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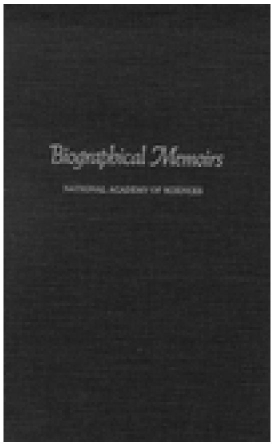


**Biographical Memoirs V.71**

Office of the Home Secretary, National Academy of Sciences

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NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

# Biographical Memoirs

## VOLUME 71

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# PREFACE

On March 3, 1863, Abraham Lincoln signed the Act of Incorporation that brought the National Academy of Sciences into being. In accordance with that original charter, the Academy is a private, honorary organization of scientists, elected for outstanding contributions to knowledge, who can be called upon to advise the federal government. As an institution the Academy's goal is to work toward increasing scientific knowledge and to further the use of that knowledge for the general good.

The *Biographical Memoirs,* begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work of our most distinguished leaders in the sciences, as witnessed and interpreted by their colleagues and peers. They form a biographical history of science in America—an important part of our nation's contribution to the intellectual heritage of the world.

PETER H. RAVEN
*Home Secretary*

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# JEROME W. CONN

*September 24, 1907-June 11, 1981*

BY WILLIAM H. DAUGHADAY

JERRY, AS HE WAS known to his contemporaries, was a member of a small group of clinical endocrinologists who applied the new knowledge in hormone structure that arose in the middle decades of this century to the definition of important clinical syndromes of hormone excess and deficiency. The contributions of these clinical scientists made this the golden age of clinical endocrinology. Almost single-handedly Jerry Conn defined the syndrome of aldosterone excess and contributed to the recognition of the renin, angiotensin, and aldosterone control mechanisms in hypertension.

Jerry was born in New York in 1907, the oldest of four children of Joseph and Dora Conn. His father operated a small shop that grew to a busy luncheonette, and his mother was a homemaker. The parents shared a common belief in the value of education and made great sacrifices to ensure that their children had the benefits of the best education possible. Jerry was an outstanding, inquisitive, and industrious student who skipped a grade in primary school. A family friend, a physician, probably influenced Jerry's choice of profession. After three successful years at Rutgers University Jerry entered the University of Michigan Medical

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School in Ann Arbor in 1928. The Great Depression, which began the following October, depleted the family resources, and his two sisters, both of whom were equally gifted, contributed materially to Jerry's education from their salaries as secretaries. Jerry never forgot this sacrifice, and when his brother Harold, who was twenty years his junior, came to the University of Michigan for his medical education, Jerry paid his tuition and expenses. Jerry had two precepts for Harold as he embarked on his own academic career: (1) Stay in one place because every move will cost him at least one year, and (2) find a good umbrella (by this he meant a supportive chairman) and stay under it—to which he added, do not be seduced into becoming an umbrella. Harold went on to a distinguished career in hepatology entirely at the Yale University School of Medicine.

In medical school Jerry met a classmate, Betty Stern, who shared his interests in clinical research. They were married after the first year. He graduated from the University of Michigan School of Medicine in 1932 with honors and as a member of the Alpha Omega Alpha Honor Society. He chose to take his internship in surgery at the university's hospital, but after one year he switched to internal medicine, which presented more of an intellectual challenge to him. After two years of medical residency he was attracted to the exciting research in the Division of Clinical Investigation in the areas of obesity, energy metabolism, and diabetes under the direction of Dr. Louis H. Newburgh. Betty joined the division at the same time. She collaborated with Jerry in important studies of the relationship between obesity and noninsulin-dependent diabetes. Jerry entered the division as a fellow in 1935 and became an assistant professor of internal medicine in 1938.

Jerry's entire professional career was at the University of Michigan. In 1943 he became director of the Division of

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Endocrinology and Metabolism, a position he held until 1973. In 1968 he was named the L. H. Newburgh Distinguished University Professor. He retired from the university in 1974.

Jerry Conn and his associates made major contributions in four areas of clinical endocrinology and metabolism: (1) dietary modification of glucose tolerance, (2) aldosterone and the regulation of salt excretion (the syndrome of hyperaldosteronism), (3) the renin-angiotensin system in hypertension, and (4) the nutritional regulation of insulin secretion.

*Dietary Modification of Glucose Tolerance.* Conn began his research career in Newburgh's laboratory with a series of studies of the relationship between diet and glucose tolerance. He reported on the comparative glycemic effects of carbohydrate and protein on glucose tolerance. He was one of the first to advocate a high-protein diet in the treatment of hypoglycemia. An important contribution of this period was the carefully conducted metabolic studies of what is now known as type II diabetes mellitus. Those studies found that the oxidation of glucose in this condition is not reduced but achieved only at an elevated level of blood sugar. This explained the rarity of ketoacidosis in this condition. Conn's group was one of the first to clearly recognize the relationship between obesity and adult-onset diabetes by showing the resumption of normal carbohydrate tolerance after attainment of normal weight in twenty of twenty-one patients. This was quite an achievement in view of the difficulty of obese patients to reach normal weight by dietary restriction.

*Aldosterone and the Regulation of Salt Excretion.* With the onset of World War II, medical research was redirected to problems of military relevance. Jerry's mentor, Louis Newburgh, left Ann Arbor for Washington to join the U.S.

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Naval Investigative Laboratories, and Jerry took over the Division of Endocrinology and Metabolism in 1943. Because acclimatization to tropical heat was a major military concern in the South Pacific, Jerry undertook a series of investigations of the regulation of salt loss in sweat in conscientious objectors exposed to elevated heat and humidity. It was possible to do detailed metabolic studies under controlled dietetic conditions with excellent clinical chemistry laboratory support. Measurements of mineral loss in body sweat, saliva, urine, and feces were made daily for long periods of heat exposure. Conn established that acclimatization involved a rapid curtailment of renal, sweat, and salivary sodium excretion. He suspected that this response was mediated by adrenal activation, but he observed that the negative nitrogen balance was transitory while the sodium retention persisted for the duration of the heat exposure. Conn suspected that the persistent sodium retention was due to increased adrenal “salt-active corticoid secretion." He postulated that "certain types of stimuli can provoke a preponderant secretion of one type of steroid depending upon the condition, type and duration of the stress imposed." This prompted his interest in the measurement of these salt-active corticosteroids even before the isolation of aldosterone by Simpson, Tait, and Bush in 1950.

Because of these studies in the hormonal regulation of salt excretion in acclimatization, Conn was well prepared for a thirty-four-year-old patient who entered the university hospital in 1954 complaining of seven years of episodic muscle weakness that often resulted in virtual paralysis of her lower legs. In addition, she noted muscle spasms and cramping of her hands. The initial laboratory studies established severe hypokalemia and alkalosis. There were no signs of cortisol excess. Selective excess of mineralocorticoid secretion was suspected, and the patient was transferred to the metabolic

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ward for intensive investigations, which occupied more than seven months. Repeated measurement of thermal sweat found the same low concentration of sodium previously found in heat-acclimatized volunteers. Balance studies established a continuing negative potassium balance despite low levels of serum potassium. A young associate in the laboratory, Dr. David Streeten, found that the patient's urine contained an excess of mineralocorticoid, as demonstrated in a bioassay using adrenalectomized rats. These and other studies convinced Conn that the patient was suffering from mineralocorticoid excess (hyperaldosteronism) and that surgical intervention was indicated.

Jerry took the occasion of his presidential address to the Central Society for Clinical Research on October 29, 1954, to present for the first time his extensive clinical investigations of this new syndrome, which he called primary aldosteronism. The author of this memoir was in attendance that day and remembers the excitement with which this brilliant clinical study was received. The following December the patient had a surgical exploration of her adrenal glands and a 4-centimeter tumor was found, much to the delight of Jerry and his colleagues. In the years that followed, Conn's clinic became a world referral center for patients with hyperaldosteronism. Many subsequent publications expanded on the clinical description, and laboratory and radiologic diagnosis of this and related conditions.

*The Renin-Angiotensin System in Hypertension.* When the work of others established that the secretion of aldosterone was under the control of angiotensin renin secretion, Jerry's group published a series of studies of this system in hypertension and related conditions. A possible role of functional hyperaldosteronism in idiopathic benign edema and periodic paralysis was suggested. More importantly, he studied the renin-angiotensin system in secondary hyperaldo

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steronism, which can be difficult to distinguish from primary hyperaldosteronism. Jerry reported that finding a suppressed serum renin activity was an important distinguishing laboratory finding in primary aldosteronism due to an adrenal tumor. Interest in the renin-angiotensin-aldosterone axis in hypertension allowed Conn to recognize one of the first cases of a renin-hypersecreting renal tumor and to contribute to our understanding of the pathophysiology of this rare tumor.

*Nutritional Regulation of Insulin Secretion.* With the advent of radioimmunoassay for insulin, members of Conn's division conducted extensive studies on the ability of certain amino acids, particularly leucine and arginine to promote insulin secretion. They speculated that leucine sensitivity might be responsible for certain cases of functional hypoglycemia.

Conn had a deep interest in recognizing individuals with normal glucose tolerance who are at risk of subsequently developing adult-onset diabetes. In his Banting Memorial Lecture presented in 1958 to the American Diabetes Association, Conn summarized the findings obtained by Stefan Fajans and other members of his division with a glucose tolerance test that followed cortisone administration. This stress to insulin secretory capacity proved quite effective in predicting which members of a diabetic family were most likely to come down with the clinical disease. Fajans, an early member of Conn's division, succeeded Jerry as head of the Division of Endocrinology and Metabolism at the University of Michigan.

During his career at Michigan, Jerry was highly productive. He authored 284 scientific papers and book chapters; but the most impressive product of his long direction of the Division of Endocrinology and Metabolism Research Unit at Michigan was the large number of bright young

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fellows that began their research career in his division. He tirelessly supported their development as independent investigators and promoted their research careers. His personal concern for those beginning their academic careers was well expressed in his 1954 presidential address to the Central Society. Before presenting his brilliant clinical studies of the patient with primary aldosteronism, Conn exhorted the members to avoid destructive criticism of young presenters. As he said:

> I am not old enough to have forgotten completely the perspective of younger colleagues. Their aspirations are pointed in your direction. They wish eventually to reach the standing and respect in clinical research which they believe you have achieved, and many of them will, and perhaps to a greater degree! Let us set for them a proper example of kindness, friendliness, and common decency.

Jerry then gave his philosophy of scientific communication:

> We speak glibly these days of "fundamental research" and extol the discovery of a fact, however disjointed from all other facts, as an addition to the sum total of knowledge acquired by mankind. Reason tells us that we must acquiesce in a vague kind of way to the statement that every fact has a potential usefulness. In the meanwhile it is regarded as background, available to all who choose to use it. Are there any of us capable of evaluating which fact, when eventually correlated with others, will have the greatest impact upon the lives of men? The answer is "No."
>
> Let us remember that we are all painting background. Regardless of how important or unimportant your contribution of today may seem, no sooner has it been expounded than it has become background. There is some solace in the fact that your brain child is not dead! The entire background is seething with life and motion, but acceptance of the idea of painting background is sufficient to remove the undesirable gusts of wind from many sails. Let us rejoice in the knowledge that to us has come the opportunity to paint background.

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Jerry received many honors, including the Claude Bernard Medal of the Institute of Experimental Medicine and Surgery of the University of Montreal (1957); the Banting Medal of the American Diabetes Association (1958); the Henry Russel Award of the University of Michigan; the Gordon Wilson Medal of the American Clinical and Climatological Association (1961) the Banting Memorial Award of the American Diabetes Association (1963); the John Phillips Memorial Award of the American College of Physicians (1965); the Elliott Proctor Joslin Award of the New England Diabetes Association (1965); the Howard Taylor Ricketts Award of the University of Chicago (1967); the Heath Memorial Award of the University of Texas, Houston (1971); and the Distinguished Achievement Award of the American College of Nutrition (1973).

He received an honorary doctor of science from Rutgers University (1964) and an honorary doctor of medicine from the University of Turin, Italy (1975).

Jerry was a member of twelve national professional societies and served as president of the American Diabetes Association (1962-63) and the Central Society for Clinical Research (1954). He was elected to the National Academy of Sciences in 1969 and was a founding member of the Institute of Medicine. He served on four committees of the National Research Council and was the chairman of its Committee for Evaluation of the National Pituitary Agency. He was also an honorary fellow of the American College of Surgeons and an honorary member of thirteen foreign medical societies. He was an invited lecturer at thirty-nine international meetings and gave more than fifty invited lectures at various institutions in the United States.

Jerry and Betty had a long and happy marriage. They had a son, William, and a daughter, Phyllis. Their Ann Arbor home was a mecca for family, students, fellows, and

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colleagues. Sunday evening barbecues with Jerry manning the grill were common and much-remembered occasions. Tennis was one of Jerry's favorite forms of relaxation and exercise. He approached tennis games with young members of his laboratory with the same intense competitive spirit as he did his medical research.

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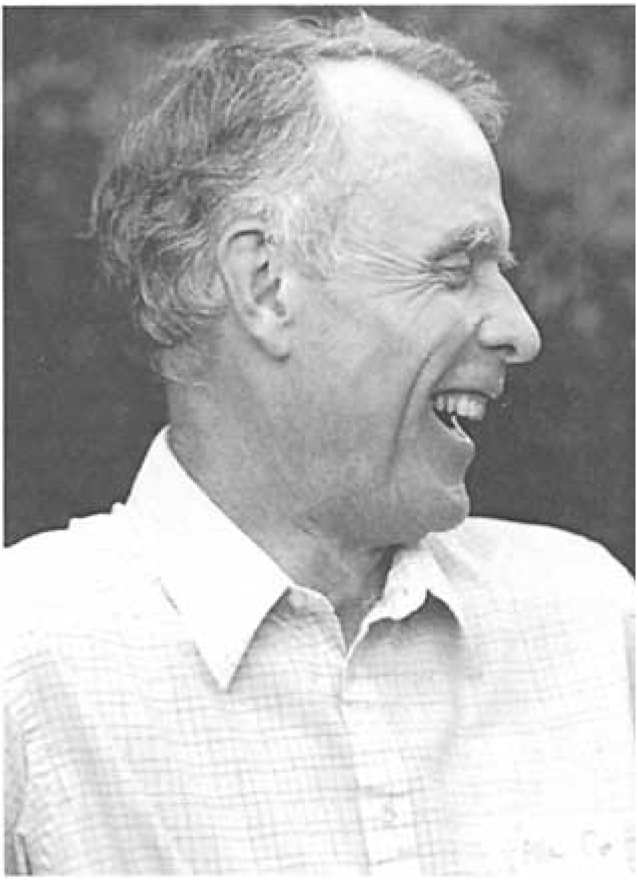

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# ALLAN V. COX

*December 17, 1926-January 27, 1987*

BY KONRAD B. KRAUSKOPF

IN THE EARLY MORNING of January 27, 1987, Allan Cox died in a bicycle accident, colliding with a large redwood tree after a steep descent on a stretch of mountain road. By this mishap geophysics lost a major contributor to the theory of plate tectonics, Stanford University lost an able and innovative dean of earth sciences, and the Stanford community lost a stimulating and compassionate teacher and counselor. More than a thousand people crowded into the Stanford chapel for the memorial service following his untimely death.

Allan was the son of a house painter, born in Santa Ana, California, in 1926. He was an avid reader and student of many fields from an early age. His chemistry teacher in a Santa Ana high school, because of other commitments, found it necessary to teach a semester's course in only six weeks. The students were really pushed, having to study chemistry three or four hours a day, five days a week-an experience that Allan found very stimulating. “Just plunging into something with enthusiasm and working hard at it was a big experience for me," he recalled later. He decided then that he wanted to be a chemist, and chose chemistry as a probable major when he entered the University of California at Berkeley. After a single quarter, however, he left school for

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a three-year spell in the merchant marine, a job that gave him ample time for pursuing his love of eclectic reading.

Chemistry remained his chosen field when he returned to Berkeley, but a summer job in Alaska with Clyde Wahrhaftig convinced him that geology was more to his liking. Continued work in chemistry proved so dull that his grades suffered, and as a result he lost his draft deferment. Two years in the Army made a return to school seem especially attractive, again at Berkeley but this time with a major in geology. More trips to Alaska with Wahrhaftig elicited an interest in glaciers, both ice glaciers and rock glaciers, and particularly in the mechanics of glacier movement. He had fond memories of those expeditions, not only for their scientific content but also for long conversations on the works of Proust and the music of Bach.

In graduate work at Berkeley Allan came under the influence of John Verhoogen and chose geophysics rather than glaciology for his specialty. Verhoogen at the time was much interested in rock magnetism, and this was the subject that became the focus of Allan's doctoral research. Verhoogen also was one of the very few at Berkeley who thought there might be some validity in the idea of continental drift.

This was an hypothesis proposed in 1912 by a German meteorologist, Alfred Lothar Wegener, suggesting that the continents are not fixed in their present positions on the Earth's surface, but over geologic time have drifted widely, joining together, breaking up, and assuming new shapes. Continental drift plays a major role, of course, in the current doctrine of plate tectonics, but in the 1950s it was generally regarded by geologists, in the northern hemisphere at least, as a wild notion with little evidence to back it up. Verhoogen's solitary support of the hypothesis made a deep impression on Allan, and he came to regard continental drift as a real possibility. In his Arthur Day Lecture many

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years later, Allan gave a vivid description of the atmosphere at Berkeley during his graduate years:

> As graduate students at the University of California in the mid-fifties, we sniffed something in the air that we were NOT hearing about in our lectures and seminars, with the exception of those of John Verhoogen. So we graduate students formed a Geology Club. The only two by-laws that I remember are (1) that we met only in places that served beer, and (2) that faculty members were not invited. .. At the end of the first year we voted on the question of whether continental drift was likely. For safety we used a secret ballot. Continental drift won, though not by a landslide. Voting as we did on a scientific issue was probably done mainly to annoy the Berkeley faculty. Luckily they had the good judgment not to acknowledge that our club existed.

Continental drift remained a strong interest for Allan, but in his graduate research he concentrated on rock magnetism.

Rocks formed near the surface are influenced by the Earth's magnetic field. As a lava flow cools and solidifies, for example, any magnetic minerals it contains become magnetized parallel with the field, and the resulting rock is then itself a weak magnet. The orientation of a rock's magnetism is thus a record of the Earth's field at the time of the rock's formation, and by measuring the magnetism of older rocks it is possible to tell whether the Earth's field has had a different direction in the past. Surprisingly, the field direction has indeed changed. The most remarkable change is an apparent complete reversal of direction at some times in the Earth's history: if the magnetism of specimens from a variety of old lava flows is measured, some show directions parallel to the present Earth's field, others a direction just reversed. Evidently the Earth's field has changed direction many times in the geologic past, the present north magnetic pole becoming the south pole and vice versa. It was this strange behavior of the Earth's field that attracted

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Allan's interest, and remained a principal subject of his research for many years.

After obtaining his doctor's degree (1959) Allan joined the U.S. Geological Survey at its western headquarters in Menlo Park. There he worked with another Survey geophysicist, Richard Doell, whom he had met as a fellow graduate student at Berkeley. From these two men in the early 1960s came many important papers on rock magnetism. A major question concerned the timing of the apparent reversals in the Earth's field: did the field stay in one direction for periods of, say, thousands of years, or millions of years? Was there a regular alternation of normal and reverse fields in the past, or was the alternation erratic? Many other questions continued to intrigue Cox and Doell, but the timing of reversals remained their major concern.

To get an answer required that the rock specimens they were using be accurately dated, and this posed a real difficulty. Many of their rocks were not very old geologically, say from a few hundred thousand to several million years, a range for which accurate methods of dating were at that time not available. Very fortunately work had begun on adapting the potassium-argon method of rock dating to this time range, and even more fortunately one of the principal workers in this enterprise was a Berkeley graduate student looking for a job, Brent Dalrymple. The Cox-Doell pair arranged for the Survey to hire Dalrymple, and it was this triumvirate that succeeded in working out details of the magnetic-reversal time scale. Much of the effort was accomplished in a small tar-paper shack set up adjacent to the main Survey building—a location that seemed at the time to reflect a low estimate of the importance of rock magnetism on the part of their Survey superiors. It turned out, however, to be a particularly favorable place for their work, because of the absence of other laboratory paraphernalia

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that would have interfered with the measurement of very weak magnetic fields.

The work was far from easy. Samples had to be collected worldwide and their magnetic direction and age carefully determined. Some samples were not suitable for measurement because their magnetic properties had been altered since they were formed, by the action of heat or lightning or changes in their chemistry produced by solutions. A few rock specimens had evidently acquired reverse magnetization not from the Earth's field but simply because of peculiarities in their chemical composition ("self-reversal"). Such deviants caused trouble in establishing the pattern to which the great majority of samples conformed.

Despite these difficulties, the job was carried through triumphantly. Reversals had indeed occurred worldwide many times, but on a very irregular schedule. Sometimes reversed polarity had lasted only a few hundred thousand years, sometimes several million. Teams elsewhere in the world contributed similar data, and for a time there was feverish competition among the different laboratories. General agreement about the times of polarity change made it clear that a time scale or calendar could be set up showing just when the changes had occurred, and the dates could be correlated with other geologic events like volcanic eruptions and changes in fossil assemblages.

This was all impressive enough, but the most spectacular application of the reversal calendar came from another source. Rock magnetism could be measured not only on specimens in the laboratory but on rocks at the bottom of the sea, by using magnetometers carried on ships. At about the time the Cox-Doell-Dalrymple calendar was being fashioned, others were exploring magnetic properties of rocks on and near the mid-ocean ridges. In general, rocks at the high point of a ridge showed normal magnetic orientation,

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but on either side were belts parallel to the ridge showing the reverse direction. Then beyond these were belts of normal direction, then more reverse directions, and so on. On a map the ridge would be at the center of a pattern of stripes, the stripes representing a symmetrical alternation of normal and reverse magnetization. And the width of the stripes showed rough proportionality to the length of the times indicated by the Cox-Doell-Dalrymple calendar. This correlation between widths of stripes and times shown by magnetic reversals became apparent to Allan at a meeting where the two records were placed side by side. He describes his reaction: "I felt cold chills. This was the most exciting moment of my scientific career."

There was ample cause for excitement. A then-current hypothesis about the mid-ocean ridges pictured them as places where lava comes up from the Earth's interior and flows to either side forming the ocean crust as it cools, and where the solidified crust is moving apart. If lava has been flowing from a ridge at intervals for a long time, and if the solidified lava of the crust is moving away from the ridge on either side, the lava would be of increasing age with increasing distance from the ridge. It should thus represent material that over time has been magnetized alternately in the normal and reverse direction. Allan's observation that the sea-bottom magnetic orientations do indeed show this alternation, with a timing parallel to that shown by the Cox-Doell-Dalrymple calendar, is strong support for the hypothesis. So the ocean crust is demonstrated to be moving, and parts of the crust on the two sides of a ridge represent huge "plates" of crustal material that are drifting apart. Here, then, is solid evidence that great areas of the Earth's crust are in motion, one of the principal kinds of evidence that paved the way to the concept of plate tectonics. Thus, Allan Cox early in his professional career played a major

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role at the beginning of a scientific revolution that within the next few years would profoundly transform the earth sciences.

In 1967 Allan moved from the Geological Survey to the geophysics department at Stanford, where he continued work on paleomagnetism and attracted the interest of many students to this field. The magnetic field preserved in rocks can provide much information about Earth history besides the simple reversal of field direction at various times in the past, and some of these other aspects proved a fertile area for Allan and his students to explore.

The magnetism of a rock records many details of the Earth's field at the time of the rock's formation: its intensity, its direction with respect to north (since the axis of the Earth's dipole field remains fairly close to the rotational axis), and its inclination to the horizontal (since lines of force of the Earth's field range from horizontal at the magnetic equator to vertical near the poles). Measurements of the direction of a rock's field and its inclination thus make it possible to determine the latitude and longitude of the place on the Earth's surface where the rock was at the time it acquired its magnetism. If the place so indicated is different from the rock's present position, this means that the crustal plate containing the rock must have moved since the rock's formation. Such measurements on rocks of different ages provide a means of tracing the movements of continents and islands and ocean basins over geologic time, and the record of these movements shows great changes in the Earth's geography during its long history.

As an example, one problem of crustal movements that particularly interested Allan and some of his students was to find an explanation for the curious pattern in the geology of the rocks that make up the west coast of North America. Here large elongated areas of rock of very different kinds

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and structures and ages ("terranes") exist side by side, as if they had come from a distance and had been successively jammed against the continent. From the magnetic directions indicated by rocks in the different terranes, it could be deduced that they had indeed moved to their present positions at different times and after long travels across the Pacific Ocean basin from various places in latitudes far to the south. Using computer graphics Allan and his students could make animated diagrams to illustrate these movements. "Earth science is highly visual, in fact almost artistic," Allan said in describing this activity. Working out details of the origin and travel of terrane components was a fascinating but difficult undertaking, an undertaking even yet not completed in all details. The magnetic properties of rocks have thus provided not only a key to the initial concept of plate tectonics, but a wealth of data on the large-scale movement of crustal plates in the Earth's past.

In 1979 Allan became dean of the School of Earth Sciences, a demanding position in which he displayed a talent for administration that had not previously been known or appreciated by his colleagues. Despite his new duties he kept up his research program and his close contact with students, at one time supervising the research of thirty student advisees, in addition to his work as teacher and administrator.

In the university environment Allan's interest broadened to other parts of earth science and to the general problems of liberal education. He gave generously of his time to student groups, helping to devise a lighting system for a student theatre and living for one period of time in a student residence. He became widely known for his teaching, in both graduate and undergraduate classes. A special interest was the designing of research projects for undergraduate students.

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Praises for his work as administrator were loud in the tributes accorded Allan by university colleagues in the memorial service that followed his death. "He was the very ideal of the teacher-scholar: the faculty resident *and* National Academy of Science Member, the theater-lighter *and* the pioneer of magnetic reversal, the living proof that we were right to insist that such capabilities and interests can come together in one person" (from the university president). "Allan greeted each problem of academic leadership with the excitement that a child brings to the exploration of a new game. Problems as diverse as the challenge of faculty development, or of dealing with prejudices that faculty and staff bring to the problems of health and safety in laboratories, or of bringing the fruits of scholarly investigation within the reach of undergraduates, or of dealing with the complex implications of divestment in South Africa ... each of these problems, and many more, challenged Allan's imagination—and he poured some part of himself into the ultimate solution of each" (from the university provost). Many other samples could be cited of the heartfelt tributes that came his way in the memorial service.

His scientific work brought Allan many honors: election to the National Academy of Sciences, the American Philosophical Society, and the American Academy of Arts and Sciences, the presidency of the American Geophysical Union, the Day Medal of the Geological Society of America, the Fleming Medal of the American Geophysical Union, and the Vetlesen Medal of Columbia University. He is the author of more than one hundred scientific papers and of two books on plate tectonics. The first of these books (1973), *Plate Tectonics and Geomagnetic Reversals,* is a reprinting of the more important early papers on plate tectonics, together with an extensive major introduction and then shorter introductions to different parts of the subject which provide

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notes on the different authors and the background of their work. The second book (1986), *Plate Tectonics: How It Works,* written with Robert Brian Hart, is intended for undergraduates or other readers with some scientific background, and explains in simple terms, with a multitude of diagrams, the basics of plate-tectonic theory and the techniques of using magnetic data to reconstruct past motions of continental and oceanic plates.

