion that such substances might be of more widespread occurrence formed the basis for a plan to extend such studies to other poisonous fungi such as *Amanita muscaria.* Although begun, this was interrupted by other work and has since been carried on in other laboratories. The ergot alkaloids may be regarded as the first group of a general pattern of such distorted polypeptides encountered in the gramicidins, penicillins and other antibiotics.

Another field of investigation entered by Dr. Jacobs was the chemistry of the aconite alkaloids, which embraced a large group of substances of undetermined structure when he began his inquiry in 1936. Some of these had long been used in medicine, and in some cases are among the most poisonous substances known. These studies included the known aconitine from *Aconitum napellus,* commonly known as monkshood or wolfsbane, and the isolation of three new alkaloids—heteratisine, hetisine and benzoylheteratisine—as well as the known atisine from *Aconitum heterophyllum* Wall. Closely related were delphinine and staphisine from *Delphinium staphisagria,* similar in action to aconitine. Degradation procedures supplemented by syntheses were reported in some thirty-five communications, until the time of Jacobs' retirement in 1957.

The final group of natural products to which Jacobs turned his attention embraces a complex family now known as the steroid bases, or veratrum alkaloids, found in various *Veratrum* species. These fall into two classes, as suggested by Fieser and Fieser*: the jerveratrum alkamines and the ceveratrum alkamines which comprise cevine and its precursors.

Of the first group, Jacobs and co-workers established that rubijervine is a hydroxyl derivative of the known solanidine by conversion to the latter substance. Assignment of the hydroxyl group to the 12-position was established by infrared and rotatory dispersion data. A structure for veratramine was also proposed, which was essentially correct except for one minor detail.

As with the saponins, interest in jervine increased with recognition of the possibility that it could serve as a starting material for the synthesis of cortisone, and it attracted extensive attention from industrial laboratories. In 1949, Jacobs had tentatively suggested a structure for jervine based on available data for this complex molecule at that time. This was shown to be in error in some respects, although in general it reflected the data then available. The accepted structure was eventually proposed by a group at the Squibb Laboratories.

The ceveratrum alkamines generally occur as esters of various acids. Four of the alkamine bases commonly found as such esters are veracevine, germine, protoverine and zygadenine. On alkaline hydrolysis, cevine, now recognized as an

* Louis F. Fieser and Mary Fieser, *Steroids* (New York: Reinhold Publishing Corp., 1959), 1118 pp.

artifact, was formed from all four. It appeared to be a logical candidate for structural studies in 1937, when Jacobs' long series of studies of these substances began. Selenium dehydrogenation yielded nine bases, five hydrocarbons and one tricyclic phenol. From study of these fragments and spectroscopic data, it was possible to formulate the ring system of cevine.

A second approach involved chromic acid oxidation of cevine which yielded a mixture of acids and lactams from which six pure acidic compounds were isolated by fractional separation of the methyl esters. Heating of the mixture of oxidation products at 200° resulted in the formation of decevinic acid. Data obtained with this substance resulted in corroboration of the structures assigned to other members of this family.

The investigations of Jacobs on the veratrum alkaloids continued for some twenty years until his retirement, and formed the basis for more extensive studies of these structurally very complicated molecules by other workers.

Walter Jacobs was a very highly regarded member of the Rockefeller Institute for some fifty years. His modesty, shyness and lack of aggressive tendencies kept even his colleagues at the Institute from realizing his innate abilities. He showed extraordinary judgment of the correctness and reliability of a chemical procedure, and an amazing ability to make most efficient use of time, especially in starting something new when another reaction had to be left for some hours. He insisted on complete and repeated experimental verification before publication of any result, a conservatism which in certain instances resulted in loss of priority.

He was not only an excellent and original organic chemist but also a very kind person, and most considerate of his fellow workers. He was fortunate in having married Laura Dreyfoos in 1908. She was the ideal understanding and sup portive wife for a retiring and dedicated scientist. A warm, outgoing, and capable woman, she furthered in unobtrusive ways the communication of his personality to friends. They often entertained his younger associates in their home in Mount Vernon. He specialized in very temperamental renditions of Beethoven on the Pianola, and in directing recordings of Wagner's Ring operas.

The Jacobs had two children, Elizabeth and Walter, Jr.

He was granted emeritus status at the Rockefeller in 1949, but continued active laboratory work until 1957, when he retired and migrated to Los Angeles.

HONORS AND DISTINCTIONS

Awards

Belgian Order of Leopold II, 1953

Professional and Honorary Societies

National Academy of Sciences (elected, 1932)
American Association for the Advancement of Science
American Chemical Society
American Society of Biological Chemists
American Society of Pharmacology and Experimental Therapeutics
Harvey Society

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Robert Kho-Seng Lim

October 15, 1897-July 8, 1969

by Horace W. Davenport

Robert K. S. Lim lived two lives. In the first, he was a physiologist with research interests in the control of gastric secretion and the neurophysiology of pain. He established Western physiology in China while teaching at the Peking Union Medical College.

In his second life, Robert K. S. Lim organized medical relief corps and trained doctors, nurses and technicians to meet the needs of China at war. He supervised medical services on the field of battle from the Great Wall to the retreat with Stilwell through the Burmese jungle. He built hospitals and medical schools on Mainland China and on Taiwan, and after the war he rebuilt his country's medical education and medical research. He was "one of the great men of China,"* the abundantly decorated Lieutenant General in the Army and Surgeon General of the Republic of China.

In both lives, Robert K. S. Lim was the vivacious, generous, charming, energetic, athletic and artistic man who spoke with a Scottish burr and was universally known as Bobby.

Robert Lim's ancestors came to Singapore from Fukien Province in southwestern China. The surname means a small

* B. Tuchman, *Stilwell and the American Experience in China, 1911-45* (New York: Macmillan, 1970).

forest represented by two trees, and the present official transliteration of the charcter is Lim. Although the Lim family, like many from their province, retained the old spelling, R. K. S. Lim sometimes appears in the indexes of books about war in China and in library catalogs as R. K. S. Lim.

Robert Lim's father, Lim Boon Keng, did so well as a poor boy at the Raffles Institution in Singapore that he won the Queen's Scholarship to Edinburgh University where he graduated in medicine. He worked briefly with W. B. Hardy in Cambridge, and together they published a paper in the *Journal of Physiology** on the origin and function of leucocytes in the frog. On returning to Singapore, Lim Boon Keng practiced medicine, but he was also active in public affairs in China as well as in Singapore. He was a Legislative Councillor, and in 1911 he was appointed Medical Advisor to the Chinese Ministry of the Interior. The next year he became physician and confidential secretary to Sun Yat-sen. He represented his country at meetings in Paris and Rome, and in 1923, with the help of a millionaire friend, he established the University of Amoy. Lim Boon Keng married Margaret Tuan-Keng Wong, one of the first Chinese women to be educated in the United States, and they had four sons, the oldest being Robert Kho-Seng. Lim Boon Keng died at the vernerable age of eighty-eight in Singapore.

Bobby Lim was born in Singapore on October 15, 1897. His father sent Bobby to Scotland when he was eight years old. The boy was in the charge of his father's apothecary, who was also an itinerant lay preacher, and in moving from parish to parish, Bobby's education was more peripatetic than substantial. Later, Bobby attended Watson's School in Edinburgh where he prepared for the University. At the outbreak of the First World War, Lim volunteered and was assigned to

* *Journal of Physiology,* 15(1894):361-74.

the Indian Army in France as a warrant officer. His job was to drill recruits, and the young sons of Maharajas who had joined the colors objected to being ordered around by a young "Chinaman." In 1916, Lim was allowed to return to Edinburgh for medical studies, and he received the M.B. and Ch.B. degrees in 1919.

In the Medical School of Edinburgh University, Lim quickly established himself as a protégé of Sir Edward Sharpey-Schafer, the Professor of Physiology, and as an undergraduate he worked in the Physiology Laboratory on problems suggested by Sharpey-Schafer. Immediately upon graduation, he was appointed Lecturer in Physiology with responsibility for teaching histology. The next year Lim presented the results of his research to earn the Ph.D.

In the tradition of British physiology, microscopic anatomy came within the purview of the Physiology Department, and Lim developed skill in histological techniques and observations. His first major publication was a study of the histology of tadpoles whose development had been accelerated by being fed thyroid. This paper is notable for Lim's drawings. Lim had considerable skill as a draughtsman, and he had transiently wanted to be an artist before his father persuaded him to try medicine first. He continued to illustrate his papers with delicate drawings. The best example is Lim's paper, published in 1922,* on the microscopic anatomy of the gastric mucosa. The paper is distinguished by its smooth style, by its thoroughness based on wide observation and meticulous attention to detail, and by its correlation of structure with function.

Lim carried the microscopic anatomy of the gastric mucosa almost as far as it could be carried until the advent of electronmicroscopy. In fact, he carried it a little further than

* *Quarterly Journal of Microscopical Science,* 66(1922): 187-212.

the resolution possible in light microscopy warranted, for in papers published later from China he described how mitochondria dissociate during secretion into a free lipid which condenses to form the Golgi apparatus and a remainder which either catalyzes or enters into the secretion.

Lim described the structure of the stomach, because he was already studying its function. The results were reported in a flood of papers in 1923.

At this time research in gastrointestinal physiology was in the doldrums. In Russia, Pavlov had turned to the study of conditioned reflexes, and in the United States, Walter B. Cannon had stopped work on the mechanical factors of digestion when he discovered he had been burned by X-rays. Cannon's observation on the supposed relation between gastric motility and the sensation of hunger had been taken up, without any notable results, by A. J. Carlson, whose reputation rests more on his picturesque behavior than on his scientific accomplishments. Carlson's industrious pupil, Andrew C. Ivy, was just beginning his long career.

In 1902 William Bayliss and Ernest H. Starling had established the fact that a hormone from the upper intestinal mucosa, *secretin,* could stimulate pancreatic secretion, but no progress had been made in purifying the hormone or in delineating its role in the course of digestion. Edkins had shown, by methods very similar to those of Bayliss and Starling, that extracts of the gastric antral mucosa stimulate secretion of acid by the oxyntic mucosa, and he had postulated that his extracts contained a hormone which he called *gastrin.* Unfortunately for Edkins, the two subsequent discoveries—that crude tissue extracts always contain histamine and that histamine stimulates acid secretion—were generally interpreted to mean that Edkins had made a ludicrous mistake.

This conclusion, which was to trouble gastroenterology for another fifty years, was not accepted by Robert Lim. He repeated Edkins's experiments with no significant improvement, and he found that extracts of the pyloric mucosa, but not extracts of other tissues, stimulate acid secretion. Recognizing that the crucial test of a gastric hormone would be demonstration of it in gastric venous blood, Lim unsuccessfully tried to find acid-stimulating properties in blood drawn from dogs digesting a meal.

Lim became interested in the properties of pyloric secretion. With his colleague, N. M. Dott, Lim prepared, in a two-stage operation, a pouch of the gastric antrum devoid of oxyntic mucosa. Dott probably contributed much of the surgical skill, for he published separately on operative techniques. The pouch was found to secrete a viscid, alkaline secretion containing a proteolytic enzyme active in acid but not in alkaline solution. A dog with such a pouch was ready for the next step: the demonstration that stimulation of the pouch causes acid secretion by the remote oxyntic mucosa, but Lim did not do the experiment.

In the autumn of 1922, Lim applied to the China Medical Board of New York for a fellowship to enable him to study in European and American universities. His application was immediately welcomed by Roger S. Greene, the Board's Secretary. Greene knew Lim Boon Keng by reputation, and the day before he received Lim's letter he had been told about Lim by the Chinese Minister to the United States. Greene asked Lim whether, if he received a fellowship, he would be willing to take a year's appointment at the Peking Union Medical College.

The Peking Union Medical College had been developed by the China Medical Board with an endowment from the Rockefeller Foundation. In 1915 the Board, with the advice of W. H. Welch and Simon Flexner, had bought the missionary-founded Union Medical College in Peking and had begun to build a medical school along Western lines.* The aims of the school were "to give medical education comparable with that provided by the best medical schools in the United States and Europe, through . . ."† an undergraduate curriculum and through graduate training in research and practice. Emphasis was always on quality, and pressure from the Chinese government for quantity was firmly resisted. The stated goal of those responsible for the College was to have Western medical science taken over by the Chinese people so that it became part of their national life. The suggestion that Lim consider an appointment at P.U.M.C. was an example of the Board's continued search for competent Orientals.

Lim replied that the chief object in his life was to return to China to teach physiology and to do research there as efficiently as it was being done in the West. However, he cautiously refused to commit himself completely to P.U.M.C. without assurances of an adequate salary and a senior appointment.

Lim received the fellowship, and he came to the United States in the autumn of 1924. Although the China Medical Board had suggested that Lim study in two departments, those of Joseph Erlanger in St. Louis and A. J. Carlson in Chicago, Lim worked only in the Department of Physiology of the University of Chicago. That laboratory was the only one in the country with a current reputation in gastrointestinal physiology. Most of the work was being done by a

* The administrative history of P.U.M.C. is fully described in M. E. Ferguson, *China Medical Board and Peking Union Medical College* (New York: China Medical Board of New York, Inc., 1970). The records of the China Medical Board and of P.U.M.C. are now in the Rockefeller Archive Center, North Tarrytown, New York. Copies of letters relating to Lim have been made available to me through the courtesy of the Center's Director and Associate Director, J. W. Ernst and J. W. Hess. The educational and scientific program of P.U.M.C. is described in J. Z. Bowers, *Western Medicine in a Chinese Palace* (New York: The Josiah Macy, Jr. Foundation, 1972). Bowers is wrong in identifying Lim's first wife as Sharpey-Schafer's daughter.

† Ferguson, p. 44.

team under A. C. Ivy, and Lim was put to work as a member of the team. Research was on the control of gastrointestinal secretion and motility, and dogs with chronically prepared pouches and fistulas were used. In one study in which Lim participated, the entire stomach was separated from the esophagus and duodenum, and made into a pouch draining to the body surface. The vagus nerves had been cut, and cephalic stimuli could not affect the pouch through them. The pouch's secretion could be collected, and thereby the efficacy of stimuli could be determined. Because the distal end of the esophagus had been anastomosed to the proximal end of the duodenum, the dog could eat naturally. With such a preparation, Ivy, Lim and McCarthy found that mixed meals, meat extracts and milk stimulated gastric secretion after a latent period of one or more hours. Fats fed inhibited basal or continuous secretion.

This team, and indeed all such teams for many years, was dominated by Ivy; Lim, as a visiting fellow, cannot be held responsible for the conclusions of papers bearing his name. He can only be judged by the use to which he later put what he had learned in Chicago. The paper just cited lamely concluded that stimulation of gastric secretion by food in the intestine must result from some vascular response. Moreover, ". . . our work proves that Edkins' pyloric hormone theory is utterly inadequate; that there is either no hormone mechanism, or, if one, that the whole gastrointestinal tract is involved."* What Lim thought when he eventually saw this paper in print is unknown, but it seems unlikely that he, who had only recently published several papers of his own affirming the existence of gastrin, had abruptly changed his mind.

* A. C. Ivy, R. K. S. Lim, and J. E. McCarthy, "Contributions to the Physiology of Gastric Secretion. II. Intestinal Phase of Gastric Secretion, "Journal *of Experimental Physiology,* 15(1925):55-68.

Having received a satisfactory appraisal of Lim from A. J. Carlson, a recommendation which said that Lim made an excellent impression even on those prejudiced against the Chinese, the China Medical Board recommended that Lim be made an Associate Professor in physiology at P.U.M.C. In the meantime, Lim's father had begun to organize the University of Amoy, and he asked his son to build a medical school from scratch. In contrast with the superb school, hospital, and staff being completed in Peking, Amoy had no buildings and no faculty, but young Lim could have at least the title of Professor. To get him for Peking, the P.U.M.C. made him a Visiting Professor with no increase in salary over that previously offered. In September of 1925 the trustees of the school made him Head of the Department of Physiology.

By the time Lim arrived in Peking in 1924, the buildings of P.U.M.C. had been completed. The preclinical and clinical departments, a hospital, and faculty residences occupied the site of a Prince's palace. The Prince's name of Wu sounded much like the Chinese word for oil, and P.U.M.C. was known to the Chinese as the Oil Prince's Palace. Lim occupied a fully equipped Physiology Department, and during his tenure from 1924 to 1938 he had a staff of seven professionals, five of them Oriental. The China Medical Board sent visiting professors to P.U.M.C., and the list is an honor roll of American medical science. In 1935 both Anton J. Carlson and Walter B. Cannon were Visiting Professors of Physiology.

Lim established a vigorous research program in collaboration with many colleagues and students. He founded the Chinese Physiological Society, and the Society began publication of the *Chinese Journal of Physiology.* Lim was managing editor, and he published many papers in the journal. He also organized a Peking branch of the Society for Experimental Biology and Medicine which gave him the opportunity of publishing summaries of his work in a journal more easily accessible to Western physiologists.

By means of transplanted and perfused stomachs, Lim studied gastric metabolism and the control of secretion. His most important result was the demonstration that feeding olive oil inhibits secretion by a transplanted pouch of the stomach. In the process of preparation, the gastric tissue forming the pouch was totally separated from the donor dog, and it was therefore completely extrinsically denervated. The inhibitory influence of fat feeding must have been carried by the blood, and Lim showed that fat absorbed into the lymph was not responsible. Lim coined the word *enterogastrone* for the putative hormone, and he showed that it is probably different from the hormone cholecystokinin which had recently been identified by Ivy. Lim attempted to purify enterogastrone, but he succeeded no better than many after him. Today, it appears that the inhibitory property of enterogastrone is only one of the properties of a number of polypeptides extractable from the intestinal mucosa. Although the hormonal mechanism described by Lim indubitably exists, his name for it is being discarded.

Working with pupils and colleagues from other departments, Lim did three other substantial pieces of physiological research at P.U.M.C. He found a pressor center in the lateral parts of the floor of the IV th ventricle between the levels of the acoustic stria and the inferior fovea. Stimulation of the center electrically or by iontophoresis of acetylcholine elicits typical and complete sympathetic responses. The efferent pathway goes unilaterally down the ventrolateral columns of the spinal cord, and through it both sympathetic neurones and the adrenal medulla are excited. Stimulation of the central end of the cut sciatic nerve has its familiar pressor effects mediated by the center Lim described. In a thorough comparative study, Lim demonstrated that a similar pressor response follows stimulation of corresponding parts of the medulla in fish, amphibians, reptiles, birds, and eight species of mammals.