Allan's influence was felt far beyond the confines of the School of Earth Sciences. To university faculty and staff and to students both in and outside the school he was widely known for his understanding and friendliness combined with exacting standards. Notable particularly was his quiet ability to elicit the best possible performance from all with whom he came in contact. A lifelong bachelor, he lived by himself in a small house in the wooded hills above the Stanford campus, where he delighted in entertaining friends and students. Perhaps from this kind of life came his often expressed concern for preservation of open space and his talent for giving wise counsel to politicians and environmental groups on problems of timber harvesting in the nearby hills. In 1970, with the help of students and other faculty members, he published a small book, *Logging in Urban Counties,* in which he analyzed at length the effect that unrestricted logging can have on soil, streams, and watersheds, with a resulting increase in the danger of fires and landslides. The book led to his appearance at many hearings on problems of open space and land management, and ultimately to the stiffening of logging regulations.

Allan had an extraordinary combination of scientific acumen, humility, concern for his fellows, and love of the natural world that makes his death at an early age seem especially tragic.

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To some of Allan's friends the occurrence of January 27, 1987, seemed unbelievable. He was a safety-conscious bicyclist of long experience, incapable of simply running off a road and smashing head-on into a redwood tree. Another possible explanation came a few days later. The collision may not have been an accident, but a carefully arranged suicide prompted by a police investigation of allegations that he had had improper relations with a teenage boy. The sad event becomes then a tragedy of true Shakespearean dimensions, for here is a character admired and loved by all about him, successful in everything he tries, who in the end may have been betrayed by a fatal weakness.

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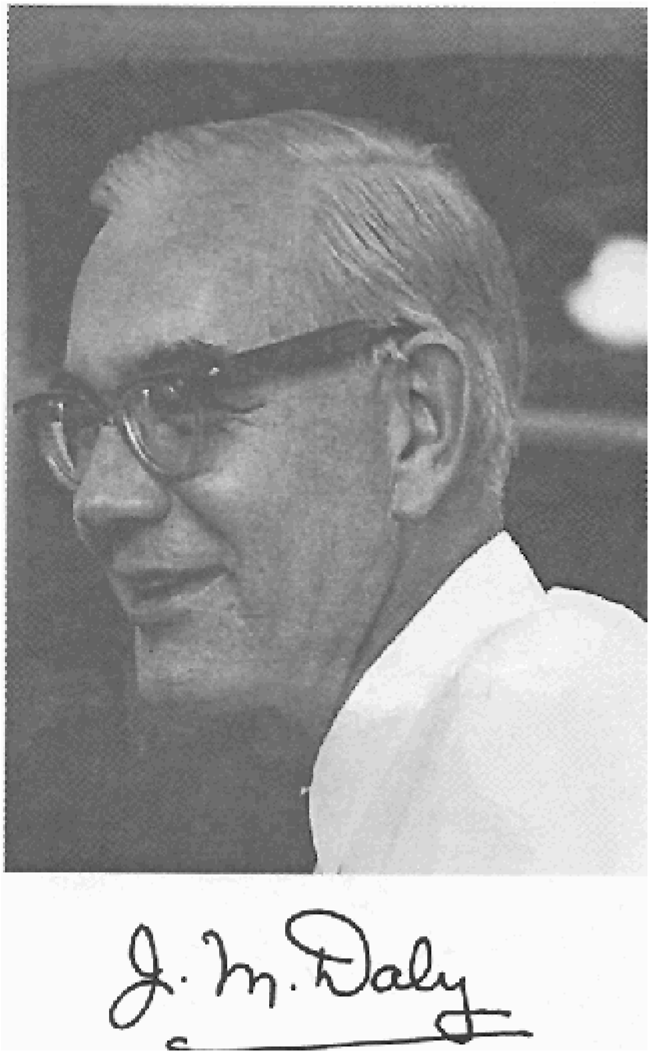

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# JOSEPH MICHAEL DALY

*April 9, 1922-August 18, 1993*

BY MYRON K. BRAKKE

JOSEPH MICHAEL (MIKE) DALY was an expert on the physiological interactions of plants and their fungal pathogens. Most of his research was on the rusts of wheat and bean, but he also investigated toxins produced by *Helminthosporium* species.

Research on rusts was difficult because the fungi could not be grown independent of the plant. Physiological differences between healthy and rusted plants reflected the metabolism of the rust fungus itself and changes in the plant's metabolism caused by the rust. Attempts to separate the two effects gave numerous theories and led to spirited discussions in which Daly reveled and excelled. His abilities to discover logical flaws in facile theories made him a respected leader in his field.

In the last part of his career, Daly turned to another system the plant diseases caused by fungi that produce toxins. Daly purified and determined the structure of the toxin of the fungus that caused the devastating southern corn leaf blight epiphytotic of 1970 and participated to varying degrees in studies of other similar toxins.

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## PERSONAL HISTORY

Joseph Michael Daly was born in Hoboken, New Jersey, to Julia (nee Yarwood) and Michael Daly on April 9, 1922, and grew up in Newport, Rhode Island. His father died when he was young, leaving support of the family to his mother. Daly spoke of her often and of how hard she worked in unskilled jobs to support the family. Daly was always Joe or Joseph to his mother, but there were too many “Joes" in plant pathology at the University of Minnesota, so Daly was always known as "Mike" to his fellow scientists.

Although he wanted to work to help support the family after high school, Daly's mother strongly encouraged him to go to college. He worked his way through Rhode Island College, where he was influenced by botanist Vernon Cheadle and plant pathologist Frank Howard to pursue a career in plant pathology. After obtaining his B.S. degree, Daly went to the University of Minnesota for graduate studies in plant pathology. There he married Cecilia Rieger, a botanist with an M.S. from Vassar. They had six daughters and two sons, all of whom survive: Katherine O'Rourke, Anne Schmidt, Melissa Hoy; Martha, Cecilia, Constance, Stephen, and Timothy Daly.

Family and religion were important parts of Daly's life. He served on the Catholic Social Service Board and was involved in educational efforts to promote awareness of the sanctity of life. At home he led the family's lively dinner-hour discussions and strongly encouraged his children in their diverse interests. He was proud of his wife when she obtained her second M.S. degree, in computer science, after raising eight children. She taught computer science and counseled students for many years at the University of Nebraska.

I first knew Mike Daly when we were both graduate stu

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dents at the University of Minnesota, he in plant pathology and I in biochemistry. Later, his recommendation helped me obtain a position with the U.S. Department of Agriculture in the plant pathology department at the University of Nebraska to start an association that lasted more than thirty years. Although we worked on different diseases of plants, it was a small department and I knew his students and his research problems as he knew mine. We shared equipment and for a while even an 8 x 12 foot office.

Mike was an intense and enthusiastic scientist, critical of his own work and that of others. Throughout his career he spent time at the laboratory bench doing crucial experiments himself. He checked and double checked his own experiments before drawing conclusions and often double checked those of others as well. He checked the calculations on most of the papers he refereed. His criticisms, both written and vocal in meetings and informal gatherings, set the standard for the field. Nobody got away with a bad conclusion as long as Mike was around.

Mike loved camaraderie and a good story. He was vastly amused by the foibles of his fellow man. One of Daly's favorite stories was of a fellow graduate student at Minnesota, who won and kept a dime from a bet on a rebroadcast of a prize fight that he had heard before. An enthusiastic sportsman and a gardener, Mike was an avid golfer with an incurable slice and a fervent fan of the Celtics basketball team and of Notre Dame football. He told me that Notre Dame players, some of whom he had had in class, were real gentlemen, not the jocks found at lesser schools!

## GRADUATE EDUCATION

The plant pathology department at Minnesota was dominated by its head, Dr. Elvin Stakman, an NAS member. Stakman had formulated the concept of physiological races

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of rust fungi to explain the variations in the patterns of disease severity caused by fungi from different sources when tested on a selection of cultivars of wheat. The susceptibility of a cultivar depended on the source of the rust fungus used to inoculate it and vice versa; the severity of the disease depended on the wheat cultivar on which it was tested. The rust pathogens from different sources were identical morphologically, and, therefore, their virulence differences were attributed to physiological differences, and they were termed "physiological races."

Many of the graduate students at Minnesota investigated some aspect of the physiology of plant disease. Physiology at the time included nutrition and environmental effects. Daly obtained an M.S. degree in 1947 under Helen Hart, after thesis research on the influence of nitrogen levels in the soil on the development of stem rust of wheat.

Daly next wanted to use biochemical methods to study the basis for the physiological differences of the rust races. To do so, he left the plant pathology department for the botany department at the University of Minnesota, but he did not leave his interest in plant diseases.

In changing departments Daly showed that he thought for himself, a characteristic that would make him a leader. I doubt that Stakman liked his departure. Stakman was a dominant figure in plant pathology, not only at Minnesota but nationally, and was deeply interested and involved in the training of graduate students and their subsequent careers. He did not like to lose good graduate students. Daly was an excellent student, willing to do the extra jobs that kept the department functioning. For example, he produced the index for 1946 through 1949 for the journal *Phytopathology,* which was edited by Helen Hart.

Daly's relations with Stakman were a bit strained until they had a cordial conversation at a meeting of the Ameri

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can Phytopathological Society in the late 1960s. Daly felt he was forgiven.

## PROFESSIONAL HISTORY

Daly completed his Ph.D. degree in 1952 under Dr. A. H. Brown in the botany department at Minnesota with a thesis on the presence of cytochrome oxidase in the leaves of higher plants and became one of the early users of mass spectrometry for physiological investigations. Upon graduation he accepted a position at the University of Notre Dame to teach botany. In 1955 Dr. Bill Allington offered him a position in the plant pathology department at the University of Nebraska with freedom to start his own research program. Daly accepted and started a research program on the physiology of rusted plants.

Daly was a professor of plant pathology at the University of Nebraska until 1964, when he moved to the Department of Biochemistry and Nutrition as professor of biochemistry. In 1966 he was named C. Petrus Peterson Professor of Biochemistry at the University of Nebraska, a position he held until he retired.

Daly was primarily a teacher and researcher, but he also was an able, though reluctant, administrator. From 1962 to 1964 he was chairman of the Department of Plant Pathology at the University of Nebraska. He provided leadership to the newly formed School of Life Sciences during 1973-74 as its first director.

Daly's advice was highly valued, and he was asked to serve on numerous committees and panels of university, government, and professional societies. He was an influential teacher through the courses he gave and the students and postdoctoral fellows he trained in his laboratory. He believed in close personal supervision of students and to that end had only a few students in his laboratory at any one time.

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Daly received many honors and gave numerous invited seminars and symposium talks, wrote many book chapters, and edited books and journals. He was elected to the National Academy of Sciences in 1984 and to the American Academy of Arts and Sciences in 1986. His publications reveal collaborations with scientists at several institutions, in addition to students and postdoctoral fellows. His most frequent collaborator was Herman Knoche, a fellow professor in the biochemistry department. Daly had close ties with Japanese scientists, particularly I. Uritani, Yoshiki Kono, and Yoshikatsu Suzuki. He was an active participant in a series of cooperative science seminars on the physiology of plant disease jointly sponsored by the National Science Foundation of the United States and the Japan Society for the Promotion of Science. Daly coorganized a seminar titled "Recognition and Specificity on Host-Parasite Interactions" held at the University of Nebraska in 1977. The last of these seminars that he attended, in Nagoya, Japan, in 1985, especially honored him.

## RESEARCH

Rusts have been important diseases, often limiting humans' food supply, since the beginning of agriculture. As obligate parasites, rust fungi obtain their nutrients and energy from host plants. The contribution of the rust fungus to the host plant is less clear, although the formation of pustules containing spores is an obvious morphological disruption of plant tissue. Some rust-infected plants produce galls, which are part plant and part fungal tissue, suggesting a growth-controlling effect of the fungus on the host. The narrow host range of the fungus and the presence of physiological races of the fungus show that subtle differences, under genetic control in both host and fungus, can determine whether disease develops.

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The above brief facts were known when Daly started his research. Daly and others tried to understand the interaction of fungus and host, partly out of curiosity and partly to help devise logical ways to control diseases. One aspect Daly investigated was the energy and nutrient supply to the fungus; another was specificity or the mutual recognition between pathogen and host that determined if the disease would develop; a third was growth stimulation of the host caused by the rust.

The hallmark of Daly's investigations was thoroughness. He investigated the respiration of rusted tissues at all stages of rust development, in near-isogenic lines differing in a gene for resistance, in lines with temperature-sensitive resistance genes, and with many approaches-inhibitors, labeled precursors, kinetic studies, anaerobic versus aerobic conditions, and others. Theories postulated by others and supported by data from a limited set of conditions often were unsupportable when tested under such a variety of conditions.

Daly produced strong evidence that the fungal tissue itself contributed to respiration of the host-parasite complex, particularly at sporulation. Furthermore, rusted leaves and hypocotyls were metabolic sinks into which sugars, sugar alcohols, and other metabolites were transported from uninfected parts of the plant. The rusted tissues not only needed extra nutrients for growth of fungal mycelium and spores, but the rusted bean leaves and safflower hypocotyls were often larger than comparable healthy tissue.

Some investigations showed that rusted tissue had high concentrations of indoleacetic acid (IAA), a growth-regulating hormone, a plausible explanation for the increased respiration and growth. However, it was hard to obtain consistent results in IAA assays because of technical difficulties in assaying leaves for hormones without interference from

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contaminating microbes. Daly investigated the metabolism of IAA in leaves and found that resistant inoculated tissues had higher levels of peroxidases and IAA decarboxylases than uninfected or susceptible rusted tissue. However, high levels of peroxidase persisted in the presence of ethylene or high temperature, both of which broke the resistance and allowed development of spore pustules. The hypothesis that high levels of peroxidase caused resistance was untenable.

In postulating reasons for the difference between resistant and susceptible cultivars, most plant pathologists consider susceptibility the norm and resistance to be an active mechanism induced by the presence of the pathogen. However, most fungi do not cause disease in most plants. The common viewpoint is to say that this is "nonhost" resistance, operating by a mechanism separate from that responsible for resistant and susceptible cultivars of a single species. Daly suggested that resistance was the norm and that susceptibility was induced by the right combination of pathogen and host. He argued that all observations, including that of Harold Flor's on flax rust, which led to Flor's gene for-gene hypothesis, could be explained by the induced susceptibility hypothesis.

## PATHOTOXINS

Widespread infection of corn with *Cochliobolus heterostrophus (Helminthosporium maydis)* race T in the United States in 1970 caused the loss of about 15 percent of the corn crop. Race T was rare in the United States before this, although it occurred elsewhere and race O was common in the United States. Maize geneticists soon showed that *H. maydis* race T devastated maize with Texas male-sterile cytoplasm, which was widely used to save the labor of detasseling in growing hybrid seed corn.

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It was soon found that culture filtrates of *H. maydis* (which is not an obligate parasite) caused typical disease lesions on T-cytoplasm corn but not on N-(normal) cytoplasm corn. This result showed that a fungal toxin caused the symptoms and that the specificity between T- and N-corn was due to the toxin. If the toxin could be purified, it would simplify the study of genetic specificity. Daly undertook the purification and characterization of the toxin with the help of several collaborators. In addition to being project leader, he got his hands dirty, literally, purifying the toxin with charcoal and chromatography. Toxin he purified has been sent to scientists worldwide for investigations on toxin action.

As a preliminary step to purification of the toxin, Daly sought a better bioassay than root growth inhibition. The toxin inhibited both photosynthesis and dark $CO_2$ fixation. Daly showed that an assay based on dark $CO_2$ fixation was quicker, more reproducible, and more accurate than assays based on root growth inhibition or ion leakage.

Purified toxin was needed for investigating mechanisms of action, and Daly concentrated on purifying the *H. maydis* toxin and determining its structure. He and his colleagues were soon successful and reported the toxin to be a family of long-chain, linear, saturated hydrocarbons with odd numbers of carbon atoms and four clusters of oxy/oxo groups. The three main constituents were $C_{42}H_{68}O_{13}$, $C_{39}H_{66}O_{12}$, and $C_{41}H_{70}O_{13}$. The oxy/oxo clusters were mixtures of 3, 5 dihydroxy ketone, and 3-hydroxy, 5-keto ketone sequences. The clusters were separated by three or five methylene groups.

Biochemist Herman Knoche gave major help in purifying the toxin, and organic chemist Yoshiki Kono assisted significantly in determining the structures during a two-year stay at Daly's lab. Kono continued to collaborate with Daly after he returned to Japan.

Daly and Yoshikatsu Suzuki synthesized the stereoisomeric

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$C_{41}$ compound and showed it to be as toxic as the natural toxin. The synthetic $C_{23}$ analog, with three methylene groups between two oxy/oxo clusters, was only slightly less toxic than the $C_{25}$ synthetic analog with five intervening methylenes, but both were 300 times less toxic than the natural toxin. All the synthetic compounds had the same specificity toward corn as the natural toxin; that is, they were toxic to T-cytoplasm corn but not to N-cytoplasm corn.

The $C_{24}$ synthetic analog with four intervening methylenes was slightly less toxic than the $C_{25}$ analog, but the $C_{26}$ analog with six intervening methylene groups was less than one-tenth as toxic as the $C_{25}$ analog, showing the importance of the length of the intervening methylene groups between the clusters of oxy/oxo groups.

*H. maydis* is not the only pathogen to cause a disease specific to corn with T-cytoplasm. An unrelated fungus, *Phyllosticta maydis,* causes a leaf blight of corn with T-cytoplasm. Daly and colleagues purified the toxin of P *maydis* and showed it to be a mixture of linear hydrocarbons, $C_{33}H_{60}O_{8,}$ $C_{33}H_{62}O_8$, $C_{35}H_{66}O_9$, and $C_{33}H_{62}O_{9.}$ As with *H. maydis* T-toxin, the PM toxin has four clusters of oxo/oxy groups separated by three or five methylene groups. However, the PM-toxin mostly has only two oxo/oxy groups per cluster rather than the three of *H. maydis* T-toxin. Purified PM-toxin and synthetic toxin were equally effective on mitochondrial oxidation in vitro and on dark $CO_2$ fixation by leaf slices. Both were a few times more active on a molar basis than the *H. maydis* T-toxin.

Daly also collaborated with Larry D. Dunkle in determining the structure of the host-selective toxins of *H. carbonum* and with V. Macko in determining the structure of the *H. victorriae* toxin.

Determining the structure of the toxins and synthesizing analogs was only the first step in Daly's planned investiga

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tions of the host-selective toxins. He wanted to use the toxins as tools to investigate the physiological basis for the host-parasite specificity. He started this investigation by studying the effect of T-toxin on dark $CO_2$ fixation, photosynthesis, and mitochondrial oxidation of NADH, succinate, and malate. His research was cut short by an untimely stroke in 1986, only two years after he was elected to the National Academy of Sciences. His contributions continued after his stroke, as his stocks of purified toxin were distributed to colleagues for investigations of action, including the identification of mutations in mitochondrial genes of T-cytoplasm corn.

## CONCLUSION

One of Daly's lasting contributions was the rigorous standards he brought to physiological investigations of plant disease. Physiology of pathogenesis was emerging when Daly became active, and he contributed more than anyone else to the establishment of high investigative standards. His critical analyses were crucial in a field where conclusive experimental evidence was difficult to obtain.

Another of Daly's main contributions was the purification, structure determination, and synthesis of the host-selective phytotoxins produced by *Helminthosporium* and other fungi. He set the stage for application of the molecular techniques that subsequently became available. But Daly's most lasting contribution may be the students and postdoctoral fellows he trained and influenced. He was an enthusiastic teacher who challenged his students with penetrating questions that stimulated thinking and lively discussions. Students who took his courses frequently came to him throughout their graduate careers for advice on research problems and professional matters. His greatest professional satisfaction came from letters written by former

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students who told him how much they appreciated his efforts in creating an environment for learning, to get them to critically read scientific literature, and to be equally critical of their own work.

I AM INDEBTED TO Mrs. Cecilia Daly for information on Mike's early history and personal life. Herman Knoche, Larry Dunkle, and M. G. Boosalis provided personal and professional information on Mike. Secretaries at the biochemistry department of the University of Nebraska kindly made their files available.

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# EDWARD SMITH DEEVEY, JR.

*December 3, 1914-November 29, 1988*

BY W. T. EDMONDSON

EDWARD S. DEEVEY, JR., converted the field of paleolimnology into a quantitative science that is a key to the immense treasure of information being cumulatively buried in the mud of lakes. The need for an absolute time scale put Deevey at the forefront of the use of 14C for dating lake sediments. Developing his central interest took him into related fields, each a major field in itself. He was a creative pioneer in several areas, including quantitative palynology, cycling of natural isotopes, biogeochemistry, population dynamics, systematics and ecology of freshwater zooplankton, and he promoted the use of life tables in ecology. In addition to research papers in professional journals and books, he published many reviews and commentaries in books and journals, and in various periodicals such as *Scientific American* (eight articles) and the *New Yorker* (one article).

Deevey was born in Albany, New York. He attended Albany High School and New York State College for Teachers before enrolling at Yale University, where he received a B.A. degree (summa cum laude) in botany in 1934. He then moved over to the zoology department where he found a congenial home. He finished a Ph.D. degree in 1938 at age twenty-three on “Typological Succession in Connecticut Lakes." He was the second student to do a Ph.D. with G.

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Evelyn Hutchinson (NAS). He held a Sterling Postdoctoral Fellowship in 1938-39. In 1938 he married Georgiana Baxter, a fellow graduate student, with whom he published several papers and had three children, Ruth, Edward Brian, and David Kevin, and three grandchildren. Georgiana died in 1982. Ed then married Dian Hitchcock, a geochemist specializing in sulfur, an interest quite compatible with his own interest in sulfur isotopes in lakes (1963;1983,2).

During the summers of 1938 and 1939 Deevey was employed by the Connecticut State Board of Fisheries to make limnological surveys of lakes. His first academic job, at Rice Institute, was ended in 1943 by a three-year stint during World War II at Woods Hole Oceanographic Institution where, as a civilian, he did research in marine biology of interest to the U. S. Navy. Much of the work consisted of identifying and counting fouling organisms from buoys, data of considerable importance to mine warfare and ship operations. In 1946 he returned to Yale where he progressed from lecturer to full professor. In 1967 he took a one-year appointment at the National Science Foundation as both head of the Section on Environmental and Systematic Biology and acting director of Environmental Biology. At that time plans for the U.S. contribution to the International Biological Program were being completed. Deevey took particular pleasure in his association with the program, but did not regret the brevity of the appointment. He told me that it would be dangerous for him to stay, explaining that he had begun to feel like God, and he was afraid it would be addictive.

In 1968 he took on the Killam professorship at Dalhousie University. That was his shortest academic appointment. In 1971 he accepted a Distinguished Graduate Research Curatorship in Paleoecology and Professorship at the Florida State Museum of Natural History at the University of Florida

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in Gainesville. He remained there, active until a heart attack following angioplasty ended his life at age seventy-five.

Through all these changes in venue Deevey's research followed a clear, continuous line of paleolimnology as a key to past global environmental conditions and human history. While he traveled widely from each of his home institutions he always took advantage of local conditions. At Rice and Woods Hole he collected information on hydroids that was useful later in his paleoecological interpretations (1950). His work in Florida and Guatemala was in karst regions heavily affected by human activity. In 1987 he went to the Peoples Republic of China to start similar work in the karst area of Yunnan Province with Chinese colleagues.

His publication record reflects more the development of his thinking, with side lines, than do his geographical movements. During his graduate work at Yale he made the first pollen stratigraphy for northeastern North America and made another for Tibet, using material collected by G. Evelyn Hutchinson on the Yale North India Expedition of 1932. They were the basis for his first two research papers, both aimed at climatic interpretation (1937;1939,1). These were soon followed in quick succession by a series of papers on modern conditions, neolimnology. One was a highly original multifactorial treatment of the regional limnology of Connecticut (1940). It used chemical and biological data gained in connection with his work for the Connecticut State Board of Fisheries and with existing geological information. Another was a major comparative study of the bottom fauna of thirty-six lakes, with ecological interpretation, equivalent in scope to a Ph.D. dissertation (1941). Still another, in collaboration with G. E. Hutchinson and A. Wollack, was a novel ecological interpretation of redox potentials at the mud-water interface suggesting that the species composition of the benthic insect population was af

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fected more by redox state of the trace metals in solution than by oxygen concentration (1939). All of these were relevant to the problems of paleological interpretation of sediment data that he was dealing with concurrently. In 1942 he published one of his major paleolimnological papers, perhaps the best both in breadth and depth, on the biostratonomy of Linsley Pond, based on his Ph.D. dissertation. The data consisted of chemical analyses of slices of several cores of lake mud and the results of intense visual examination and counting of pollen, diatoms, and of every fragment of invertebrate remains. The glory of the paper is in the richness of the data and the imaginative interpretation of changes in the lake and its surroundings. The most direct information about the ancient conditions within the lake was given by the remains of organisms, from organic molecules to visually recognizable fragments. The pollen content of the cores told about vegetation around the lake and led to interpretation of changes in climate, hydrology, and human influence. Remains of algae, crustaceans, and insects gave a species list of the community that he could interpret in terms of chemical and physical conditions in the lake. From all this he could read the 12,000-year history of changes of conditions and communities within the lake, changes in the climate above it, and some of the activities of human populations around it. The prevalence of *Bosmina* remains led him to study the systematics and biogeography of the genus.

Deevey became involved in population concepts while Georgiana was doing her Ph.D. study of the hematology of the black widow spider. Her records had data on the length of life of many individuals, males and females, giving a basis for a joint paper presenting a life table analysis, the first for an arachnid (1945). He is credited with introducing life table concepts into ecology with a paper that became a

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citation classic (1947). He continued to publish on neolimnology, particularly after his move to Florida, but there were about twice as many papers on paleolimnology, paleontology, and paleoclimate. His remarkable 1949 review paper on Pleistocene biogeography was a major and influential synthesis of existing knowledge.

The time scale of the events recorded in the cores was only relative, which was strong motivation for him to seize on the work of Willard F. Libby (NAS) on the use of 14C for dating archaeological samples. With a grant from the Rockefeller Foundation he founded the Yale Geochronometric Laboratory in 1951 and was its director until 1962 (1984). The goal of a worldwide paleoclimatology dominated the approach of the laboratory. The first spectacular discoveries, beyond simply getting firm dates for various events that had been known only in a relative way, were coordinations of climatic changes on both sides of the Atlantic. This opened the way to getting a real global climatic history. Dating made possible the calculation of absolute rates of deposition of pollen, and Deevey helped Margaret B. Davis (NAS) with her development of the method.

During all his time at Yale Deevey was in close communication with G. Evelyn Hutchinson. Starting with the establishment of the Geochronometric Laboratory, Deevey worked and published increasingly with new collaborators. He was associated with Richard Foster Flint of the geology department, an authority on the Pleistocene in North America. Both were close to Libby and all four helped each other, making an "informal institute. . .really getting carbon 14 on its feet. Very largely, the extent and speed of the spread of its use was due to Ed Deevey" (Hutchinson, 1984). One of Deevey's most important contributions to the interpretation of carbon dates in lake sediments was the demonstration that the basis of a discrepancy in dates from some

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lakes was a source of bicarbonate originating in ancient deposits of limestone (1954). He was assiduous in developing the laboratory (1984). He brought Minze Stuiver to it from Holland and Matsuo Tsukada from Japan. In 1969 the Laboratory closed, and both moved to the newly created Quaternary Research Center at the University of Washington in Seattle.

In 1964-65 Deevey spent a year in New Zealand. He took cores from several locations including Upper Pyramid Swamp, famous as a rich repository of bones of the extinct moa. He had already had vicarious experience with Pyramid Swamp paleolimnology nearly twenty years before when Robert Cushman Murphy provided him with cores taken in 1947. Deevey exploited them with a detailed analysis of the whole aquatic community as he had done in Linsley Pond many years earlier (1955,2). The sediments were highly unusual in the abundance of remains of ostracods. Two species coexisted for hundreds of years, varying reciprocally in abundance and population age structure. He was careful to point out that his studies had not explained the demise of the moa unless it was that the ostracods had nibbled them to death, an hypothesis he never published, although the moa was the basis of a *Scientific American* article in February 1954.

Deevey had a long-standing interest in the new world tropics, and made many trips to Mexico and Central America. His attention had spread from eastern North America to the Atlantic basin and then to the whole world. The climax of his research development was the project at Florida, Historical Ecology of the Maya, that melded paleolimnology, archaeology, and climatology to interpret the record of environmental consequences of prolonged human activity in a changing climate (1967;1979;1987,1).

It was characteristic of Deevey's way of thinking sometimes to compare the successional changes of a lake over

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time with embryonic ontogeny, possibly an effect of his experience in a department headed by Ross Harrison (NAS). Deevey had a high respect for intellectual processes in the historical development of concepts. This occasionally led him to hang onto ideas past their time, most notably in a tendency to identify eutrophication with the resultant increase of production and population density in lakes. He was a bit too impressed by the beauty of the sigmoid curve and relied heavily on it in his 1942 paper on the development of Linsley Pond for an interpretation that was refuted by one of his students many years later (Livingstone, 1957).

Deevey participated responsibly in many professional activities. He was on five editorial boards and was a member of eleven diverse societies, serving various functions, including the presidency of two, the American Society of Limnology and Oceanography and the Ecological Society of America. While at Dalhousie he was a member of the Fisheries Research Board of Canada and the Canadian Committee on the International Biological Programme. He received much recognition. He held a Guggenheim Fellowship and a Fulbright Research Award in Denmark in 1953-54. In 196768 he had a National Science Foundation Senior Postdoctoral Fellowship and a Fulbright travel award to New Zealand. He was elected to the National Academy of Sciences in 1981. In 1982 he received the Eminent Ecologist Award from the Ecological Society of America. The Florida Board of Regents awarded him a commendation in 1983.

Deevey had considerable influence as a teacher through his graduate students. He had nine doctoral and seven masters students and more than twenty postdoctoral associates. Many of them have gone on to distinguished careers. Through his teaching and publications he developed a large, admiring following. A consistent theme in comments by students in recounting their experience with Deevey is his kindness.

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Criticism was delivered gently and with respect (Livingstone, 1991). He taught by example. Some graduate students were surprised when they found that they were to work on their own research problem with his help, not on pieces of his problems. He was always accessible for questions, and sometimes the answer took hours, ". . . tucked in amongst related facts, personal anecdotes, and ajoke now and then .... We often kidded that Ed had the uncanny ability to go right to the periphery of an issue," said one.

A measure of his appeal was given by a seventieth birthday symposium on "Topics in Historical Ecology" in 1984, which was attended by about 300 people, some crossing the Atlantic. G. Evelyn Hutchinson gave a laudatory review of his career (Hutchinson, 1984). He stressed Deevey's contribution to the use of radiocarbon for dating and characterized his speaking and writing style as "verbal play and deep understanding of highly important truths," referring to his 1970 presidential address to the Ecological Society of America, "In Defense of Mud" (1970). In it Deevey pointed out that while we can refer to "pure air" and "pure water” one never refers to "pure earth."

Indeed, Deevey had an admiration for words that expressed itself not only in the frequent use of unusual words, especially in his book reviews and popular writings, but also in puns and a seemingly limitless stock of limericks, some of his own composition. His pleasure in literature and the arts was expressed in his writings by an abundance of learned and obscure allusions. A combination of admiration for Thoreau and scientific interest took him on a pilgrimage to Walden Pond, where one day of sampling resulted in a scholarly paper on its present limnological condition, including a comparison with Thoreau's own observations of temperature and transparency (1942,2). The work was done on a holiday from his job with the Connecticut fisheries board.

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His lecturing style was not his best feature. He had a quiet voice and hesitant manner, with a tendency to let sentences trail away. This was unfortunate, because he had many good things to say and some of his best humor was displayed in the dropped ends of sentences. His friends knew to sit in the front row at lectures. Deevey had a dry sense of humor, a capability of amusing and being amused, expressed in many ways. He appreciated similar traits in others. For instance, one of his colleagues at Yale established a principle that many of us recognize: Poulson's Principle states that "The day after you give a lecture on some topic an important publication on the same topic arrives in the mail." Deevey's corollary states "The next year when you try to get it from the library, it is at the bindery."

He also had a sense of value. Early one morning at a meeting, sitting on a stool in a restaurant to order breakfast, he noticed on the menu "One egg 50 cents. Second egg 25 cents". He asked for a second egg, and was served it, with a bill for 25 cents, accompanied by giggles from the waitress.

IN ADDITION TO PERSONAL knowledge from my many years of association with Ed Deevey, I received valuable information from Dian Hitchcock Deevey, Michael W. Binford, Mark Brenner, Margaret B. Davis, Daniel A. Livingstone, Minze Stuiver, and A. L. Washburn. Additional information came from tape-recorded remarks by Edward Deevey and G. Evelyn Hutchinson at the seventieth birthday celebration. The photograph was supplied by Brian Deevey.

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# BERNARD N. FIELDS

*March 24, 1938-January 31, 1995*

BY SONDRA SCHLESINGER

BERNARD N. FIELDS was a recognized leader in the field of viral pathogenesis—an area of medicine that dates from the time of Jenner and his development of a vaccine against smallpox to the present day and the pandemic of HIV—and, as I'll mention, Bernie had something important to say about both of these viruses. Bernie was diagnosed with pancreatic cancer in the summer of 1992 and died of the disease on January 31, 1995, at the age of fifty-six. His illness and death were deeply felt not only by his family and friends but also by the many scientists who had been influenced by his contributions to the field of virology.

In particular, Bernie will be remembered for emphasizing the importance of basic research in the area of clinical medicine and in helping to define molecular parameters that affect disease. Bernie was known to have an optimistic view of life, and I don't want to dwell on the tragedy of his death but on the contributions he made during his life. Before I start with a brief history of Bernie's life and career I should tell my readers one of the reasons I am the writer of this memoir. In 1992, just a few months after Bernie had been diagnosed with pancreatic cancer, he agreed to my conducting an oral history with him. Much of what I have

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written here comes from that source and permits me to quote him directly.

## COLLEGE AND MEDICAL SCHOOL

It is interesting to reflect on the influences in one's life that directly or indirectly lead one to career choices and, for scientists, research directions. Some of these influences were very clear for Bernie, and some were more subtle. He recalled that he had always been interested in the nervous system and how things injure it and realized that this interest almost certainly derived from the knowledge that his younger brother developed epilepsy when he was very young. His brother is now fine and has been for many years, but those episodes of seizures left an indelible impression. Bernie felt that the connection between his research and his own history had a strong and important effect on him.

These interests in research and the nervous system were not apparent when Bernie was growing up in Brooklyn in the late 1940s and early 1950s. He was not a particularly good student, and, although his math scores on the standardized tests were high, his high school grades were not. He thought he was lucky to have been accepted at Brandeis University. The person who interviewed him said that, even though he didn't have the record to be accepted at Brandeis, something "felt right" and they would take a chance.

Bernie entered Brandeis University in 1954 at the age of sixteen. This was the time when Herbert Marcuse taught international Communism and the history of the Chinese Revolution, Irving Howe taught English, and Max Lerner taught American Civilization. Bernie described Brandeis as an extraordinarily interesting small school that was totally alive and spirited: "I had suddenly learned how to learn, and I began to trust myself and enjoy college." Although he loved biology and became a biology major and premedicine

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concentrator, his reasons for choosing medicine as a career were rather vague. A major influence was the (Jewish) culture in which he grew up. His parents had lived through the insecurities of the Depression; their families were still in Europe during World War II and were killed in the Holocaust. A medical career track seemed to be a very secure future to choose.

Bernie attended NYU Medical School. He claimed that during the time he was in medical school he always planned to be a clinician and that he often felt cheated during the coursework because, as an example, instead of learning about infectious diseases in microbiology, he learned about bacteriophage. He did show some interest in research, however, as he spent the summers while in medical school doing research first at NYU and then at the Brookhaven National Laboratory.

After two years of intern and resident training at Beth Israel Hospital in Boston, Bernie took a fellowship in infectious diseases at Massachusetts General Hospital under the guidance of Mort Schwartz. That experience led him to seek further training in what he said was "the new discipline of molecular biology." He arranged to become a postdoctoral fellow with Bill Joklik at Albert Einstein College of Medicine in New York; but this was the 1960s, and before starting his training in molecular biology, Bernie had to do his military service, which he was able to do at the Communicable Disease Center (now the Centers for Disease Control and Prevention) in Atlanta.

It was during his stay in Atlanta that Bernie met his wife Ruth. Marriage to Ruth brought instant family: the three sons from Ruth's first marriage-John, Edward, and Michael-were adopted by Bernie. The family increased when Ruth and Bernie had two sons of their own-Daniel and Joshua.

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## RESEARCH CONTRIBUTIONS

Bernie's first publication came from his work at the CDC, but I suspect that if he were asked what his first scientific contributions were he would cite his work on reovirus and not those endeavors that included papers titled “The Isolation of Vesicular Stomatitis Virus from Mosquitoes in New Mexico" and "Pahayokee and Shark River, Two New Arboviruses Related to Patois and Zegla from the Florida Everglades."[1] His two years of training at the CDC were important. That experience provided Bernie with a broad view of the biology of viruses, and this served him in good stead as his research began to explore pathogenesis at a molecular level.

In 1976 Bernie moved to the Albert Einstein College of Medicine, where he began postdoctoral training with Bill Joklik. Joklik was well known for his work on vaccinia virus (perhaps this is one reason for Bernie's later interest in the smallpox virus), but when Bernie came to the lab he chose to work on reovirus. Although reoviruses are found in humans, they are not associated with any human diseases, as one can understand from the name: reo = respiratory enteric orphan. This virus was considered an orphan because, although humans were often found to be infected with it, it did not cause disease. In spite of—or perhaps because of —this, reovirus was an attractive entity to study in the laboratory.

Reoviruses are relatively easy to grow in the laboratory, and it is important to remember that at this time working with cultured cells was just changing from something of an art form to a controlled and reproducible science. The tools of molecular biology were well enough advanced so that it was possible to study the structure and replication of viruses such as reovirus, and already there was important in

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formation available about this virus. The genome of reoviruses was known to be composed of RNA, but it was different from other RNA-containing viruses: it was double-stranded RNA. Furthermore, it appeared that the genome was not a single molecule of RNA, as, for example, is the genome of poliovirus. Instead, it was segmented-a characteristic that had previously been described only for the genome of influenza virus.

It is the segmented nature of the genome of reovirus that permitted Bernie to exploit reoviruses in his genetic studies and later in his work on pathogenesis. Originally he was interested in obtaining mutants of reovirus and attempting to correlate genetic mutations with specific phenotypes. In the present day of recombinant DNA technology, the ability to generate mutations and identify the specific changes in both the gene and protein of a particular virus doesn't represent quite the challenge it did in the late 1960s and early 1970s. Bernie first isolated and characterized mutants that were temperature sensitive (they could grow at a temperature of 31°C but not at 39°C). Cells infected with two different temperature-sensitive mutants of reovirus could recombine to produce viruses that were no longer temperature sensitive if the mutations were in different genes. This type of recombination represented a physical reassortment of the double-strand RNA segments. Reovirus particles contain ten different segments of RNA. Each segment codes for a particular reovirus protein. In the assembly of new virus, segments from two different reoviruses can reassort, so that in cells infected with two different reoviruses the newly synthesized particles will be genetic hybrids containing some combination of segments from each parent.

Bernie was aware that different strains of reovirus could be distinguished by differences in their ability to cause disease in mice. Reovirus type 3 will cause acute encephalitis

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when injected directly into the brain of a newborn and is considerably more neurovirulent that reovirus types 1 and 2, which produce a clinically silent infection of ependyma in newborn mice.

Now directing his own lab, Bernie set out to determine whether these different phenotypes could be associated with a single gene and thus a single protein of reovirus. The tool that made this possible was gel electrophoresis: an RNA segment derived from one strain could be distinguished from its homologue in a different strain by differences in their mobilities. These first studies showed that when the gene coding for the virus protein sigma 1 came from the type 3 virus, the virus was neurovirulent. The sigma 1 protein is now known to be the protein responsible for attachment of the virus to the cell. Some of Bernie's more recent work focused on the structure of this protein.

Bernie and his colleagues, first at Albert Einstein and then at Harvard, continued to analyze the genetics of pathogenesis. Their work included identifying the gene responsible for the ability of the virus to grow well in the intestine and the gene most associated with the spread of the virus in the bloodstream. In addition to what were becoming almost classical pathogenic studies, Bernie's lab was exploring other directions. The role of the immune response is clearly a crucial factor in infection, and in the past few years work in the lab has included studies on the neutralization of reovirus by antibodies and T cell responses to the virus. At the same time, other members of the lab were beginning to look more closely at the structure of the virus and the viral proteins. These directions of research are continuing, and Bernie's contributions to the important initial work have provided a valuable framework for the future.

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## TEACHING AND OTHER SCIENTIFIC CONTRIBUTIONS

Bernie moved from the Albert Einstein College of Medicine to Harvard Medical School in 1975 and served as chairman of its Department of Microbiology and Molecular Genetics from 1982 until his death. His influence on the outstanding scientific reputation of that department is evident. An even more important contribution, however, was his concern for and training of young scientists. He summed his philosophy this way:

> One of the first things that needs to be really emphasized is that students and postdocs have been absolutely central in the most exciting discoveries that I feel we've made. They are the people who have done the experiments. I have been extremely fortunate in having a large number of outstanding students and postdocs. What do I do with a student when they come to my lab? Here is where intuition is not just scientific; it's got to be personal because people don't realize that running a laboratory is a very interpersonal process. One of the things I try to learn from the student is what are they like. How can you encourage them to find their own internal scientific voice? Because it seems to me that the students, who at any level often make the most profound discoveries, are talking from a very unique perspective, which is often their own metaphors, their own insights. The first thing that I like to find out is who the student is, where are they coming from, what they are excited about. And if you get the student to really dig in, choose a project, understand it, and come to grips with it, then I think you have done the most important initial steps. Later, you want to help them over the times that experiments don't work and you want to make sure they understand that if an experiment doesn't work, it's an experiment, it's not them. Separating and personalizing a failure at the bench from personal failure is a critical later point. No experiment works all the time and students don't know that; they haven't had enough successes. This problem of personalizing is often true for postdocs, and it's even true for faculty. The role of teacher and mentor has probably been one of the most satisfying aspects of my scientific career.[2]

I mentioned earlier that Bernie had an interest in both the smallpox virus and HIV. One of the triumphs of modern medicine has been the elimination of smallpox as a

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disease. For the past few years there has been a debate about whether the virus causing smallpox should be eradicated as well. The arguments over this issue have been more intense than most virologists might have expected. It is interesting to note that Bernie was against its loss to the world. He was very much influenced by the studies demonstrating how complex viruses are and how many of them have evolved mechanisms by which they can evade the immune system. One of the areas of pathogenesis that is just beginning to be explored is the realization that mutations in some genes of a virus may not have a phenotype in cultured cells, but that doesn't mean that the genes are nonessential in an organism. There are genes that produce a protein that can interact with a major histocompatibility protein and thereby affect the immune response. The complexity of the smallpox genome almost certainly means that there will be genes in this virus that have important—perhaps unique—functions in causing disease. This was Bernie's argument, which has many supporters, but there are also many scientists who strongly believe that it is better to rid the world of this hazard than to risk the possibility that it could somehow escape into the environment. Before leaving the subject of smallpox, it is worth mentioning that in 1721 smallpox wiped out half the population of Boston; this virus was truly devastating.

HIV illustrates how complicated viruses can be. It has humbled virologists who thought that they knew enough about viruses to keep them under control. In May 1994, just a few months before his disease again became apparent, Bernie wrote an editorial in *Nature* titled "AIDS: Time to Turn to Basic Science."[3] He argued that it was essential that we reevaluate the approaches initially taken in the early years of AIDS research. He felt that there were still so many

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gaps in our fundamental knowledge that it was critical to broaden the definition of AIDS-related research.

## FAMILY

Family was very important to Bernie, and he was very proud of his family's accomplishments. When the family moved to Boston, Ruth began to pursue an interest in art, particularly painting. She is now a well-established artist and has had numerous exhibits. Her work can be seen in galleries in the Boston area as well as in the homes of some of their friends. Bernie and Ruth's interests and careers were complementary. Ruth sometimes accompanied Bernie to conferences, especially those held in interesting locations, and enjoyed the interactions with Bernie's colleagues and his former students and postdocs. Traveling also gave them time to look at art and for Bernie to learn more about Ruth's perspectives.

## CODA

In ending this memoir I want to quote from Bernie's response when I asked him about his illness.

> Sure, I'm happy to say a little about my illness. I developed some symptoms about nine months ago of malabsorption that eventually led to a diagnosis of cancer of the pancreas. That diagnosis was made in July of 1992 and was obviously upsetting. The word upsetting does not describe my feelings, which were very powerful. My diagnosis was made at the time that I was planning to go to ASV [American Society for Virology] to host two dinners, to give a talk, and be with friends. Thus, my scientific community knew about my illness rather earlier than they might have. It was a very difficult experience because suddenly whatever future we all think we have was removed from me since cancer of the pancreas has a rather grim prognosis. In my own personal case, I was fortunate to go to a physician at the Dana Farber—Bob Mayer—who immediately changed my perspective and pointed out that I was a statistic of one, and even though I know the statistics of cancer of the pancreas, he said let's see what happens with you.

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> I started chemotherapy in the summer of 1992 and started trying to deal with my feelings about the disease. It was a process that I had to go through that involved intense pain, anxiety, and the need to find comfort. My wife and the rest of my family were very important and critical in the process. I started to meditate, which was extremely helpful to me in finding comfort. The amazing thing is that I am still alive, and we are now talking eight, nine months after the onset of my illness. I can honestly say on December 8, 1992, that after a horrendous beginning of the summer, I've had a nice fall. For whatever multiplicity of reasons, the tumor has not progressed the way pancreatic cancer usually does. I've had chemotherapy. I may have been fortunate in having a brisk immune response at the outset of the disease—the pancreatitis it started with. And for those or whatever other reasons that I can't fully account for, the disease regressed. Even though surgery seemed not to be feasible in July, I will be undergoing surgery next week. I can only say that the mind is a rather extraordinary organ. I would never have thought five, six months ago that I would have had a productive and fun fall. I also wouldn't have thought that I would have been here and would have had a future. Now I am gently taking steps that involve projecting a little longer into the future since it seems that my tumor has been indolent enough to even regress. . . . We'll see what the next step is.
>
> But regardless, there is an interesting literature about cancer that exists and is quite helpful. I think the most important thing is to say I have had a quite remarkably wonderful fall, in spite of knowing that I have this tumor. I guess I should thank the tumor and accept the fact that it's very important never to really give up hope when you have a disease like cancer because you don't really know the future. It's very easy to talk yourself into giving up. Also, be lucky in your doctor, be lucky in your friends and spouse, and hang in there because there are no absolute numbers that relate to you as an individual. These thoughts have been very helpful to me, and we'll see what happens. I think that's probably about all I can say, other than I wish myself luck next week as I have some pretty big surgery. I hope that I continue to be luckier than I thought I would be.

In many respects Bernie was lucky. He underwent surgery and chemotherapy in the winter of 1993 and after recovering was in relatively good health for more than a

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year. Most importantly, there was enough time for his family, friends, and colleagues to show him how much they cared.

## NOTES

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2. Bernard N. Fields, an oral history. This is an edited quote from the interview conducted by me on December 8, 1992.
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# RAYMOND MATTHEW FUOSS

*September 28, 1905-December 1, 1987*

BY MICHAEL A. COPLAN

RAYMOND MATTHEW FUOSS was born on September 28, 1905, in Bellwood, Pennsylvania, son of Jacob Fuoss and Berdie Zimmermann Fuoss. He attended Altoona High School in Altoona, Pennsylvania, which at the time was a thriving manufacturing and railroad center.

Fuoss entered Harvard University in 1922 at the age of seventeen. At Harvard his main interest was organic chemistry, but, while working as a private laboratory assistant for Professor G. S. Forbes the summer before his senior year, he developed an interest in electrochemistry. His first published paper, with Forbes and S. W. Glass, was on the topic of oxidation potentials and equilibria in the system chlorine, iodine, hydrochloric acid, and water. It appeared in the *Journal of the American Chemical Society* in 1925. A second paper with Forbes on the reaction of bromine and the chloride ion in hydrochloric acid appeared in the *Journal of the American Chemical Society* in 1927.

While at Harvard he studied mathematics with Coolidge and Birkhoff. Those were the days when every undergraduate was expected to take science and mathematics for each of the four undergraduate years. Fuoss also studied German, and this may very well have been the beginning of his

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lifelong interest in languages. It is interesting to speculate how Fuoss, a small energetic boy from an industrial city in central Pennsylvania, fit in at Harvard? Fuoss graduated from Harvard summa cum laude and Phi Beta Kappa in 1925 at the age of twenty after completing the four-year curriculum in three years. He seemed to have had an interest in football, at least as a spectator. He retained a loyalty for Harvard, participating in the small Harvard Club in New Haven during his Yale years.

From 1925 to 1926 Fuoss had a Sheldon Fellowship to study at the University of Munich. It was in Munich that he found his calling. He had a good opinion of his abilities in organic chemistry, as what summa cum laude Harvard graduate would not, but this changed in the course of his work with Wieland on the structure of the bile acids. His initial idea was to do Ph.D. research in organic chemistry, but exposure to lectures in thermodynamics and physical chemistry, along with some difficulty in finding the position of the double bond in the bile acids, caused him to change to physical chemistry. He attended lectures by Fajans on thermodynamics. Lange was also at Munich at the time. The only publication to come from the year in Munich was with Lange on the concentration dependence of the heat of precipitation of silver chloride.

Upon returning to the United States Fuoss married Rose Elizabeth Harrington. For one semester he was an Austin teaching fellow at Harvard in Chemistry B, and then in the winter of 1927 became a consulting chemist with the firm of Skinner, Sherman, and Esselen in Boston. He earned extra money by tutoring undergraduates evenings and weekends in a variety of subjects from chemistry to naval science. During this period his son, Raymond Matthew, Jr., was born only to die after two days of life. The consulting

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and tutoring brought him enough money so that by September 1930 he was able to resume his graduate studies.

Fuoss entered Brown University for his graduate work in order to study with Professor C. A. Kraus. Fuoss had ideas about organizing the large amount of data on the conductance of electrolytes in a variety of solvents, and Kraus was an inventive, original thinker. Scientific interest in electrolytes had been stimulated by the 1923 theory of Debye, but there seemed to be no systematic way of understanding the different experimental results. Kraus was willing to experiment with many different solvents and solutes and measured the properties of solvents as varied as liquid ammonia and liquid hydrogen cyanide. By astute choices of solvent, conductance data over a wide range of dielectric constant and viscosity were acquired. Conductance was also measured as a function of temperature.

Lars Onsager was at Brown at the time. Fuoss attended his lectures and this began an association that was to span more than thirty-five years. The 1932 paper that they wrote together on irreversible processes in electrolytes took up eighty-nine pages in the *Journal of Physical Chemistry* and remained the definitive treatment of the topic until it was taken up again by the two of them in the 1950s. Fuoss recognized that conductance for the wide variety of solvents and solutes for which there were data could be understood in terms of the electrostatic interactions between the ions in solution with the solvent taken as a continuum with the microscopic dielectric constant and viscosity taken to be equal to the macroscopic values.

The topic of Fuoss's Ph.D. thesis was the properties of electrolytes in non-aqueous solvents. It was completed in two years in 1932 under the direction of Onsager. The pace was accelerating. Fuoss was appointed research instructor at Brown in 1932 and later assistant professor for research,

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positions created by Kraus especially for Fuoss. Fully aware of Fuoss's abilities, Kraus encouraged him to broaden his education. Fuoss was sent to Michigan for a summer to attend the lectures of Sommerfeld and Pauli on quantum mechanics. In 1933 he took a leave of absence from Brown and, with an International Research Fellowship, returned to Germany to work with P. Debye in Leipzig for a year and with M. Wein for a short time at Jena. He also spent a summer with Fowler at Cambridge gaining experience with statistical mechanics. Quantum mechanics was not to play any role in Fuoss's research, but for many years he taught first-semester quantum mechanics for physical chemists at Yale.

Research during the Brown years resulted in thirty publications-many with Kraus. Several were published a number of years after Fuoss had left Brown. The papers are almost equally divided between experimental and theoretical research. The experimental work was mostly high precision conductance measurements of a wide variety of organic and inorganic salts in pure and mixed solvents. Of the irreversible processes in solution that can be measured, conductance can be done with the highest precision and provides the best test of theory. The goal was to collect data over a wide range of the parameters that affect conductance. Strong, moderate, and weak electrolytes were used, with the anions and cations chosen to cover a range of sizes and shapes. Solvents low to high dielectric constant and viscosity were used . Miscible mixtures provided the means for continuously varying the physical properties of the solvent. The work was characterized by careful attention to purity of the salts and solvents. Fuoss's experience with organic synthesis and purification was essential. The measurements themselves were of the highest precision attainable at the time and once again showed a meticulousness and

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zattention to detail that was the mark of all of Fuoss's research. Temperatures were controlled to within 0.01°C; concentrations had to be determined to better than 0.1%; the conductance cells, many of original design, had to be stable; and the measurements themselves had to be done in a way that eliminated all electrode effects and avoided any heating of the solution.

The theoretical work was an extension of the Debye-Hückel-Onsager treatment of the conductance of strong electrolytes in high dielectric constant solvents. The publications were mostly on the topic of weak electrolytes and the relations between association constants and the electrostatic forces between the solute particles. There were also mathematical papers on the solution of the conductance equation and the evaluation of the constants, the limiting conductance $A_O$ and association constant KA. Because Fuoss had available his own conductance measurements, the comparison between theory and experiment was straightforward, with each new system providing both answers and questions that could only be answered by further experiments and refinement of the theory. Some of his papers published during the period are reviews of the progress that was made both with the experiments and theory. Exceptionally clear, these papers are both a compilation of the data and summary of the theoretical understanding of the experiments.

The research program that Fuoss undertook required experiments, calculations, and theory, all at the highest levels. Only a scientist who combined the skills of an organic chemist, a physical chemist, a theorist in continuum mechanics and a mathematician could hope to succeed. For Fuoss it was the ideal choice.

In 1935 The American Chemical Society presented Fuoss its award for promising young chemists. Fuoss was thirty at the time. In the spring of the same year his second child,

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Patricia Rose, was born, and he was contacted by the General Electric Research Laboratory, then and for many years to come the premier industrial research laboratory in the country. He spent the summer in Schenectady as a consultant. This was the middle of the Great Depression with no money available for university research. The General Electric Research Laboratory provided a level of equipment and support that no academic institution could match, and Fuoss made the most of the opportunity, joining the permanent staff of the laboratory the next year.