Lim's efforts to identify circulating hormones released from the gastrointestinal mucosa made him a master of the techniques of cross circulation and vivi-perfusion. In vivi-perfusion, an organ removed from a donor animal, usually a dog, is perfused by way of the carotid arteries and jugular veins of another animal. The perfusing animal is frequently unanesthetized, its vessels being isolated under local anesthetic. Lim used this method to study humoral transmission in the central nervous system. In this case, the organ perfused was the severed head of a donor dog. Stimulation of the central end of the vagus nerves of the perfused head is followed by a small and brief fall in the blood pressure of the perfusing dog and then by a large and prolonged rise in its blood pressure. Lim showed, using standard pharmacological and physiological methods, that the response is mediated by acetylcholine liberated by the perfused head. The transient fall in blood pressure is the direct effect of acetylcholine on the cardiovascular system, and the rise is caused by epinephrine liberated from the adrenal medulla under the stimulus of acetylcholine.

Using the same vivi-perfused preparation, but one in which the life of acetylcholine was not prolonged by eserine, Lim found that when afferent fibers of the vagus nerve are stimulated there is also a pressor response, but one which is abolished by extirpation of the donor's pituitary gland. Furthermore, blood draining the perfused head also contains an oxytocic and an antidiuretic principle. Lim, returning to his histological methods, found that exhaustion of the reflex is correlated with disappearance of secretory granules from the posterior pituitary gland, and that the reflex returns when the granules do. He believed that he had discovered a vago-posterior-pituitary reflex. Knowledge of this reflex seems to have died with Homer Smith, for in the 1970's renal physiologists interested in reflex control of antidiuresis do not refer to Lim's work.

In the 30's, Lim turned toward serving his country on a larger scale. He became President of the Chinese Medical Association and Chairman of the North China Council for Rural Reconstruction. Lim organized a training corps for reserve medical officers. As the Japanese attacks began, Lim founded the Chinese Red Cross Medical Relief Commission, and its field units first saw service when the Japanese moved against Shanghai. When fighting spread along the Great Wall, Lim had twelve medical units which treated over 20,000 casualties. He knew that China would require a vast number of persons at all levels of training, and he pressed upon P.U.M.C. the need for mass education of technicians and sanitarians. P.U.M.C., which conceived its mission to be the teaching of teachers, refused to change its standards, and Lim left it for good in 1938.

By 1940, the Chinese Red Cross, under Lim's direction, operated convoys, depots, and medical units. The units, now forty-nine in number, provided treatment and nursing services for the wounded; ambulance units, each with 120 stretcher bearers, brought the wounded, who otherwise would have been left on the field to die, into makeshift hospitals. Lim had by then inaugurated a school designed to train 200 men a month as hospital attendants and stretcher bearers. This and the similar schools he built in the next few years were intended to be the nuclei of future medical schools.

Lim built at Kweiyang the largest medical center in wartime China, and he was appointed Inspector General of the Medical Services in 1941. Following the defeat of the Chinese armies in 1942, Lim accompanied General Joseph Stilwell in the retreat through Burma. He earned the friendship and admiration of Stilwell. When President Roosevelt ordered Stilwell to confer the Order of Merit upon Chiang Kai-shek, Stilwell said: "It will make me want to throw up."* Stilwell was allowed, as an anti-emetic, to pin the same decoration on Lim. In the many memoirs of the period, General Bobby Lim occasionally appears, distinguished amidst the surrounding chaos by his honesty, industry and accomplishments.

When the Nationalist Government was on the point of collapse on the Mainland, Lim was offered the Ministry of Health. After a debate with his staff, all men and women of great integrity and dedication, Lim refused the job. Seeing that Mainland China was untenable, Lim proposed that the medical units be moved to Taiwan and that the government follow. He was able to save equipment and supplies, and he diverted from Shanghai to Taiwan a ship sailing to China with supplies he had ordered. On Taiwan, Lim built the National Defense Medical College and ten hospitals throughout the island.

Lim regretted that he had lost touch with teaching and research, and after twelve years of fighting under desperate circumstances, he wanted to return to the academic life. He resigned as Surgeon General and Lieutenant General and came to the United States. He remained *persona grata* with the government on Taiwan,† and on cordial terms with General

* Tuchman, p. 378.

† The statement in Tuchman, *op. cit.,* that Lim was dismissed in 1943 as the result of political pressure is clearly wrong. A man of Lim's vigor was bound to get into scrapes with the government. The 1943 episode may have been a temporary one from which he was rescued, as he often was, by Chiang's deputy and Lim's immediate superior, Chen Cheng, who befriended the intellectuals of China. Although Chiang's and Chen's background and education were totally different from Lim's, they appreciated Lim's ability.

and Madame Chiang Kai-shek. He revisited the island several times to do research and to arrange for postgraduate training of Chinese physicians in this country. The year before his death, he spent six months on Taiwan, setting up a neurophysiological laboratory.

After working briefly in Chicago and Omaha, Lim was invited by Miles Laboratories of Elkhart, Indiana to join its research team. Miles had a proprietary interest in preparations of acetylsalicylic acid, and Lim worked on analgesia. Eventually he was made Senior Research Fellow, and then he did the work on the neurophysiology of pain for which he will probably be best remembered.

In his most important experiment, Lim carried his method of cross-circulation into neurophysiology. Using two dogs, a donor and a recipient, Lim arranged for the circulation of the spleen of the recipient dog to be supplied entirely by the donor dog. A catheter permitted close intra-arterial injections into the spleen. Nerves from the spleen of the recipient dog were intact, and in some instances Lim placed electrodes on the nerves so that afferent impulses could be recorded. Intra-arterial injection of a minute amount of bradykinin into the spleen had no effect upon the donor dog, but the recipient dog gave a brief affective response, that is, it howled, struggled, and bit.

Using this method, Lim found that the non-narcotic analgesic, aspirin, eliminated the affective response of the recipient dog when it was given to the donor dog in appropriate dose. Afferent impulses in the recipient dog's splenic nerve were suppressed. Given to the recipient dog in the same dose, aspirin had no effect. Aspirin, therefore, is an analgesic because it blocks the generation of impulses in the receptor endings of the afferent nerves mediating the sensation of pain. Narcotic analgesics, such as morphine, block centrally and not peripherally. Lim confirmed his distinction between central and peripheral action by experiments on man in which he injected bradykinin intraperitoneally. Lim's last work was an attempt to discover by means of fluorescent microscopy the pain receptors that had absorbed acetylsalicylic acid.

Robert K. S. Lim was elected a Foreign Associate of the National Academy of Sciences in 1942, when he was deeply involved in his war work. The nomination lists as his qualifications his scientific accomplishments, his stimulation of physiological research in China and his promotion of Western medicine there. It also cites his services to China, then our ally, in organizing the Medical Relief Corps, in providing medical and surgical services for the Chinese armies and in establishing military medical training schools. The relative importance of the two different kinds of qualifications in securing his election cannot now be determined. When he became a United States citizen in 1955, he automatically became a regular member of the Academy.

In May of 1967, a brief period of dysphagia led to the discovery of squamous cell carcinoma in the mid-third of Lim's esophagus. He responded well to cobalt-60 therapy, and in early 1968 his colleagues in Taiwan found his esophagus to be almost normal. Later that year, repeated mechanical dilatation was necessary, and in April 1969 a gastrostomy was performed in Chicago. His wife and a physician took him to his son's home in Jamaica, and his daughter came from England. He had a few weeks in which he enjoyed the company of his family before he died on July 8, 1969.

Lim married Margaret Torrance in Scotland on July 10, 1920. They had two children, a daughter Effie (Mrs. O. Philip Edwards) and a son, James T. After his first wife's death, Lim married Tsing-Ying Tsang in Shanghai on July 2, 1946. She and the children survived him.

In addition to the staff of the Rockefeller Archives Center already identified, I thank Ms. Opal Gunter of Miles Laboratories, Inc., M. I. Grossman, S. C. Wang, and T-M Lim for supplying information. I am especially grateful to Tsing-Ying Lim (Mrs. R. K. S. Lim) for her many kindnesses.

HONORS AND DISTINCTIONS

Degrees

M.B., Ch.B., 1919, Edinburgh University

Ph.D., 1920, Edinburgh University

D.Sc., 1924, Edinburgh University

D.Sc. (Hon. Causa), 1961, Hong Kong

Professional Record

1919-1923 Lecturer in Physiology, Edinburgh University

1920 Goodsir Fellow, Edinburgh University

1923-1924 Rockefeller Foundation Fellow, University of Chicago

1924-1938 Professor and Head, Department of Physiology, Peking Union Medical College

1939-1941 Director, Emergency Medical Service Training School

1944-1947 Special Lecturer in Physiology, Columbia University

1945 Organizing Director, Institute of Medicine, Academia Sinica

1946-1949 Director, National Defense Medical Center, Republic of China

1949-1950 Visiting Research Professor of Clinical Science, University of Illinois, Chicago

1950-1951 Professor and Head, Department of Physiology and Pharmacology, Creighton University

1952-1967 Miles Laboratories, Inc., Elkhart, Indiana, Director, Medical Sciences Research, Senior Research Fellow

1968-1969 Visiting Professor of Physiology, University of California, Los Angeles, and Senior Medical Investigator, Veterans Administration Center, Los Angeles

Professional and Honorary Societies

British Physiological Society, 1919

Fellow, Royal Society of Edinburgh, 1923

American Physiological Society, 1923

Sigma Xi, 1924

Society for Experimental Biology and Medicine, 1925

President, Chinese Physiological Society, 1927

President, Chinese Medical Association, 1928-1930

Honorary Member, Deutsche Akademie der Naturforscher, Halle, 1932

Corresponding Member, Royal Academy of Sciences, Bologna, 1932

Member, Permanent Commission for Biological Standardization, League of Nations, 1935

Counsellor, Academia Sinica, 1936

Foreign Associate, National Academy of Sciences, Washington, 1942; Member, 1955

Honorary Member, American Gastroenterological Association, 1946

Honorary Fellow, American College of Surgeons, 1947

Member, Permanent Committee of the International Congress of Physiology, 1947

Honorary Member, Association of Military Surgeons of the United States, 1948

American Society for Pharmacology and Experimental Therapeutics, 1952

Society of Toxicology, 1963

Fellow, American College of Clinical Pharmacology and Chemotherapy, 1964

Military Record

Warrant Officer, Indian Army, 1914-1916

Lieutenant, RAMC, 1919

Field Director, Chinese Red Cross Medical Relief Commission, North China, 1933

Director, Chinese Red Cross Medical Relief Corps, 1937-1943

Inspector General of Medical Service, Chinese Army, 1942, 1944

Deputy Surgeon General, Chinese Army, 1944-1945

Surgeon General and Lieutenant General, Chinese Army, 1945-1949

Decorations

Great Britain: 1914-1915 Star; General Service Medal, Victory Medal, 1918

United States: Legion of Merit, Officer Grade, 1943; Medal of Freedom with Silver Palms, 1946

Republic of China: Kan Ching Medal; Chung Ching Medal; Sheng Li Medal; Yun Hui Order, 1st Class; Victory Medal, 1945

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A method for recording gastric secretion in acute experiments on normal animals. Q. J. Exp. Physiol., 13:71-78.

The question of a gastric hormone. Q. J. Exp. Physiol., 13:79-103.

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The source of the proteolytic enzyme in extracts of the pyloric mucous membrane. Q. J. Exp. Physiol., 13:139-44.

With A. R. Matheson and W. Schlapp. An improved method for investigating the secretory function of the stomach and duodenum in the human subject. Q. J. Exp. Physiol., 13:333-45.

With A. R. Matheson and W. Schlapp. Observations on the human gastro-duodenal secretions with special reference to the action of histamine. Q. J. Exp. Physiol., 13:361-91.

With W. Schlapp. The effect of histamine, gastrin and secretin on the gastro-duodenal secretions in animals. Q. J. Exp. Physiol., 13:393-404.

1924 On the relationship between the gastric acid response and basal secretion of the stomach. Am. J. Physiol., 69:318-33.

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Alfred Lee Loomis

November 4, 1887-August 11, 1975

by Luis W. Alvarez

The beginning of this century marked a profound change in the manner in which science was pursued. Before that time, most scientists were independently wealthy gentlemen who could afford to devote their lives to the search for scientific truth. The following paradigms come to mind: Lord Cavendish, Charles Darwin, Count Rumford, and Lord Rayleigh. But after the turn of the century, university scientists found it possible to earn a living teaching students, while doing research "on the side." So the true amateur has almost disappeared—Alfred Loomis may well be remembered as the last of the great amateurs of science. He had distinguished careers as a lawyer, as an Army officer, and as an investment banker before he turned his full energies to the pursuit of scientific knowledge, first in the field of physics, and later as a biologist. By any measure that can be employed, he was one of the most influential physical scientists of this century. In support of that assessment, one can note: (1) his election to this Academy when he was 53 years old, (2) his honorary degrees from prestigious universities, (3) his crucial wartime role as director of all NDRC-OSRD radar research in World War II, and (4) his exceedingly close personal relationships with many of the leaders of American science and government in the mid-twentieth century.

With that brief introduction to the remarkable career of Alfred L. Loomis, we will now examine the man himself, to find, as one might expect, that he was indeed as extraordinary as his unique accomplishments would suggest.

He was born in New York City on November 4, 1887. His father was Dr. Henry Patterson Loomis, a well-known physician and professor of clinical medicine at New York and Cornell medical colleges. His grandfather, for whom he was named, was the great nineteenth century tuberculosis specialist whose work was commemorated in the naming of the Loomis Laboratory at Cornell Medical College, and the Loomis Sanatorium at Liberty, New York. His maternal uncle was also a physician, as well as the father of Alfred Loomis' favorite cousin, Henry L. Stimson, who was Secretary of State under Herbert Hoover, and Secretary of War throughout World War II.

From Alfred Loomis' educational background, one would correctly judge that he came from a prosperous, but not exceedingly wealthy family. He attended St. Matthew's Military Academy in Tarrytown, New York from the age of nine until he entered Andover at thirteen. His early interests were chess and magic; in both fields, he attained near professional status. He was a child prodigy in chess, and could play two simultaneous blindfold games. He was an expert card and coin manipulator, and he also possessed a collection of magic apparatus of the kind used by stage magicians. On one of the family summer trips to Europe, young Alfred spent most of his money on a large box filled to the brim with folded paper flowers, each of which would spring into shape when released from a confined hiding place. His unhappiest moment came when a customs inspector, noting the protective manner in which the box was being held, insisted that it be opened—over the strong protests of its owner. It took a whole afternoon to retrieve all the flowers.

The story of the paper flowers is included in this biographical memoir because it is the only story of Alfred's childhood I can remember hearing from him. (Since all his friends called him Alfred, and since the story of his life is for his friends in the Academy, I will refer to him most often as Alfred. Those who knew him less well called him Dr. Loomis.) In the thirty-five years during which I knew him rather intimately, I never heard him mention the game of chess, and his homes contained not a single visible chessboard or set. (When I checked this point recently with Mrs. Loomis, she wrote, "Alfred kept a small chess set in a drawer by his chair and would use it, on and off, to relax from other intellectual pursuits. He preferred solving chess problems or inventing new ones to playing games with other people.")

He loved all intellectual challenges and most particularly, mathematical puzzles. He made a serious attempt to learn the Japanese game of Go, so that he could share more fully in the life of his son Farney, who was one of the best Go players in the United States. But his chess background wasn't transferable to the quite different intricacies of Go, and he had to be content to collaborate with his son in their researches on the physiology of hydra. As he grew older, his manual dexterity lessened, but he still enjoyed showing his sleight of hand tricks to the children of his friends and to his grandchildren—but never to adults.

It was characteristic of Alfred that he lived in the present, and not in the past the way so many members of his generation do. On the very few occasions when he shared one of the many closed chapters of his life with me, I was enchanted by what he had to say about the captains of industry and the defenders of the America's Cup, who were many years ago his most intimate friends. He apparently felt it would sound as though he were bragging if he alluded to the great power he once wielded in the financial world when in the company of a university professor. In 1940, I casually asked him what he thought of Wendell Willkie, the Republican presidential candidate, and he said, "I guess I'll have to say I approve of him because I appointed him head of Commonwealth and Southern." Alfred was the major stockholder of that utility, so there was certainly an element of truth in his flip and very uncharacteristic remark. He was immediately and obviously embarrassed by what he had said, and it would be another twenty years before he made another reference to his financial career in my presence.

Alfred entered Yale in 1905, where he excelled in mathematics, but he was not interested enough in the formalities of science to enter Sheffield Scientific School. He took the standard gentlemen's courses in liberal arts, and without giving much thought to his career, felt he would probably engage in some kind of scientific work after he graduated. But one afternoon, a close friend came to him for advice on choosing a career. Alfred strongly urged him to go to law school, pointing out that a broad knowledge of the law was a wonderful springboard to a variety of careers; in addition to formal legal work, a lawyer was well prepared for careers in business, politics, or government administration. Alfred was so impressed by the arguments he marshaled for his friend that he enrolled in Harvard Law School. He never regretted that decision, because it gave him a breadth of vision that he applied to many fields.

In his senior year at Yale, he was secretary of his class, but he had the time and the financial resources to pursue his life-long hobby of "gadgeteering." His extracurricular activities involved technical matters such as the building of gliders, model airplanes, and radio-controlled automobiles. He was fascinated by artillery weapons, and we shall learn that the great store of information he accumulated in that field played a crucial role in changing the major focus in his life from business to the world of science. A glider he built and tested from the dunes near his summer home at East Hampton stayed in the air several minutes. It was obvious to his friends that he was distinguished by a wide-ranging mind and the ability to "learn all about" a completely new field in a remarkably short time through independent reading. That facet of his personality and intellect was the most immutable throughout his life—a life that would be characterized by periodic and major changes of interest.

Alfred's decision to become a lawyer was certainly influenced by his cousin, Henry Stimson, in whose firm of Winthrop and Stimson he was assured a law clerkship. But after his distinguished performance at Harvard Law School, where he was in the "top ten," helped edit the *Harvard Law Review,* and graduated cum laude in 1912, he would have been welcomed in any New York law firm. As one would guess from his later interests, he specialized in corporate law and its financial aspects.

By 1915, he was a member of the firm, and married to the former Ellen Farnsworth of Dedham, Massachusetts. They lived in Tuxedo Park and raised three fine sons, each of whom shared one or more of his father's major interests. Alfred's ideas on child rearing were unorthodox, but very successful. He thought that his sons should learn at an early age to manage all their own affairs, so he gave each of them a large sum of money at age fourteen, with no controls whatsoever. Each one planned his own education, and decided what hobbies to pursue, after much consultation with, but no veto power from, Alfred. The oldest son, Lee (Alfred Lee Loomis, Jr.), is a successful financier and famous deep sea sailor. The second son, the late Farney (William Farnsworth Loomis), was a physician and later Professor of Biochemistry at Brandeis University. He was a Himalayan climber, pilot, and as an OSS doctor, parachuted into China beyond Japanese lines in World War II. The third son is Henry, a radar officer in World War II who gave up a career in physics for one in public service administration. He was assistant to the President of MIT, later Director of the Voice of America, and is now President of the Corporation for Public Broadcasting.

Alfred's career as a young corporation lawyer was interrupted by World War I. When he joined the Army, his fellow officers were surprised to learn that he knew much more about modern field artillery than anyone they had ever met. He had made good use of the special communication channels available to Wall Street lawyers, and had accumulated a vast store of up-to-the-minute data on the latest ordnance equipment available to the warring European powers. His expertise in such matters led to his assignment to the Aberdeen Proving Grounds, and before long, he was put in charge of experimental research on exterior ballistics, with the rank of major. At Aberdeen, he was thrown into daily contact with some of the best physicists and astronomers of this country, and he and they benefited from each other's talents.

In those days, before photoelectric cells and radar sets came to the aid of exterior ballasticians, there was no convenient way to measure the velocity of shells fired from large guns. Alfred invented the Aberdeen Chronograph, which satisfied that need for many years after its invention. It is hard for someone like me, who came into a scene long after an ingenious device had been invented, and later supplanted, to appreciate what made that device so special. But the fact that Alfred singled out the Aberdeen Chronograph for mention in his entries in *Who's Who* and *American Men and Women of Science,* and mentioned it on a number of occasions in conversations with me, makes me believe that it must have been a remarkably successful and important invention.

Alfred set such high standards for his own performance that no other interpretation of the value of the Aberdeen Chronograph would be consistent with his pride in it.

One of the friends Alfred made at Aberdeen was R. W. Wood, who was considered by many to be the most brilliant American experimental physicist then alive. They had known each other casually from the circumstance that each of their families had summer homes at East Hampton, on Long Island. But at Aberdeen, they initiated a symbiotic relationship that lasted many years. Wood became, in effect, Alfred's private tutor, and Alfred responded by becoming Wood's scientific patron. The following paragraphs from Wood's biography, including some direct quotations from R. W. Wood, tell of this relationship better than anyone of the present era could.

> It was a consequence of Wood's scientific zest and social strenuousness that fate brought him, about this time, the facilities of a great private laboratory backed by a great private fortune. He had met Alfred Loomis during the war at the Aberdeen Proving Grounds, and later they became neighbors on Long Island. Loomis was a multimillionaire New York banker whose lifelong hobby had been physics and chemistry. Loomis was an *amateur* in the original French sense of the word, for which there is no English equivalent. During the war, he had invented the "Loomis Chronograph" for measuring the velocity of shells. Their friendship, resulting in the equipment of a princely private laboratory at Tuxedo Park, was a grand thing for them both.
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> A happy collaboration began, which came to its full flower in 1924. Here is Wood's story of what happened.
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> "Loomis was visiting his aunts at East Hampton and called on me one afternoon, while I was at work with something or other in my barn laboratory. We had a long talk and swapped stories of what we had seen or heard of 'science in warfare.' Then we got onto the subject of postwar research, and after that he was in the habit of dropping in for a talk almost every afternoon, evidently finding the atmosphere of the old barn more interesting if less refreshing than that of the beach and the country club.
>
> "One day he suggested that if I contemplated any research we might do together which required more money than the budget of the Physics

> Department could supply, he would like to underwrite it. I told him about Langevin's experiments with supersonics* during the war and the killing of fish at the Toulon Arsenal. It offered a wide field for research in physics, chemistry, and biology, as Langevin had studied only the high-frequency waves as a means of submarine detection. Loomis was enthusiastic, and we made a trip to the research laboratory of General Electric to discuss it with Whitney and Hull.
>
> "The resulting apparatus was built at Schenectady and installed at first in a large room in Loomis' garage at Tuxedo Park, New York, where we worked together, killing fish and mice, and trying to find out whether the waves destroyed tissue or acted on the nerves or what.
>
> "As the scope of the work expanded we were pressed for room in the garage and Mr. Loomis purchased the Spencer Trask house, a huge stone mansion with a tower, like an English country house, perched on the summit of one of the foothills of the Ramapo Mountains in Tuxedo Park. This he transformed into a private laboratory deluxe, with rooms for guests or collaborators, a complete machine shop with mechanic and a dozen or more research rooms large and small. I moved my forty-foot spectrograph from East Hampton and installed it in the basement of: the laboratory so that I could continue my spectroscopic work in a better environment..."
>
> Loomis, who was anxious to meet some of the celebrated European physicists and visit their laboratories, asked Wood to go abroad with him. They made two trips together, one in the summer of 1926, the other in 1928....†

After World War I, Alfred formed a lifelong business partnership with Landon K. Thorne, his sister Julia's husband. In the thirty-five years I was so personally close to Alfred, I met Landon Thorne on only two occasions. Alfred kept his business friends and his scientific friends quite separate. For a long time, he apparently reasoned that while his broad range of interests made both groups exceedingly interesting to him, the two disparate groups might not feel about

* At the present time, the word "supersonic" is reserved for the characterization of objects that move faster than the velocity of sound. The subject pioneered by Langevin, Loomis and Wood—sound waves with frequencies above the audible range—is now called "ultrasonics."

† William B. Seabrook, Dr. Wood, Modern Wizard of the Laboratory (New York: Harcourt, Brace and Company, 1941), pp. 213-17.

each other as he did about them. So Alfred had many business friends about whom I have heard in the greatest detail, but never met. As he grew older, Alfred's personal ties to the scientific world became the dominant ones, and I find that his last entry in *Who's Who in America* lists his occupation simply as "Physicist."

Alfred was proud of the fact that he and Landon Thorne were in many kinds of business deals, and in every one of them, they were equal partners. First of all, they had equal shares in the very profitable Bonbright and Co., the investment banking firm of which Landon was the president, and Alfred the vice-president. This firm was instrumental in putting together and financing many of the largest public utilities in the country.

The two partners also built a very innovative racing sloop of the J-class, which they hoped would win the right to race against Sir Thomas Lipton in one of his periodic attempts to capture the America's Cup from the New York Yacht Club. To cut down on wind resistance, the partners arranged to have most of the crew below decks at all times, working levers in the fashion of galley slaves, rather than hauling on wet lines on the deck. With the help of the MIT Naval Architecture Department, they did a thorough study of hull shapes, and there were several changes in the location of the mast—made of strongest and lightest aluminum alloy—during the test program. But in spite of all these efforts, *Whirlwind* wasn't a success. Perhaps the best indicator of Alfred Loomis' financial state at that time is that J-boats were then almost always built by "syndicates" of wealthy men such as the Vanderbilts. But in order to have complete control of their J-boat, Alfred and Landon paid for the whole project, 50-50 as always. After World War II, J-boats became too expensive even for syndicates of rich men, so the America's Cup races are now sailed in the smaller "12-meter" boats.

Another of Alfred and Landon's partnerships was the ownership of Hilton Head, an island off the coast of South Carolina. Hilton Head is now a famous resort area, with luxurious hotels and golf courses. But when Alfred and Landon owned it, it was completely rustic. They used it only for riding and hunting, and invited their friends to share the beauties of the place with them. They also owned a large oceangoing steam yacht, which they donated to the Navy at the start of World War II. I can count on the fingers of one hand the number of times I've seen Alfred's name in the public press—he believed that the ideal life was one of "prosperous anonymity." The first time I saw Alfred's name in print was when *Time* identified him as a "dollar-a-yacht man," one of several who had given their yachts to the Navy in return for a dollar. Recently, I've found in the library two old articles about Alfred. The first was a popular article on the unusual J-boat and its owners. The second was an article in the very first issue of *Fortune* concerning Wall Street firms, and telling of the great success of Bonbright and Co., its well-known president, Landon Thorne, and its shadowy and brilliant vice-president, Alfred Loomis, who kept in the background and planned their financial coups. According to the article, "Bonbright . . . rose in the twenties from near bankruptcy to a status as the leading U.S. investment-banking house specializing in public-utility securities."

Another joint endeavor was the Thorne-Loomis Foundation which sent ten boys at a time (2,000 in all) on six-week tours of industrial plants in special trucks, designed by Alfred.

When the *Fortune* article appeared, Alfred was leading a double life; his days were spent on Wall Street, but his evenings and weekends were devoted to his hilltop laboratory in the huge stone castle in Tuxedo Park. The laboratory was abandoned in November 1940, so those who worked in it could join the newly established MIT Radiation Laboratory which Alfred was instrumental in founding, and which reported directly to him, in his wartime role as head of the NDRC's Radar Division. I arrived at MIT at the same time, so I learned much about the Tuxedo Park laboratory from the young scientists and engineers who worked there throughout the year, and from the former laboratory manager, P. H. Miller. The following account of a laboratory I never visited is based on those recollections, and on stories I heard from older physicists who had been Alfred's guests during summers at Tuxedo, and finally on the countless reminiscences of Alfred and other members of his family.

Because of the strong influence of R. W. Wood, the laboratory concentrated at first on problems that interested him. As the quotations from his biography tell, the first major work was in ultrasonics. Loomis and Wood are still mentioned in the introductory chapters of textbooks on ultrasonics and sometimes referred to as the "fathers of ultrasonics." The field has grown enormously since they did their pioneering work, and it now has practical applications in industrial cleaning, emulsifying, and most recently in medical imaging, in place of X-rays when the required moving pictures would involve excessive radiation doses. Imaging ultrasonic scanners are now in common use to watch the motion of heart valves, to observe fetuses, and at the highest frequencies, they serve as high resolution microscopes.

A bound volume of the "Loomis Laboratory Publications" (1927-1937) includes reprints of sixty-six scientific papers, of which twenty-one were on ultrasonics; Alfred was a co-author of the first four, and of four later ones. The first is the classic 1927 paper by Wood and Loomis, some of whose results were described by R. W. Wood in the quotation above.

The laboratory was well equipped for work in Wood's specialty of optical spectroscopy. Ten papers in this field came from the laboratory, including one by Alfred and

George B. Kistiakowsky entitled "A Large Grating Spectrograph," which illustrates Alfred's talents as an innovative designer of precision mechanical devices. None of the spectroscopic papers bears Alfred's name; it wasn't in his nature to publish in a mature field. Although Alfred admired those who could do the involved spectroscopic analyses that came from his laboratory, he preferred to do the pioneering work in some new field. His admiration for the real professionals of this era is shown by the fact that he arranged a series of conferences in honor of visiting European physicists. Guests at the conferences were transported to Tuxedo Park in a private train, and entertained in lavish style at the laboratory. The *Journal of the Franklin Institute,* in the issue of April 1928, has a sixty-five page section entitled "Papers Read at a Conference in Honor of Professor (James) Franck, at the Loomis Laboratories, Tuxedo, New York, January 6, 1928." Included are papers by J. Franck, R. W. Wood, K. T. Compton, and several others.

I have no records of the other conferences, but Alfred once showed me the guest book from the laboratory. (It had just been returned to him by his son, Farney, when the latter had closed his "Loomis Laboratory" to join the Brandeis University faculty.) The book showed the names of most of the well-known American and European physicists of the period. On some occasions, a page with many famous names would be headed by the name of the man in whose honor the group had assembled. Most often such an honored guest was a visiting European physicist, for example, Einstein, Bohr, Heisenberg, or Franck.

Alfred's main interest at that time was in accurate timekeeping. The following quotation from R. W. Wood's biography will serve to introduce that subject:

> Wood's second trip abroad with Alfred Loomis was made in 1928. They called first on Sir Oliver Lodge, who presented each of them with an autographed copy of his latest book, *Evidence of Immortality....*

> One of the things Loomis hoped to obtain in England was an astronomical "Shortt clock," a new instrument for improving accuracy in measurement of time. It had a "free pendulum" swinging in a vacuum in an enormous glass cylinder—and was so expensive that only five of the big, endowed observatories yet possessed one. Says Wood:
>
> "I took Loomis to Mr. Hoke-Jones, who made the clocks. His workshop was reached by climbing a dusty staircase, and there was little or no machinery in sight, but one of the wonderful clocks was standing in the corner, almost completed, which made the total production to date six. Mr. Loomis asked casually what the price of the clock was, and on being told that it was two hundred and forty pounds (about $1,200), said casually, 'That's very nice. I'll take three.' Mr. Jones leaned forward, as if he had not heard, and said, 'I beg your pardon?' 'I am ordering three,' replied Mr. Loomis. 'When can you have them finished? I'll write you a check in payment for the first clock now.'
>
> Mr. Jones, who up to then had the expression of one who thinks he is conversing with a maniac, became apologetic. 'Oh, no,' he said, 'I couldn't think of having you do that, sir. Later on, when we make the delivery, will be quite time enough.' But Loomis handed him the check nevertheless."
>
> Back in America, they learned that Professor James Franck, Nobel prize winner, was coming over in January to give lectures at various universities. Wood suggested to Loomis that he hold a congress of physicists in his Tuxedo Park laboratory in Franck's honor. Franck accepted and the meeting was held in the library, a room of cathedral-like proportions, with stained-glass windows. Franck gave his first lecture in America there. Wood, Loomis, and others made subsequent addresses. The visiting American physicists were conducted through the laboratory and shown the supersonic and other experiments. The congress in this palace of science proved such a success that it was repeated the following year.*

Alfred's interest in accurate timekeeping probably resulted from his seagoing background, and his fascination with the art and science of navigation. He installed the three Shortt clocks on separate brick piers that were isolated from the laboratory structure, and extended down to bedrock. He was surprised to find that the clocks beat for long times in exact synchronism, and thought at first that they were locked together by gravitational interactions between the pendula.

* Seabrook, p. 221.

But he found that the coupling was through the bedrock, so the clocks were then placed at the corners of an equilateral triangle, facing inward, and the coupling was broken.

The Bell Telephone Laboratories had recently been developing quartz crystal oscillators with low temperature coefficients, and they came to surpass the Shortt clocks for short-term accuracy, but not for periods greater than a day. Alfred had a private line installed to carry the Bell oscillator signals to his horological laboratory, and he designed an ingenious chronograph to compare the timekeeping abilities of the Shortt pendulum clocks with the quartz oscillators. Since the first of these types was sensitive to gravity but the second was not, Alfred used his chronograph to demonstrate the expected but previously undetected effect of the moon on pendulum clocks. The observational data were accumulated by Alfred, but the data analysis required the services of a battery of "computers"—women who operated desk top computing machines, and whose salaries were paid by Alfred. The results of the analysis were published by Ernest W. Brown and Dirk Brouwer in a paper immediately following Alfred's "The Precise Measurement of Time," in the *Monthly Notices of the Royal Astronomical Society,* March 1931.

Alfred published several papers on biology and physiology with E. Newton Harvey and Ronald V. Christie. I never heard him speak of the physiological work, but he was obviously proud of the microscope-centrifuge he developed with Harvey. This was a typical Loomis "gadget" of the kind he enjoyed building all his life. The device made it possible for a biologist to watch for the first time the deformation of cells under high "g-forces." As Harvey and Loomis said in the introduction to their first paper on the subject,

> The previous procedure has been to centrifuge the cell in a capillary tube, remove it from the tube and observe it under a microscope to determine what happens. It would obviously be far better to observe the

> effect of centrifugal force while the force was acting . . . Our communication describes a practical means of attaining this end.*

In typical Loomis fashion, Alfred's name appears on only the first of thirteen papers on the microscope-centrifuge that are to be found in the collected reprints of the laboratory.

In the mid-thirties, Alfred turned his attention to the newly discovered brain waves. Berger had published his observations in the German literature, but American physiologists were unable to duplicate his results, and most of them apparently doubted the existence of the very low voltage signals that Berger described. From his contacts with industry, Alfred had available the best amplifiers, and he did his work inside "a screen cage," to eliminate interfering electrical noise. He had by this time retired from his Wall Street firm, and was devoting his full attention to his scientific work. For this reason, his name appears on all of the laboratory papers on brain waves, many of which were of great importance. His work erased any lingering doubts concerning the value of Berger's discovery; electroencephalograms are now used routinely in the diagnosis of epilepsy and many other diseases. In fact, one finds advertisements in magazines for "bio-feedback devices" that let the user observe his Berger "alpha waves," and learn to control them, "leading to greater creativity." (In kit form, $34.95.)

Alfred and his co-workers investigated many aspects of brain waves and did particularly important work with sleeping subjects that involved the abrupt changes in the character of the waves as the subject underwent "quantum jumps" in his "depth of sleep." It was then possible to tell precisely when a subject dropped from one of five states of sleep from which he could instantly be awakened by a small disturbing noise, into one in which he would fail to respond to the loudest

* E. Newton Harvey and Alfred L. Loomis, "A Microscope-Centrifuge," *Science,* 72 (1930):42-44.

noises that Alfred's high fidelity amplifiers could produce. (Alfred was one of the first "hi-fi buffs"; his homes were always filled to overflowing with a changing parade of the latest and most advanced high fidelity sound reproducing equipment. Avery Fisher and Alfred were close personally, and on at least one occasion, Mr. Fisher improved his superb product line with an idea that Alfred had devised to make the fidelity even higher.)

The only formal scientific talk I ever heard Alfred give was at the weekly Physics Department Colloquium in Berkeley, in 1939. He described his important brain wave experiments on sleeping, hypnotized, and blind subjects. My brief description of this work derives from my memory of Alfred's talk, but if space permitted, I could expand greatly on those observations. Henry Loomis' first important exposure to science came in those experiments, and he shared his experience with me on more than one occasion. (One of the papers lists a sixteen-year-old subject with the initials H.L.)

In 1939, Alfred's scientific interests changed drastically. He became deeply involved in Ernest Lawrence's projects and he shifted the emphasis of his own laboratory from pure science to war-related technology, by starting the construction of a microwave radar system to detect airplanes. The Sperry Gyroscope Company had bought an interest in the klystron patents that were owned by the Varian brothers, who invented the klystron, and Stanford University, which had supported the development work. Sperry built a small klystron plant in San Carlos, near Stanford, and their first customer was Alfred Loomis, who appeared, checkbook in hand, as he had years before at the small plant making Shortt clocks. (I'll temporarily interrupt this story to tell of Alfred's concurrent involvement with Ernest Lawrence's Radiation Laboratory.)

I was not surprised to meet Alfred in Berkeley, on his first visit to the Radiation Laboratory, in 1939. Francis Jenkins of the Berkeley Physics Department had spent a summer at Tuxedo as Alfred's guest, and he had told me in wide-eyed amazement about the fantastic laboratory at Tuxedo Park, and about the mysterious millionaire-physicist who owned it. Everyone who had submitted an article to the *Physical Review* in the depression years had received a bill for page charges together with a note saying that in the event the author or his institution was unable to pay the charges, they would be paid by an "anonymous friend" of the American Physical Society. There was of course no way to break the veil of secrecy surrounding the "anonymous friend," but "Pan" Jenkins told me in confidence that he felt sure that Alfred Loomis was the Society's benefactor. (That was a correct surmise.) Pan told me that Alfred was a wonderful person, but he didn't like the other residents of Tuxedo Park. He thought they were too "snooty," and looked down on the scientists as barbarians who "didn't even dress for dinner." (The gentlemen in Tuxedo Park followed the aristocratic British tradition of dressing for dinner in what most people would call tuxedo's, but which were called dinner jackets in Tuxedo Park.)