The situation with polymers at the time Fuoss joined General Electric was similar to that in electrochemistry several years earlier. There was a large amount of data on the electrical properties of polymers, but it was difficult to assess their quality; until this could be done there was little hope of correlating the composition of the polymer with its physical and electrical properties. By eliminating surface effects and controlling the ionic content of the polymers Fuoss was able to obtain consistent reproducible measurements. It then became clear that the dielectric response of polar polymers depended on the nature of the polar substituents and the degree to which they were able to follow a time-varying external electric field. The frequency region over which the dielectric properties of a given polar polymer changed was found to depend on the concentration of the plasticizer as well as the size and shape of the plasticizer molecules. Between 1937 and 1945 Fuoss published twenty-six papers on the electrical properties of solids, almost all of which were based on work done before the beginning of World War II; during the war he worked on classified research topics.

Once the war was over in 1945, Fuoss accepted the first Sterling professorship in the Department of Chemistry of Yale University. His qualifications were perfectly matched

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to the department at the time. At Yale were Lars Onsager and John Kirkwood, two of the world's leading theorists in statistical mechanics, both of whom had published important articles with Fuoss. Also there were Herbert Harned and Benton Owen, supremely talented experimentalists in electrochemistry. Andrew Patterson and Phillip Lyons would be added to the faculty a few years later. With Fuoss joining the faculty, the department could well be considered the best in country in the areas of electrolytes, polymers, and statistical mechanics research. Fuoss at age forty still had unlimited energy and had made important contributions in each of the areas. For Fuoss, after the war years, the appointment was the chance he was seeking to return to academic research, teaching, and the training of graduate students. Once again he would work on problems that interested him rather than those suggested by others. He was, however, neither impractical nor opposed to applied research. Industrial consulting was an important activity for him in the years after graduating from Harvard; at Yale he continued his contacts with industry by consulting for DuPont, Monsanto, the California Research Corporation, and Arthur D. Little.

Upon arriving at Yale, Fuoss took up a new line of research that drew on his experience with electrolytes and polymers. This research was concerned with the properties of polyelectrolytes, high polymers with positive charge sites all along the length of the polymer chain. The mutual repulsion of the positive charges could change the conformation of the polymer chain and dramatically alter the viscosity and conductivity of polyelectrolyte solutions as a function of solute concentration. Fuoss synthesized a number of polyelectrolytes and measured their properties in a variety of pure solvents and solvents to which simple electrolytes had been added. He was able to explain the results with a mo

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lecular model that took into account the structure of the polymer and the inter- and intramolecular electrostatic interactions. The similarity between polyelectrolytes, proteins, and membranes was obvious, but the way to apply the results of the polyelectrolyte research to proteins was not clear at the time. This research, continuing well into the 1950s, was gradually replaced by theoretical and experimental work on 1:1 electrolytes in a variety of solvents. In 1951 he was elected to the National Academy of Sciences.

With Onsager, Fuoss systematically reexamined the assumptions and approximations in the Debye-Hückel-Onsager theory of electrolytic conductance. By retaining higher order terms, Fuoss found that the ad hoc assumption of ionic association for electrolytes in low dielectric solvents was no longer needed. A term analogous to the ionic association term now appeared in the revised conductance equation. The price to be paid for this theoretical rigor was the appearance of a second parameter, the ion radius $a_o$, and a much more complex equation. The parameter $a_o$ appeared both in the new ion association term and in one of the hydrodynamic terms. In principle, the values of $a_o$ derived from both terms should have been the same. Whether this was the case depended on suitable experimental tests based on higher precision conductance data than ever before and a reliable numerical method for extracting the physical parameters from the data. With typical energy and planning Fuoss and his students constructed a conductance laboratory where measurements with a relative precision of 0.01% were routine. Data analysis was done on a series of Yale computers at the computer center. The new theory was not a complete success. For a large number of solute-solvent combinations the ionic radius derived from the hydrodynamic term in the conductance equation was essentially equal to the hydrodynamic radius, but for low dielectric

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constant solvents there were significant differences. Much of the remainder of Fuoss's scientific career was devoted to exploring the origin of these differences and seeking better approximations in the derivation of the conductance equation. In 1974 he retired from Yale University but continued active research in electrolytes. From 1974 to 1980, the date of his last publication, he published twenty-two articles in refereed journals.

The legacy of this phase of research is over eighty research and review papers that cover all aspects of electrolytic conductivity from the development of new instruments and techniques to high quality conductance data and the theoretical development of ever more refined conductance equations. In addition to the research papers, Fuoss wrote *Electrolytic Conductance* in 1959 with F. Accascina of the University of Rome. This small book of 279 pages is written in the Fuoss style: direct, clear, complete, and precise, with no gaps between equations to be filled in by the reader. Introductory chapters on hydrodynamics, statistical mechanics, thermodynamics, and distribution functions are excellent summaries that neither oversimplify nor confuse with excessive extraneous detail.

A great deal has been made of Fuoss's extraordinary facility with languages. He could read, speak, and understand nineteen languages; he spoke of his interest in foreign languages as an avocation or hobby, but went about learning them with the same resolve, energy, and organization that served him so well in research. He traveled widely and collaborated with scientists in several countries, always mastering the language of his host country. He lectured in French at the University of Paris, in Italian at the universities of Rome and Palermo, in Turkish at the Technical University of Ankara, and in Hebrew at the Weizman Institute and Hebrew University. This was as much an effort to under

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stand what was going on around him as it was an interest in linguistics. Language, of course, requires one to mentally organize a large number of objects, words in this case, and find patterns. This was what Fuoss was best at in his research, and it is not surprising that he was able to apply his skills to this area. He saw his intellect as a tool to be applied to whatever problem was at hand.

Fuoss excelled at teaching and he enjoyed it. He routinely taught graduate courses in mathematics for physical chemists, quantum mechanics (essentially taking the viewpoint of applied mathematics), polymer chemistry, and electrochemistry. The lectures, planned with precision, contained no digressions; everything was well organized and clear. One came away from them, if not inspired, at the very least confident that the material could be mastered by diligent study; there were no mysteries nor imaginative leaps. In his research group organization prevailed and each member, whether an experienced research associate or first year graduate student, had a well defined project. Monthly reports, organized according to a strict format, were due from everyone the first Monday of each month. They were promptly read and returned with corrections. Three times per week there were group literature seminars at lunch. Although it never seemed so from the graduate student perspective, Fuoss was keenly aware of the abilities and limitations of the members of his group. This was reflected in the problems he gave them and the help he provided. Often impatient and abrupt, he was also encouraging and kind. His philosophy was that a research career was not made in graduate school but afterward. He provided the training and taught the skills; whether a student would go on to be a successful researcher depended on what was done after obtaining the Ph.D. degree. With the Fuoss system a Ph.D. routinely took three to four years. It is noteworthy that Fuoss himself fin

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ished his Ph.D. degree in two years. There was no spare time with this schedule, but the outcome was assured. A large fraction of former Fuoss students hold academic and research positions.

What kind of man was Raymond Fuoss? He was energetic, forceful, and impatient. There was no time to be lost. All routine tasks were accomplished on the spot at top speed to have time for the more important research tasks. He was single minded with respect to his work. While he would listen to others it was mostly to find the errors in their reasoning. He aggressively defended his work and his publications and was fair but uncompromising in assessing the work of others. As a self-made man he was a staunch conservative and an opponent of taxes.

He was devoted to his second wife, Ann Stein Fuoss, whom he married in 1947 after having been divorced from his first wife for some years. The Fuosses understood and complimented each other. Mrs. Fuoss provided a vivaciousness and ease that were appreciated by the professor. When Mrs. Fuoss died in 1979 it left a deep void in his life.

Raymond Fuoss left a number of contributions to chemistry and physics that are the basis of important fields of current research. He presented his conductance equation in a form that was accessible to experimentalists. He treated the hydrodynamics and electrostatics of ions in solution in a rigorous mathematical way and at the same time made high precision measurements to test the theoretical results. He took the initiative in providing experimenters the source code of the computer program used for the analysis of conductance. There were other theories at the time, but none in such a useful form. Fuoss recognized the biological implications of polyelectrolyte studies, and his formulation of the properties of polyelectrolytes remain a useful starting point for theoretical and experimental work on proteins. It

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is indeed rare to find a scientist with exceptional abilities in such a wide range of topics. Those who knew Raymond Fuoss appreciated his intelligence, hard work, and uncompromising devotion to scientific truth.

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# LESTER ORVILLE KRAMPITZ

*1909-1993*

BY ROBERT HOGG, CHARLES G. MILLER, AND C. WILLARD SHUSTER

LESTER ORVILLE KRAMPITZ was born in the family home at Maple Lake, Minnesota, on July 9, 1909. His father, Henry Richard Krampitz, was the son of German immigrants who had settled and farmed in the Albion region of Minnesota. His mother, Selma Wolff Krampitz, also came from a farming family of the same region. Had economic conditions been better, they too would have raised their family on a farm in what would now be considered a rural northwest Minneapolis suburb. Henry chose to seek his fortune in town, however, and was employed for many years in the grocery business in Buffalo, Minnesota.

Les benefited from Minnesota's high-quality public school system. In 1915 he entered grammar school in the South Haven system and in 1919 transferred to the Buffalo system. In 1926 he played guard on the Buffalo High School basketball team that won the state championship. This was in the days when a short, wiry guard could still make a varsity team. Les's younger sister Ione was born in 1917 while he was in grammar school; a second sister, Kathryn, was born in 1923 while he was occupied with high school and basketball. Although he lived his entire youth on the shores of Buffalo Lake, Les never learned to swim, a situa

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tion that nearly proved disastrous when one winter evening he and some friends opted to drive his old car across the ice on the lake. The car did not make it; the occupants did.

Les matriculated high school in 1927 and managed to scrape together enough money to enroll at Macalester College in St. Paul, Minnesota. He had to work his way through college because there was no help coming from home and no athletic scholarships, even for a former state champion basketball player. The tuition was $87.50 a semester and anyone with the money was accepted. Finding the money to continue in school was not easy as the country entered the depression years. Summer employment in a Detroit automotive plant provided funds for one year.

At Macalester Les met two of the most important people in his life. One was Norma Peterson, who became his wife in 1931. The Peterson family traditionally sent their children to Macalester. The other was Harland Goff Wood, who became his colleague and lifelong friend. Les and Harland both went out for football, with Les playing guard and Harland a running back. Together they probably weighed less than a present-day freshman lineman. They were both employed in the college kitchen, and Les often said he lettered in football and potato peeling. During college Les began to hunt deer with the Wood boys of Mankato,an annual event that continued into the 1980s. Harland's brother, Earl, later met and married Norma's sister, Ada Peterson.

Les graduated from Macalester in the spring of 1931 with a Bachelor of Arts degree, having majored in biology and chemistry and minored in German. He accepted a position as a science teacher in the Taunton, Minnesota, school system. However, this proved to be a short-lived career because the citizens of the area voted to bus the students to the next town to save money and Les ended up unemployed. Having just married Norma, he needed gainful employment,

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and so took a position managing a “Quick Lunch" in Buffalo. This lasted only a few months until he located a position in the school system of Sacred Heart, Minnesota. In 1932 his only daughter, Joyce, was born. The following year he left Sacred Heart for a higher-paying position in Crystal Lake, near Mankato, where he coached basketball and spent six years teaching in the high school. One summer he set a door-to-door sales record for World Book encyclopedias. During this time he renewed his friendship with the Wood boys. In 1938 he resigned his position as teacher and principal to join Harland Wood at Iowa State University in Ames.

Harland's graduate advisor, C. H. Werkman, was initially reluctant to gamble on a student who had been out of the academic environment for eight years. So Les enrolled in summer school at Iowa State and did so well by dint of hard labor that Werkman provided him an assistantship at the end of the summer. Thus began the relationship of Les Krampitz, Harland Wood, and Merton Utter in the cornfields of Iowa and eventually in the medical school at Western Reserve University in Cleveland, Ohio.

Werkman's research laboratories were spread between the Industrial Science Research Institute and the Agricultural Experimental Farm. Les became affiliated with the Bacteriology Section of the institute. Werkman's research interests centered on microbial metabolism. The organisms of choice were *Micrococcus lysodeikticus* or *propionibacterium.* Harland Wood, now an assistant professor in the Department of Bacteriology, had shown earlier the fixation of $CO_2$ by the heterotrophic bacteria. He proposed that pyruvate plus $CO_2$ yielded oxaloacetate. Les's research demonstrated a magnesium-dependent enzymatic activity capable of decarboxylating oxaloacetic acid. With advice from A. O. Nier (a physicist at the University of Minnesota) Krampitz and Wood built a 60-foot thermal diffusion column to enrich ($^{13}C$)

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methane, which they converted to $^{13}CO_2$. Using a mass spectrometer of their own construction to measure the isotope, they demonstrated the exchange of $^{13}CO_2$ into the carboxyl group of oxaloacetate. One story claims that Mert Utter inadvertently used the elevator whose shaft housed the diffusion column and set progress back at least a week for reconstruction. Les received his Ph.D in Bacteriology with a minor in Biochemistry from Iowa State in 1942.

His interest in microbial metabolism had kept Les abreast of the work of D. W. Woolley at the Rockefeller Institute in New York and he was able to obtain a research assistant's position in Woolley's laboratory for what we would now refer to as postdoctoral studies. Research centered on naturally occurring vitamin antagonists. Les worked on the artificial generation of scurvy in rats by feeding glucoascorbic acid, which appeared to irreversibly inactivate ascorbic-acid utilizing enzymes, possibly the mixed-function oxygenases involved in the conversion of proline to hydroxyproline in collagen. He also studied an enzyme in fish tissue that hydrolyzed thiamine between the pyrimidine and thiazole rings. This was not microbial biochemistry, but there was one project that involved the study of the inhibition of growth of *Lactobacillus casei* by a crystalline protein isolated from wheat. Les showed that the inhibition could be reversed by the addition of lecithin and other phosphatides to the media. At the end of the year Les loaded his family into the 1932 Chevrolet that had brought them to New York (and was parked for a year behind the Rockefeller building) and returned to Iowa to accept an assistant professorship offered by his mentor C. H. Werkman in the Department of Bacteriology at Iowa State.

Les stayed at Iowa State for three years and focused his research once again on *Micrococcus lysodeikticus* and its respiratory enzymes. He renewed his collaboration with Werkman

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and together they studied acetyl phosphate as a reactive compound, developing assays for diacetyl, and demonstrating the reversibility of the "phosphoroclastic" reaction in which pyruvate is converted to acetate and formate with acetyl phosphate as an intermediate.

In 1946 Les received a call from Harland Wood, then Chair of the newly formed Biochemistry Department in the Medical School at Western Reserve University, offering him an associate professorship. Within the year Les and his wife and daughter loaded the 1932 Chevrolet once again and moved to Cleveland. As a member of the biochemistry department in the medical school Les continued to pursue his research interests in microbial systems. Within two years he was asked by the Dean of the Medical School, Dr. Joseph Wearn, to move downstairs to the second floor to establish and become Director and Professor of the new Department of Microbiology.

It was as department chairman and through the people he brought together and the environment he established that Les had his greatest influence on the field of microbiology. The research support facilities of the department were the envy of many and included centralized media preparation and dish washing, a stockroom, and photography and electron microscope capabilities. He insisted on a policy of shared equipment and space, not only for efficiency and economy, but also to foster an interactive departmental spirit and to counteract the tendency for insularity among faculty and students. Thursday morning faculty meetings were followed by a brown-bag departmental lunch where students and faculty informally talked about their research problems to garner departmental input, and if no one rose voluntarily, Les did not hesitate to point a finger. Everyone soon learned it was better to come prepared and leap forward than to hang back. The annual Christmas party was notori

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ous: the anonymous poems and gifts given out for all to enjoy—Les kept his faculty and students interacting.

Over the years Les was able to attract an outstanding faculty. Some of the people he gathered to the department included Leon Cambell, Irving Crawford, Michael Fanger, Howard Gest, Joseph Lampen, Charles Miller, G. David Novelli, John Spizizen, Abram Stavitsky, Morris Tager, and Charles Yanofsky. Individuals who trained in this environment included Tom Brock, a young postdoctoral fellow, John DeMoss, Elliot Juni, Eugene Nester, Howard Peck, Peter Plagemann, Earl Swim, T. P. Wang, Neil Welker, and Frank Young.

Work in Les's laboratory continued to focus on questions in microbial metabolism. Howard Saz, an early student, established the oxidation of acetate by *Micrococcus lysodeikticus* and critically evaluated the possibility that the tricarboxylic acid cycle existed in bacteria. Until this time it was generally held that bacteria used a dicarboxylic acid cycle. Using isotopic methods, Earl Swim and Les went on to clearly establish the tricarboxylic acid cycle as a functionally significant metabolic pathway in bacteria. Elliot Juni and Les established the role of acetolactate as an intermediate in the production of acetoin. During this time the entire department worked on problems of microbial metabolism and many interactions and collaborations existed within the faculty.

In 1950 his daughter Joyce followed in her father's footsteps and went to Iowa State, where she met and married John A. Hansen. They eventually settled in the Indianapolis area and raised Les's two grandchildren, Leslie and Scott. In his later years Les knew his great-grandchildren, Leslie's two daughters, Kisha and Shawna Burk.

In 1955 Les received a Fulbright Fellowship to go to Munich, where he worked in the laboratory of Feodor Lynen

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on the utilization of tartrate. The highlight of his year there was his visit with Otto Warburg, perhaps one of the few times Les was at a loss for words. Lynen introduced Les to skiing, which he continued to enjoy on his return to the United States. Eventually he purchased a ski cabin in the Holiday Valley region of upstate New York which was also enjoyed by his friends Harland and Millie Wood, Warwick and Adi Sakami, and Fitzi and Eva Lynen.

Les was an active participant in the reorganization of the medical school curriculum at Western Reserve to the organ-system based, integrated teaching method. It was the first time that the material presented to medical students was not presented by discipline. Clinical studies were introduced in the first year and many of the faculty attended each other's lectures to facilitate interaction and references to previously described systems and conditions. This new approach had a major influence on medical education in this country. Medical students were required to pursue research projects and submit a written thesis. Les and Harland were the active motivators of the clinical faculty to get this process off the ground. These revisions still stand today, although there is no longer a thesis requirement. In 1958 Les was awarded a honorary doctorate of science by his alma mater Macalester.

In the 1960s his research interests moved back to the subjects of his postdoctoral days in Woolley's laboratory, particularly the mechanism of thiamine in enzymatic reactions. He pursued the mechanism of hydroxyethyl thiamine diphosphate in reactions of alpha-keto acids and dihydroxyethyl thiamine diphosphate as a possible intermediate in the transketolase reaction. During the late 1970s Les's research interests took a major shift when he became interested in the generation of hydrogen by the biophotolysis of water as a potential renewable energy source. He pursued

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this interest until his retirement. He stepped down as chair in 1978 and theoretically retired; however he maintained a laboratory on the floor and came in daily until illness forced him to stop in the early 1990s. It was not until the mid-1980s that he stopped hunting with the Wood boys of Mankato. Venison stew was often on the menu at Thursday lunch.

Throughout his career he pulled his weight in service, serving on the editorial boards of the *Journal of Bacteriology* and the *Proceedings of the Society for Experimental Biology and Medicine.* He sat on both the Biochemistry and Bacteriology Study Sections of the National Institutes of Health, as well as the Research Career Development Award Committee and the Microbiology Training Grant Committee. In 1963 he enlisted the entire department, including students, to host the sixty-third annual meeting of the American Society for Microbiology in Cleveland. He was elected to the National Academy of Sciences in May 1978.

Les had a lasting influence on the development of microbiology in this country through his scientific pursuits, the students he trained, and the young faculty whose careers he fostered. Many have gone on to chair departments (15 at last count). They took with them the philosophy and ideals of what a department should be like: interesting, exciting, and fun. Les lived for his department and his ideals continue to shape the careers of the scientists he trained and influenced.

THE AUTHORS gratefully acknowledge the participation of Joyce Hansen of Claremont, Indiana, in the preparation of this memoir.

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# ERNEST GEORGE MERRITT

*April 28, 1865-June 5, 1948*

BY PAUL L. HARTMAN

ERNEST GEORGE MERRITT, emeritus professor of physics at Cornell University and long-time member of that department and personification of American physics, died June 5, 1948, in Ithaca, New York, after a short illness. He was born April 28, 1865, (two weeks after Lincoln's assassination) in Indianapolis. As a youngster he already showed an inclination toward his adult vocations of editor and scientist. By the age of eight or so, in Indiana no less, he had founded and edited two journals, *Sea Breeze,* and its successor, *The Mountain Echo,* which he also printed and bound; and he had obtained a modest telescope which he housed in his small observatory. The *Indianapolis Journal* reported on “the boy astronomer (whose) eager study of the stars may some day make him famous."

In high school Merritt showed a talent for mathematics, winning a small prize. After one year at Purdue he went to Cornell's engineering school, graduating with a degree in mechanical engineering. When the mathematical and astronomical inclinations disappeared is not clear. Whatever transpired, he failed his first course in physics but became so enamored of the subject (and possibly of Cornell's demonstration lectures) that he decided to make physics his

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career, earning after graduation a master's degree in physics and becoming an instructor in 1889, followed by assistant professor in 1892, all in the Cornell physics department. After this last appointment he spent a year in Berlin studying with Max Planck, among others. He remained a friend of Planck's to the time of Planck's death, sending relief packages to the family following the end of World War II. The extant notes he took on Planck's thermodynamics course are beautifully done. He became a full professor in 1903 and head of the department in 1919, succeeding Edward L. Nichols. He held that position until his retirement in 1935, when he became "E. Merritt, emeritus," a title he had long looked forward to.

In 1893 Nichols, with Merritt as co-editor, founded *The Physical Review,* recognized today as the world's premier journal of physics. They were joined shortly by their colleague, Frederick Bedell. The three ran the journal until it was taken over by the American Physical Society in 1913, Bedell continuing as managing editor for another decade. Not only did they manage the journal but were major contributors, along with other Cornellians, then past and then present. They wrote scientific articles, short communications, notes of interest, book reviews, and obituaries. Merritt, like his colleagues, reviewed many books on physics-from Dolbear's *Matter, Ether, and Motion* (a treatise not widely referred to these days) to one of his last, Boltzmann's *Populare Schrifter,* a collection of his talks and magazine articles, including some on thermodynamics. In his review Merritt was critical of the inclusion of "Reise einer deutschen Professors in Eldorado" on the author's visit in the summer of 1904 for a course of lectures he gave at the University of California. In his last contribution to the journal, Merritt (1938) extolled in a memorial for Edward Leamington Nichols, then recently deceased, the accomplishments and attributes of his

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colleague. Reading it, one is reminded of the writer himself.

Of Merritt's many technical papers the first appeared in volume I of *The Physical Review* and was titled "On a method of photographing the manometric flame with applications to the study of the vowel A." For this paper he used a translated photographic plate and the manometric flame—a primitive oscillograph in which a small bright flame made sensitive to sound vibrations is imaged on the moving plate.

In 1898, with Nichols, Bedell, and Professor Shearer, he heeded the call of Professor A. G. Webster of Clark University to about forty physicists in the country to meet at Columbia University to form an American Physical Society. The meeting was held and Rowland was named its first president, Merritt its secretary, and Nichols a member of its council. Merritt had been secretary of Section B (Physical Sciences) of the American Association for the Advancement of Science and later chairman of the section. He served the new society well and after fifteen years as secretary he became president for two years (1914-16) and a member of its council for still more years. Twenty years after its founding the society took over the *Review*. Thus, altogether, beyond his university duties, Merritt served American physics one way or another for over forty years. As early American Physical Society secretary he had responsibility for organizing the frequent meetings of the body; in many a crisis he had to deliver a paper himself to fill out a program. Physics has changed over the years.

His interests were diverse. Perhaps his major research contribution was the extended series of investigations he did with Nichols on the luminescent properties of over 100 materials: phosphorescence and fluorescence at low and moderate temperatures, the decays and recoveries, etc. In their work together over the years Merritt and Nichols were

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seen almost as a single individual. The work resulted in a long Carnegie Report (No. 298, 1912) followed a decade and half later by another (No. 284, 1928) of Nichols, Howes, and Wilber, a continuation of the first. But Merritt had other interests: acoustics (exemplified by his manometric flame), electromagnetic oscillations, radio propagation (particularly on either side of sunset and during auroras and a notable Ithaca total solar eclipse), gaseous discharges, and many "cathode" ray experiments.

Early on, Merritt was much interested in the fundamental particles of the gas discharge, as were others here and in Europe. Recall that the electron had been discovered by the turn of the century but there was still much to elucidate, such as whether the "cathode" ray entity was that of photoelectricity and that of the emission from a heated filament. With filaments obtained from Edison (used in his incandescent lamps) Merritt and O. M. Stewart went beyond the inventor's Edison effect and studied the phenomenon in rarefied gases and in their best vacuum. The vacuum plots seem remarkably modern.

He studied the reflection of cathode rays at a metal surface. It was known that charged particles came from a surface bombarded by a cathode ray beam, but it was not clear whether they arose from another mechanism of emission or were part of the original beam merely bouncing off. In a rather neat and simple arrangement Merritt agreed on the reflection hypothesis. In a tube, highly evacuated for those days, a beam from a concave cathode was focused on a pin hole in a plate located at the intersection of the main column and a side arm of his vacuum tube, the plate set at an angle of 45° to the incident beam. In either arm beyond this plate was another plate also perforated such that any beam passing through the incident pin hole or that reflected from the surface surrounding it would be restricted

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in divergence to form a small visible light area on the glass envelope at the end of each arm. He found that the reflected spot in the side arm was deflected with a horseshoe magnet by the same amount as the primary beam in the main arm, supporting the ballistic nature of the reflection of the particles. Independently, Lenard in Europe was coming to the same conclusion, but all investigators were a long way from Davisson and Germer. The experiments and techniques then available now seem very primitive. Attainable vacua were hopelessly inadequate, but the concepts were sound and results were achieved.

With Stewart again he studied the photo effect, about the nature of which there was still question. Particles leaving a metal plate illuminated obliquely by ultraviolet light incident through a quartz window on their vacuum tube impinged after acceleration onto a collector at some distance in front of the illuminated plate. On either side of the collector was another similar electrode. The small collected currents were measured with an electrometer by the rates of charge accumulation. With an applied magnetic field the charging rates of the three collectors could be changed corresponding to the deflection of particles in their traverse from the emitter to the collectors, allowing their identification as cathode rays.

As noted in the memorial statement for Merritt in the Cornell Faculty Necrology for the year of his death, he "sought always to analyze his results and interpret them in the simplest possible terms .... When demonstrating the then new phenomena of electric waves to graduate students, he was the envy and the inspiration of his pupils because of his skill in throwing together crude pieces of apparatus that could work perfectly to demonstrate the point in mind." As might be gathered from his first Cornell exposure to physics, he took great delight in lecture demonstrations. One of

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his most successful was on Hertzian waves. Instead of the feeble spark of Hertz's receiver spark gap, barely visible except to a few hovering over it, Merritt combined it with another gap and a Geissler tube such that when the receiver gap broke down the Geissler tube flashed, easily visible to a large audience.