The relationship that quickly developed between Alfred and Ernest Lawrence had all the earmarks of a "perfect marriage"; they were completely compatible in every sense of the word, and their backgrounds and talents complemented each other's almost exactly. Ernest was a country boy from South Dakota who was the first faculty member of a state university to win a Nobel Prize. He had developed an entirely new way of doing what came to be called "big science," and that development stemmed from his ebullient nature plus his scientific insight and his charisma; he was more the natural leader than any man I've met. These characteristics attracted Alfred to him, and Alfred in turn introduced Ernest to worlds he had never known before, and found equally fascinating. Anyone who was in their company from 1940 until Ernest died in 1958 would have thought that they were lifelong intimate friends with all manner of shared experiences going back to childhood.

I was impressed by the way Alfred would seek out the younger members of the laboratory to learn everything he could about us and what we were doing and planning to do in our next round of experiments. I had never before had any serious discussions of physics with anyone as old as Alfred, and I was pleased that he liked to visit with me after I had taught a freshman class and was sitting out my required "office hour"—waiting to talk with the students who seldom came by. We talked a lot about physics, and found we were simpatico. He taught me an important lesson that I have put to good use in my life; the only way a man can stay active as a scientist as he grows older is to keep his communication channels open to the youngest generation—the front line soldiers.

Although Alfred's real mission in coming to Berkeley was to help Ernest raise the funds to build the 184-inch cyclotron, he also used the time to learn everything he could about cyclotron engineering and nuclear physics. I remember one occasion when I mentioned in passing that because of the war in Europe, the price of copper had risen to almost twice that of aluminum, for a given volume. Since aluminum had only 60 percent more specific resistivity than copper, I suggested to Alfred that aluminum might now be the preferred metal for the magnet windings of the 184-inch cyclotron. It seemed obvious to me, from elementary scaling laws, that an aluminum coil would be larger but would cost less. I had completely forgotten the suggestion, when a few days later, Alfred showed me a long set of calculations based on several altered designs of the 184-inch cyclotron that proved my snap judgment wrong. I came to appreciate for the first time the difference between the world of business, where a 20 percent decrease in cost was a major triumph, and the world of science, where nothing seemed worth doing unless it promised an improvement of a factor of ten. I hadn't done the calculations concerning the cyclotron cost because they obviously didn't permit a "large" savings in cost. But Alfred considered it worth a day or two of his time to see if he could cut the cost of the magnet windings by $50,000.

Ernest once told me of spending some time with Alfred in New York, after the Rockefeller Foundation had allocated $2.5 million to build the 184-inch cyclotron. Earlier, Alfred had been instrumental in securing the virtually unanimous backing of the "scientific establishment" for the proposal, thus relieving the Rockefeller Foundation of any necessity for acting as a judge between factions competing for the largest funds ever given to any physics project. So after acting as a senior statesman in the worlds of science and philanthropy, Alfred was ready to help Ernest obtain the best possible bargains in the purchase of iron and copper for the giant cyclotron. Ernest recalled that after spending some time with the Guggenheims, during which a favorable price for copper was negotiated, Alfred said, "Well, now we have to go after the iron. I think Ed Stettinius is the right man." (Stettinius was then Chairman of U.S. Steel, and later Secretary of State.) Ernest was impressed when a call was put through and Alfred said, "Hello Ed, this is Alfred. I have someone with me I think you'd like to meet. When can we come over?" They were soon in Mr. Stettinius' office, and shortly after Ernest had given him a pitch on the great cyclotron, Ernest and Alfred were in the latter's apartment celebrating their success with a drink.

In early 1940, Alfred was back in Berkeley, and he told me that his next big project was to arrange for the funding of Enrico Fermi's embryonic plans to build a nuclear chain reactor. I hadn't given any thought to the problems involved in designing or building such a device, so everything Alfred told me was most interesting. But Alfred's involvement in reactors was cut short in the summer of 1940 by the dramatic appearance in Washington of the "Tizard Mission." The purpose of this group of visiting British scientists was to enlist the help of the United States in developing and building the new devices needed to meet the military requirements of a war that had become technologically oriented to a degree quite unappreciated by our military-industrial-scientific establishment. As an example, radar had been invented independently in the United States by the Navy and the Army, and in England by Robert Watson-Watt. The U.S. military departments treated the subject with such excessive secrecy that no "outsiders" learned of it. Since the outsiders were the real professionals in radio engineering, they were the ones who could have developed American radar into the useful military tool that the insiders didn't manage to achieve. (The dismal state of U.S. radar was demonstrated at Pearl Harbor, a year and a half after the Tizard Mission had revealed all the British successes to the U.S. armed forces.)

The world now knows that the operational success of the long wave British radar was the foundation on which the RAF triumphs of the Spitfire and Hurricane pilots were based. A second generation of VHF radar, in the 200-megahertz (1.5-meter) band, could be fitted into planes to turn them into night fighters and anti-submarine patrols. Everyone agreed that microwave radar in the 3,000-megahertz (10-cm) band would be vastly superior to the 1.5-meter equipment then available. But there appeared to be little chance that a powerful generator of such pulsed microwaves could be developed.

When Randall and Boot made their breakthrough with the cavity magnetron in Mark Oliphant's laboratory in Birmingham, it was suddenly clear that microwave radar was there for the asking, but Britain had no spare "bodies" who could be asked to do the development—everyone with applicable skills was working at breakneck speed on the immediate problems of a desperate war that could be lost any day by the starvation of the submarine-blockaded British people. So, in a great and successful gamble, Winston Churchill made the decision to share all of his country's technical secrets with the United States, in the hope that the potential gain would offset the loss in compromised security. Sir Henry Tizard was sent to Washington with a committee of experts, including such luminaries as Sir John Cockcroft, to brief their American counterparts on all aspects of the scientific war.

Alfred Loomis was included in the briefings not only because of his unique position in the scientific establishment, but because his laboratory had built one of the two microwave radar sets then existing in the United States. Both were based on the klystron tube recently invented by the Varian brothers at Stanford University, and both were "continuous wave Doppler radars" of the type now used by police departments to apprehend speeders. William Hansen, who designed the first of these microwave radar sets, attempted for the next few years to find a wartime niche for such a device, but without much success. Alfred immediately sensed the great superiority of the pulsed microwave radar devices that could be based on the new magnetron, so he dropped his work on the klystron-powered radar set, and devoted all his energies to pulsed microwaves for the next five years. But his klystron radar could detect planes, as he demonstrated to the "founding fathers" of the MIT Radiation Laboratory in the winter of 1940—in fact, it was the first working radar set that any of us had ever seen. But immediately after that demonstration, it was junked.

The Tizard Committee spent some time in Tuxedo Park as Alfred's guests, and on that occasion, Alfred brought a number of friends, including Ernest Lawrence, into the newly formed Microwave Committee of the fledgling National Defense Research Committee (NDRC) which had just been established by President Roosevelt on the advice of Vannevar Bush. Alfred was chairman of the Committee which took the responsibility for establishing the MIT Radiation Laboratory, one of the world's most successful scientific and engineering undertakings. Alfred made the arrangements with industry for equipping the laboratory with the necessary hardware to make several flyable night-fighter intercept radar sets, and Ernest Lawrence took the responsibility of staffing the laboratory, mostly with young nuclear physicists. (The Tizard Mission suggested this, because the British had found nuclear physicists to be more quickly adaptable to a radically new set of "ground rules" than were professional radio engineers.) Lawrence persuaded Lee DuBridge to become the director of the new laboratory, and that was a most fortunate choice. He also traveled all over the country, recruiting his former students and their colleagues from the cyclotron laboratories they had modeled after his own, and he didn't spare his own laboratory; Edwin McMillan, Winfield Salisbury and I all rushed off to Cambridge in November of 1940, and didn't return to Berkeley for five years.

But this is the story of Alfred Loomis, and not that of his friends, nor of the great laboratory he founded and guided so successfully with a loose rein. So I will single out from the many successes of the laboratory only two projects, one invented by Alfred, and the other invented by Lawrence Johnston and me, but in which Alfred played a key role. The first was Loran (for Long Range Navigation), which was of great importance during the war, and is still a major navigational aid in use all over the world. Loran is a pulsed, "hyperbolic system," and in its original form, made use of Alfred's great store of knowledge about accurate timekeeping. In fact, the Loran concept of a master station and two slave stations can be traced to the Shortt clocks, which had a master pendulum swinging in a vacuum chamber, and a heavy-duty pendulum "slaved" to it, oscillating in the air.

To obtain a navigational "fix" with Loran requires the measurement of the time difference in arrival of pulses from two pairs of transmitting stations. Each such time difference places the observer on a particular hyperbola. The observer's position is fixed by the intersection of two such hyperbolas, each derived from signals originating from a pair of long wave transmitting stations. It is common for a Loran fix to derive from only three transmitters, with the middle one serving as a member of two different transmitter pairs. All of the wartime Loran stations operated at the same radio frequency, and different pairs of transmissions were distinguished by characteristic repetition rates for their pulses. The techniques for separating the signals and for measuring their differences in arrival time were "state of the art" at that time, but the problem of synchronizing the transmissions to within a microsecond, at points hundreds of miles apart, was a new one in radio engineering. Alfred proposed the following solution: the central station was to be the master station, and its transmissions were timed from a quartz crystal. The other stations also used quartz crystals, but in addition, monitored the arrival times of the pulses from the master station. When the operators noted that the arrival time of the master pulses was drifting from its correct value, relative to the transmitting time at that particular "slave station," the phase of the slave's quartz crystal oscillator was changed to bring the two stations back into proper synchronization. This procedure was able to bridge over periods when the signals at one station "faded out," and it was also what made Loran a practical system during World War II, rather than an interesting idea that would have to await the invention of cesium beam clocks, which were introduced in the 1950's.

The second project of interest in this biographical sketch is Ground Controlled Appraoch (GCA), the "radar talk-down system for landing planes in bad weather." The basic idea behind GCA came to me one day in the summer of 1941 as I watched the first microwave fire control radar track an airplane, automatically, from the roof of MIT. It occurred to me that if a radar set could track a plane accurately enough in range, azimuth and elevation to shoot it down, it could use that same information to give landing instructions to a friendly plane caught up in bad weather.

Starting from that simple concept, my associates and I, with strong backing from Alfred, showed that the technique would work if the radar set gave angular information that was as reliable as the optical information we used in our tests. We had to wait several months for the radar set to become available for landing tests, but in one early demonstration, the radar did track several planes successfully as they executed their approach and landing. But in the scheduled radar tests, the equipment was found to be quite unable to track planes near the ground; it would suddenly break away from the line of sight to the plane, and point instead down at the image of the plane, reflected in the surface of the ground.

At the conclusion of this disastrous set of tests, Alfred invited me to have dinner with him in his suite at the Ritz Carlton in Boston and he did an amazing job in restoring my morale, which was at its lowest ever. He said, "We both know that GCA is the only way planes will be blind-landed in this war, so we have to find some way to make it work. I don't want you to go home tonight until we're satisfied that you've come up with a design that will do the job." We both contributed ideas to the system that eventually worked, and that involved a complete departure from all previous antenna configurations. I'm sure that had it not been for Alfred's actions that night, there would have been no effective blind landing system in World War II, and many lives would have been lost unnecessarily. I would have immersed myself in the other interesting projects that concerned me, and would soon have forgotten my disappointment and my embarrassment.

Alfred played another interesting role in GCA by ordering ten preproduction models of the embryonic device we had invented at the Ritz-Carlton from a small radio company on the West Coast. He did this for two reasons: in the first place, the laboratory had failed badly in transferring its first airborne radar set to industry for production. The industrial engineers predictably developed a bad case of NIH (Not Invented Here), and promptly decided that everything had to be re-engineered. The final product came out so late and was so heavy that it never saw any action. Because of that experience, Alfred and Rowan Gaither (later the first president of the Ford Foundation) set up the "Transition Office," whose job was to avoid the problems mentioned above. Rowan became head of the Transition Office, and GCA was selected as the first test case of the new technique. Its basic idea was that a company would be selected to produce a new radar set before the original ideas had been worked out in any detail. The chief engineer of the designated company, plus a few of his assistants, would come to the laboratory and participate in the design and testing of the new device, as members of an MIT-company team. In this way, when they returned to their factory to produce the device, everything in it would be "our ideas" and "our design." The Transition Office was a spectacular success, and in the process, Rowan Gaither became extraordinarily close, personally, both to Alfred and me.

The second reason that Alfred ordered the ten preproduction sets, using NDRC-OSRD funds, was that the Army and Navy as well as the RAF had all said, independently, that their pilots would "never obey landing instructions from someone sitting in comfort on the ground," and that they would con tinue pressing for something like the ILS (Instrument Landing System) that is now in general use throughout the world. Alfred was confident that as soon as the three services saw GCA work, they would immediately accept it, and want working models to test, "yesterday."

After some very successful tests at Washington National Airport, in which high service officials watched pilots land "under the hood," when those pilots had never even heard of the system until after they were in the air, there was a rush to order several hundred GCA sets. When the three services learned that NDRC had ten sets almost built, they called a meeting at the Pentagon to allocate them for tests in this country and in England. Alfred was invited, and he asked me to sit in. Neither of us said a word as the admirals, generals, and air marshalls engaged in a horse-trading session that ended up with all ten sets allocated to the services, and none to MIT or to the NDRC. The meeting was about to break up when Alfred said quietly, "Gentlemen, there seems to be some misapprehension concerning the ownership of these radar sets; it is my understanding that they belong to NDRC, and I am here to represent that organization." His training as a lawyer was immediately apparent, and after he had shown in his gentle manner that he held all the cards, an allocation that was satisfactory to all concerned was quickly worked out. And NDRC even ended up with one of its own GCA sets!

At the end of the war, Ernest Lawrence was asked for his evaluation of Alfred's contribution to radar, and he had this to say:

> He had the vision and courage to lead his committee as no other man could have led it. He used his wealth very effectively in the way of entertaining the right people and making things easy to accomplish. His prestige and persuasiveness helped break the patent jams that held up radar development. He exercised his tact and diplomacy to overcome all obstacles. He's that kind of man. I've never seen him lose his temper or heard him raise his voice. He steers a mathematically straight course and succeeds in

> having his own way by force, logic and by being right. I am perfectly sure that if Alfred Loomis had not existed, radar development would have been retarded greatly, at an enormous cost in American lives.*

Alfred's other important role during the war is so little known that its only mention in print is in a brief obituary notice I wrote for *Physics Today.* Many authors have commented on the remarkable lack of administrative roadblocks experienced by the Army's Manhattan District, the builders of the atomic bombs. In my opinion, this smooth sailing was due in large part to the mutual trust and respect that Secretary of War Stimson and Alfred had. Alfred was in effect Stimson's minister without portfolio to the scientific leadership of the Manhattan District—his old friends Ernest Lawrence, Arthur Compton, Enrico Fermi, and Robert Oppenheimer. Alfred maintained a hotel room in Washington throughout the war, which his friends used when they couldn't find other accommodations, and one of the reasons for this was so that he could be available to talk with the Secretary on short notice. Alfred was also a member of a small committee set up by the Secretary to advise him concerning the V-1 and V-2 weapons being developed by the Germans, and just coming to the attention of military intelligence. At the committee's suggestion, the V-1 menace was largely blunted by a combination of the SCR-584 developed in Alfred's laboratory, an advanced computer developed by the Bell Telephone Laboratory, the proximity fuses developed by Merle Tuve and his associates working under NDRC sponsorship, and the Army's anti-aircraft guns. The V-2 rockets could not be defended against, and the committee recommended the only course of action possible, and the one that was followed—capture of the firing sites.

Toward the end of the war, Alfred was able to relax for the first time in five years, and he concurrently made an

* "Amateur of the Sciences," *Fortune,* 33(March 1946): 132-35.

important change in his personal life. He and Ellen were divorced, and he married Manette Seeldrayers Hobart. They had an extraordinarily happy time together during the final thirty-two years of Alfred's life. His lifestyle underwent a dramatic change from one of multiple homes staffed by many servants to a very simple one, in which he and Manette cooked dinner every evening in East Hampton, side by side in the kitchen. Alfred designed a special rolling cart that brought the food to one end of the table, where he and Manette sat opposite each other, and served themselves from the cart. If there were guests, the plates were passed down each side of the table to them, from the cart. This new style of servantless elegance was written up in a magazine devoted to "good living."

Alfred's principal scientific interests changed at this time from the physical to the biological. As an example, I've mentioned his contributions to research on hydra. In that period, one of the bathrooms in his Park Avenue apartment was filled with petri dishes containing hydra. Alfred spent hours each day examining the hydra under a microscope, and comparing his observations with those of his son, Farney. He and Farney organized small meetings to which they invited specialists in subjects about which they wished to learn more. As in the old Loomis Laboratory days, the invitations included first class round trip transportation, plus luxurious living at the resort hotels where the meetings were held.

Alfred enjoyed introducing his scientific friends to the pleasures that are normally known only to the very wealthy. For many years, he and Manette visited California each spring, and invited several couples from Ernest Lawrence's laboratory to be their guests at the Del Monte Lodge at Pebble Beach, and to play golf at the Cypress Point Golf Club. In later years, the Loomises spent their winters in Jamaica, where their friends were invited, a week at a time, to share with their hosts the sun, the beach, and good food and good conversation. As often happens with men as they grow older, Alfred's circle of closest friends shrank to those he called "my other sons." I was fortunate to be included, along with John S. Foster, Jr., Walter O. Roberts, Ronald Christie, and Julius A. Stratton. Had Ernest Lawrence and Rowan Gaither outlived Alfred, they would have continued to visit the Loomises each winter in Jamaica, as members of the "other sons."

I can think of no better way to end this biographical memoir than by quoting the last paragraph of my *Physics Today* obituary:

> For those of us who were fortunate to know him well, he will be remembered as a warm and wise friend, always interested in learning new things. I was his guest for three days in May of this year, and what he most wanted to learn from me concerned programming tricks for the Hewlett-Packard model 65 hand-held computer that was his constant companion. I think it most fitting that my last visual memories of this renaissance man, whose life encompassed and contributed much to the electronic age, should have him operating a hand-held electronic computer containing tens of thousands of transistors.*

* Luis W. Alvarez, "Alfred L. Loomis" (obituary), *Physics Today,* 28(11):84-87.

HONORS AND DISTINCTIONS

Honorary Degrees

D.Sc., Wesleyan University, 1932
M.Sc., Yale University, 1933
LL.D., University of California, 1941

Awards and Medals

Wetherhill Medal of Franklin Institute, 1934
Medal for Merit, 1948
His Majesty's Medal for Service in the Cause of Freedom, 1948

Boards of Trustees

Massachusetts Institute of Technology (Life Member)
Carnegie Institution of Washington
Rand Corporation
Research Corporation
New York Hospital

Scientific Societies

National Academy of Sciences
American Philosophical Society
American Physical Society
American Chemical Society
American Association for the Advancement of Science
American Astronomical Society
Audio Engineering Society
Institute of Electrical and Electronic Engineers
Royal Astronomical Society

Administrative Posts

Chief, National Defense Research Committee, Division 14 (Radar)
Director, Loomis Laboratories
President, Loomis Institute for Scientific Research
Vice President, Bonbright and Company

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With H. T. Stetson. An apparent lunar effect in time determinations at Greenwich and Washington. Mon. Not. R. Astron. Soc., 93:444-48.

1935 With E. Newton Harvey and Garret Hobart. Potential rhythms of the cerebral cortex during sleep. Science, 81:597-98.

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Howard Percy Robertson

January 27, 1903-August 26, 1961

by Jesse L. Greenstein

Howard Percy Robertson, one of the most original workers in relativity and cosmology, was born to George Duncan Robertson and Anna McLeod in Hoquiam, Washington, January 27, 1903. He died of a pulmonary embolism, after injury in a minor automobile accident, on August 26, 1961. To his many friends he was, and still is, "Bob," a warm memory of a good and great man, a patriot, and a scientist. At the height of his scientific productivity in 1939, he turned his attention to the military application of science and mathematics. He never fully cut his ties to such national and international service. He served both as Chairman of the Defense Science Board and as Foreign Secretary of the National Academy of Sciences, in his last year, while still lecturing on general relativity as Professor of Mathematical Physics at the California Institute of Technology. His public service may have reduced his scientific output, but his two lives together made him a complete and remarkable man, both admired and loved.

On his death in 1961, Detlev Bronk sent the following message to Bob's wife Angela:

> Distinguished scientist, selfless servant of the national interest, courageous champion of the good and the right, warm human being, he gave

richly to us and to all from his own great gifts. We are grateful for the years with him. We mourn the loss of his presence but rejoice in the legacy of his wisdom and strength.

BEGINNINGS

His family was middle-class, and his father, descended from a Scottish family of Maryland, became a well-loved county engineer, building bridges in a wide area of rural Washington. His mother, also of Scottish descent, attended Johns Hopkins and became a nurse. She was widowed and left with five children. Bob and his father had been very close, and Bob remained close to his MacLeod grandmother. Bob, only fifteen, was the oldest. Bob's mother became the local postmistress and was active in politics. Although Bob worked to help his mother support the family, he graduated from the University of Washington in 1922 and took a master's degree in 1923. All the children attended the University. He lived in a small lumber town, Monteseno, somewhat excluded by work from the normal youthful fun of university life. But in that same year, 1923, he married Angela Turinsky of Sandpoint, Idaho, the daughter of a captain in the Austrian Army who was by then a landscape architect in Idaho. She was born in Budapest, worked her way through the Idaho State Normal School, and had taught in a one-room schoolhouse before she became a student of philosophy and psychology at the University of Washington.

Bob's studies soon turned from engineering to mathematics and physics under the strenuous influence of the mathematician E. T. Bell and the University of Washington physicists. His relation with Bell was, and remained, a stormy one. Bell pressed him to take a graduate course by correspondence from the University of Chicago and helped him to find his real challenge by urging him to enter graduate work at the California Institute of Technology (Caltech).

After a few years of Bob's Caltech career, Robert A. Millikan brought his teacher, Bell, to Pasadena from Washington. Throughout their lives, and in spite of intense and clashing personalities, the relationship between them was deep. In his old age and illness, Bell was cared for daily by Angela and Bob, until Bell moved to his son's hospital. (Taine Bell was a physician in Watsonville, California.)

CAREER POSITIONS

From 1927 to 1929, Bob held the position of Assistant Professor of Mathematics. Between 1929 and 1947 he was Assistant, Associate, and then full Professor of Mathematical Physics at Princeton, with a sabbatical in 1936 at Caltech. After World War II, he became Professor of Mathematical Physics at Caltech (1947-1961). But as early as 1939, under the urging of Richard Tolman, he began to concern himself with what later became Divisions of the National Defense Research Committee and the Office of Scientific Research and Development (OSRD) (1940-1943). He was Scientific Liaison Officer of the London Mission of the OSRD (1943-1946) and Technical Consultant to the Secretary of War. In 1945 he was Chief of the Scientific Intelligence Advisory Section of the Allied Forces Supreme Headquarters. He received the Medal of Merit in 1946 for his contributions. From 1950 to 1952 he was Director of the Weapons Systems Evaluation Group for the Secretary of Defense, while continuing to teach relativity at Caltech. Another stay in Europe, as Scientific Advisor to the NATO Commander, occupied 1954 to 1956. After returning to Caltech he was Chairman of the Defense Science Board and member of the President's Scientific Advisory Committee. The strength of mind and body this career required was matched by his versatility. His wit, kindness, and ability to deal with all kinds of people survived the strain of these and the many other responsibili ties now buried in the history of the enlistment of science in the art of war. I will discuss his scientific career separately, but when we see that most of his publications considerably predate our entry into the war, we must recognize how great a loss to science was his career of public service.

MATHEMATICS, PHYSICS, AND THE UNIVERSE

Robertson's scientific contributions were largely derived from his interest and ability in differential geometry and group theory, which he applied to atomic physics, quantum physics, general relativity, and cosmology.

In 1925 Bob received his Ph.D. from Caltech and a National Research Council Fellowship to Göttingen, 1925-1928, which included a half-year at Munich. As a mathematical physicist in Germany, he met D. Hilbert, R. Courant, K. Schwarzschild, J. von Neumann, E. Wigner, E. Schrödinger, W. Heisenberg, and A. Einstein, and worked with some of them. The transition from Bell, Brakel, and Utterbeck at the University of Washington, through Caltech to Göttingen, meant a transition from engineering through pure mathematics to applications of mathematics in the "new" atomic, quantum, and relativistic physics. In this pursuit Bob had energy without bounds and a sense of involvement with the history of philosophy and science. Although capable of mathematical elegance, he worked through in detail solutions of some of the first classic, difficult problems of relativistic mechanics. His scientific work evolved parallel to his career. Although a student in mathematics, at Princeton he was in both the physics and mathematics departments. As a Caltech physicist he advised several generations of observing astronomers at the Mount Wilson and Palomar Observatories on the critical tests of relativistic cosmology, as had Tolman. Tolman and Robertson had the clarity of mind that permitted them to translate abstract mathematical concepts into terms physicists and astronomers could understand.

At Caltech he had a wide variety of friends such as Paul Epstein, Graham Lang, Willy Fowler, Ira Bowen, Todor von Karman, and, naturally, Bell. The early years in Göttingen and Munich in the great period brought fruition to his graduate study of differential geometry. Much influenced by Weyl, with whom he worked, he translated Weyl's *Theory of Groups and Quantum Mechanics* in 1931. His bibliography from 1924 to 1929 includes differential geometry, the theory of continuous groups, atomic and quantum physics, and general-relativistic cosmology. In Göttingen he was a good enough mathematician to impress Courant and, to quote Bob, "even Hilbert." American science and scientists had not yet attained international prestige, but Bob learned German well enough for student life and could even make a sufficiently elegant German pun to be printed in *Simplicissimus.* As if this was not enough the student's life, he rolled a barrel of beer through cobbled Munich streets at 2:00 A.M. and thus earned a police citation for "disturbing the citizenry." About this time he became friends with von Neumann and with Martin Schwarzschild (son of the relativist Karl) and later was instrumental in bringing von Neumann and Wigner to Princeton University.

Along with physicists like Heisenberg and Max Born, Bob had a short but important involvement with the growth of quantum theory, especially in the relation of quantum mechanics to the theory of groups, their representations, and commutation operators. The Göttingen period gave him an excellent knowledge of quantum physics, but relativity theory and its applications had the stronger, longer impact. At Princeton he had a long contact with Einstein. Bob's realistic philosophy, in spite of his mathematical skill, made him skeptical of those who "thought they could invent the universe out of their own head." Bob loved mathematics mainly for its application to physical problems.

In relativity he found his life work. He lectured on it for years; I have seen and studied some of his lecture notes, continually revised, modernized, and made more elegant. Pages of a detailed derivation in colored ink were refined to a few lines. One of his last, students, Thomas W. Noonan, prepared these notes as a book, *Relativity and Cosmology,* published in 1968. His discovery of the (first order) theory of the linear cosmological redshift dates from 1928. The creators of special and general relativity theory were faced not only by an immediate hostile reception, but also by a fundamental uncertainty intrinsic to the theory. Its application to the enormous real universe (of which our knowledge was and still remains so limited), required simplifications. Large-scale homogeneity and isotropy of the unknown are postulates. Progress requires some postulate of the uniformity of the universe of matter and space-time, called the "cosmological principle." A possible nonzero cosmological constant, which Einstein introduced as a complication into the field equations, took the form of a cosmic repulsion of unknown magnitude. In the theory of gravitation, the interaction of matter with the geometry of space occurs in the form of singular points (matter) imbedded in a curved space-time whose metric properties are to be determined. The propagation of a photon in this is along a minimal path, a geodesic. The solution for an empty universe could be static (W. de Sitter). In 1928 and 1929 Robertson developed fully the "postulate of uniformity" so as to obtain the complete family of line-elements from the theory of continuous groups in Riemannian space. These Robertson-Walker cosmological spaces are still fundamental; A. G. Walker rediscovered them in 1936, and W. Mattig studied their further consequences in 1957 and 1958. These metrics have a line-element and a geometry which is homogeneous and isotropic in space but which changes in time at a rate to be determined from physical considerations rather than symmetry arguments.

The early years of relativistic cosmology were marked by a great uncertainty: was the universe static or expanding (W. de Sitter, Hermann Weyl, A. Friedman, the Abbé G. Lemaître, K. Schwarzschild)? With a nonzero cosmological constant the universe may be stationary but is not static. Dynamic (expanding) universes, with zero cosmological constant, were possible and could be finite or infinite, and of positive or negative curvature. Knowledge, however, is limited to a sphere of finite radius; i.e., there is an event horizon. The *Review of Modern Physics* article in 1933 is a classical presentation of the problem and its solutions. With the assumed overall uniformity, Robertson's line-element depends on the local behavior of matter. "This rawest of all possible approximations may be considered as an attempt to set up an ideal structural background on which are to be superimposed the local irregularities due to the actual distribution of matter and energy in the actual world." The detailed working out of the consequences requires the close interplay of mathematics and physics. In 1933 he solved the field equations using the cosmological principle and mathematical ingenuity. His exact solution of the two-body problem, including the advance of the perihelion of an eccentric planetary orbit, has stood the test of time.

The final observational tests of Robertson's expressions have not yet been made in the larger universe. Such cosmological tests (by Allan Sandage and others) are major goals in the observation of galaxies, radio galaxies and quasars by the largest radio and optical telescopes. The first observational test involves the possible nonlinearity (after suitable correction) of the relation between the apparent brightness and redshifts. At present, other less practical tests involve the number of objects at a given brightness (the number-flux relation found by radio astronomers) and the apparent diameter-redshift relation, all produced by non-Euclidean departures from the metric. For successful application, the evolution of brightness and size of galaxies in earlier phases of their history (at the "look-back time") is needed. For the deceleration parameter, nonlinear effects could appear significant at observable values of the redshift when we understand all evolutionary effects.

Robertson's interest in the prediction of these effects led him (1928) to predict a linear redshift-apparent magnitude (i.e., brightness) relation and even to plot the first such diagram from the sparse available data. Edwin Hubble, in 1929, independently discovered this relation, central to the observational approach to cosmology. Later followed cooperation between Tolman and Hubble in the early days of the observation of the expanding universe. With the 100-inch telescope and ordinary galaxies, Hubble was active when the observations reached out to 13 percent redshift; Milton Humason found objects at 20 percent, with the 200-inch. In 1956 Robertson took an active interest in the discussion of the redshift results of Humason, Nicholas Mayall, and Sandage at Mount Wilson, Palomar, and Lick. The discoveries of radio astronomy further enlarged horizons, and a galaxy at 46 percent redshift was found by R. Minkowski. Galaxies to over 60 percent redshift have since been detected. Sandage and others are searching for still more distant galaxies. The quasars (perhaps themselves symbols of a relativistic collapse or singularity) have been traced to over 350 percent redshift, but seem too variable in intrinsic luminosity to be as useful in determining cosmological parameters.

In 1953, Bob's paper discussing tests of cosmology on an "elementary" level characteristically issued from the California Institute of Technology and Supreme Headquarters Allied Powers in Europe. When Bob was working in Washington and Paris and also teaching in Pasadena, he still discussed consequences of evolutionary changes and pro posed new tests of cosmologies and the still very weak evidence for nonlinearity in the brightness-redshift relation with observers at Mount Wilson and Palomar.

The complexity of the evolution of the brightness and colors of stars is compounded in predicting the global brightness and color of a hundred billion stars, as they are born and evolve. Galaxy observations look back halfway in time to the "beginning," and quasars to 90 percent. When the universe was younger and denser, galaxies probably interacted more, i.e., have not always been closed systems but may have grown in mass. The stellar part of galaxy evolution can be modeled, but the model for galaxy growth is new and only partly studied. A major novelty in observational cosmology that would have given Bob a special pleasure is the radio-frequency discovery of the 2°7 K all-pervading isotropic, background radiation, greatly redshifted evidence of the cosmic fireball soon after the beginning. This radiation was implicit in the work of Bob's friends, the Abbe Lemaître, and George Gamow; Gamow, R. A. Alpher, and Robert Herman predicted a nearly correct value, but it then seemed unobservable. The other major problem that has surfaced in relativity is the existence of singularities, discussed by Karl Schwarzschild. Now we are beginning with some confidence to study less-than-cosmic-scale singularities—black holes—by their effect on nearby matter. Galaxies, quasars, globular clusters, and even stars seem to be scenes of violent energy releases connected with fall into a singularity. Such complications make the straightforward answer to the "cosmological question" more remote, but present fascinating byways. A central question for observational cosmology is whether the second-order term in the expansion is positive, zero, or negative. This depends fundamentally on the density of matter; if the expansion is to be stopped, we must find some twenty times the matter that we now know. If the origin of inertia is the existence of an external universe, the latter must also be more massive than we think. Theoretical general relativity and cosmology are in full flower (partly based on the evidence of the violence of events) and in many areas still rest on Robertson's work. Among his unpublished works Noonan lists: rigid body motion in special relativity, a study of Gödel's model, orbits around a variable mass, oscillation through a Schwarzschild singularity, and second-order plane gravitational waves.

Robertson's attention was not limited to tests of general relativity at the cosmological level. He was equally interested in solar-system tests of general relativity. Following the 1922 work of Arthur Eddington, no one had more to do in the early days than Robertson with developing a so-called "parametrization" of the spherically symmetric geometry about a center of attraction, to test general relativity by comparing its predictions with those of conceivable alternative theories of gravitation. In contrast to the line-element derived by Karl Schwarzschild from Einstein's standard general relativity for this geometry, Robertson analyzed a generalization of this geometry characterized by three disposable parameters. In Robertson's time and subsequently, and especially actively today, with the help of satellites and radar limits of greater and greater stringency are being placed on the departures of these three parameters from their Einstein values. One type of test has to do with the advance of the perihelion of an eccentric planetary orbit, especially the advance of the perihelion of Mercury. A second has to do with a precession of the local inertial frame of a small body in free orbit around the sun; this precession is predicted to have approximately three times the Newtonian value. It could be measured by a gyroscope in an artificial satellite. A third conceivable departure from Einsteinian predictions can be tested by the gravitational redshift of the photons of a light-ray. The accuracy of Mössbauer effect measurements of gamma rays confirmed this predicted redshift over a height difference of only 25 meters. It is also confirmed for white dwarf stars with lower accuracy, where it amounts to 0.02 percent. A fourth potential departure from Einsteinian predictions can be determined in principle by measuring the deflection passing the limb of the sun. This effect is of special interest because the Einstein value is twice the Newtonian value. Radio frequency observations of the apparent position of small radio sources as the sun passes near them have amply justified this prediction to one percent, and the less accurate stellar optical observations agree. Today at least ten effects are known that also allow tests of Einstein's theory and of such imagined variants from it as are describable by Robertson's now famous three parameters. The tests steadily improve in precision as the sophistication of measuring equipment increases. Clearly there will never be a last test. Will there ever be a first test to show a discrepancy?

Quite another relativistic effect interested Robertson and is the focus of attention in an ambitious experiment under preparation by Francis Everitt and William Hamilton. It concerns the Einstein-Mach theory of the origin of inertia. The Earth's rotation is expected to cause the rotation of the inertial frame—and the axis of a spin of a gyroscope in polar orbit around the Earth—by the fantastically small amount of about 0.1 seconds of arc per year.

Robertson delighted to talk also about what others have called the Poynting-Robertson effect. It has nothing to do with general relativity. However it also causes a departure of an orbit from the Newtonian prediction of constant radius. A small dust particle in orbit around the sun is constantly scattering sunlight. More photons are sent in the direction of travel than against it. In consequence, the particle suffers a small but significant backward push. This takes angular momentum away from the particle; therefore, the particle spirals inward toward the sun.

In cosmology, after the realization that the universe was expanding, i.e., nonstatic, and probably nonstationary, and contained material test points (galaxies), the important further step was to evaluate crucial tests of general relativity that might be supplied by observation. Robertson's papers from 1938 to 1940 were supplemented by detailed studies of methods of comparison with observation (1955).

A beautiful summary of the effects of general relativity and curved space in 1949 ("Geometry as a Branch of Physics") uses only school mathematics. He discusses the effects of space curvature on astronomical observables and says, "The success of the general relativity theory of gravitation as a physical geometry of space-time is attributable to the fact that the gravitational and inertial masses of any body are observed to be rigorously proportional for all matter." Note his characteristic approach: a test by external reality. He continued to study attempted revisions, Leigh Page's or E. A. Milne's static solutions and Fred Hoyle's steady-state solution, all with skepticism. He emphasized the basic importance of the Michelson-Morley and Ives-Stillwell experiments in reducing the number of postulates required.

No contribution that Robertson made to physics and astronomy is of more enduring importance than the geometrical line-element. A local space-time interval is a general mathematical expression with a meaningful separation of time-like and space-like coordinates. The "postulate of uniformity" was a necessary first approximation, stating in essence that matter and energy in the universe had no preferential axis and were on a large scale homogeneous and isotropic. The general class of Robertson-Walker line-elements predicts a first-order term, the expansion rate; a second-order term, the deceleration or acceleration of the expansion; and another second-order term representing the space curvature.