He brought out the essential sameness of the particles emitted from metal cathodes by light as by temperature. He was early in the use of silicon as a detector of short radio waves. Strict linearity with intensity having been shown in photoemission, he, with Nichols, was prompt in using the effect in film photometry. The 1921 (and 1951) cumulative index of *The Physical Review* illustrates the diversity of his interests (and the variety of books he reviewed).

He was concerned with what he saw as the inadequacy of the university in support of research and sought to enhance it. And he was a good teacher. He would lecture and arrive at a result, appearing to be as surprised at the outcome as he wished his audience to be. One recalls his last lecture; it related naturally enough to the gas discharge. He had a demonstration, his wife watching from the back row. The pump started up; with voltage across the discharge tube it presently broke into color and, beaming, he looked up at those watching as if to say, "Look at that, would you?" and then went on to the rest of his lecture.

While Merritt and his wife were of the Quaker persuasion, they did not hesitate to support their country during the two world wars. In World War I at New London he directed experimental and development work related to submarine detection. After Pearl Harbor he wrote to the Navy to inquire why in protecting the harbor it had not used some of the methods developed earlier. Secretary of the Navy Frank Knox responded that such had been used, with some positive results, but things were so chaotic during the

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attack that no great success could be claimed for the installations. Merritt suggested to DuBridge some subjects which the Radiation Laboratory at MIT might pursue, but, then in his late seventies, he took no active part in World War II efforts. During the war he and his wife were busy with Bundles for Britain and elsewhere. Following the war they took active roles in alleviating the distress of war victims, both enemy and friend. It was during this time that they were helpful to the Plancks.

His university duties were many. While it remained for his successors to implement, it was under his chairmanship that the Cornell physics department decided to get into nuclear research and later the high energy field, areas in which it became preeminent. Beyond the department leadership he was the first dean of the Cornell graduate school and for three years served as faculty member on the Cornell Board of Trustees. He and his wife were gracious people, hosts to many, truly of the "old school." During his Cornell years more than 400 physicists received training in the department, many of them going on to become heads of physics departments, spreading the physics "word," a not insignificant factor in the growth of the enterprise in this country.

Merritt had a long interest in photography, not only in his science but also as a hobby. He took delight in making movies on a winter's day, and then showing the hilariously amusing antics of motorists attempting to negotiate one of Ithaca's steep hills after a sudden heavy snow. And to catch on film from a constant-velocity boat alongside a Cornell racing crew, the surges backward and forward of the rowed shell relative to a fixed marker on his own vessel—ahead each time the backward facing crew, oars out of the water, slid toward the coxswain, bracing itself for the next pull on the oars, and the seeming retardation as the crew slid back on the pull stroke. A nice show of momentum conservation

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complicated by the friction of the shell-water interface and the movement of the oars themselves. The effect would have been greater with a heavyweight crew sliding back and forth than with featherweights, but that variation on the theme was not captured by his camera.

Following a visit to Cornell by Max Born on a miserable winter's day, Merritt sent him a photo of a more benign Ithaca and a strip of film taken of Millikan in a Cornell lecture—alongside, a strip of density variation "conveying the lecturer's words," a technique "developed in a laboratory not far from here," he wrote Born.

One amusing story he enjoyed recalling and which he wrote up (to be found in the Cornell Library Archives) may not be inappropriate. Following the gift of a liquid air machine to the Cornell physics department there was to be a public lecture on low temperature. In preparation, burned out ceiling lamps in the lecture hall were replaced. The rejects, and there were quite a few of these, were where the janitor had left them, in a carton on the floor near the lectern and below a fire extinguisher on the wall. The hall was so crowded that Merritt could see the demonstrations only through a door at the front. All was going well until a spectator sitting on the front table, squirming about for a better view, knocked the fire extinguisher off the wall onto the box of old lamp bulbs, spilling out the contents. Some of the bulbs popped and the extinguisher went into play, spraying the audience. There was panic. Liquid air was obviously on the loose. People scrambled out, crunching more lamp bulbs, adding to the pandemonium. Merritt said he had not learned much but "it was sufficiently interesting."

Ernest Merritt was a rather small pixieish man, somewhat hard of hearing, eyes sparkling, who obviously enjoyed what he was doing, just a delightful person. In his research he did not make momentous discoveries but in his associations

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and activities he made a large contribution to American physics. It is for that and for his long useful service to the enterprise that he should be remembered.

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Merritt published almost exclusively in *The Physical Review*. A partial list follows—what one hopes he might consider his most important papers. Not included are book reviews and many abstracts of papers given at meetings. A complete list is in *The Physical Review* cumulative index published in 1921, followed by another in 1951.

1893 Photography of manometric flame. *Phys. Rev.* 1:166.

1895 On the absorption of certain crystals in the infra-red as dependent on the direction of the plane of polarization. *Phys. Rev.* 2:424.

1897 The distribution of alternating current in cylindrical wires. *Phys. Rev.* 5:47.

1898 The magnetic deflection of reflected cathode rays. *Phys. Rev.* 7:217. A vacuum tube to illustrate the slow diffusion of the residual gases in high vacua. *Phys. Rev.* 6:167.

1899 A lecture demonstration to show the influence of ultra-violet light on the spark discharge. *Phys. Rev.* 5:306. The resistance offered by iron wires to alternating currents. *Phys. Rev.* 9:294.

1900 With O. M. Stewart. The development of cathode rays by ultraviolet light. *Phys. Rev.* 11:230.

1904 With O. M. Stewart. Conductivity produced in rarefied gases by an incandescent cathode. *Phys. Rev.* 18:239.

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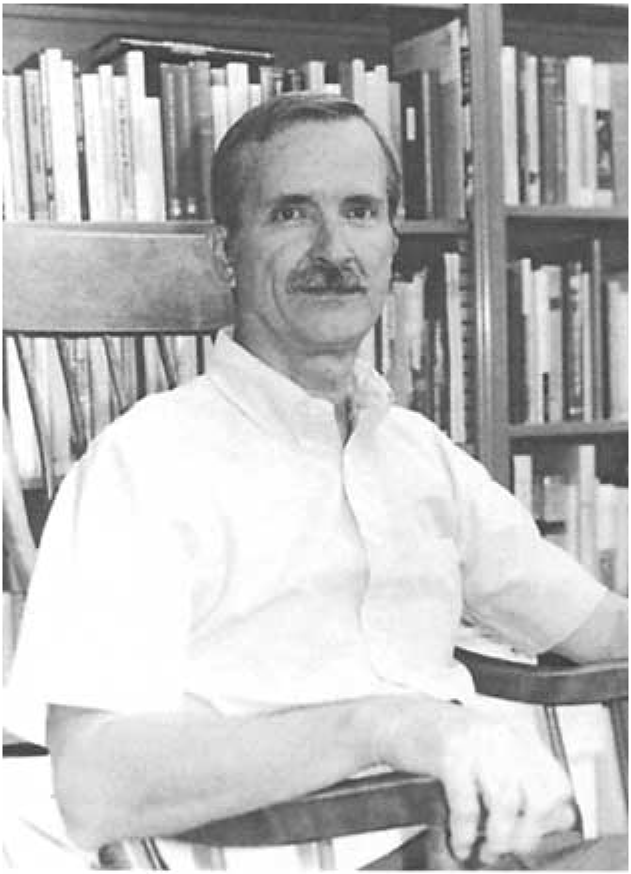

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# ROBERT McC. NETTING

*October 14, 1934-February 4, 1995*

BY OLGA F. LINARES

WITH THE DEATH of Robert McC. Netting on February 4, 1995, at the age of sixty, anthropology lost one of its most respected members. A distinguished cultural ecologist, Netting conducted lifelong studies of the vital relationships linking peoples' social institutions, individual behaviors, and collective beliefs to their production practices. Focusing on the effects of population growth on land tenure and agricultural use, Netting championed the cause of the smallholder—the peasant farmer who intensifies production on a small plot of land by using household labor to achieve an energy-efficient, low-input, successful adaptation. Managing the household patrimony wisely and sustainably, smallholders can achieve yearlong use of their land with minimal ecological damage. They can make a decent and honorable living in farming without experiencing the marked instabilities and inequalities that plague capitalistic (or for that matter also collectivistic) export-oriented farming enterprises elsewhere in the world.

Born in Racine, Wisconsin, on October 14, 1934, Netting received his undergraduate training at Yale University. He graduated in 1957 (summa cum laude and a member of the Phi Beta Kappa Society) with a B.A. in English. His back

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ground in the humanities served him well. Netting was highly literate; a skillful and engaging writer, his lucid prose was devoid of the turgid constructions marring so much of social science writing today.

For graduate studies Netting went to Chicago, where he obtained his M.A. in anthropology in 1959 and his Ph.D. in 1963. In the summer of 1958 he conducted fieldwork in the Ft. Berthold Reservation in North Dakota. There he investigated sources of conflict in Indian voluntary organizations for his M.A. thesis. For his Ph.D. dissertation research Netting spent eighteen months from 1960 to 1962 studying the agrarian practices of the Kofyar, a people living in the hilly escarpments of the Jos Plateau in northern Nigeria. Netting's Kofyar study, published in 1968, was to become a classic monograph on the cultural ecology of intensive cultivators. Time and again, Netting was to return to this part of West Africa: he visited the Kofyar for nine months in 1966-67, for six months in 1984, for one month in 1992, and for three months in 1994. More recently he collaborated with M. P. and G. D. Stone in the publication of much of the quantitative data gathered these past few years on Kofyar demography, expansion, and cash cropping.

In addition to his African research, Netting carried out protracted field studies among Alpine villagers in the Törbel community of Valais, Switzerland. The fourteen months he spent in Törbel in 1970 and 1971, followed by two months in 1974 and 1977, resulted in numerous publications, including his famous book, *Balancing on an Alp* (1981). This is a superb analysis of the historical demography and intensive land use of a European agricultural and herding community.

The consummate teacher-beloved by his students, admired by his colleagues-Netting's first full-time academic appointment was in the anthropology department of the

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University of Pennsylvania. At Penn he served as assistant professor from 1963 to 1968 and as associate professor from 1968 to 1972. He and I first met in Philadelphia in 1966. Fresh from the field, where I had been studying the agrarian practices of the Jola of Casamance, Senegal, Netting received me with characteristic grace: “Finally, a colleague who is also working on intensive farmers." With this remark he promptly placed my research in comparative perspective, making me feel appreciated and welcomed. In the years that followed I learned as much from his lectures on cultural ecology in the introductory course that we taught together at Penn as did his students. It was a great loss to the department when Netting left in 1972 for the University of Arizona.

With the exception of 1994, which he spent as a research scholar at the Institute for Advanced Studies at Indiana University, and extended periods in the field or shorter summer consultant jobs, Netting served continuously as professor of anthropology at the University of Arizona. Since 1991, in fact, he was regent's professor in that distinguished department.

Netting's honors were multiple: Guggenheim fellow (197071), fellow at the Center for Advanced Study in the Behavioral Sciences (1986-87), the Heizer Prize for the best journal article on ethnohistory (1987), and the Wenner Social and Behavioral Sciences Research Institute Best Book Award (1994) for *Smallholders.* He also performed important services for the profession: editor of the University of Arizona Press's Studies in Human Ecology since 1984; member of the AAA Executive Board (1981-84), including chairman of its Committee on Scientific Communication (1983-84); and president of the International Association for the Study of Common Property (1991-92). In April 1993 Netting was elected to the National Academy of Sciences.

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At the November 1994 meetings of the American Anthropological Association, Netting complained to his friends of a persistent backache. Shortly after, in December, his condition was diagnosed as bone marrow cancer. The treatment he underwent lowered his defenses, and a few months later he died of valley fever, a lung infection common in the southwestern United States. A person of great personal integrity, compassion, and commitment, he is survived by his wife, Rhonda Gillett-Netting, an accomplished biological anthropologist in her own right, and five children: Robert F., Jessa F., and Laurel M. from a previous marriage and the twins, Piers and Juliet, born after he died. His mother, Martha M. Netting, and brother, the Rev. William J., also mourn their loss.

Netting will always be remembered for his prodigious scholarship. His field research was informed by past theoretical problems and future orientations. His methodology was sound and appropriate. The seminal articles, meticulous monographs, and important theoretical works he produced shaped the course of ecological anthropology from the 1970s onward. They brought him international fame and a wide readership. His publications and the field studies upon which they are based merit detailed consideration.

## EARLY WORKS

Netting's early work is decidedly empirical in focus, comparative in execution, and functionalist in orientation. In the 1960s he undertook research among the Kofyar of northern Nigeria with a clear problem in mind. Could the ecological approach that Julian Steward pioneered in his 1938 study of mobile hunting societies in the Great Basin of the southwestern United States be profitable when applied to agricultural peoples, he asked? What reformulations and refinements of Steward's theory would be necessary to ac

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commodate the sedentary, intensive cultivators of the hilly Nigerian escarpment? At his return from the field Netting wrote two papers answering these questions (full references are given in the Selected Bibliography). In "Trial Model of Cultural Ecology" (1965,1) Netting argues that social and cultural factors, and not only biological and physical factors (i.e., Steward's "relevant environmental features"), must be included in the definition of effective environment. The innermost core of Netting's functionalist model is taken up by what he calls "social instrumentalities"—namely, demography, productive groups, and rights in resources—precisely those aspects of social organization that have direct adaptive significance. In a second article published the same year (1965,2) Netting illustrates further what he means by "social instrumentality." He uses the example of the small independent family among the Kofyar, an institution admirably adapted to the labor needs of intensive agriculture in small parcels of land. He contrasts it with the extended family of the neighboring Chokfem people, who are better adapted to shifting cultivation on dispersed lands. Here we see Netting's early interest in the functional links that relate household composition and labor requirements to the ways in which land is put to productive use.

Netting's first book, *Hill Farmers of Nigeria* (1968), is a more complete analysis of the intensive ways in which the Kofyar manage their homestead gardens on hilly slopes by terracing and fertilizing. In this highly readable description of Kofyar ecology and agrarian economy Netting demonstrates that population density, division of labor, and rights to land and labor are functionally interrelated with crucial aspects of the ecosystem. In addition to making cross-cultural comparisons and correlations he marshals an impressive amount of quantitative data on farm size, labor composition, and yields to test culture-environment relationships.

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Netting further compares the social institutions of Kofyar hill farmers with those of their lowland bush relatives who migrated to the lowlands in large numbers after the 1950s. Their enlarged extended households are better suited to the production of commercial crops under a system of shifting cultivation. Intricate feedback mechanisms between population density, land availability, and household structures have facilitated adjustment and adaptation among these migrants. According to Netting these are widespread phenomena. In another landmark article (1969,2) he compares Kofyar adaptation to that of the Igbo of eastern Nigeria. He concludes that in both cases the emergence of polygynous extended families and communal tenure is directly related to the easing of population pressure, as more cultivable land became available. Hence, population pressure becomes the critical variable, the mechanism that sets in motion related technological and social variables.

Netting's writings about the Kofyar covered many aspects of their life besides agriculture. He wrote wonderful pieces on the social value of drinking beer, on the politics of gender and domesticity, and on warfare. On the latter theme he emphasized the causal links between war and shifts in settlements, including their abandonment. Tsetse fly infestations increase as bush takes over unused land, and deaths lower population densities.

Already engaged in the study of Törbel (see below), Netting found time to write a useful little book explaining his own anthropological perspective. *Cultural Ecology* (1977) is a clear exposition of the theoretical underpinnings and methodological commitments of what Netting calls an ecological "way of seeing." Hunter-gatherers, northwest coast fishermen, East African cattle-raising peoples, and subsistence farmers are analyzed with an eye to exposing the complex, reciprocal interactions that underlie subsistence

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technologies and local ecosystems. For this task a comparative historical approach and a cross-cultural perspective are essential.

## HISTORICAL ECOLOGY: THE TÖRBEL YEARS

Feeling the need for more complete historical records than were available for northern Nigeria on long-term relationships between demographic fluctuations, land tenure regimes, and ecological adaptations, Netting undertook research during the early 1970s on the German-speaking Alpine community of Törbel in the Vispertal of the Valais canton of southern Switzerland. The Törbel inhabitants practice an intensive, largely self-sufficient, mixed farming and herding economy. Starting in 1972, Netting published a series of important articles covering land-use practices, including the intricate irrigation system, forms of communal tenure, and the marriage system of these remarkable Swiss alpine villagers. His most salient contribution was in the analysis of historical records on household structure and migration in this largely endogamous community. Census data contained in enumerator's books listed village residents by household and covered several periods during the nineteenth century. Netting copied, cross-checked, and subjected the data—with the help of Walter Elias and Larry Manire—to computer analysis using software developed by the Cambridge Center for the Study of Population and Social Structure.

In the fifty years encompassed by the censuses of 1829 and 1880 three-fourths of Törbel's households continued to encompass four to eight members, with the modal number being four or five. There were minor increases in the age of marriage, frequency of celibacy, and average life span of the parents. But the number of households remained fairly stable. The formation of new households was seri

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ously constrained by the limit placed on resources—namely, meadows, gardens, grain fields, and water to irrigate them. Households with extended family units that included maiden aunts, or celibate uncles became more common through time; but when emigration and wage labor opportunities presented themselves, households contracted in size. The ideal continued to be the nuclear household, well adapted to a relatively static agrarian economy. Within relatively narrow boundaries, therefore, the household serves as the main institution through which individuals responded to short-term social and economic changes.

In 1981 Netting's Alpine research was published by Cambridge University under the clever title, *Balancing on an Alp,* an allusion to the closed corporate nature of Törbel, a community in demographic and ecological equilibrium. The book assembled eight of his most important articles, appropriately revised, plus three new chapters that structure and relate the ecology and economy to the social organization. Rather than replacing each other, as scholars have assumed, in Törbel communal land tenure practices—held in the alpine grazing lands and in the irrigation system—coexisted for 300 years with private tenure practices exercised in the intensively cultivated agricultural plots. Discussion of population dynamics covers four chapters and is based on a staggering array of quantitative data forming the core of the book. The concluding chapters present a skillful analysis of the relationship between demographic trends and ecological processes. An outstanding work of "ecological anthropology," *Balancing on an Alp* is also a charming study of how Swiss peasants endured and even thrived in their special Alpine environment. The book has become a classic, referred to by economic historians and students of rural European life as often as by anthropologists and cultural ecologists.

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## A SYNTHESIS: THE LAST FEW YEARS

In 1993 Netting published Smallholders, Householders, an impressive scholarly work analyzing the role that farm families play in the ecology of intensive, permanent, and productive agriculture. The book documents the food-producing practices of small rural cultivators from the Far East, Southeast Asia, Africa, and Latin America. Their environmentally appropriate and efficient ways of mobilizing labor, reducing external inputs, and diminishing risk contrast with the wasteful procedures inherent in agribusiness and large-scale industrial farms. Under conditions of expanding population, changing agricultural technologies, and the penetration of a market economy the smallholder alternative has proven to be "economically efficient, environmentally sustainable, and socially integrative" (p. 27). Using the example of two systems he knew well—the Kofyar of northern Nigeria and the Swiss Alpine villagers of the Valais Canton—Netting outlines the common features of technology and knowledge they share that are essential characteristics of intensive farming practices everywhere. He then extends his sample to include Asian irrigated rice economies and intensively managed dooryard gardens to demonstrate that an intimate knowledge of the environment combined with a wide array of soil restoration, water control, and plant-management practices are employed to solve ecological and economic problems. The social unit that most effectively carries out these intensive tasks is the coresident household, with its bounded resources and acknowledged property rights, its committed labor force, and its clear production goals. Despite their demonstrated efficiency, households are not uniformly endowed. There are significant disparities between them in productive property and accumulated wealth. But these differences are not predetermined or fixed:

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they do not prevent deliberate mobility or the unintended consequences of reduction in size. Actually, economic disparity is lower in areas where population density is higher and land use most intensive. It is in sparsely populated hinterlands and agricultural frontiers where wealth is more polarized and poverty most paralyzing.

Consistent with previous writings, Netting finds in the theories of Ester Boserup, the Danish economist, the most intellectually satisfying reason for the smallholder way of life. Boserup was the first scholar to argue that a positive relationship exists between population density (and land scarcity) and intensive forms of agriculture. This is exactly the opposite of Malthus's argument that environmental potential and carrying capacity determine population densities. It is more compatible, however, with the microlevel analysis of the composition of peasant households by the Russian economist Chayanov. One of the great strengths of Netting's last book is that he shows where the theories of great economists such as Boserup, Malthus, Marx, and Chayanov are relevant, or where they go astray, in an effort to explain the logic and persistence of smallholder adaptations. In the complex world economy in which all farmers participate nowadays there is no imminent danger that the smallholders' way of life will disappear so long as their activities continue to make good economic sense.

## CONCLUSION

Netting's life and work were characterized by integrity, intellect, and involvement. Ever so much the social scientist, he painstakingly gathered quantitative ethnographic data with which to test middle-range theories about the processes that relate social institutions to underlying forces in the environment. His focus on the role of households in the economy of intensive agriculture led him to discover sys

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tematic relationships between population dynamics, ecological change, and the form and function of rural family structures. His enduring contribution was to explore—systematically and quantitatively—the social and ecological consequences of long-term resource use.

Netting's respect for the inventiveness and tenacity of rural producers won him affection and respect. "The wonderful work he did for our community will forever remain a legacy that will not be forgotten" (letter of September 5, 1995, from J. Daduut of the Kofyar Federation to Rhonda Gillett-Netting). "Professor Netting was loved by the people of Törbel—there remains nothing but to think of this man, who was such a devoted friend to our simple mountain folk, with thankful high esteem . . ." (letter by R. Wyss of Törbel to Rhonda Gillett-Netting).

I SHOULD LIKE TO thank Rhonda Gillett-Netting for her kindness in sharing with me essential documents on her husband. These included an up-to-date CV, published newspaper articles on Bob, letters from friends in the field, and a memorial article (B. J. McCay, Robert McC. Netting and Human Ecology: An Appreciation. Hum. Ecol. 24(1996):125-35). Two additional forthcoming pieces by Richard Wilk and Priscilla Stone, both devoted students of Netting, also were helpful. The review essay by J. Martinez-Alier, "In Praise of Smallholders" (prepared for the Conference on Agrarian Questions, Wageningen, 1995) is critical of Netting's work but not unfairly so. Although I consulted several reviews of Netting's publications, the assessment here of his work is solely my own.

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# ALLEN NEWELL

*March 19, 1927-July 19, 1992*

BY HERBERT A. SIMON

WITH THE DEATH from cancer on July 19, 1992, of Allen Newell the field of artificial intelligence lost one of its premier scientists, who was at the forefront of the field from its first stirrings to the time of his death and whose research momentum had not shown the slightest diminution up to the premature end of his career. The history of his scientific work is partly my history also, during forty years of friendship and nearly twenty of collaboration, as well as the history of the late J. C. (Cliff) Shaw, a longtime colleague; but I will strive to make this account Allen-centric and not intrude myself too far into it. I hope I will be pardoned if I occasionally fail.[1]

If you asked Allen Newell what he was, he would say, “I am a scientist." He played that role almost every waking hour of every day of his adult life. How would he have answered the question, "What kind of scientist?" We humans have long been obsessed with four great questions: the nature of matter, the origins of the universe, the nature of life, the workings of mind. Allen Newell chose for his life's work answering the fourth of these questions. He was a person who not only dreamt but gave body to his dream, brought it to life. He had a vision of what human thinking

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is. He spent his life enlarging that vision, shaping it, materializing it in a sequence of computer programs that exhibited the very intelligence they explained.

## THE CAREER

In a remarkable talk about his research strategies and history given at Carnegie Mellon University in December 1991, seven months before his death,[2] Allen described his career as aimed single-mindedly at understanding the human mind, but he also confessed to four or five substantial diversions from that goal —almost all of which produced major scientific products of their own. These "diversions" included his work with Gordon Bell on computer hardware architectures, the work with Stu Card and Tom Moran on the psychology of human-computer interaction, a major advisory role in the ARPA program of research on speech recognition, and his leadership in establishing computer science at Carnegie Mellon University and in creating the pioneering computer networking of that university's campus.

For the rest, Allen's work aimed steadily, from the autumn of 1955 onward, at using computer simulation as the key research tool for understanding and modeling the human mind. After the first burst of activity, which produced the Logic Theorist, the General Problem Solver, and the NSS chess program, he focused increasingly on identifying and overcoming the limitations and inflexibilities of these models that impeded their extension into a wholly general theory of the mind. His final book, *Unified Theories of Cognition* (1990), records the vast progress that he and others made over thirty years toward such generality, progress that in the final decade of his life focused on the emerging Soar system that he and his colleagues built.

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## HOW IT BEGAN

Allen Newell was born in San Francisco on March 19, 1927, the son of Dr. Robert R. Newell, a distinguished professor of radiology at Stanford Medical School, and Jeanette Le Valley Newell. An older sister was his only sibling. His father provided an important model for his son. In an interview (McCorduck, 1979, p. 122), Allen once said of him: "He was in many respects a complete man.... He'd built a log cabin up in the mountains.... He could fish, pan for gold, the whole bit. At the same time, he was the complete intellectual.... Within the environment where I was raised, he was a great man. He was extremely idealistic. He used to write poetry."

Allen's childhood was uneventful enough, many of the summers being spent in the Sierra Nevada at the log cabin his father built. Allen acquired a love of the mountains that never left him (an early ambition was to become a forest ranger) and a love of sports that, combined with his 6'1" height and sturdy build, led to the high school football team. He said of his own high school career (Newell, 1986, p. 347): "Allen was an indifferent pupil, though some people seemed to think he was bright. He went to Lowell High School-the intellectual high school of San Francisco-where he turned on academically. He also fell in love at age 16 with a fellow student, Noel McKenna, and married her as soon as tactically possible (age 20)." The marriage demonstrated that Allen and Noel were excellent decision makers even at that early age, for they formed a close and mutually supporting pair throughout the forty-five years of their marriage.

Allen graduated from high school just as World War II was ending, worked for the summer in a shipyard, and then enlisted in the U.S. Navy. Although close to his father and

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acquainted with many other scientists who were family friends, he had no intention, up to that time, of following a scientific career. Adoption of science as his vocation came, he said, rather suddenly, when, serving on a U.S. Navy ship that carried scientific observers to the Bikini nuclear tests and assigned the task of making maps of the radiation distribution over the atolls, he was infected with the excitement of the scientific enterprise.

On completing his service in the Navy, Allen enrolled in Stanford University, where he majored in physics. Undergraduate research led to his first paper, on X-ray optics (Newell and Baez, 1949). Stanford also exposed him in the classroom to George Polya, who was not only a distinguished mathematician but also a thoughtful student of mathematical discovery. Polya's widely read book, *How to Solve It,* published in 1945, had introduced many people (including me) to heuristic, the art of discovery. Allen came away from that experience aware that the processes of discovery could be investigated and analyzed and that heuristic-the art of guided search-played a key role in creative thinking. (Our common fascination with heuristic helps account for the rapidity with which Allen and I established common ground on first meeting early in 1952.)

## RAND

A year in mathematics (1949-50) as a graduate student at Princeton and exposure to game theory, invented shortly before by von Neumann and Morgenstern, convinced Allen that he preferred a combination of experimental and theoretical research to pure mathematics. Taking a leave from Princeton, he found a position at the RAND Corporation, the then-new think tank in Santa Monica, in a group that was studying logistics problems of the Air Force. Two technical reports he coauthored with Joseph B. Kruskal *(A Model*

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*for Organization Theory* [1950] and *Formulating Precise Concepts in Organization Theory* [1951]) demonstrate his interest at that time in applying formal methods to complex empirical phenomena. Both papers adopt a style of axiomatization that was fashionable then in game theory and economics.

A six-week field visit to the Munitions Board in Washington impressed Allen with the distance that separated the formal models from reality, and his trip report, *Observations on the Science of Supply* (1951), exhibits his sensitivity to and sophistication about the organizational realities that he observed (probably reinforcing his brief naval experience and summer's work in the wartime shipyard). Somewhat disillusioned with axiomatization as the route to reality, Allen then turned to the design and conduct of laboratory experiments on decision making in small groups, a topic of considerable active interest in RAND at that time.

Dissatisfied also with small-group experiments as a way of studying organizations, the RAND team of John Kennedy, Bob Chapman, Bill Biel, and Allen conceived of constructing and operating a full-scale simulation of an Air Force Early Warning Station in order to study the organizational processes of the station crews. This effort, funded by the Air Force in 1952, led to the creation of the Systems Research Laboratory at RAND (eventually spun off as the Systems Development Corporation) (Chapman et al., 1959). Central to the research was recording and analyzing the crew's interactions with their radar screens, with interception aircraft, and with each other. These data focused Allen's attention on the information-handling and decision-making processes of the crew members and led to a search for an appropriate technique for analyzing and modeling the process. I met Allen when I became a consultant to the laboratory, and in the first minutes of our initial meeting

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he and I found common ground in the study of information processes as a route to understanding human decision making in organizations.

One of Allen's special responsibilities in the project was to find a way to simulate a radar display of air traffic, for no technology was available to the lab for making appropriate simulated patterns of blips as they move over radar screens. While searching for computational alternatives, Allen met Cliff (. C.) Shaw, a RAND systems programmer, then working with the Card-Programmed Calculator, a prehistoric device that just preceded the first stored-program computers. Allen and Cliff conceived the idea of having the CPC calculate the successive air pictures and print out simulated radar maps. This not only provided the required laboratory simulation but also demonstrated to Al and Cliff (and to me when I learned of it) that computers, even prehistoric computers, could do more than arithmetic: they could produce spatial arrangements of nonnumerical symbols representing airplanes.

Now two of the preconditions were in place for Allen's move to the goal of understanding human thinking. He clearly saw information processing as a central activity in organizations, and he had had a first experience in symbolic computing. A third precondition derived from contact with the stored-program computer Johnniac that John von Neumann was building at RAND in about 1954.

At this time the ideas of cybernetics and artificial life were abroad. W. Ross Ashby had published in 1952 his *Design for a Brain.* W. Grey Walter (1953) in England had constructed some mechanical "turtles" that wandered about the room searching for a wall outlet when their batteries ran low, and similar creatures were built by Merrill Flood's group at RAND. By 1950 both Turing and Shannon had described (but not actually programmed) strategies for com

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puter chess, and in 1952 I described (but did not implement) a program extending Shannon's ideas. On an auto trip en route to observing some Air Force exercises in the summer of 1954, Allen and I discussed at length the possibilities of using a computer to simulate human problem solving, but we were not then diverted from our current research on organizations.

## THE COMMITMENT

In September 1954 Allen attended a seminar at RAND in which Oliver Selfridge of Lincoln Laboratories described a running computer program that learned to recognize letters and other patterns. While listening to Selfridge characterizing his rather primitive but operative system, Allen experienced what he always referred to as his "conversion experience." It became instantly clear to him "that intelligent adaptive systems could be built that were far more complex than anything yet done." To the knowledge Allen already had about computers (including their symbolic capabilities), about heuristic, about information processing in organizations, about cybernetics, and proposals for chess programs was now added a concrete demonstration of the feasibility of computer simulation of complex processes. Right then he committed himself to understanding human learning and thinking by simulating it. The student of organizations became a student of the mind.

In the months immediately following Selfridge's visit Allen wrote (1955) "The Chess Machine: An Example of Dealing with a Complex Task by Adaptation," which outlined an imaginative design for a computer program to play chess in humanoid fashion, incorporating notions of goals, aspiration levels for terminating search, satisfying with "good enough" moves, multidimensional evaluation functions, the generation of subgoals to implement goals, and something

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like best first search. Information about the board was to be expressed symbolically in a language resembling the predicate calculus. The design was never implemented, but ideas were later borrowed from it for use in the NSS chess program in 1958.

Newell's goals already extended far beyond chess: "The aim of this effort, then, is to program a current computer to learn to play good chess. This is the means to understanding more about the kinds of computers, mechanisms, and programs that are necessary to handle ultracomplicated problems (Newell, 1955). When the paper was presented in March 1955 at the Western Joint Computer Conference, Walter Pitts, the commentator for the session, said, "But, whereas [the authors of the other papers] are imitating the nervous system, Mr. Newell prefers to imitate the hierarchy of final causes traditionally called the mind. It will come to the same thing in the end, no doubt. .. ." From the very beginning something like the physical symbol system hypothesis was embedded in the research.

## THE LOGIC THEORIST AND LIST PROCESSING

Even before his "conversion" Allen had been making plans to move to Pittsburgh early in 1955, with Noel and their new son Paul, to work with me in organizational research and earn a doctoral degree (in industrial management!). RAND agreed to continue to employ Allen as its (one-man) Pittsburgh outpost. This plan was duly executed but with the crucial alteration that the research was to be on programming a chess machine. It was arranged that Cliff Shaw at RAND would collaborate with us, and the program would run on RAND's Johnniac. For various technical and accidental reasons chess soon changed to geometry and geometry to logic, and the Logic Theory Machine (LTM), which discovered proofs for theorems in the propositional calcu

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lus, emerged as a hand simulation by December 15, 1955, and a running program in the summer of 1956.

Work was pursued simultaneously on a programming language that would be adequate for implementing the design, leading to the invention of the Information Processing Languages (IPLs), the first list-processing languages for computers. It is fair to say that the LTM and its successor, the General Problem Solver, laid the foundation for most of the artificial intelligence programs of the following decade. A genuine computer program performing a task of some sophistication has much more persuasive and educational powers than do verbal discussions of ideas. A running program is the moment of truth.

LTM was not a "deduction machine"—in fact, it worked backwards, inductively, from hypothesized theorem to the axioms. Discovering proofs is much like discovering anything else, a process of selective search. The fact that the task involves symbolic logic does not make the problem solving process any more "logical" or less "intuitive" than if some other task (e.g., looking for a law that would connect the distances of planets from the sun with their periods of revolution) were in question.

Although this work was incorporated in Allen's doctoral dissertation, I never regarded him as my "student." Allen, Cliff, and I were research partners, each contributing his knowledge to a wholly joint product. Allen, when he arrived in Pittsburgh, already had five years of scientific work under his belt and needed colleagues more than teachers. I do not suggest that he did not learn-he never stopped growing and learning throughout his life-but he learned as scientists learn, from everyone and everything around them, especially from observation of nature itself.

Why did this particular work, which was part of an already existing Zeitgeist that had engaged the efforts of many

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able scientists, become highly visible and influential? An essential element in its impact was the actual running program. In addition, LTM and its successors were not directed at a single task. The specific programs were steps toward the solution of the general problem: understanding the human mind. The strategy is stated clearly in the first publication on LTM: "In this paper we describe a complex information processing system ... capable of discovering proofs for theorems in symbolic logic. This system, in contrast to the systematic algorithms . . . ordinarily employed in computation, relies heavily on heuristic methods similar to those that have been observed in human problem solving activity. The specification is written in a formal language, of the nature of a pseudo-code . . . for digital computers .... The logic theory machine is part of a program of research to understand complex information processing systems by specifying and synthesizing a substantial variety of such systems for empirical study" (Newell and Simon, 1956).[3]

It is all there: complex information processing, symbolic computation, heuristic methods, human problem solving, a programming language, empirical exploration. These are the components of the fundamental research strategy of the Carnegie-RAND group in 1955 and 1956 that continued to guide Allen Newell's scientific work throughout his career. It led him continually to identify and diagnose the limitations of the programs he built and to ponder about architectures that would remove those limitations, and it led him in the last decade to Soar-not as the final answer, for he knew that there are no final answers in science, but as the next step of progress along a path that he followed as long as he was able to work.

## EXPLOITING THE FIRST SUCCESS

For about five years after 1955 the Newell-Shaw-Simon

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team, aided by a growing circle of graduate students, pushed forward the research ideas opened up by LTM and the IPL programming languages. Among the main thrusts in which Allen was involved were thinking-aloud protocols, the General Problem Solver, information-processing languages and production systems, the NSS chess-playing program, and human problem solving. Until 1961 he remained on the staff of RAND (in Pittsburgh); in that year he accepted appointment as an institute professor at Carnegie Institute of Technology.

## THINKING-ALOUD PROTOCOLS

There are severe difficulties in testing a theory of human thinking that predicts the sequence of thought processes each of only a few hundred milliseconds duration. Apart from neurological evidence, which is only now beginning to become available for tracing some processes, there were few obvious ways of obtaining data while a task was being performed, even at a density of one data point per second. It occurred to the team to instruct subjects to think aloud while performing problem-solving tasks. However, fifty years earlier the method called "introspection" had been thoroughly discredited as a means of obtaining reliable data in psychology. Hence, it was necessary to show that the thinking-aloud method was quite different from classical introspection and to determine the circumstances under which it could provide objective evidence about thought processes. A program of laboratory experimentation using thinking-aloud methods was launched by the beginning of 1957; formal methods were developed for encoding protocol data (problem behavior graphs); and a decade later Allen and Don Waterman made the first, only partially successful, attempt at automating protocol analysis (Waterman and Newell, 1971).

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## THE GENERAL PROBLEM SOLVER (GPS)

In the summer of 1957, during a workshop at Carnegie Tech on organizational behavior, Al and I extracted from the protocol of a single subject solving logic problems what proved to be a key mechanism in human problem solving: means-ends analysis. In M-E analysis the problem solver compares the current situation with the goal situation; finds a difference between them; finds in memory an operator that experience has taught reduces differences of this kind; and applies the operator to change the situation. Repeating this process the goal may gradually be attained, although there are generally no guarantees that the process will succeed. The idea of M-E analysis led to the General Problem Solver (Newell, Shaw, and Simon, 1960), a program that could solve problems in a number of domains after being provided with a problem space (domain representation), operators to move through the space, and information about which operators were relevant for reducing which differences. The research also discovered schemes that permitted GPS to produce its own operators from a small set of primitives and to learn which operators were relevant for reducing which differences.

## THE INFORMATION PROCESSING LANGUAGES (IPLS)

The IPL languages in artificial intelligence and their contemporary FORTRAN in numerical computing settled once and for all the essentiality of higher-level languages for sophisticated programming. The IPLs were designed to meet the needs for flexibility and generality: flexibility, because it is impossible in these kinds of computations to anticipate before run time what sorts of data structures will be needed and what memory allocations will be required for them; generality, because the goal is not to construct programs

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that can solve problems in particular domains, but to discover and extract general problem-solving mechanisms that can operate over a range of domains whenever they are provided with an appropriate definition for each domain.

To achieve this flexibility and generality the IPLs introduced many ideas that have become fundamental for computer science in general, including lists, associations, schemas (frames), dynamic memory allocation, data types, recursion, associative retrieval, functions as arguments, and generators (streams). The IPL-V Manual (Newell, 1961), exploiting the closed subroutine structure of the language, advocated a programming strategy that years later would be reinvented independently as structured programming—mainly top-down programming that avoided go-to's. LISP, developed by John McCarthy in 1958, which embedded these list-processing ideas in the lambda calculus, improved their syntax and incorporated a "garbage collector" to recover unused memory, soon became the standard programming language for artificial intelligence (AI).

## PRODUCTION SYSTEM LANGUAGES (OPS5)

Allen did not regard the IPLs or their successors as final solutions to the problems of organizing Al programs. Experience with the General Problem Solver revealed a tendency for the program to burrow into a deep pit of successive subgoals, with no way for the top program levels to regain control. A way out of the dilemma began to appear in the middle 1960s in the form of production system languages, introduced into computing by Bob Floyd and others to aid in compiling compilers. In a production system each instruction in the language takes the form of a condition followed by an action: "IF [such and such is the case] THEN [do so and so]." Completely general programming languages can be constructed on this plan.

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The Carnegie-RAND group saw in production system languages a solution to the control problem, and Allen took leadership in the development of a succession of such languages, the best known and most widely used of which is OPS5. OPS5 in turn provided the central ideas for the language employed to program the Soar system. A closely related set of ideas that we developed at about the same time, out of concern with the program control problem, led to a decentralized system in which independent processes add information to a common memory ("blackboard") and obtain information they need from that memory. The blackboard idea has achieved wide use in speech recognition, vision programs, and elsewhere.

## CHESS: THE NSS PROGRAM

The third main substantive product of the Carnegie-RAND group was a chess program named NSS, the initials of its authors (Newell, Shaw, and Simon, 1958). It was not the first chess program to be implemented and run (Alex Bernstein, among others, completed programs somewhat earlier), nor was it a very strong player: as critics of artificial intelligence were fond of pointing out, it was once beaten by a ten-year-old child. What the critics failed to understand was its purpose: to demonstrate how highly selective search guided by heuristics and by goals evoked by cues in the problem situation could achieve intelligent behavior in a complex task.

## HUMAN PROBLEM SOLVING

With the completion of LTM, GPS, the list-processing languages and production systems, and NSS, Al, Cliff, and I began more and more to pursue separate projects in collaboration with other colleagues and graduate students. The last major project Allen and I undertook together was to

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summarize our research on problem solving—experiments, simulation, and theory—in *Human Problem Solving,* which was published in 1972. The gradual cessation of close collaboration reflected no rift, as is evident from our joint 1975 Turing Lecture (Newell and Simon, 1976) and the weekly or more frequent conversations that continued until a few days before Allen's death, but a natural drift as each of us interacted with different graduate students and faculty colleagues and built our research strategies to reflect different bets about the locus of the biggest payoffs from studying intelligence.

## COGNITIVE ARCHITECTURE

Allen, from an early stage of his research and increasingly as the years passed, was especially concerned with computational architecture and modeling the control structures underlying intelligence.

> An architecture is a fixed set of mechanisms that enable the acquisition and use of content in a memory to guide behavior in pursuit of goals. In effect, this is the hardware-software distinction.... This is the essence of the computational theory of mind. (Newell, 1992, p. 27)

The early attention of the RAND-Carnegie group to flexibility and generality and the realization of these properties in the programming languages the group invented have already been noticed. The languages became part of the "hardware" that supported the underlying structure for the Al programs, anticipating the much later efforts of others to embed list processing in actual physical hardware. The languages also built into the AI systems some of the salient characteristics of human memory as revealed by psychological research, for example, its associative structure embodied in lists and schemas and the production-like character of stimulus-response connections.

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## UNSOLVED ARCHITECTURAL PROBLEMS

But important architectural problems remained unsolved. The experience with GPS underlined the importance of control structures for keeping a problem-solving system on course, neither dissipating its efforts in scattered random search nor following long narrow paths that often led, after much wasted effort, to dead ends. The concern for these problems can be traced through a series of Allen's publications beginning in the early 1960s and continuing through most of his career: "Some Problems of Basic Organization in Problem-Solving Programs" (1962), "Learning, Generality and Problem Solving" (1963), "The Search for Generality" (with G. Ernst, 1965), "Limitations of the Current Stock of Ideas for Problem Solving" (1965), "On the Representation of Problems" (1966), "The Trip Towards Flexibility" (1968), "A Model for Functional Reasoning in Design" (with P. Freeman, 1971), "A Theoretical Exploration of Mechanisms for Coding the Stimulus" (1972), "Production Systems: Model of Control Structures" (1973), and "How Can Merlin Understand?" (description of a "unified" architecture based on matching) (with J. Moore, 1974); then, after about an eight-year interval, "Learning by Chunking: Summary of a Task and a Model" (with P. S. Rosenbloom, 1982) and "A Universal Weak Method" (with J. Laird, 1983)these last two papers being early descriptions of crucial components of what became the Soar system, which occupied the last decade of Allen's life.

## THE MERLIN PROGRAM

MERLIN, an architectural enterprise undertaken about 1967, began as an attempt to build a pedagogical tool but became a serious effort to construct a system that had understanding. "MERLIN," Newell wrote, "was originally con

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ceived . . . out of an interest in building an assistance-program for a course in AI. The task was to make it easy to construct and play with simple . . . instances of AI programs. . . . [T]he effort transmuted into . . .building a program that would understand AI—that would be able to explain and run programs, ask and answer questions about them.... The intent was to tackle a real domain of knowledge as the area of constructing a system that understood."

The basic ideas around which MERLIN was built were analogy and matching: "the construction of maps from the structure that represents what MERLIN knows to be the structure that MERLIN seeks to understand." The difficulties that were encountered en route to this goal were so severe that Newell regarded MERLIN as a failure, not reaching its practical goals and not producing results that had an impact on the rest of the field. It is described in a single published article (Moore and Newell, 1974). Many innovative AI ideas were embedded in MERLIN, but Allen was reluctant to publish them prior to building a complete running system that incorporated them all.

## DIVERSIONS

The important work that Allen described as his "diversions" included research on computer hardware structures, the fostering of research on speech understanding, and research on human-computer interaction. Later I will mention other diversions in the form of institution-building activities.

## COMPUTER STRUCTURES

It is perhaps not surprising that someone deeply concerned with program organization would become interested in computer hardware architectures, and Allen did. Nevertheless he regarded his work on this topic, which began

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with Gordon Bell's invitation in about 1968 to collaborate on a book on computer systems, as a diversion from his main objective. The strategy of simulating human thinking did not rest on any assumption of similarity between computer architectures and the architecture of the brain beyond the very general assumptions that both were physical symbol systems and that therefore the computer could be programmed to behave like the mind. Nevertheless, there are fundamental architectural problems common to all computing that reveal themselves in hardware and software at every level, for example, how to organize systems so that they can operate in parallel on multiple tasks with due respect for priorities and precedence constraints between processes.

Newell and Bell undertook to describe architectures at two levels: (1) the system level in terms of memories, processors, switches, controls, transducers, data operators, and links (the PMS language) and (2) the instruction level in terms of the detailed operations of the instruction set (the ISP language). Their book, *Computer Structures: Readings and Examples,* using the PMS and ISP languages to characterize a large number of computers, appeared in 1971. A revised edition coauthored with Bell and Siewiorek was published in 1981.

The work with Bell led Allen to other projects on computer and software systems design, and a number of his publications up to about 1982 were devoted to these projects. In 1970-71 Newell, McCracken, Robertson, and others built a language, L*, that aimed at providing systems programmers with a kernel that would facilitate building operating systems and user interface.

In 1972, in connection with an AI workshop that we organized, Newell, Robertson, and McCracken built a pioneering hierarchical menu system that gave the workshop par

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ticipants access to demonstrations of an assortment of AI programs.

Some years later in 1978-82, using the new touch-screen technology, this idea developed into the hypermedia ZOG system, which became a tool for accessing the administrative data base on the newly launched aircraft carrier *USS Carl Vinson* (Newell et al., 1982). Other computer and systems design diversions for Allen included work with colleagues in the computer science department around 1971 on C.mmp and other parallel hardware cum software systems that were being designed there (Bell et al., 1971).

## SPEECH UNDERSTANDING

A further major diversion for Allen in the 1970s resulted from ARPA's interest in the possibility of launching a program in automatic speech recognition. Specifically because he was not an active speech researcher and hence stood in a neutral corner, Allen was asked to chair a study group whose 1971 report formed the basis for a major ARPA research effort (Newell et al., 1971). Allen then became chair of the steering committee for the project and produced a progress report in 1975 (Newell et al.) and a final evaluation in 1977 (Medress et al.). His role in the speech effort illustrates both his stature in the profession and his willingness to accept "citizenship" responsibilities for the growth of artificial intelligence.

## HUMAN-COMPUTER INTERACTION

When the Xerox PARC laboratory was formed in 1970, Allen, consulted about its research program, proposed a project that would apply psychological theory to human-computer interaction and, in particular, to the design of computer interfaces. Beginning in 1974, Allen with two of his former students, Stu Card and Tom Moran, began to

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bring together existing psychological data, examine them for regularities (such as Fitts's Law and the power law for learning), construct an engineering-level model of routine cognitive skills (MODEL HUMAN PROCESSOR) and a methodology, "GOMS," standing for goals, operators, methods, and selection, for analyzing new tasks in terms of the basic processes required to perform them. This work was brought together in *The Psychology of Human-Computer Interaction* (Card et al., 1983).

Though in one sense a diversion from his main concerns, the Xerox PARC activity brought Allen back from a preoccupation with computers to concern for the human mental architecture. Moreover, the requirement of modeling entire human tasks required the group to think in terms of a broad-gauged, unified theory. In this sense the project was a step toward the Soar system, planning for which began in the later 1970s before publication of the human-computer interaction book.

## SOAR

Allen came to doubt that lack of experimental evidence was the limiting factor in the progress of cognitive psychology. Sufficient data, he thought, already existed to pin down much of the structure of the mind at the architectural level. Moreover, further experimental work would be well aimed and useful only if guided, not by particularistic microtheories, but by a broad theoretical framework. In his final book, *Unified Theories of Cognition* (1990), based on his William James Lectures at Harvard in 1987, he called for such theories and drew the bold outlines of what such a theory might look like, taking Soar as his model. He was careful not to refer to Soar as "*the* unified theory of cognition," but introduced it as "a candidate unified theory." Indeed, in his final chapter he gives a reasoned argument as to why "there

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must be many unified theories" on the road to developing a veridical one.

When existing unified theories are viewed closely, each can be seen to be built around a core cognitive activity, which is then extended to handle other cognitive tasks. In Anderson's Act* the core is semantic memory; in EPAM, perception and memory; in connectionist models, concept learning. In Soar as in GPS the core is problem solving, and the central GPS concept of problem space is taken over and expanded to allow the system to use multiple problem spaces in solving a single problem. The Soar program is a production system. To this were added two key components developed in collaboration with graduate students: learning by chunking (Rosenbloom and Newell, 1982), which produced a wide variety of kinds of learning obeying the empirically observed power law, and a universal weak method (Laird and Newell, 1983), which incorporated a method for universal subgoaling.

Learning by chunking derived from previous AI work on memory organization in terms of chunks and on learning by adaptive production systems (systems that created and assimilated new productions). What was new in Soar was the use of this mechanism as the sole learning mechanism and the demonstration that it was both powerful and consistent with the power law of learning.

The universal weak method of problem solving consisted at each step of finding which operators were then executable; if there were none or if there were more than one, declaring an impasse, and moving to a new problem space with a new subgoal to resolve the impasse. This procedure generalized the idea of problem spaces and established a consistent semantics for the possible relations among them.

The Soar project continued to grow through the 1980s and 1990s with steadily increasing numbers of active par

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ticipants at Carnegie Mellon and elsewhere (including the University of Michigan, the Information Systems Institute at the University of Southern California, and several European sites). The effort was directed at extending and strengthening the basic Soar architecture and simultaneously demonstrating its capacity for handling a widening range of tasks, including language comprehension, complex problem solving, and even cryptarithmetic—one of GPS's initial tasks. The scope of the system at the time of Allen's death can be seen from his *Unified Theories* book, and work on it continues actively today on numerous fronts.

While it would be hazardous to predict what resemblance there will be between Soar and the "ultimate" unified theory of cognition, it is already evident that Allen's strategy of putting all of his (and many other people's) energies into Soar has intensified interest in building broad-gauged theories that cover a wide range of cognitive processes and has left an important permanent mark on cognitive science.

## SCIENCE STATESMANSHIP

It is hard to know whether to classify the time Allen spent as a citizen of the university and of the wider science community as one of his diversions or as part of the mainstream of his scientific work. From the time of his employment at RAND he was keenly aware of the dependence of progress in science upon the institutions that housed and nourished it and he identified closely with the institutions in which he worked. During the early years of his stay at RAND he was persuaded that the think tank was the preferred research organization of the future, but he gradually came to believe that universities had capacities for self-renewal that were hard to maintain in independent laboratories. This change in belief played an important part in his decision to move

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in 1961 from RAND to the faculty of the Carnegie Institute of Technology.

Allen played an important leadership role in every organizational setting in which he found himself: RAND, the computer science department (later a school) at Carnegie Mellon, the whole university, the national and international computer science research community, and ARPA as a part of it. In general he did not do this by occupying formal administrative positions but by taking on specific assignments and by serving as a very active and highly valued elder statesman. For these purposes he was, as I have remarked, "elder" all his life.

## THE COMPUTER SCIENCE DEPARTMENT

While still a doctoral student Allen was already called on for advice as we first brought computers to Carnegie Mellon University. (The first one arrived with Alan Perlis in about 1956.) By 1961, when an informal graduate program in computer science was set up by mutual agreement among four departments, Allen was a major figure along with Perlis and myself in pushing its development and then creating a computer science department, involved deeply in decisions about curriculum and the acquisition of equipment.

With Bert Green, then chairman of the psychology department, Allen was instrumental in obtaining the first large, continuing NIMH research grant for cognitive science research in that department. He was a principal figure, initially along with Alan Perlis, in obtaining and renewing the large ARPA grants that provided the core funding for what quickly became one of the nation's leading computer science departments. For the ensuing quarter century or more Allen played a major role in both departments through his research, his teaching, his guidance of graduate students, and his participation in policy.

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## THE CAMPUS NETWORK

From about 1972 the experience of members of the computer science department with the ARPA network convinced the community that a network of electronic communications was essential not only for the department but for the university. With the department having persuaded the university administration that Carnegie Mellon was in a unique position to offer national leadership in this direction, Allen agreed to serve as chairman of a task force that was appointed to prepare a plan and to educate the campus community about its potential. In February 1982 the task force issued its report, *The Future of Computing at Carnegie Mellon University*. An agreement was reached with IBM for collaboration in designing and installing the system, and the Andrew system, CMU's campus-wide network—one of the first in the nation—came into being. (The Andrews were Andrew Carnegie and Andrew Mellon.)

## ARPA

From its beginnings artificial intelligence and simulation of human thinking have been foci of controversy, eliciting disbelief and anger from those who find the idea of a machine thinking either incredible or threatening. Decisions about funding AI research inevitably became enmeshed in this controversy about its worth, and the support by ARPA of computer science in general and AI in particular was periodically under attack throughout a long and stormy history.

A very large slice of Allen's life was spent preparing research proposals and budget defenses for computer science at Carnegie Mellon, as well as participating in ARPA planning exercises and interpreting AI and cognitive science research to the broader scientific community. This, too, is a

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normal part of institution building in science, but not its pleasantest part. Allen, while resenting the time lost in these duties, never shirked them. However, his belief (and mine) was that propaganda of the deed was more important than propaganda of the word: that in the longer run the fate of AI and cognitive simulation would be determined not by debates with philosophers about what was possible, a priori, but by our success or failure in building programs that demonstrably simulate and thereby provide theoretical explanations for human thought processes. Every possible waking moment was to be reserved for that task.

## COGNITIVE SCIENCE AND AAAI

Professional organizations are important among the institutions of science, and Allen played his role in them also. It was an honor that he was proud of, but no surprise, that he was elected the first president of the American Association for Artificial Intelligence and received the first Award for Research Excellence from the International Joint Conference on Artificial Intelligence. Editorships, however, were not for him.

## ALLEN, THE PERSON

Allen Newell was a memorable person in the most literal meaning of that phrase. I will draw here on my own impressions as recorded in my autobiography (Simon, 1991) and follow these with some comments by others who knew him well.