In this memoir I have not attempted to separate Bob's contributions at Princeton from those at Caltech. His greatest contributions to general relativity were made at Princeton University, and his greatest impact on astronomy at Caltech. I first met him when I was a Harvard graduate student attending his series of lectures at the Harvard College Observatory summer school in 1937. The lectures were unforgettable, as was his personality. One hot summer Sunday my wife and I managed to buy him bathing trunks still many inches too small for his massive frame. He talked our way into a private beach club on the North Shore. There we enjoyed a picnic, drinking wine which was a sudden gift from newfound friends. Bob later invited us to Princeton, where like so many people my wife and I were immersed in the Robertson household, near-neighbors of whom were the Johnny von Neumanns, and others of the influx of scientists from Hitler's Europe. Professor Hubert Alyea of Princeton recalls one such evening party at the Robertson home where Herman Weyl, John Wheeler, and Eugene Wigner were present, and the conversation turned to the analogies and differences between computers and brains. As the talk went on, von Neumann got more interested in analyzing the philosophy of a computer. When the party broke up at a late hour and he said goodbye, he stated that he was going to look into the matter further. That was the beginning of a famous chapter in history. Bob's distaste for pretense made parties with such stars comfortable for a graduate student and wife. Angela was full of stories about her work for the office of the overseer of the poor in the city of Princeton and as referee for the juvenile court. She has always remained enormously interested in people.

Bob took me to see Einstein. I completely failed, however, to communicate my small observational discovery about galaxies to that great, kind man. He shook his head and said the equivalent of "very complicated." It was in this important sense that Bob's approach differed from that late phase of Einstein's work. Things might be complicated but he would work them through.

He had become close friends with von Neumann and a diverse group from Moe Berg to Solly Zuckerman, Stanislaus Ulam, and Todor von Karman. An evening might be spent creating limericks or variants of known limericks and telling stories about the struggle between mathematicians and engineers. Bob once said, "I left Princeton because someone came better at limericks than I." He taught engineering mathematics, probably betraying both his pure-mathematics and his engineering colleagues, but his students gave him a bottle of Teacher's Scotch at his last lecture.

In 1947 Bob and Angela renewed our friendship by lending us their apartment in the Athenaeum (and a bottle of Scotch), when we came to see Caltech. He told me I should set up a department of astronomy, in connection with the completion of the Palomar 200-inch telescope. I obeyed.

We came to Caltech when Bob seemed nearly free from his responsibility to the military and to the nation. He had been elected to the National Academy of Sciences in 1951 and Foreign Secretary of the Academy in 1958. In spite of his difficult war experiences, he had developed a number of close friendships with Europeans, friendships which served him well after the war and in his position as Foreign Secretary. He traded birthday poems in German with Albrecht Unsöld of Kiel. Sir Solly Zuckerman was a frequent visitor from England to the house in Sierra Madre. Bob understood European university and scientific life and worked to rebuild it as Science Advisor to NATO. His service to the Academy is memorialized by the H. P. Robertson Memorial Fund, estab lished in 1962 by a group of personal friends and companies he advised. The fund is used for a lecture on any topic, at the Academy meeting, every third year. The first Robertson lecture was, suitably, by John Wheeler, of Princeton, on relativity and geometry. Detlev Bronk gave an eloquent personal tribute. The next by Paul Doty, of Harvard, on "The Community of Science in the Search for Peace" was one that Bob would have enjoyed, on a topic to which he had given his life.

WORLD WAR II AND SCIENCE

The Society of Industrial and Applied Mathematics sponsored a symposium on cosmology and relativity in his memory in 1962. A letter from General Lauris Norstad, Supreme Allied Commander, Europe, in 1962 is quoted in A. H. Taub's memoir in the *Journal of the Society of Industrial and Applied Mathematics* (10:741-50).

> Dr. Robertson had a remarkable ability for getting to the crux of a problem and presenting his conclusions in such a manner that all could understand and appreciate them. He inspired the utmost confidence in all those who were privileged to work with him, and after his departure we frequently had occasion to call for his advice and assistance which was always forthcoming, frequently at great personal inconvenience and sacrifice. His contribution to the United States and the North Atlantic Treaty Organization was noteworthy and reflected his deep dedication to the ideals of the Free World.
>
> We at SHAPE [Supreme Headquarters, Allied Powers Europe] feel that we have lost a true friend and are most grateful for what your society is doing to keep his memory alive.

The great variety of national affairs to which Bob devoted so much of his life after 1938 is hard to describe; the military history of the contribution of science to World War II and the relation between British and U. S. operations research and its aftermaths have not been written. He received the Presidential Medal for Merit in 1946 for "solving complex technical problems in the fields of bomb ballistics, penetrations and patterns, and enemy secret weapons." Sir Solly Zuckerman mentioned Bob's work in England with R. V. Jones on scrambling radar beams and beacons. Bob was deeply involved with British colleagues in understanding the V-2 attacks and with the effectiveness (or lack of it) of large-scale strategic bombing on military production. In spite of his apparent ease and self-confidence, those important issues placed severe stress on what, underneath the jolliness, was a sensitive, temperamental, and humane personality.

In addition, he had a warmth centered on a long, romantic, and happy family life. Continuing her studies in philosophy when she was with Bob in Germany, Angela had become a psychological social worker for the city of Princeton and raised their two children, George Duncan, who is a surgeon in Arizona, and Marietta, wife of Caltech historian Peter Fay. In spite of the strain of the continual family separations forced by Bob's activities in Europe and Washington, Angela was always ready with food, drink, and wise, good talk for Bob's many and varied friends at home in Princeton and later in Sierra Madre. All became *her* friends and so remain. There are now seven grandchildren.

I am fortunate in having an outline from Frederick Seitz of the American side of Bob's career in defense, in the post World War II development of military and basic research, and of the federal support of science. This phase covered over twenty-two years, longer than he had for his own fundamental contributions to science. Since his activities were so long and complex, I quote below Fred's letter to me (dated August 27, 1975) with only minor deletions. As all accounts of Bob's life and work are, it is a personal, warm recognition of the fullness of Bob's personality.

> I first met Bob Robertson when I went to Princeton for graduate work in January of 1932. He was already established as a distinguished mathematical physicist, particularly for his work in cosmology. He was widely

admired among the students as a gifted lecturer. Since we both came from the West Coast he went out of his way to note my presence in a somewhat bantering manner. I never worked closely with him on any research problem and in fact I do not think he ever had any thesis students at Princeton. As you know he was always somewhat temperamental and could combine humor with sharp disputations on matters scientific or political when the spirit moved him. Although he liked to appear hail-fellow-well-met, and even garrulous at times, later experience suggested to me that he had strong aspects of the loner. His great forte in physics was mathematical elegance and, unlike Johnny von Neumann, he rarely dabbled in a quantitative way with relatively mundane problems. Back-of-the-envelope calculations were not his style.

I was at the University of Pennsylvania by the time World War II broke out in 1939. Robertson was one of a small group at Princeton, which included Harry Smyth and Walker Bleakney, which started work with the government on problems of conventional ordinance. I was asked to join them and we began by spending a certain number of days per month worrying about problems related to the effectiveness of explosives, ballistics and the like. This association eventually grew into Division 2 of NDRC [National Defense Research Committee] and became headquartered on the east side of the fountain court on the main floor of the Academy [National Academy of Sciences].

In the very early war years Bob focused his research attention on the mathematical theory of explosion damage. He reviewed the rather voluminous literature available and tried to tie it together. After the fall of France in 1940, however, he was asked to serve as a liaison scientist with the U.K. and we saw less and less of him in the United States, although he did occasionally attend the steering committee meetings of Division 2 chaired by John Burchard. He was, of course, quite secretive about his official activities but it was clear that he was deeply involved in most of the important scientific issues connecting our government and the U.K. He moved to the U.K. full time in 1943.

Although I remained on the steering committee of Division 2 through most of the war, I joined the Chicago division of the Manhattan district in 1943 and gradually saw less and less of the group centered in Washington.

During the winter of 1944-45, when it was clear that the European phase of the war was nearly over, the Secretary of the Army decided to establish a field intelligence agency (FIAT) in Europe to study German technology and I was asked to become part of the staff. Bob had agreed to head the office and we met in France to pull the organization together. At that time he was in a state near physical and nervous exhaustion since among his numerous other activities he had headed the intelligence team which focused on the V-2 problem. Our office, although relatively tiny, was the focal point for an enormous amount of traffic as scientific intelligence teams from the U.S. and U.K. surged across Europe. Bob himself was the principle attraction for many of the visitors.

I returned to the United States at the end of the summer when it became clear that academic life would begin to get started again but Bob remained involved in Europe until well into 1946. Although he returned to Princeton it was clear that his life had been radically changed by the wartime experience. He continued to accept appointments for special studies usually at the Secretary's or Chief of Staffs level. Among other things he helped set up the Weapons Systems Evaluation Group which was advisory to the Chiefs of Staff on matters involving science and technology. This organization eventually became a component of the Institute for Defense Analysis. As a sequel to this he spent 1954 and 1955 in Paris as Scientific Advisor to the Supreme Allied Command in Europe.

Conant, Bush and K. T. Compton had recommended the creation of the Research and Development Board [RDB] with the Department of Defense to replace the work of the Office of Scientific Research and Development. When it became clear in the mid 1950's that a large full time staff, such as the RDB had, was really out of place in the Pentagon, a Defense Science Board composed of part time advisors and representatives from various agencies and organizations, including the National Academy of Sciences, was created to take its place. Bob not only helped in the process of pulling the DSB together but was its Chairman from 1956 to 1960. I served on study panels of the DSB. Bob was quite remarkable as a chairman, but not merely because he had a comprehensive understanding of large areas of military planning, particularly those involving research and development. He was also widely admired as an individual. In a sense he shared the somewhat unusual type of position both von Neumann and von Karman held in governmental circles. The roster of participants in his day represented something in the nature of a Who's Who in the appropriate circuit.

Following Sputnik and the agitation produced by it in the United States, Bob spent an extended full period in Washington as one of the key White House advisors, being attached to the PSAC staff.

I was appointed Science Advisor to NATO in 1959 when the headquarters were still in Paris. Bob not only came regularly to the quarterly meetings of the Science Advisory Committee of NATO, but passed through

> Paris on innumerable missions both for the Department of Defense and other Washington based agencies. Looking backward I would judge that the opportunity these jaunts gave him to see many of his old associates was as much an incentive to travel as was his interest in the problems involved.
>
> At the time I returned from Paris at the end of the summer of 1960, he declared that he was going to give up his Washington connections and remain in Pasadena. I am not certain whether he would have been able to do this to the extent he hoped, but his unfortunate and premature death closed the book on the issue.

I would like to thank Mrs. H. P. Robertson and Professor Frederick Seitz for their kind reminiscences and my wife for editorial assistance.

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Ernest Harry Vestine

May 9, 1906—July 18, 1968

by Scott E. Forbush

Ernest Harry Vestine was born in Minneapolis, Minnesota on May 9, 1906, the son of Swedish parents, Frieda Christine (Lund) and Olaf Vestine, who left the United States to live near Edmonton, Alberta. Here he received all his early education and a B.Sc. degree from the University of Alberta in 1931. In 1932 he joined the Canadian Meteorological Office in Toronto, where he was occupied with meteorological and geomagnetic measurements.

During the Second International Polar Year (1932-33), Vestine led the Canadian expedition to Meanook in northern Alberta, Canada, where he established and operated a new magnetic observatory, an installation which continues to provide important magnetic data from the auroral region. While at Meanook, he made some of the most significant observations of noctilucent clouds—authoritatively described in his comprehensive 1934 review article.

In September 1934 he left the Canadian Meteorological Office for graduate study in England, and in 1937 received a Ph.D. and a Diploma of the Imperial College of Science and Technology from the University of London. His Ph.D. thesis, done under Professor Sydney Chapman, dealt with the electric current-systems responsible for geomagnetic field variations during magnetic storms. There is no doubt that this association with Professor Chapman profoundly influenced Dr. Vestine, who greatly admired Chapman's numerous classical contributions to geomagnetism and related subjects. Both had the greatest respect for thoroughness and objectivity, and each devoted his entire life to active research. There was even some similarity in their psychologically calm and logically objective approaches to problems.

Between July 1937 and January 1938, Dr. Vestine again became associated with the Canadian Meteorological Office in Toronto and later lectured in physics at the University of Toronto. In January 1938 he joined the Carnegie Institution of Washington's Department of Terrestrial Magnetism.

On May 20, 1943 Dr. Vestine married Lois Anne Reid. Their only child, Henry Charles Vestine, is a successful popular musician. Dr. Vestine was a persistent reader of scientific literature with a keen interest in history and biography. He and his wife often enjoyed cruising and fishing on the Chesapeake Bay in their comfortably equipped forty-foot motor launch. Dr. Vestine was quite proficient in maintaining his boat in excellent condition. He never appeared perturbed by ordinary misfortunes and, like his wife, was always interesting, courteous, and affable.

At the Department of Terrestrial Magnetism he made numerous outstanding, comprehensive contributions to the understanding of the earth's magnetic field, its secular, diurnal, storm-time, and other variations and related phenomena in the Earth's interior and in the auora and the ionosphere. In recognition of these accomplishments, he was given the sixth John A. Fleming Award in April, 1957 by the American Geophysical Union of The National Academy of Sciences-National Research Council. In his citation, Professor Chapman, world authority on geomagnetism and related subjects, characterized Dr. Vestine as a world leader in geomagnetism and auroral science.

The association of Dr. Vestine with the Department of

Terrestrial Magnetism of the Carnegie Institution of Washington was extremely fortunate and beneficial for both. This Department was established in 1904, two years before Dr. Vestine's birth. Since its founding, the Department had completed an extensive world-wide survey comprising an enormous number of measurements of the earth's magnetic field on land and sea. At many points, these measurements were repeated at intervals to provide data for secular variation, while the observatories of many countries provided continuous data and additional land survey information.

The prodigious task of systematically organizing practically all of the useful aspects from this multitude of data covering a period of four decades into reliable, comprehensive, and usable forms was carried out under the direction of Dr. Vestine, who effected many original, comprehensive analyses of the results. Those who assisted him in this exacting task were so devoted to him—because of his humane and considerate appreciation of the involved details and reliability required—that all contributed their diligent, enthusiastic and vigorous cooperation. Thus, in 1947 the Department of Terrestrial Magnetism published the resulting two volumes, described by Professor Chapman as "two great collections of modern geomagnetic data, including brief but cogent analyses and discussions of the data." The two large volumes, containing over 900 pages, are *The Description of the Earth's Main Magnetic Field and Its Secular Change, 1905-1945* and *The Geomagnetic Field, Its Description and Analysis.*

The contribution of these two volumes is best summarized in the following principal parts a and b of the two corresponding prefaces by Dr. Vestine. These show not only that he was thorough and most competent, but also charmingly modest and self-effacing:

> a) The present volume summarizes a descriptive study of the Earth's main field and its secular change. It is the result of a very considerable outlay of persistent effort, with much attention to detail, on the part of

those who have tried to fit the many published observations of magnetic surveys into a consistent picture.

Perhaps students of geophysics will welcome most the comprehensive new world-charts descriptive of secular change. These have been drawn complete in all magnetic elements for the first time. They are also, we believe, the first set of isoporic charts reasonably consistent with all available carefully assessed measurements with each other and with the known character of electromagnetic fields. Since they are drawn at four epochs a decade apart, the phenomenon is apparent with good continuity for almost half a century. A new and rich store of information is thus afforded respecting deep-seated, rapid, and mysterious physical processes of the Earth's interior which to the best of our present knowledge are not reflected in any other way.

The new charts of secular change have permitted the use of the great majority of survey-measurements made since the beginning of the present century in constructing isomagnetic charts in seven elements for the epoch 1945.0. The rather successful use of older as well as more recent data has thereby increased by a thousand or more the number of observational points that would ordinarily determine the isomagnetic lines. In this way, a somewhat more detailed description of the Earth's main field is afforded, bringing into a little sharper focus a major geophysical phenomenon of unknown cause.

It is not implied that this new series of charts represents an accurate description of the geomagnetic field. There are many regions in which magnetic measurements have never been made. Much use was made of uncertain interpolations, particularly across polar and ocean areas.

I have not troubled the reader with the multitudinous details incidental to a project of this kind. To have done so would have extended the present book to many volumes. The aim rather has been that of providing a condensed readable account highlighting features of importance and interest.

b) This book continues a descriptive study of geomagnetism begun with Carnegie Institution of Washington Publication 578, which was principally concerned with the description of the Earth's main magnetic field and its secular change. The present volume extends this work to the various known geomagnetic variations, with inclusion of some analyses.

To a considerable extent, the present book is actually a by-product of Publication 578, since extensive information on geomagnetic variations was required for the improving of estimates therein of geomagnetic secular change for the period 1905 to 1945. Because the latter required descriptive

> information respecting shorter-period time-variations on a world-wide scale and over these many years, the general scope of coverage is considerable. Moreover, the emphasis has been upon the description rather than upon the interpretation of results.
>
> It is believed that the two volumes together comprise the first convenient detailed compendium of geomagnetic data especially suited to the needs of those engineering workers who are mainly concerned with the practical applications of geomagnetism. The wide use of illustrative diagrams (many initially drawn as a training exercise for the draftsmen who drew the maps of the first volume) enhances the effective description of geomagnetic phenomena of our environment. The books emerge therefore as a kind of picture supplement to the standard treatise *Geomagnetism;* the writer hopes that his teacher, Professor Chapman, senior author of that treatise, will not object to such suggestion, provided he be not held at fault for any mistakes we may have made.
>
> In the course of pursuing the major descriptive objectives of this war project, the writers could not resist the temptation to undertake some serious investigations of the extensive new data available. Hence attempts at explanation of certain phenomena will be found at intervals, between the stacks of figures and tables, along with some short discussions linking the present with previous work. The writers hope that in this way a more interesting and readable account has been provided.

Dr. Vestine's logical, objective, and imperturbable approach to perplexing problems characterized all his activities and his attitude in personal discussions of scientific questions with colleagues. He always searched for independent tests of conclusions, which he made without personal bias or preferences. This accounted for the many fruitful, pleasant, and profitable discussions enjoyed by his colleagues. From his many investigations throughout his career of the secular change of the geomagnetic field and its rate of change with time, Dr. Vestine made several fundamental contributions. His improved determination of the westward drift showed this to be correlated with the previously unexplained variations in the rate of the earth's rotation. When he considered independent geophysical evidence on the rigidity of the earth, Dr. Vestine concluded that "the source of the geo magnetic field lies within a large-scale fluid-circulation inside the central core of the earth and that this fluid circulation in the core (relative to the mantle) must be considered established as real, since no other adequate large source needed to conserve the total angular momentum (core plus mantle) is apparently available."* Over a period of about 120 years, the geomagnetic field pattern was found to have drifted (with variations in the rate of drift) about 3300 km west and about 2900 km north. He also showed that surface fluid motions of the earth's core that can closely approximate secular change also show features compatible with four of the generator models that might account for the geomagnetic field—but that these comparisons did not indicate a preferred choice among these models. Such tests of models for secular change are presently of much interest, since they provide some basis for reliable estimation of the time scale for reversals of the earth's dipole field. This reversal time is a useful tool in geological investigations involving plate movements and related phenomena. Thus, as in the nature of most research, the studies initiated by Dr. Vestine have come to have important consequences for other phenomena.

The phenomena of secular change was only one of Dr. Vestine's many interests. He reliably located the northern and southern auroral zones and showed the dependence of their morphology upon the geomagnetic field. Related investigations provided estimates on the maximum total energy of particles in the Van Allen trapped radiation belts.

In 1944 and 1945, Dr. Vestine published the results of a thorough, comprehensive investigation of the geographical incidence of aurora and magnetic disturbances in the northern and southern hemispheres respectively. The study derived detailed curves showing the three geomagnetic com

* Ernest Vestine, "On Variations of the Geomagnetic Field, Fluid Motions, and Rate of the Earth's Rotation," *Journal of Geophysical Research,* 58 (1953):127.

ponents of the disturbance diurnal variation, S_D(difference for magnetically disturbed, less that for quiet days), and the variation from pole to pole of the maxima and minima of S_D These outstanding, authoritative studies indicate the thoroughness and reliability for which Dr. Vestine's work was regarded with the highest esteem by geophysicists everywhere.

Dr. Vestine contributed much to the mathematical methodology of techniques for analyses of the geomagnetic field. In addition, he developed theoretical models for aspects of magnetic storms and for the geomagnetic control of the aurora. His investigations included the effect of solar influences on magnetic storms and other geomagnetic phenomena. His analytical investigation of seismic waves and waves from blasts was most useful in seismology.

In addition to his research contributions, Dr. Vestine wrote many excellent survey articles on geomagnetism and related phenomena for encyclopedias, handbooks, dictionaries, and some survey books.

After he joined the Rand Corporation in January 1959, Dr. Vestine's interests were logically extended to include the use of rockets for measuring the geomagnetic field at great heights and for determining the lunar magnetic field. His later work in space science included scientific uses of satellites, astronautics and its applications, space vehicle environment, and the evolution and nature of the lunar atmosphere.

A most fitting tribute to the memory of Dr. Vestine is the dedication to him of the publication *World Magnetic Survey 1957-1969,* published in 1971 as IAGA Bulletin No. 28 of the International Union of Geodesy and Geophysics International Association of Geomagnetism and Aeronomy World Magnetic Survey Board. Dr. Vestine served as secretary general of the World Magnetic Survey Board and formed the center for planning and guidance of the activities of this international enterprise. *World Magnetic Survey* was edited by the late Dr. Alfred J. Zmuda, a very close friend of Dr. Vestine. This interesting, authoritative volume describes geomagnetic surveys by land, sea, air, and satellite, and presents charts, discussions of survey results, theories for the origin of the geomagnetic field, discussion of the interpretation of magnetic anomolies, and comments on seafloor spreading. It is an outstanding monument to the memory of Dr. Vestine.

To foster investigation on a national and international scale in his own and related fields, he unselfishly contributed the benefits of his knowledge, experience, and judgment to the work of numerous committees. As a participant in the International Geophysical Year, Dr. Vestine was an alternate member of the Executive Committee of the U.S. National Committee for the IGY, a member of the Committee on Aurora and Airglow, and a member of the Committee on Geomagnetism. As a member of the National Academy of Sciences, he served as a member of the Committee on Particles and Fields, the Committee on International Relations, the Space Science Board, and U.S. Commission IV to the Union Radio Scientifique Internationale, as chairman of the U.S. Panel on World Magnetic Survey, as a member of the Committee on Polar Research, and as a member of the U.S. Committee for the Year of the Quiet Sun. Dr. Vestine's involvement with the International Union of Geodesy and Geophysics included terms as chairman of the Committee on Magnetic Secular Variation Stations, chairman of the Committee on World Magnetic Survey and Magnetic Charts, chairman of Commission II, Magnetic Charts, and secretary general of the World Magnetic Survey Board, International Association of Geomagnetism and Aeronomy. For the American Geophysical Union, Dr. Vestine served as a member of Working Group II, Committee on Space Research, as chairman of the Committee on Cosmic Terrestrial Relationships, as a member of the Committee on Planetary Sciences, president of the Section on Geomagnetism and Aeronomy, and as a member of the Council.

Few investigators have enjoyed such a lifetime of extraordinarily fruitful research that has contributed so solidly to the understanding of so many phenomena in a wide field of geophysical interest. This achievement resulted from Dr. Vestine's sustained singleness of purpose and persistent effort, without deflection by irrelevant activities, toward his laudable goal.

HONORS AND DISTINCTIONS

Education

University of Alberta, 1928-32; B.Sc., 1931

University of Toronto, 1933-34

Imperial College of Science and Technology, University of London, 1934-37; D.I.C. (Diploma of Imperial College) and Ph.D., 1937

Professional Positions

Canadian Meterological Office, 1937

University of Toronto, Instructor in Geophysics and Meteorology, 1937

Carnegie Institution of Washington, Department of Terrestrial Magnetism, 1938-56

Johns Hopkins University, Applied Physics Laboratory, Consultant on Missile Guidance, 1946-56

Battelle Memorial Institute, Consultant, 1956-59

National Aeronautics and Space Administration, Consultant, 1959-66

National Science Foundation, Consultant, 1960-66

The Rand Corporation, 1957-68

University of California, Los Angeles, Professor of Meteorology, 1966-68

Professional Societies

American Geophysical Union

American Seismological Society

Institute of Electrical and Electronics Engineering

Society of Terrestrial Magnetism and Electricity Japan)

Washington Academy of Sciences

International Scientific Radio Union (URSI)

Honors

National Academy of Sciences, Member, 1954

John A. Fleming Award by the American Geophysical Union of the National Academy of Sciences-National Research Council, 1967

Moon Crater named Crater Vestine by the International Astronomical Union

World Magnetic Survey Summary Volume of the International Union of Geodesy and Geophysics dedicated to the memory of Dr. Ernest Harry Vestine, 1971

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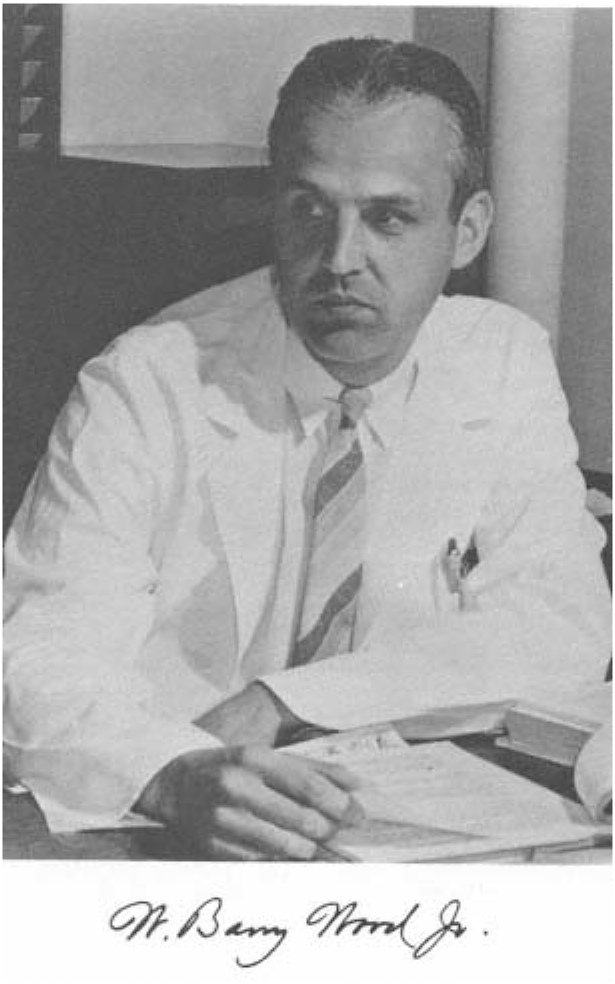
W. Barry Wood Jr.

William Barry Wood, Jr.

May 4, 1910—March 9, 1971

by James G. Hirsch

Barry Wood was born May 4, 1910 in Milton, Massachusetts, of parents from established Boston families. His father was a Harvard graduate and a business man. Little information is available about Barry's early childhood, but it was apparently an enjoyable and uneventful one; he grew up along with a sister and a younger brother in a pleasant suburban environment. He was enrolled as a day student in the nearby Milton Academy, where one finds the first records of his exceptional talents as a star performer in several sports, a brilliant student, and a natural leader. Young Wood had no special interest in science or medicine. He took a science course as a part of the standard curriculum his senior year at Milton and somewhat to his surprise won a prize as the best student in the course. This event signaled the start of his interest in a career in science.

In view of his family background and his prep school record it was a foregone conclusion that he would attend Harvard, but Barry was only seventeen years old when he graduated from Milton, and his parents decided he might profit from an opportunity to broaden his outlook and mature further before entering college. He was sent to The Thatcher School in California, an experience he recalled later as highly enjoyable and successful, with much exposure to outdoor sports and activities.

The record Wood made at Harvard (1928-1932) was truly phenomenal, leading understandably to national fame at the time. He was a star athlete, winning nine letters in three major sports (football, baseball, and hockey) and a tenth letter in tennis. He was named to the All-American football team as quarterback in 1931 and was captain of the football team in his senior year. So much time was taken with his football play, his position as center of the hockey squad, and as first baseman of the baseball team, that he didn't participate in track, one of the sports he excelled in at Milton. He did, however, make time for a little tennis, achieved national ranking, and was chosen on one occasion as a member of the Davis Cup squad.

In the face of this record of athletic accomplishments, which obviously consumed a good deal of time, Barry somehow managed not to neglect his academic work. He graduated summa cum laude and did honors thesis research in biochemistry as I shall discuss in a moment. Barry was asked time and again in later life how he managed to do both sports and schoolwork and do both so well in prep school and in college. His answer was deceptively simple, namely that he devoted all of his time to these two activities and organized himself so as to avoid incursions on his time by anything else. This undoubtedly was true or nearly true, but nevertheless his record of accomplishments in both fields is likely never to be equalled.

Barry's experience in chemistry at Harvard was influenced by a chance encounter with James Conant, described by Barry as follows:

> I can still remember vividly coming out of the chemistry library ... walking down the hall one day, and Mr. Conant met me and asked me what I was doing in the biochemistry field, whether I was having a good time, and

> whether I planned to write an honors thesis. I told him I really hadn't made any plans.... Mr. Conant was then chairman of the department, and I was just an undergraduate student ... but as was so typical of Mr. Conant he still knew that I was there and that I was trying to concentrate in biochemistry. He said that he knew just where I ought to work on my honors thesis, with Professor L. J. Henderson. Well, I had read Henderson's *Fitness of the Environment,* which was a book that anyone concentrating in biochemistry would read, and I had also read his monograph on the blood, which was his great work as a scientist, and the idea of working with L. J. Henderson just seemed too good to be true. As a result of Mr. Conant's efforts, I was introduced to L. J. and went to work with him in the fatigue laboratory, which was located in the basement of the Harvard Business School across the river.*

Barry's description of L. J. Henderson was also of some interest.

> He was not a laboratory scientist. As a matter of fact, he used to tell me that he was no good in the lab; he broke all the test tubes! He stayed in a room above the laboratory. L. J. took the data that came out of the laboratory, he would work with his slide rule and put it together and write it up. He really was one of the first theoretical biologists, who didn't do a lick of work with his own hands. And later he became a philosopher, interested in Plato. Of all the people on the Harvard faculty at that particular time, he had more influence over President Lowell than anyone else. Lowell went to him, consulted him about every major decision. To be allowed to work in his laboratory was a tremendous privilege.†

Barry's honors thesis work was selected to take advantage of his athletic as well as his academic activities. The laboratory had studied previously changes in certain physiological or biochemical parameters in athletes such as marathon runners. Henderson advised Wood to study changes in the white blood cell count during strenuous physical exercise. The study thus involved obtaining from his teammates blood sam

* "Leaders in American Medicine," audiovisual memoir T/V2107, W. Barry Wood, Jr., 1971; Alpha Omega Alpha Honor Medical Society and National Library of Medicine/ National Medical Audiovisual Center.

† Ibid.

pies for white counts before, during, and after the height of physical exertion in football or in hockey contests. The findings were impressive: white counts in a football game or after a period on the ice would go from a normal of 5000 to 24,000, a change as great as that seen in pneumonia or acute appendicitis. This thesis work, properly evaluated and written up, was published in a German physiology journal and was the first of a long series of publications by W. Barry Wood, Jr., nearly all of which had to do directly or indirectly with the same white cells that were the subjects of this college project.

Barry also described with some relish his experience in writing for publication this first research work, an experience strikingly similar to that recalled by many of us in similar circumstances.

> The thing I can remember most clearly about this experience was that I had to write this honors thesis, and I worked very hard on it. I tried to make every sentence perfect. When I got through I thought I had a masterpiece to give to L. J. to read. Well, he called me back about three days after I had given it to him. It was there on his desk, and it was just *covered* with red marks—corrections—starting with the title. He began by pointing out that the title didn't say what was in the paper, that when you give a title to a paper it should tell the reader what is in the paper. He must have spent hours correcting the manuscript, every single word. My first reaction was one of anger. But it was a wonderful lesson to me in writing. I did the whole thing over again, taking into consideration all of his corrections. And then, of course, he liked it a little better. That had a lasting impression on me. I always tried later on to do the same thing, to help junior faculty or students write scientific papers properly.*

Two important decisions were made by Barry in 1932 when he graduated from Harvard. The first was the decision that he and Mary Lee ("Leal") Hutchins would be married. Barry and Leal had been close friends since childhood; the Woods and the Hutchins shared with several other Boston families a summer camp in southern Maine. Secondly, Barry

* *Ibid.*

decided to enter medical school at Johns Hopkins. He reminisced about the choice between Harvard and Hopkins as follows:

> I decided after college that it would be a good thing to get away from Boston. After all, I'd been there nearly all my life, and it seemed to me wise to go somewhere else. I had just been married, and my wife's father was a physician who had been trained at Johns Hopkins. I heard him talk about it, what a wonderful place it was. He had been there in the days of Osler and Welch and Kelly—he was Kelly's first resident on the gynecology service. I also looked at the Hopkins catalogue and noticed that they had a lot of free time for special studies and research. I was interested in the idea of doing research, so it seemed to me that I ought to go to Johns Hopkins. So I went to talk to some of the Boston Brahmin physicians. And they told me I was crazy! They said that John Hopkins was a second-rate place, that there was no medical school that could measure up to Harvard Medical School. But despite that advice I decided to go to Hopkins, and I never regretted it.*

Barry and Leal lived in a boarding house near The Hopkins. Leal went to Goucher and then to The Johns Hopkins School of Public Health, for her graduate work. Both Barry and Leal often spoke of these happy days, commenting that it was a great experience to go through graduate school together. They kept careful records of expenses; total costs, including tuition, room and board, and maintenance on a secondhand car, were $1100 for the first year, for both!

The medical school curriculum was a demanding one, but Barry found some time to spend in the laboratory of W. Mansfield Clark, working in biochemistry and metabolism, in particular on pH and oxidation-reduction potentials. One summer during medical school was spent at the University of Wisconsin working in microbiology and visiting with Polly Bunting, a close Vassar College friend of Leal's who was a graduate student at Wisconsin. The following summer Barry took a clerkship at The Boston City Hospital,

* *Ibid.*

where he was exposed to interesting clinical material and stimulating teachers: Soma Weiss, William Castle, and Chester Keefer. As Barry put it, he was "bitten by the clinical bug" that summer.

When graduation from medical school drew near, Barry found himself facing a difficult choice in terms of postdoctoral training. He was interested in everything ranging from clinical medicine through clinical research to the basic sciences and pure laboratory work. Mansfield Clark urged Barry to go straight into biochemistry and start his scientific career, but Barry finally decided, after considerable debate and soul-searching, to pursue his clinical bent. He went on to internship and assistant resident appointments at Hopkins on the medical service directed by Warfield Longcope.

Longcope encouraged each of his resident physicians to select a speciality for clinical and laboratory study in depth during their stay. Barry wanted the metabolism and biochemistry speciality, but it had been taken by someone else. Longcope suggested as an alternate the field of immunity and gave Barry recent issues of The Rockefeller Institute for Medical Research annual reports, directing his attention to the work of Oswald Avery. Wood was utterly fascinated with these reports. Longcope arranged for Barry to visit Avery in New York. Barry described his visit as follows:

> I can still remember to this day going into Avery's office. He sat me down at a table.... He was a tiny little man, and he had on a long white coat, and he paced the floor. He told me the whole story of the pneumococcus capsule and the polysaccharides in such a way that I was just entranced by it, and I went back with great enthusiasm for getting into this infectious disease field.*

Barry had little time for lab research during his clinical residency, but as soon as this had been completed he returned to Boston as a National Research Council Fellow in

* *Ibid.*

the bacteriology department of Hans Zinsser. Wood had made a choice between A. Baird Hastings in biochemistry and Zinsser and selected the latter because he was still captivated by the pneumococcus and wanted to do research on this organism. It is somewhat surprising that Barry did not seek a position with Avery, the acknowledged pneumococcus expert who had stimulated his interest, but there is no evidence that he considered this course of action.

Zinsser was in the terminal stages of leukemia during Barry's stay in the department, so arrangements were made for him to work with John Enders, who was then studying pneumococcal infections. Wood and Enders developed a laboratory model of pneumococcal pneumonia in rats, based on earlier work done by Nungester at Michigan. This model allowed them to study in a fruitful manner several experimental aspects of the infection. They demonstrated that leukocytes played a primary role in recovery from pneumococcal pneumonia and were not merely scavenger cells that cleaned up the damage after antibodies or other agencies had killed the microbes. They studied the effects of antiserum on the recovery process and confirmed the earlier reports that antibody promoted phagocytosis, although they also noted that some phagocytosis occurred before demonstrable antibody was present. These experiments laid the groundwork for continuing studies on relationships between pathogenic microorganisms and phagocytic cells, studies that occupied Barry for approximately half of his scientific career.

After only one year at Harvard, Barry returned to Hopkins, accepting a junior staff position in the department of medicine. It was wartime and he was busy with clinical and administrative duties, including service with a special commission on primary atypical pneumonia of the Armed Forces Epidemiology Board.

During his second year on the staff at Hopkins, Wood was offered the position of professor and chairman of the department of medicine at Washington University in St. Louis. Barry was only thirty-two years of age. He was, in his own words, "flabbergasted" at the offer. He was only two years beyond house-officer training, and he felt a distinct uneasiness about his ability to handle the professional responsibilities. Furthermore, St. Louis was far outside the Boston Baltimore axis he had been on his entire life, and all of his Hopkins friends and mentors advised against taking it because it would preclude any chance to develop a research program. Despite these negative aspects, the challenge and the attraction of the offer were too great to resist, and he accepted.