> When I first met Al at RAND in 1952, he was 25 years old and fully qualified for tenure at any university—full of imagination and technique... His energy was prodigious, he was completely dedicated to science, and he had an unerring instinct for important (and difficult) problems. If these remarks suggest that he was not only bright but brash, they are not misleading.

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> If imagination and technique make a scientist, we must also add dollars. I learned . . . [from Al] . . . how to position the decimal point in a research proposal.... Thinking big has characterized Al's whole research career, not thinking big for bigness' sake, but thinking as big as the task invites...
>
> From our earliest collaborations, Al has kept atrocious working hours. By this I . . . mean . . . that he works at the wrong time of day .... He preferred sessions that began at eight in the evening and stretched almost to dawn. I would have done most of my day's work by ten that morning, and by ten in the evening was ready to sleep, and not always able not to.
>
> Perhaps his greatest pleasure . . . is an “emergency" that requires him to stay up all night or two consecutive nights. I recall his euphoria on our visit to March Air Force Base in 1954, when the air exercise extended over a whole weekend, twenty-four hours per day.
>
> Some of these memories are frivolous, but high spirits, good humor, and hard work have characterized my relations with Al from the beginning.

Allen was serious but not solemn. Whimsy and laughter came easily and often to him. Life, sometimes perplexing, was not a plodding march but a vivid drama in which he acted with brilliance and eclat, quite aware of the dramatic effects he was producing. This too was obvious early on. The Systems Research Laboratory operated on the grandest scale, its cast an entire Air Force unit. Only Allen and his codirectors could have dreamed up theater on this megabuck scale at a time when behavioral scientists might timidly request $5,000 or $10,000 for their research. His forceful qualities and his exuberance impressed themselves on all who met him.

As Cliff Shaw recalled (McCorduck, 1979): "Energy is the thing I remember mainly about working with Al. Energy and brilliance. Long phone calls and long sessions on the teletype were typical. We would have sessions late into the night at Al's home. I felt like I was tagging along behind, trying to get that Johnniac to do what we already knew

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could be done. And with Al's energy, it was a good thing he had IPL-V, the programming language, as another outlet, so all that energy didn't descend on me."

And much later, from a graduate student: "Allen Newell was not my adviser ... and was not on my thesis committee. But still Allen shaped my thinking: from him I learned every day more and more what research is. Through him I understood how to work dynamically towards my research goals.... Very rapidly, the initial intimidation I felt ... was transformed into admiration, friendship and respect." That testimonial, given at his memorial service, could be duplicated dozens of times over by his co-workers-from full professors to new graduate students. In his work with students he was patient, his criticism was constructive, he never lost his temper. If he had any faults as a mentor (and who does not?), it was probably in becoming so involved himself in his students' research problems that he sometimes provided them with more structure and more insights than was good for them. They had to work very hard and fast to retain the strategic initiative.

Everything he attended to he attended to with energy and depth-whether it was his current research problem or an inquiry directed to him by a student or a visitor. In fact, it was this inability to address matters superficially that made the diversions weigh so heavily on Allen, taking him from his main research for considerable periods of time. But he handled the diversions with the same cheery enthusiasm and eclat as he did the mainstream tasks. It is hard to recall a lackluster Newell performance, whether it be a public address, a conversation in his office, or the analysis of a thinking-aloud protocol.

I have said nothing about Allen's family life or leisure. Noel and Allen with their son Paul formed a close-knit family. As much of his work was done on the computer at home,

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he was not at all an absentee husband or father, but shared his activities with his family in spite of his marathon work week. The Newells enjoyed entertaining their friends, most of them from CMU or other academic communities. The categories of introvert or extrovert don't quite seem to fit Allen; he worked long hours in his study, but he spent enormous amounts of time with other people—usually engaged in professional tasks.

There was little evidence of, or time for, the simpler kinds of leisure or hobbies unrelated to his work. He traveled fairly often abroad, usually with Noel, but mostly to professional meetings, and only occasionally did he add more than a few vacation days to these trips. At the very end of his life I learned—much to my astonishment, as I had had no inkling of it—that he frequently watched Sunday afternoon (or was it Saturday afternoon?) TV football games, perhaps a bit of fond nostalgia for his high school athletic days.

It is fitting to conclude this account with a selection from Allen Newell's own set of maxims for the dedicated scientist, proposed in his "Desires and Diversions" talk of December 1991, for these maxims describe his own life:

> To each scientific life, its own style; and each style defines a life. Science is in the details.
> To work with the results of field X, you must be a professional in X.
> There is no substitute for working hard—very hard.
> A scientist is a transducer from nature to theory; seek out nature and listen to her.
> The scientific problem chooses you; you don't choose it.
> New things get their start by evolution or change, not design.
> Everything must wait until its time; science is the art of the possible.
> Diversions occur, make them count; salvage what is possible for the main goal.
> Solve whatever problems must be solved; but do not be seduced by them.

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Deep scientific ideas are exceedingly simple; others usually see them as trivial.

and, finally:

Choose a final project to outlast you.

For Allen, Soar was that project.

## NOTES

1. In preparing this account of Allen Newell's life I have drawn heavily on a briefer memorial (Simon, 1993) published in *Artificial Intelligence* and on a more complete one published by John Laird and Paul Rosenbloom (1992) in *AI Magazine.* Newell's papers are deposited in the Archives of Hunt Library at Carnegie Mellon University, where can also be found the transcripts of lengthy interviews with Newell by Pamela McCorduck, which were used extensively in her *Machines Who Think* (1979), and by Arthur L. Norberg, who interviewed Newell about his activities in connection with ARPA.

2. This talk was videotaped and is available by writing to University Video Communications, P.O. Box 5129, Stanford CA 94309.

3. Although, for reasons that are no longer obvious, Cliff Shaw was not a coauthor of this paper; he was a full partner in the entire research effort.

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# J. ROBERT OPPENHEIMER

*April 22, 1904-February 18, 1967*

BY H. A. BETHE

J. ROBERT OPPENHEIMER died on 18 February 1967 in Princeton, N.J. More than any other man, he was responsible for issuing American theoretical physics from a provincial adjunct of Europe to world leadership. Robert Oppenheimer was born on 22 April 1904 in New York. His father, who had come to the United States from Germany at the age of 17, was a prosperous textile importer. By inheritance, Robert was well-to-do all his life. The father was quite active in many community affairs, and much interested in art and music. He had a good collection of paintings, including three Van Goghs. Oppenheimer's mother, Ella Freedman, came from Baltimore. She was a painter who had studied in Paris, and was a very sensitive person. Robert had one younger brother, Frank, who also became a physicist; he is Professor of Experimental Physics at the University of Colorado, Boulder, Colo. Oppenheimer had close ties both with his parents and his brother.

As a boy, Robert was already most interested in matters of

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the mind. He attended the Ethical Culture School in New York, one of the best in the city. He was more interested in his homework, in poetry and in science than in mixing with other boys. He has said, 'It is characteristic that I do not remember any of my classmates.'

Already at the age of 5, Robert collected mineralogical specimens, some of which came from his grandfather in Germany. By the time he was 11 years old his collection was so good and his knowledge so extensive that he was admitted to membership in the Mineralogical Club in New York.

He entered Harvard in 1922 intending to become a chemist, but soon switched to physics. It was characteristic of him not to abandon a subject once he had become interested. Familiarity with chemistry was very useful to him in his Los Alamos days when purification of fissionable materials was one of the main problems of the laboratory. He also retained a lifetime affection for Harvard University, where he was a Member of the Board of Overseers from 1949 to 1955.

At Harvard he was strongly influenced by Professor Percy W. Bridgman, a great and very original experimental physicist. Apart from this, he kept much to himself and devoured knowledge. 'I had a real chance to learn,' he said. 'I loved it. I almost came alive. I took more courses than I was supposed to, lived in the library stacks, just raided the place intellectually.' In addition to studying physics and chemistry, he learned Latin and Greek and was graduated *summa cum laude* in 1925, having taken three years for the normal four-year course.

His work for the Ph.D. was even more astonishingly rapid: two years sufficed while the present average time required in the United States is four to five years.

After his B.A. degree he travelled for four years to the great centres of physics in Europe. The year 1925 to 1926

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he spent at Cambridge University, where he was exposed to the great personality of Lord Rutherford. It was the time when Heisenberg, Born and Schroedinger were developing quantum mechanics. Robert was fascinated and immediately accepted when an invitation came from Max Born to work with him at Gottingen. Here he took his Ph.D. in the spring of 1927.

Next he became a Fellow of the National Research Council, first at Harvard University, then at the California Institute of Technology. In the year 1928 to 1929, he was a Fellow of the International Education Board and visited Leiden and Zurich. He worked with Professor Pauli, an association which greatly influenced his further scientific life.

On his return to the United States in 1929, Oppenheimer received many offers of positions. He accepted two and became an Assistant Professor in Physics, simultaneously at the University of California in Berkeley and at the California Institute of Technology. In the ensuing 13 years, he 'commuted' between the two places, spending the fall and winter in Berkeley, and the spring term, beginning in April, in Pasadena. Many of his associates and students commuted with him.

It was here, in Berkeley, that he created his great School of Theoretical Physics. The majority of the best American theoretical physicists who grew up in those years were trained by Oppenheimer at one stage of their lives. Many were his graduate students, others came to him as Post-doctoral Fellows. They affectionately called him 'Oppie'. His teaching, his style and his example formed the scientific attitude of all of them.

## EARLY SCIENTIFIC WORK

Oppenheimer was most fortunate to enter physics in 1925, just when modern quantum mechanics came into being.

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While he was too young to take part in its formulation, he was one of the first to use it for the exploration of problems which had been insoluble with the old quantum theory.

In 1927, he wrote with Born a famous paper on the 'Quantum theory of molecules'. In this they showed how to separate the problem into one describing the motion of the electrons around fixed nuclei, and another to describe the motion (vibrations and rotation) of the nuclear skeleton. Their method still forms the basis of any treatment of molecules.

Oppenheimer's main interest until 1929 was the theory of continuous spectra. This was unexplored territory. He had to develop the method to normalize the eigen-functions in the continuous spectrum, and to do calculations of transition probabilities. Here as well as later in his work, his great knowledge of mathematical tools was most useful. He calculated the photoelectric effect for hydrogen and for X-rays. Even today this is a complicated calculation, beyond the scope of most quantum mechanics textbooks. Naturally, his calculations were later improved upon, but he correctly obtained the absorption coefficient at the *K* edge and the frequency dependence in its neighbourhood. It was disturbing that his theory, while agreeing well with measurements of X-ray absorption coefficients, did not seem to be in accord with the opacity of hydrogen in the sun. This, however, was the fault of the limited understanding of the solar atmosphere in 1926. It was then believed that the sun consisted mostly of heavy elements from oxygen on up, like the earth. Many years later, Strömgren suggested that the main constituent was hydrogen. This brought Oppenheimer's calculations of opacity into agreement with astrophysical data. Nowadays the opacity, calculated essentially on the lines of 'Oppie's' theory, is one of the main ingredients of all understanding of stellar interiors. In the course of his calcula

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tion of opacity, he also calculated the bremsstrahlung from electrons in the field of nuclei.

His work with the nuclear physicists at Cambridge motivated him to calculate the capture of electrons by ions from other atoms, i.e. such charge exchange processes as

$$He^{2+} + H = He^{+} + H^{+} \quad (1)$$

For this work he had to develop a method for the treatment of collision processes involving non-orthogonal save functions.

This work led him on to a treatment of the ionization of the hydrogen atom by electric fields, probably the first paper describing the penetration of a potential barrier, well before the theory of the alpha disintegration. Discussions with Millikan and Lauritsen at CalTech who had just observed the extraction of electrons from metal surfaces by very strong electric fields, motivated him to extend his theory to a description of this effect (1928).

Studying collisions between electrons and atoms, using the Born approximation, he pointed out that the incident electron can exchange with the atomic electron. This effect is indeed important for the understanding of the scattering of low energy electrons from such atoms as helium, as well as in high energy collisions. He could also make mistakes: he believed that exchange could explain the Ramsauer effect while actually this effect is due to the fact that an integral number of half-waves fit into the atom.

## THE BERKELEY PERIOD

Pauli, who all his life emphasized the problems at the very frontier of physics, exerted a lasting influence on Oppenheimer. As the frontier shifted from ordinary quantum mechanics to the relativistic quantum mechanics of

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Dirac, and the theory of electromagnetic fields, the work of Oppenheimer and his great school in Berkeley became chiefly devoted to these subjects.

As early as 1930, Oppenheimer wrote a fundamental paper which essentially predicted the positive electron. One year before, Dirac had reinterpreted the negative energy solutions of his relativistic equation for the electron as indicating the existence of positive charges. Dirac had believed that these were protons. Oppenheimer showed, by very cogent arguments involving symmetry, that the positive charges could not have the mass of the proton, but must have the same mass as the electron. This implicitly predicted the existence of the positron which was discovered three years later. Unfortunately Oppie was prevented from drawing this conclusion by his skepticism concerning the validity of the Dirac equation, a skepticism which had been engendered by another calculation (with Harvey Hall, his student) on the photoelectric effect at high energies, which appeared to disagree with experiment.

Also in 1930, Oppenheimer investigated radiative transitions, making use of the newly developed quantum electrodynamics of Pauli and Heisenberg. He had hoped that the infinite perturbations which Heisenberg and Pauli had found in their theory would not occur in observable processes like the scattering of light. To his disappointment they did. Only the mass renormalization of the late 1940's permitted physicists to eliminate these troubles.

His association with the CalTech experimenters stimulated him to calculate the energy loss of relativistic electrons (1932, with his student Carlson). Their result has proved correct but, at the time, it was believed in contradiction with the evidence from cosmic radiation. In 1933, cosmic radiation yielded the first new particle: Carl Anderson at CalTech discovered the positron which Oppie had almost

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predicted three years earlier. Oppie immediately proceeded to calculate the cross section for production of positrons at low energy, with his student Milton Plesset. His great knowledge of the continuous spectrum wave functions in the Coulomb field was most useful for this purpose. A more thorough theory with Nedelsky followed.

A little later, he extended the theory of electron pair production to a theory of the showers which are such a prominent phenomenon in cosmic radiation. It had been pointed out by Nordheim, Heitler and Bhabha that these showers could be explained as follows: electrons emit electromagnetic radiation (gamma rays) and these gamma rays in turn produce electron pairs in the electric field of atomic nuclei. Oppenheimer, with his associates Carlson and H. Snyder, developed a most elegant mathematical theory of the multiplicity of air showers, a masterpiece of mathematical treatment of a physical phenomena.

All the time, however, Oppenheimer was worried about the likely breakdown of quantum electrodynamics at energies above 137 $mc^2$. Indeed, laboratory experiments on the penetration of cosmic ray particles through slabs of lead and similar substances seemed to indicate this breakdown very clearly, provided the particles were electrons. It was only in 1937 that it was discovered that the particles were in fact not electrons but mesons. While most physicists were troubled by the supposed breakdown, it dominated Oppie's thoughts, more than anybody else's and he impressed his worries on his students. A number of his papers deal with this problem. We know now that there is no such breakdown and that in fact quantum electrodynamics holds at least up to about a hundred times this energy, probably higher.

Oppie was also very active in other aspects of fundamental quantum theory. In 1931, he attempted to get a first-

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order differential equation for light quanta, similar to Dirac's equation for the electron. He failed, but in the process recognized the fundamental difference between particles of spin one-half and of integral spin. This was later a basis of Pauli's theory of the relation between spin and statistics.

In 1934, with Furry, he developed a field theory of the Dirac equation, treating electrons and positrons as of equal status. This paper contains essentially the modern form of the electron-positron theory. He was much concerned with other consequences of the existence of the positron. He and his collaborators found that the observable charge of the electron is not the true charge, foreshadowing charge renormalization. They pointed out the effect of vacuum polarization by virtual pairs of electrons and positrons being formed in strong electric fields. Similar ideas were simultaneously discussed by Dirac and others, but the most explicit calculation of vacuum polarization was made by Oppenheimer's student, Uehling.

In 1937 Anderson and others discovered the meson which had been predicted two years earlier by Yukawa in an effort to explain nuclear forces. Making use of Yukawa's theory, Oppie had suggested that the 'hard component' of cosmic rays, i.e. that which penetrates to sea-level, might consist of mesons which, being much heavier than electrons, would have greater penetrating power, while the soft component was interpreted as electrons and positrons, on the basis of the success of shower theory. Now, after Anderson's discovery, he immediately turned his attention to the properties of mesons. Oppenheimer and two of his students, Christy and Kusaka, showed that the meson could not have a spin of 1 or greater, because otherwise it would radiate too fast when penetrating underground. Oppie carefully discussed why he believed the theory of radiation to be valid in this case.

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With Serber, he discussed the production of mesons from primary cosmic rays in the upper atmosphere. With Christy, he postulated that together with the penetrating, charged mesons, other particles should be produced in the upper atmosphere which have a short life and then decay into gamma rays, thus giving rise to the soft component of cosmic rays. In 1947 he postulated that these intermediate particles are neutral mesons ( °), well before the discovery of that particle.

Both at Berkeley under Ernest Lawrence, and at Pasadena under Lauritsen, experimental nuclear physics was developing rapidly. Oppenheimer and his students turned their attention to this field from 1933 on. He calculated the excitation function for collisions between protons and nuclei, thus helping much in the interpretation of experiments. His most important contribution was the 'Oppenheimer-Phillips process' in which a deuteron, entering a heavy nucleus, is split into proton and neutron, one of these particles being retained by the nucleus while the other is re-emitted. He gave the first quantitative description of this very prominent process which after the war became an important tool in the study of nucleon energy levels and their properties. He also calculated the density of nuclear energy levels, the nuclear photo-effect and the properties of nuclear resonances. When Lauritsen observed that fluorine, bombarded with protons, gave electron pairs, Oppenheimer contributed much to the explanation: the nuclear reaction is

$$^{19}F + H = {}^{16}O^* + {}^{4}H \quad (2)$$

$^{16}$O is formed in an excited state of angular momentum 0. By selection rules a transition from such a state to the ground

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state can most easily be accomplished by converting a virtual gamma ray into a pair of electrons.

At Pasadena one of the most important activities was astronomy, through the Mount Wilson Observatory. Richard Tolman worked on general relativity. Oppenheimer became interested in neutron stars, and with Snyder, in the gravitational contraction of massive stars until they disappear from observability.

In 1940 and 1941, Oppenheimer's attention was turned to meson theory and the attempt to explain nuclear forces by mesons. He attempted to deal with strong coupling, using his own theories as well as that of Wentzel. He predicted the existence of nucleon isobars with an excitation energy slightly below the rest energy of the meson.

In addition to this massive scientific work, Oppenheimer created the greatest school of theoretical physics that the United States has ever known. Before him, theoretical physics in America was a fairly modest enterprise, although there were a few outstanding representatives. Probably the most important ingredient he brought to his teaching was his exquisite taste. He always knew what were the important problems, as shown by his choice of subjects. He truly lived with these problems, struggling for a solution, and he communicated his concern to his group. In its heyday, there were about eight or ten graduate students in his group and about six Post-doctoral Fellows. He met this group once a day in his office, and discussed with one after another the status of the student's research problem. He was interested in everything, and in one afternoon they might discuss quantum electrodynamics, cosmic rays, electron pair production and nuclear physics.

In his classroom teaching he always applied the highest standards. He was much influenced by Pauli's article in the *Handbuch de Physik,* which provided the deepest understanding

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of quantum mechanics then and even now. Among his students was Leonard Schiff who incorporated much of Oppenheimer's spirit into his excellent textbook on quantum mechanics. New problems were constantly introduced into the quantum mechanics lectures. The lectures were never easy but they gave his students a feeling of the beauty of the subject and conveyed his excitement about its development. Almost every student went through his course more than once.

Oppie saw much of his students and associates after working hours. He would frequently treat them to an exquisite dinner in San Francisco, or to a less ambitious one in a Mexican restaurant in Oakland. His most constant collaborator of these years, Serber, writes of these excursions: 'One should remember that these were post-depression days when students were poor. The world of good food, good wines and gracious living was far from the experience of many of them, and Oppie was introducing them to an unfamiliar way of life. We acquired something of his tastes. We went to concerts together and listened to chamber music. Oppie and Arnold Nordsieck read Plato in the original Greek. During many evening parties we drank, talked and danced until late, and, when Oppie was supplying the food, the novices suffered from the hot chilli that social example required them to eat.'

The magnetism and force of his personality was such that many of his students copied his gestures and mannerisms. Among his students, in addition to those already mentioned, were Fritz Kalckar, George Volkoff, Sid Dancoff, Phil Morrison,Joe Keller, Willis Lamb, Bernard Peters, Bill Rarita, and many others. As Oppenheimer himself has written: 'As the number of students increased, so in general did their quality. The men who worked with me during those years held chairs in many of the great centers of physics in the

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United States; they have made important contributions to science, and in many cases to the atomic energy project.'

During his Berkeley time, Oppie had also many friends in the faculty, scientists, classicists and artists. He studied and read Sanskrit with a colleague, and his private reading ranged over the classics, novels, plays and poetry.

Most of the time he was indifferent to the events around him; he never read a newspaper, he had no radio or telephone, he learned of the stock market crash in 1929 only long after the event.

His interest in politics began in 1936. He had been much disturbed by the treatment of the Jews of Germany, including some of his relatives. He saw the effect of the American depression on his students, and had great compassion with them and others who could not find any jobs.

In these days, Oppie's sympathies were quite left-wing. He contributed to a strike fund of the Longshoremen's Union and to various committees helping the Spanish Loyalists in the Civil War. His brother and his sister-in-law were members of the Communist Party for some time; he himself apparently never joined. As far as I can tell, he moved away from the party in 1939 and 1940. He was disgusted by the pact between Stalin and Hitler which permitted Hitler to start the Second World War. He was deeply distressed by the fall of France in 1940. I saw him shortly thereafter at an evening party when he spoke long and eloquently about the terrible tragedy that the fall of France meant to Western civilization. Clearly he entirely disagreed with the Communist slogan that this was 'An imperialist war'.

In 1936 he was promoted to a full professorship at Berkeley and CalTech. In 1941 he was elected to the United States National Academy of Sciences.

In 1940 Oppenheimer married Katherine Harrison. They had one son, Peter, and a daughter Katherine. They lived

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in a most beautiful house on Eagle Hill, overlooking all of San Francisco Bay, where I (and of course even more his Berkeley friends) spent many happy hours.

## LOS ALAMOS

In 1942, Oppenheimer felt the deep urge to contribute to the American war effort. The opportunity came soon. He was appointed the leader of a theoretical effort to design the atomic bomb.

By the summer of 1942 it was very likely that Fermi's atomic pile would work, that Dupont would build a production reactor, and that useful quantities of plutonium would be produced. The separation of uranium-235 by the electromagnetic method, though extremely expensive, also seemed very likely to succeed; the separation by gaseous diffusion was less certain. In any case, the committee in charge of the uranium project considered it advisable to begin a serious study of the assembly of a weapon. It proved accurate timing. In 1945, the preparations for the assembly of the weapon were finished just about the same time that the necessary amounts of material became available.

Oppenheimer assembled a small group of theoretical physicists: Teller, who had been working on the atomic pile in Chicago, Van Vleck and myself who had been working on radar, Konopinski, Serber who was then associated with Oppenheimer, and three of his own graduate students. Some members of our group, under the leadership of Serber, did calculations on the actual subject of our study, the neutron diffusion in an atomic bomb and the energy yield obtainable from it. The rest of us, especially Teller, Oppenheimer and I, indulged ourselves in a far-off project-namely, the question of whether and how an atomic bomb could be used to trigger an H-bomb. Grim as the subject was, it was a most exciting enterprise. We were forever inventing new

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tricks, findings ways to calculate, and rejecting most of the tricks on the basis of the calculations. Now I could see at first-hand the tremendous intellectual power of Oppenheimer who was the unquestioned leader of our group. The ideas we had about triggering an H-bomb later turned out to be all wrong, but the intellectual experience was unforgettable.

In the fall of 1942 plans were started for a more permanent laboratory to investigate the assembly of a nuclear bomb. Oppenheimer chose its location, together with General Groves who was by then in charge of the 'Manhattan Project'. General Groves wanted a remote place in order to keep the secrecy of the project. Oppie knew just the place. He had spent many happy summers in the Pecos Valley in New Mexico, on a ranch, owned by him and his brother. He knew about the Los Alamos Ranch School, an expensive boarding school for boys, which was in bad financial condition. The school was bought out and the Government established its laboratory on one of the most beautiful mesas in New Mexico, with a splendid view of the Sangre de Cristo Mountain Range across 30 miles of the Rio Grande Valley. Pleasant aspen forests rose from Los Alamos to the crest of a minor mountain range, the Jemez, and gave the inhabitants of Los Alamos many opportunities for pleasant hikes, horseback rides and ski expeditions.

Oppenheimer searched the country for the best experimental and theoretical nuclear physicists, for general physicists, chemists and engineers. The task was difficult because many of the best people were already deeply engaged in war work, and some were reluctant to leave this work which promised immediate applicability in World War II, for the remote possibility of an atomic bomb. Nevertheless a magnificent staff was assembled.

Oppenheimer had the great desire to identify with the U.S. war effort, and was quite ready to accept a commission

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as a Lt.-Colonel in the U.S. Army as was desired by General Groves. The better judgment of some of his colleagues, more experienced in scientific war work, prevented him and the rest of us from becoming integrated into the Army machinery. Of course the Army had charge of guarding the laboratory, of construction of both laboratory and civilian housing, of the civil administration of the town and essentially of all our lives. But in scientific matters the laboratory remained independent.

It was not obvious that Oppenheimer would be its director. He had, after all, no experience in directing a large group of people. The laboratory would be devoted primarily to experiment and to engineering, and Oppenheimer was a theorist. It is greatly to the credit of General Groves that he overruled all these objections and made Oppenheimer the director.

It was a marvellous choice. Los Alamos might have succeeded without him, but certainly only with much greater strain, less enthusiasm, and less speed. As it was, it was an unforgettable experience for all the members of the laboratory. There were other wartime laboratories of high achievement, like the Metallurgical Laboratory at Chicago, the Radiation Laboratory at M.I.T., and others, both here and abroad. But I have never observed in any of these other groups quite the spirit of belonging together, quite the urge to reminisce about the days of the laboratory, quite the feeling that this was really the great time of their lives. The scientific work at Los Alamos has often been described. I will quote the description by Victor Weisskopf in *Physics Today:*

> 'The task facing Oppenheimer and his collaborators was stupendous. When the work started at Los Alamos not much more was known than the fundamental ideas of a chain reaction. What happens in a nuclear explosion had to be theoretically predicted in all details for the design of the

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> bomb since there was no time to wait for experiments; no fissionable material was available yet. The details of the fission process had to be understood. The slowing down of neutrons in matter and the theory of explosions and implosions under completely novel conditions had to be investigated. Nuclear physicists had to become experts in fields of technology unknown to them such as shock waves and hydrodynamics. Oppenheimer directed these studies, theoretical and experimental, in the real sense of the words. Here his uncanny speed in grasping the main points of any subject was a decisive factor; he could acquaint himself with the essential details of every part of the work.
>
> 'He did not direct from the head office. He was intellectually and even physically present at each decisive step. He was present in the laboratory or in the seminar rooms, when a new effect was measured, when a new idea was conceived. It was not that he contributed so many ideas or suggestions; he did so sometimes, but his main influence came from something else. It was his continuous and intense presence, which produced a sense of direct participation in all of us; it created that unique atmosphere of enthusiasm and challenge that pervaded the place throughout its time.'