He started slowly in St. Louis, watching and learning the ropes from Harry Alexander and other experienced clinicians. His success in the early phases of the new job was helped in no small measure by his strict adherence to the admonition of his wife, Leal, that he never mention Johns Hopkins! Within a few years Barry had established one of the best teaching and research medical services in the world at Washington University. The service was small, by today's standards, with an unusual degree of intimate and stimulating contact between professors and house staff.

Wood was determined to continue his laboratory research in his new position. He enjoyed clinical medicine and teaching and was extraordinarily talented in both of these activities, but basic research was his first love. He devised an unusual plan for sharing the clinical and administrative leadership of the department with his close friend and colleague, Carl Moore. Each was the professor in charge of clinical and administrative duties for six months of the year, during which the time available for research was nil, or at best catch-as-catch-can, and each enjoyed six months of the year for fulltime research, uninterrupted save by emergencies. Such a plan can be expected to work only if the two men are completely compatible and trusting of one another; it worked very well indeed for more than a decade with Barry Wood and Carl Moore sharing the captain's role.

Although limited to half-time for research activities, Wood was remarkably productive during his thirteen years in St. Louis. His laboratory published during this period approximately thirty papers reporting new findings on mechanisms involved in the pathogenesis of diseases produced by the pneumococcus or closely related microorganisms. He studied the mechanisms by which the outcome of experimental pneumococcal pneumonia was altered by various agencies: coexisting influenza virus infections, various drugs and antibiotics, and serum antibodies.

Perhaps the most important contribution was the discovery of the phenomenon of surface phagocytosis. This discovery grew out of observations made on lungs of animals infected with encapsulated pneumococci early in the course of the disease, before the animals were able to produce antibody to render the microorganisms susceptible to phagocytosis. It was noted that considerable phagocytosis was occurring even at these early time points. Further study on interactions between encapsulated pneumococci and phagocytes *in vivo* and in several situations *in vitro* established the fact that the nature of the environment was important to the outcome. On smooth surfaces the phagocytes were unable to engulf the bacteria, whereas on rough surfaces they were often able to wedge the slippery microbes in a blind alley or a corner and accomplish phagocytosis. This phenomenon of surface phagocytosis contributed to understanding of the early events of infection; more important in theoretical terms was the demonstration that physical as well as chemical parameters influenced the phagocytic process.

Toward the end of his stay in St. Louis, Wood embarked on a new line of research, leaving pneumococcal disease in favor of experiments dealing with what appeared to be quite a different area, the pathogenesis of fever. When asked to comment on the factors that accounted for this change in direction of his research, Wood began his reply with another comment on his visit, during his house-officer days at Hopkins, with Oswald Avery at The Rockefeller Institute.

> When I went to visit Avery that time I mentioned earlier, he pointed out that there are two kinds of investigators. There are investigators who go around picking up surface nuggets, and wherever they spot a surface nugget of gold, they grab it and put it in their collection. And, he said, there is another kind of investigator who is not interested in these surface nuggets, but rather is interested in digging a deep hole in one place, hoping to hit a vein. Of course, if he strikes a vein of gold he makes a tremendous advance. Dr. Avery was such a wonderful example of this second type of investigator. ... If you look at his bibliography essentially everything was on the pneumococcus—I think there was one paper on the streptococcus, which is a sort of cousin of the pneumococcus. And yet Avery was the father of modern molecular genetics. Avery made the extraordinary discovery that the molecule important in heredity is DNA, in the course of studying his dear old pneumococcus, which he stayed with all of his life.*

Wood continued,

> Having been so impressed by Avery's doctrine and his career, I was very hesitant to leave the pneumococcus. But I rationalized by saying that I was just as much interested in the leukocyte that I started working with in college. This double interest also seemed justified because the leukocyte, after all, is the thing that destroys the pneumococcus and makes the patient recover. One of the things that commonly happens to patients sick with infections or other diseases is that they develop fever. And I was impressed as a clinician with how little we know about fever. This became particularly fascinating when it was found that the leukocyte is the cell that makes pyrogen, which is a hormone that acts on the hypothalamus to reset the body thermostat so that the temperature goes up. So it was really a logical progression to get involved in this area.†

* *Ibid.*
† *Ibid.*

At this time Wood changed not only his primary field of interest, but also his job. He was offered the post of vice-president of Johns Hopkins, a challenging position in which he was expected to coordinate the medical school, the school of public health, and the hospital and to revise the medical school curriculum. The decision to return to Hopkins was a difficult one for him to make. His life in St. Louis had been a happy and gratifying one from both personal and professional points of view. He and Leal enjoyed their home and friends and found it ideal for their children. He had developed the department of medicine at Washington University into one of excellence as judged by both clinical and research authorities. All was going very well indeed. What then led Barry to accept the offer from Hopkins? His own comments on this question indicate that he was probably lured by the new challenge, as well as by the old ties to The Hopkins:

> Decisions of that kind ... are not made up here in the head by logic, they're made down here in the middle of your solar plexus. I had a feeling at that time that I might be able to contribute something if I went back to Hopkins. . . I'd been in St. Louis for thirteen years. I knew that Carl Moore was there and would take over. I didn't have any worries about leaving the ship when it wasn't in good shape. Dr. Lowell Reid, who had been the vice president and was then acting president of Hopkins, persuaded me very convincingly that I could make a contribution.*

Wood commented on his experiences on returning to Hopkins as follows:

> I had reservations about whether I could be effective in administration, whether I would be happy in it, so I left myself an escape hatch. The trustees very kindly permitted me to keep a laboratory in the microbiology department, where I could continue to do research. I tried to do my administrative work in the morning and to keep the afternoons free for the lab. But when you are doing research, it's not only what you think about when in the laboratory that is important, it's also what you think about after

* *Ibid.*

> dinner, or in the morning while shaving. You've got to have your mind on these research problems. What I found happened to me was that in all these hours outside the working day I was thinking about the administrative problems, not the research. I found this very difficult, and after four years of it, when circumstances made it possible for me to return full time to teaching and research [as chairman of the department of microbiology at Hopkins], I jumped at the opportunity.*

It must be said that some of Wood's attempts to bring about administrative changes met with opposition. He said in describing his efforts to modernize the curriculum that "trying to change the curriculum is like trying to move a graveyard!"

Barry's stint as an administrator did not leave him with a jaundiced attitude toward this activity, since he went on to say: "Administrative problems have to do with the welfare of other people. That's what an administrator is, he's trying to do things so that other people can operate properly. It seems to me that it is one of the most unselfish of occupations, to be a good administrator." Furthermore, after Wood returned to teaching and the research laboratory, he continued to give generously of his time and wisdom in many outside advisory and administrative activities such as The Board of Overseers of Harvard College (1944-1955); Armed Forces Epidemiologic Board (1950-1962); Board of Trustees, Rockefeller Foundation (1954-1971); National Academy of Sciences (1956-1971, elected 1959; Council Member 1959-1965); and The President's Science Advisory Committee (1960-1962); to list only a few of the most prestigious. Wood was also a dedicated member of The American Society for Clinical Investigation and the Association of American Physicians, serving as President of both.

One outside activity deserves special mention, for it was an unusual one and one that Barry prized perhaps above all others. This was the so-called Interplanetary Society, also

* *Ibid.*

known more widely as the Pus Club. Shortly after Barry returned to Hopkins, he and two of his closest friends, Paul Beeson and Walsh McDermott, decided it would be pleasant and rewarding to gather once or twice a year, bringing along their research groups to spend a day in informal scientific discussions and good fellowship. This was convenient, since Beeson was in New Haven and McDermott in New York. A very special relationship existed among these three men: each was endowed with unusual qualities for leadership and each had wide-ranging interests and abilities, from clinical medicine or public health through microbiology to modern science at the molecular level. And furthermore they constituted an unusual three-way mutual admiration society, apparently completely free of any friction or ill will that might have resulted from their common interests and competition. These meetings were remarkably successful and were slowly enlarged to include selected outside groups, such as René Dubos and his colleagues in New York. With passage of time junior people from the three departments moved into positions of independence. These alumni continued to participate in the Pus Club meetings, and the meetings grew larger and larger, but the special quality persisted. At no time was there any formal organization or charter; throughout, the only official members were Barry, Paul, and Walsh!

The last decade of Barry Wood's life was spent with his "first loves," research and teaching. He built an outstanding microbiology department. He took a personal interest in the departmental courses, attending all lectures and student labs and accepting responsibility for many of them himself. His lectures, like his research papers or review articles, were models of clarity and precision. His style of lecturing was calm and refined, not flamboyant, but the overall effect was nonetheless inspiring to, and appreciated by, the students.

His research in these last years at Hopkins consisted in the main of a series of studies on leucocytic pyrogen. He and his colleagues demonstrated convincingly that neutrophil leukocytes and mononuclear phagocytes are cellular sources of pyrogen. They investigated the stimuli and conditions required for pyrogen production and release, and they established the site of action and many of the pharmacological properties of endogenous (white cell) pyrogen, as distinguished from various exogenous substances that produce fever. Finally they launched a successful attack on the isolation of endogenous pyrogen and the study of its chemical nature. Although the major effort was on pyrogen, in these Hopkins years an occasional paper appeared on other topics, such as surface influences on phagocytosis of streptococci, mechanism of action of penicillin, and nature of heat labile serum opsonins. All in all Barry Wood was author or coauthor of over 100 papers reporting original scientific observations; nearly all of these were published in *The Journal of Experimental Medicine.*

His reputation as a scientist and as an excellent writer and speaker led to many invitations for review articles or name lectureships. He authored two articles in *Scientific American,* "White Cells and Bacteria," in 1951 and "Fever" in 1957; he gave a Harvey Lecture entitled "Studies on the Cellular Immunity of Acute Bacterial Infections" in 1951; he contributed to *Physiological Reviews* in 1960 an article entitled "Phagocytosis, with Particular Reference to Encapsulated Bacteria." His published name lectures and monographs included the Shattuck Lectures on "Studies on the Causes of Fever" in 1958; the Durham Lectures on "Miasmas to Molecules," 1961; and Expo '67 Noranda Lectures on "Report to Metchnikoff," 1968. He also contributed to several textbooks, the most significant being his part in the Harper and Row *Microbiology,* co-authored with Davis, Dulbecco, Eisen, and Ginsberg.

Wood's later years in Baltimore were happy and successful ones, marred only by family illnesses. He remained active as an athlete throughout his life. In his fifties he was lean and fit, the picture of good health and certainly an unlikely candidate indeed to suffer a heart attack. But he was an example of how deceiving appearances can be. His father had died at an early age, probably of coronary artery disease, and Barry inherited the tendency, which made its appearance despite his excellent general physical condition and his physiognomy. He suffered a severe, debilitating coronary artery occlusion in 1969. He was incapacitated for several months but recovered nearly completely and was back to a reasonably normal life. His wife's illness, leading to her death in the summer of 1970, added to the tragic burden. Then came the final, fatal heart attack on March 9, 1971, while Barry was in Boston to attend a dinner honoring President Pusey. His life thus ended in Boston where it had started some sixty years earlier, a life cut short, but a life nevertheless full and rewarding in the extreme.

What qualities made Barry Wood the special person, the natural leader that he so obviously was? Let me quote from some of his close friends and colleagues who attempted to answer this question when they paid memorial tribute to Wood shortly after his death.

Dr. A. McGehee Harvey said:

> That he was a rare human being was clear to all who knew him. What were the ingredients of his greatness? Barry aimed constantly for excellence in everything he undertook—in research, in teaching, in administration and in sports. He had ability in abundance and the self-discipline to succeed. He was a great teacher, for whom teaching was a sacred trust. Every student was given special care. He was no mere theoretician of medical education; he spent long hours preparing for lectures and conferences, clarifying his writing and sifting his courses so that only the finest remained. Giving everything his best, he inspired others to do their best; and so he led by example, softly. Another ingredient of his greatness was that rare quality

of speaking with one voice and listening to all—presidents and first year medical students alike. This quality above all brought respect and trust from countless students and colleagues who sought his help and advice. Barry's unique combination of talent in teaching, research and clinical medicine, together with his sense of humility and natural benevolence of spirit, gave him that degree of wisdom which is the basis of effective leadership.

Polly Bunting wrote:

I have thought often . . . about what made Barry so special. One of the elements certainly was his capacity for growth, growth professionally and in his personal relations. Success never tended to arrest his development or dull his interest in the new adventures ahead. Growth is the right word, for new skills, new findings, new relations with friends and family, implied no rejection of the past but were built on it and into it, contributing to the whole. Barry's powers of concentration were prodigious, as was his ability to organize his work and his life. These have never been endearing qualities . . . yet somehow in his case they did not offend. Rather they made possible time for those memorable discussions of problems and plans, the companionable golf games, the leisurely Sunday afternoons at Owings Mills. Somehow, very early in life Barry had ordered his priorities to give highest place not to being organized, nor to any specific set of achievements nor any ideology, nor even to people as such, but to those particular individuals who were close to him, those whom he taught, those with whom he taught, those with whom he worked, his friends and his family .... Barry was a far more complicated person than was generally realized.... He was a very private person, choosing to work through personal problems alone or with those who were directly involved. It must have taken hours of careful thinking as well as a touch of genius to move so surely among us. I suspect it was not as easy as it looked. Love never is easy, only wonderful. Slowly, over the years, one came to realize that what made Barry special was his love.

Walsh McDermott commented on Barry's greatness as follows:

Two qualities stand out as the key essentials for greatness and Barry had them both. The first is that the holder is widely acknowledged to be set off

> from contemporaries by some set of distinctions. The second is that the holder has an unusual capacity to bestow some of this distinction on many others. I shall content myself with only one example of the first, the general acceptance that he was different. Barry was a frequent subject of conversation among his colleagues. Yet whenever he attained a particular success, and there were many, I never heard it said even in the most indirect way that "there but for ... a few minor breaks go I." He stood alone in this immunity to critical comment based on jealousy or envy. But it is on Barry's transmission of his greatness to so many others, that I wish to dwell... Barry had a truly extraordinary ability to bring himself into the lives of others and make them the better for it.... To do this for another person must have required reflection, sometimes considerable reflection, about that person. Yet he made it seem as if it were a wholly spur of the moment affair.... The striking phenomenon of transmission of himself, came as an act of enthusiasm. It seemed no more and no less than that he couldn't bear to see you not share the fun ... of high quality.... He made you think about yourself, and what you were doing, in such a way that in effect you lifted yourself by your own bootstraps to his generous applause. Clearly Barry saw himself as he was and others as they could be. How does this transmission of himself differ from the fine influence on young scientists and professionals exerted by so many of our top-rank leaders through the years? In many ways it really doesn't differ—and well that is, or we should be in far more trouble than we are. But it does differ in two important ways. First, in the extraordinary personal nature of the relationship without its ever being a possessive intimate relationship. For Barry was deeply convinced that it was the personal relationship that counted. Indeed he believed the transmission of respect for quality could only be made in this way. And second, it differs in that those whose lives were touched by Barry came to know not necessarily the world's greatest scientist or professional in this or that area of learning or creativity, but a truly great man.

The last point made by Walsh McDermott, that Barry was convinced that it was the personal relationship that counted, is borne out by Barry's own remarks when he reviewed his career shortly before his death. In a response to a question from Dr. Robert Glaser, Barry said, "Again it comes back to people, Bob. You look back on your life and you see that the things that really influence you are your experiences with specific people." At every turn of his career, Barry gave credit to someone who had exerted personal influence, intentionally or otherwise, to lead him along the path he took. The list of people mentioned in this regard by Barry is an impressive one indeed: James Conant, L. J. Henderson, Mansfield Clark, Polly Bunting, Soma Weiss, Chester Keefer, William Castle, Warfield Longcope, Oswald Avery, Hans Zinsser, and John Enders. In turn many of us, myself included, are indebted to Barry for having exerted by his personal touch a telling influence in determining the course of our careers.

It is difficult or impossible to expand in a meaningful way on the incisive analyses of Barry Wood's unique qualities as seen by Mac Harvey, Polly Bunting, and Walsh McDermott. My own comments can only reiterate theirs in a more simple summary. The features that stand out in my mind are: his unusual endowment of physical and mental talent and the drive to make the best use of these talents, a genuine interest in other people and extraordinary ability to influence them for their own good, and versatility—an All-American triple threat in his career accomplishments as well as in his college football days.

The many direct quotations from Barry Wood contained in this memoir are all derived from a motion picture made only two months before his death. This film,* an informal discussion between Barry and Dr. Robert J. Glaser, provides an overview of Wood's career and is a marvelous record of his special personal qualities, which come across well on the screen. Barry's closing words in this interview were especially prophetic and touching in view of his imminent death: "Well,

* *Ibid.*

the only thing I'd like to say at the end is that it has all been wonderful fun. I wouldn't change one thing; it has been a tremendous privilege. And I hope that in the next generation, where things are going to be more complicated, that it will still be possible for people to have as much fun and reward as I had."

HONORS AND DISTINCTIONS

Professional and Honorary Societies

American Society for Clinical Investigation, 1941-1971

Board of Overseers of Harvard College, 1944-1955

Council on Pharmacy and Chemistry, American Medical Association, 1944-1949

American College of Physicians, Fellow, 1945-1971

Association of American Physicians, 1947-1971; President, 1962-1963

Experimental Therapeutics Study Section, USPHS, 1949-1954

Central Board, Armed Forces Epidemiological Board, 1950-1962

Scientific Advisory Committee, Common Cold Foundation, New York, N.Y. 1951-1955

Citizens Committee on Human Rights, St. Louis, Missouri, 1953-1955

Walter Reed Army Medical Center, Consultant, 1953-1960 Board of Trustees, Rockefeller Foundation, 1954-1971

Armed Forces Epidemiological Board, Commission on Epidemiological Survey, 1954-1964, 1966-1969

Medical Fellowship Board, National Research Council, 1955-1957

Interurban Clinical Club, Honorary member, 1956-1971

National Academy of Sciences, elected 1959; Council member, 1959-1965

National Advisory Allergy and Infectious Diseases Council, 1957-1961

Dartmouth Medical School Policy Committee, 1957-1962 The President's Science Advisory Committee, 1960-1962

Visiting Committee of the Medical Department, Brookhaven National Laboratory, 1962-1964

Scientific Advisory Committee, Massachusetts General Hospital, 1961-1963

Advisory Committee, Mount Sinai Hospital, 1961-1964 World Health Organization, 1963-1964

Armed Forces Epidemiological Board, Commission on Radiation and Infection, 1963-1964

Advisory Committee, Milton S. Hershey Medical Center, 1963-1964

Board of Advisory Editors of *The Journal of Experimental Medicine,* 1963-1971

Society for Experimental Biology and Medicine

American Society for Microbiology

Population Crisis Committee, 1967

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