He was everywhere at all times, and he worked incredibly long hours. Nevertheless, he still had time for some social life; in fact, the Oppenheimer house with his attractive wife was a social centre. He lived, as far as we could see, on his nervous energy. Always quite thin, he lost another twenty pounds and during a bout with measles reportedly got down to 104 lb., being six feet tall. It is remarkable that his health could stand this pace, because he was never physically strong. The one sport he loved was horseback riding. But in the three years at Los Alamos there was time only for one overnight ride on the two horses his wife fed and groomed for their use. Before Los Alamos, on his ranch, he used to keep five horses for himself and his guests.

One of the factors contributing to the success of the laboratory was its democratic organization. The governing board, where questions of general and technical laboratory policy were discussed, consisted of the division leaders (about eight of them). The coordinating council included all the group

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leaders, about 50 in number, and kept all of them informed on the most important technical progress and problems of the various groups in the laboratory. All scientists having a B.A. degree were admitted to the colloquium in which specialized talks about laboratory problems were given. Each of these three assemblies met once a week. In this manner everybody in the laboratory felt a part of the whole and felt that he should contribute to the success of the programme. Very often a problem discussed in one of these meetings would intrigue a scientist in a completely different branch of the laboratory, and he would come up with an unexpected solution.

This free interchange of ideas was entirely contrary to the organization of the Manhattan District as a whole. As organized by General Groves, the work was strictly compartmentalized, with one laboratory having little or no knowledge of the problems or progress of the other. Oppenheimer had to fight hard for the free discussion among all qualified members of the Los Alamos Laboratory, but the free flow of information and discussion, together with Oppenheimer's personality, kept morale at its highest throughout the war.

Weisskopf says 'One of the most important factors that kept us at work was the common awareness of the great danger of the bomb in the hands of an irresponsible dictator. After his defeat, it turned out that this danger was in fact not so great; still the work and the spirit continued until the task was accomplished, until in the desert of Alamogordo for the first time a nuclear fire was kindled by man. Every one of us, and Oppenheimer more than anyone, was deeply shaken by this event.'

For his work at Los Alamos, Oppenheimer received the Medal of Merit from President Truman in 1946, 'for his great scientific experience and ability, his inexhaustible en

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ergy, his rare capacity as an organizer and executive, his initiative and resourcefulness, and his unswerving devotion to duty....'

## HIS PUBLIC LIFE

It was obvious that a community like Los Alamos would be deeply concerned with the ominous implications of the atomic bomb. Oppenheimer was one of the most concerned, and had many discussions about this problem with Niels Bohr. Bohr had come to the United States in 1944 and had been asked to help us at Los Alamos. He was quite interested in our work and gave us some advice. However, his main interest was in talking to statesmen and trying to persuade them that international control of the atom was the only way to avoid a pernicious arms race or worse, atomic war. Bohr did not succeed with statesmen but he greatly impressed Oppenheimer and through him the rest of us at Los Alamos.

After the war the American scientists exerted much pressure in Washington. One of their wishes was civilian control of atomic energy rather than continued control by the Army. The Senate responded to the urging of Szilard, Condon and of the American Federation of Scientists, by setting up the McMahon Committee which after long labour, devised the Atomic Energy Act of 1946. Oppenheimer, although originally in favour of military control because it would provide a smoother transition, was an effective witness before the McMahon Committee.

More urgent still seemed the problem of international control. By the intervention of some far-sighted statesmen, President Truman was persuaded to appoint a committee to study this problem, under David Lilienthal. Oppenheimer was one of the members. Lilienthal describes the work of the committee impressively in his 'Journal'. All five mem

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bers were outstanding men in business, engineering or science. But Oppenheimer brought to it the years of experience of creation of the atomic bomb. The work of the committee, although all its members contributed, was primarily that of Oppenheimer. Lilienthal said of him, 'He was the only authentic genius I have ever met.'

The Lilienthal Report which was then endorsed by Under-Secretary of State Dean Acheson called for the creation of an international authority to control all atomic-energy work. The plan emphasized the need for a positive task for the international authority. It should develop atomic reactors for power and other peaceful uses, and also atomic weapons if desired; it should not have merely the function of a policeman preventing individual nations from developing atomic energy and weapons on their own. This wise plan became official U.S. policy. Its presentation to the United Nations was entrusted to Bernard Baruch, a very respected and very conservative elder statesman. Unfortunately Baruch's advisers and Baruch himself, changed the emphasis: instead of pointing to the great joint task of developing peaceful uses of atomic energy, Baruch placed the main emphasis on the 'condign' punishment of violators of the agreement to be concluded. I do not know whether there was ever any chance of acceptance of the plan by the Soviet Union, that country being at the time exclusively concerned with its own national interest. But if there ever was a chance it was lost by the manner of Baruch's presentation.

Oppenheimer was one of the first to see that the plan would be rejected by the U.S.S.R. Most of the members of the Federation of American Scientists held on to hope beyond hope. His realism, as well as his official duties, kept Oppenheimer rather separate from the Federation and other political organizations of the scientists.

His first government appointment was in 1945, as a mem

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ber of Secretary of War Stimson's Scientific Panel of the War Department's Interim Committee on Atomic Energy. This panel was asked, before Hiroshima, whether there was any technically effective alternative to dropping the bomb on Japan; its answer was negative. Later, an enlarged panel was asked what to do with atomic energy after the war. The members of this enlarged panel were Oppenheimer, members of the other wartime laboratories of the Manhattan District, and several elder-statesmen scientists. One of the committee's meetings took place at Los Alamos, and some other Los Alamos scientists were asked to participate. I remember this meeting very vividly. All of the participants were impressive people who had made great contributions. Nevertheless, whenever Oppenheimer left the room, discussion slid back into fairly routine problems, such as the specific nuclear reactions one should investigate and the kind of research that could be done with a nuclear reactor. On his return, the level of the discussion immediately rose and we all had the feeling that now the meeting had become really worth while.

Oppenheimer's most important Government task was to be Chairman of the Atomic Energy Commission's (AEC) General Advisory Committee (GAC) from 1946 to 1952. This most important body included Fermi, Rabi, Conant, Dubridge, Smythe and Seaborg (both later AEC Commissioners) and two industrialists, Worthington and Rowe. It advised the Commission not only on scientific matters but also on matters of general policy. It was a hard-working committee, having about six sessions a year, of three days each, mostly over week-ends. In the words of Seaborg 'At the conclusion of each session, when the AEC Commissioners came in to review our work, Oppie presented a masterful summary of the proceedings. I know that my fellow members of the GAC remember with me that this was pure

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Oppenheimer at his very best. I regret that tape-recordings were not made of these eloquent summations of our deliberations, for I believe that these would provide fascinating historical material.'

The first task of the GAC and AEC was to strengthen the position of the U.S. in the production and military use of fissionable material. The plutonium production plants at Hanford had to be improved and further ones had to be built. Oppenheimer devoted much time to strengthening the Los Alamos Laboratory after many of its members had left at the end of the war, as well as supporting the other AEC laboratories, Oak Ridge and Argonne.

These latter two laboratories were given the specific task of developing nuclear power. Oppenheimer had the great desire to foster peaceful applications but, like most of his colleagues on the GAC, he was overly pessimistic about economic possibilities. In a talk at this time, he thought that the application of isotopes in research would for a long time remain the most important peaceful application of atomic energy. In a sense he was right; it took about ten years before large-scale power reactors were constructed in the United States and only recently have they become economical.

Oppenheimer was deeply devoted to the support of fundamental research in nuclear physics. The Brookhaven National Laboratory was established for this specific purpose, the Radiation Laboratory at Berkeley was generously supported, and many university projects for the construction of high energy accelerators and their use were financed. The AEC was one of the chief contributors to the tremendous expansion in research in physics in the United States, and Oppenheimer and his GAC gave much encouragement to the Commission to do so. Oppenheimer strongly advocated to make fundamental scientific information available

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to scientists all over the world and distributing special materials, such as radio-isotopes, freely to scientists abroad.

In military applications, Oppenheimer was one of the first advocates of a system to detect foreign nuclear weapons tests. He proposed this while still at Los Alamos. He then supported strongly the programme to develop techniques for detection in 1948 to 1950. This was one of his many functions as Chairman of the Committee on Atomic Energy of the Joint Research and Development Board of the Armed Services. In addition this committee was concerned with the proper application of atomic weapons in warfare. Its membership was half civilian, half military. His efforts to get a detection system established bore fruit on 29 August 1949 when the first Soviet atom bomb was exploded. A panel of the Committee on Atomic Energy including Oppie himself, scrutinized the evidence presented and concluded that indeed a weapons test had taken place in the Soviet Union.

He served the Joint Research and Development Board from 1947 to 1952, also in other capacities. He was a member of the National Research Advisory Committee from 1949 to 1952, and of the Secretary of State's Panel on Disarmament in 1952 and 1953. Most important of these committees was the Science Advisory Committee (1951-1954). It was then part of the Office of Defense Mobilization and later developed into the President's Science Advisory Committee.

More important still, he participated in many summer studies on the effect of nuclear weapons on military tactics and strategy. In particular, in the Vista project, the study group urged that the U.S. should not place its main reliance on strategic atomic weapons and massive retaliation, but should rather develop tactical nuclear weapons to defend Western Europe against possible Russian attack. This

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advice was very unpopular in many quarters of the Air Force, devoted primarily to strategic bombing.

In 1949, after the U.S.S.R. had exploded its first atomic weapon, the work of the GAC reached a crisis. As a response to the Soviet explosion, Edward Teller and Ernest Lawrence proposed that the U.S. should develop H-bombs. The GAC wrote a strong recommendation against the crash development of the 'super'. All members of the Committee agreed on this (Seaborg did not attend, after writing a letter stating that he was quite undecided).

One important argument of the GAC was that there was, at that time, no sufficient technical basis for this development (the crucial invention was made in 1951, by Teller). Another strong argument was that the U.S. should not deliberately step up the arms race, and should at least first make an effort to discuss with Soviet Russia the possibility of an agreement not to develop hydrogen weapons. A more radical minority report was written by Fermi and Rabi.

For about three months the issue was hotly debated in Washington. The Joint Committee on Atomic Energy of the Congress enthusiastically endorsed the proposal by Teller and Lawrence. Lilienthal, Chairman of the AEC, supported the GAC position and writes in his 'Journal' about the nervous strain of this battle. The decision probably came when Acheson, the Secretary of State, endorsed the H-bomb plan. At the end of January 1950 President Truman decided to pursue with full vigour the design and manufacture of an H-bomb.

He probably could not have decided any other way at the time. However, it is most deplorable that time and again nations have decided in favour of another step in armament without first trying to obtain mutual agreement with other nations to refrain from new escalation of death. The effort of Oppenheimer and the GAC to make the U.S. Gov

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ernment pause and think about this step stands as a most important milestone.

After President Truman had overruled the committee, Oppenheimer tried to resign as Chairman of the Committee, but the resignation was not accepted, probably wrongly.

## THE SECURITY INVESTIGATION

1953 was a difficult year in U.S. politics. Senator Joseph McCarthy charged nearly anyone he could think of with being a Communist, and hence a traitor to the United States. Since McCarthy's charges had contributed much to the defeat of the Democrats in the Presidential elections of 1952, the new Republican government let him have free rein for a long time.

That Robert Oppenheimer would be one of the victims was foreshadowed in a scurrilous article in *Fortune* in 1953. The author had collected much material from disgruntled officers of the Air Force who were opposed to Oppenheimer's defense policy. Although they had won the battle for massive retaliation they wanted to defeat the 'enemy' completely. A former employee of the Joint Congressional Committee on Atomic Energy in a nearly paranoic letter, accused Oppenheimer of being a Communist and working against the interest of the U.S. Oppie had also made some personal enemies, and on the basis of all this, in December 1953, President Eisenhower ordered that Oppenheimer's clearance for secret government work be terminated. This was communicated to him by the AEC in December 1953. Oppenheimer answered the charges in a long letter, and both charges and answer were published in the *New York Times,* on 13 April, 1954.

Oppenheimer chose to have a security investigation which was organized essentially like a Court of Law with a Board of three judges, and lawyers both for the government and

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for the defense. He chose to face this investigation in spite of the fact that he was quite convinced from the beginning that he would lose his case.

The ensuing, long-protracted security investigations became a *cause celebre.* Many of his scientist friends came out in his defense, a few against him. The *Proceedings,* published by the AEC, give a vivid story of the discussions within the U.S. Government on defense policy between 1947 and 1953. They have been avidly read by friend and foe, at home and abroad.

Both the Security Hearing Board, by a vote of 2 to 1, and the AEC, by a vote of 4 to 1 decided to withhold security clearance from Oppenheimer. In the final majority opinion by the Commission the only real argument against granting him clearance was the grotesque story involving Haakon Chevalier in 1942. Intrinsically this 'espionage attempt' was of no importance whatever; the counter-intelligence corps did not even bother to investigate the lead until May 1946. But apparently Oppenheimer, in an effort to shield his friend Chevalier, and at the same time not to endanger his position as Head of the Los Alamos Laboratory, had first invented a very foolish 'cock-and-bull-story' and then later denied it.

The importance attached to this incident is the more astonishing as (1) these facts had all been known to General Groves who had cleared him for wartime work; (2) the same facts were scrutinized by the whole AEC in 1947 and again clearance was granted for the most delicate atomic energy work. One of the members of the AEC in 1947 was Lewis Strauss who, in 1954, wrote the majority opinion of the AEC against him. It is hard to imagine that this old story could have attained so much greater importance between 1947 and 1954.

The scientific community, with few exceptions, was deeply

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shocked by the decision of the AEC. An eloquent discussion was given by Bush, the wartime leader of the U.S. Science Defense effort, in the New York *Times Magazine,* 13 June, 1954. Personally I felt that the AEC which I had always regarded as our, the scientists', agency in the government, had become a hostile body.

The AEC soon made efforts to reconcile the scientific community. Perhaps most important was the appointment of John Von Neumann, the noted mathematician, as a second scientific member of the Commission. He was universally respected, by the friends of Oppenheimer as well as those of Teller. Soon afterwards Joseph McCarthy's agitation ended when a Senate Committee investigated his own behaviour as a committee chairman, and this led to McCarthy's censure by the Senate. The political climate generally improved.

But it took until 1961 for the Government to make amends to Oppenheimer, President John F. Kennedy invited Oppenheimer to a White House dinner given in honour of Nobel Prize Winners. The most important recognition, however, was the presentation to him of the Fermi Award of the AEC, the highest honour that body can bestow. It carries a prize of $50 000.

## THE FERMI AWARD

The decision to present the Award was made by President Kennedy, the actual presentation by President Johnson in December 1963. On the presentation President Johnson said in part: 'Dr. Oppenheimer, I am pleased that you are here today to receive formal recognition for your many contributions to theoretical physics and to the advancement of science in our nation. Your leadership in the development of an outstanding school of theoretical physics in the

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United States and your contributions to our basic knowledge make your achievements unique in the scientific world.'

In his acceptance remarks Oppenheimer said, 'I think it is just possible, Mr President, that it has taken some charity and some courage for you to make this award today.'

## THE PRINCETON PERIOD

In 1947, Oppenheimer was appointed Director of the Institute for Advanced Study in Princeton. The Institute had always included prominent physicists: Albert Einstein had been one of its Charter Members appointed in 1933. Bohr and Dirac had been frequent visitors, and Pauli spent the war years there. A number of other well-known physicists had worked at the Institute at one time or another.

But on Oppenheimer's arrival, the physics department of the Institute changed. While its emphasis had been on well established professors before, it now became a centre for young physicists. Five research associates from Berkeley came with him in 1947. Thereafter the Institute was open to dozens of post-doctoral fellows, from the United States and abroad. Even more than Berkeley in the 1930's, the Princeton Institute became the centre for physics. Nearly everybody who was anybody passed through its stimulating atmosphere. Murray Gell-Mann, Marvin L. Goldberger, Geoffrey F. Chew, Frances E. Low, Yoichira Nambu, were among the American visitors, Maurice Levy came from France, Lehman and Symancik from Germany, and countless visitors from Great Britain, Italy, Japan and other countries. There was a distinguished permanent staff including Pais, Dyson, Placzek, T. D. Lee and C. N. Yang. The distinguished visitors of old times continued to come.

Oppenheimer brought to the Institute his whole method of inspired teaching. He no longer did much research of his own, but he constantly inspired his collaborators. The

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seminars which he directed were always very lively. In 1948 I gave one of these seminars, on some calculations concerning the Lamb shift. I spoke for less than half the time and this, I was told, was a much larger fraction of the time than was customary in the seminar. The rest was discussion by the many bright young physicists, and especially by Oppenheimer himself. Ideas developed fast in this atmosphere of intense discussion and stimulation.

Vigourous discussion as well as emphasis on fundamental problems remained Oppenheimer's style. All through his life he was able to convey to all around him a sense of excitement in the quest of science.

He could also irritate the people who worked with him. His great mind was able to read and digest physics much faster than the minds of his less gifted colleagues. In scientific conversation he always assumed that others knew as much as he. This being seldom the case and few persons being willing to admit their ignorance, his partner often felt at a disadvantage. Yet, when asked directly, he explained willingly.

Abraham Pais writes of his influence at the Institute: 'He could convey to young men a sense of extraordinary relevance of the physics of their day and give them a sense of their participation in a great adventure, as for example in the Richtmyer lecture: “There are rich days ahead for physics; we may hope, I think, to be living in one of the heroic ages of physical science, whereas, in the past, a vast new field of experience has taught us its new lessons and its new order."

'He could define and thereby enhance their dedication, by words such as these: "People who practice science, who try to learn, believe that knowledge is good. They have a sense of guilt when they try to acquire it. This keeps them busy . . . It seems hard to live any other way than thinking

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that it was better to know something than not to know it; and that the more you know the better, provided you know it honestly."

'To an unusual degree, Oppenheimer possessed the ability to instill such attitudes in the young physicists around him, to urge them not to let up. He could be critical, sharply critical at times, of their efforts. But there was no greater satisfaction for him than to see such efforts bear fruit and then to tell others of the work that someone had done.'

In addition to his work at the Institute, he was a leading spirit for many years at the Conferences on Physics which started from a small basis and then expanded into international scope.

Pais writes: 'The first such conference in physics took place on 2-4 June 1947, on Shelter Island, New York. For this meeting Oppenheimer wrote the outline of topics for discussion entitled "The foundations of quantum mechanics". As was to happen so often in the following years, Oppenheimer showed himself to be the three-fold master: by stressing the important problems, by directing the discussion and by summarizing the findings.

'In his outline he discussed the copiousness of meson production in cosmic radiation in terms of meson theories then current and concluded that "no reasonable formulation along this line will satisfactorily account for the smallness of the subsequent interaction of mesons with nuclear matter". In the discussion of this point, Robert Marshak got up to propose that there should be two kinds of mesons. It was, one may recall, in September of that year that Cecil F. Powell reported the discovery of  decay at a Copenhagen conference.'

The Shelter Island Conference witnessed the opening of a new chapter in quantum electrodynamics. Willis Lamb, one of Oppenheimer's Ph.D students (1938), gave an ac

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count of his experiment on the upward energy shift of the 2-S state of hydrogen. Rabi reported on a deviation in the hyper-fine structure of hydrogen and deuterium from theory.

Immediately Oppenheimer emphasized that here one might be faced with self-energy effects. This subject was close to his mind: as early as 1930 he had been concerned with atomic level displacements due to radiative effects. Oppenheimer's remarks, and a talk by Kramers, stimulated me, immediately after the Shelter Island Conference, to explain the Lamb shift as a residual self-energy effect due to the interaction of the electron with the electromagnetic field. My theory was only non-relativistic. But at the next conference, at Pocono Manor in April 1948, Schwinger and Feynman discussed their different, relativistic solutions for the self-energy effects. The Old-Stone-On-Hudson meeting, a year later, discussed further development of the theory.

At these conferences Oppenheimer was the undisputed leader. Pais found some comments in old notes from the Pocono Conference. By Oppenheimer: 'Now it doesn't matter that things are infinite.' By Rabi: 'What the hell should I measure now?' Pais remarks: 'They reflect the sense of optimism of the late forties, especially the expectation that with the new theoretical tools other than electromagnetic interactions would soon give sensible results.'

Oppenheimer continued to play a leading role in the Conferences, which from then on developed into the Rochester Conferences. The latter soon became international. They were among the first conferences which brought together the scientists from East and West. And they have continued to do so, through easy and difficult political times. This role of science to bring together people of different countries and different political opinions, was very much Oppie's desire.

Oppenheimer had become widely known as a principal

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representative figure of the natural sciences. Thus, when in 1948 the American Institute of Physics inaugurated a new journal, *Physics Today,* the dialogue between theory and experiment was symbolized on the cover of its first issue by a picture of a pork-pie sombrero, Oppenheimer's well-known symbol, tossed on a cyclotron. When in 1950 the *Scientific American* devoted an issue to summarize that incredibly full half-century in science, 1900-1950, it was fitting that Oppenheimer should write its general introduction.

In the 1950's, the Institute at Princeton continued to play its leading role. One of the main problems was the profusion of new particles which had been discovered. Pais was one of the men who brought some order into this chaos. Later on Yang & Lee solved a great puzzle in the behaviour of the K-meson by postulating that parity need not be conserved in weak decays. Astrophysics and statistical mechanics were also successfully pursued at the Institute. Oppenheimer was always there to stimulate, criticize, encourage and clarify. Even to the last days (I saw him a few months before his death, when he was already very ill) he followed all of particle and theoretical physics with avidity, and discussed the problems with profundity, and with curiosity about the next step.

## WRITINGS ON GENERAL TOPICS

Ever since the Second World War, Oppenheimer's own writings and talks were concerned with general subjects rather than with physics. There is an impressive list of them, about 125. He was invited to give lectures at many universities, and in other distinguished settings, like the Reith Lectures of the B.B.C. In his lectures he cast a spell over his audience with his marvellous command of the English language. It was a pleasure just to listen to him and watch how he formulated his thoughts. He added much wit and a store of

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good anecdotes, and most importantly, the signs of deep concern about humanity.

Probably his greatest concern was the relation between modern science and the general culture of our time. He was troubled that the tremendous increase of knowledge makes it impossible for an intelligent, educated man to cope with even the more important parts of knowledge. His concern resembled that of C. P. Snow about the 'two cultures', but was more profound, partly I think because Oppie himself was a creative scientist. He worried about the increasing gap between specialized knowledge and common sense, the increasing gap between neighbouring sciences, and even between different branches within his own science of physics. He said: 'Even in physics we do not entirely succeed in spite of a passion for unity which is very strong.'

This activity has again been well summarized by Pais: 'Briefly, then, what Oppenheimer had in mind was this. First, he addressed himself to what is loosely called the intellectual community. He wished to foster a common understanding primarily within this community. Second, as a example of what in his opinion could profitably be shared, he mentions the lesson of quantum theory which we call complementarity. He wished and in fact tried to explain this lesson to the biologist, the statesman and the artist because he believed that what to the physicist is a technique represents at the same time a general way of thinking that could be liberating to all. Third, he saw a two-fold duty for our education system. In the face of increasing demands on education we should continue to stress that the cultural life of sciences lies almost entirely in the intimate view of the professional. At the same time, "no man should escape our universities without . . . some sense of the fact that not through his fault, but in the nature of things, he is going to be an ignorant man, and so is everyone else".

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'Of the great effort needed to achieve these aims he said the following: "I think that, with the growing wealth of the world, and the possibility that it will not all be used to make new committees, there may indeed be genuine leisure, and that a high commitment on this leisure is that we re-knit the discourse and the understanding between the members of our community. As a start, we must learn again, without contempt and with great patience, to talk to one another; and we must hear."'

As a move toward bridging the gap between various disciplines he invited many psychologists and historians for temporary visits to the Institute. He talked enthusiastically of the progress psychologists were making in understanding the process of learning.

Another subject of great concern to him was atomic power and the politics related to it. He gave many lectures on this, before colleges, general audiences and to young people. He wrote about it in the prestigious journal *Foreign Affairs.* He discussed the decision to drop the atomic bomb, international control of atomic energy, and Secretary Stimson's role in the development of the bomb. His opinion was always moderate; he thought that the development of the bomb and its drop had been inevitable, but that the world should make every effort that the bomb should not be used again. He also wrote about specific subjects, such as the functions of the International Agency on Atomic Energy to which he was much devoted.

Some of his writings are in response to the many honours he received, and the many interviews he was asked to give. Others are personal tributes to other scientists: he was a very good friend who would not forget his friends.

Other writings are predictions of the development of physics in the future, summaries of conferences and of develop

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ments in physics such as 'symmetries of forces', and '30 years of mesons'.

His reputation as a scientist and a symbol was at least as great in Great Britain and France as it was in the United States. He paid frequent visits to both countries, and was much honoured in both.

Again I would like to quote Pais: 'Any single one of the following contributions would have marked Oppenheimer out as a pre-eminent scientist: his own research work in physics; his influence as a teacher; his leadership at Los Alamos; the growth of the Institute for Advanced Study to a leading centre of theoretical physics under his directorship; and his efforts to promote a more common understanding of science. When all is combined, we honour Oppenheimer as a great leader of science in our time. When all is interwoven with the dramatic events that centred around him, we remember Oppenheimer as one of the most remarkable personalities of this century.'

Oppenheimer will be remembered by the world and by his country. He will leave a lasting memory in all the scientists who have worked with him, and in the many who have passed through his school and whose taste in physics was formed by him. His was a truly brilliant mind, best described by his long-time associate Charles Lauritsen: 'This man was unbelievable. He always gave you the answer before you had time to formulate the question.'

The photograph of Oppenheimer was taken by Ulli Steltzer.

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Linus Pauling

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# LINUS CARL PAULING

*February 28, 1901-August 19, 1994*

BY JACK D. DUNITZ

LINUS CARL PAULING was born in Portland, Oregon, on February 28, 1901, and died at his ranch at Big Sur, California, on August 19, 1994. In 1922 he married Ava Helen Miller (died 1981), who bore him four children: Linus Carl, Peter Jeffress, Linda Helen (Kamb), and Edward Crellin.

Pauling is widely considered the greatest chemist of this century. Most scientists create a niche for themselves, an area where they feel secure, but Pauling had an enormously wide range of scientific interests: quantum mechanics, crystallography, mineralogy, structural chemistry, anesthesia, immunology, medicine, evolution. In all these fields and especially in the border regions between them, he saw where the problems lay, and, backed by his speedy assimilation of the essential facts and by his prodigious memory, he made distinctive and decisive contributions. He is best known, perhaps, for his insights into chemical bonding, for the discovery of the principal elements of protein secondary structure, the alpha-helix and the beta-sheet, and for the first identification of a molecular disease (sickle-cell anemia), but there are a multitude of other important contri

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butions. Pauling was one of the founders of molecular biology in the true sense of the term. For these achievements he was awarded the 1954 Nobel Prize in chemistry. But Pauling was famous not only in the world of science. In the second half of his life he devoted his time and energy mainly to questions of health and the necessity to eliminate the possibility of war in the nuclear age. His active opposition to nuclear testing brought him political persecution in his own country, but he was finally influential in bringing about the 1963 international treaty banning atmospheric tests. With the award of the 1962 Nobel Peace Prize, Pauling became the first person to win two unshared Nobel Prizes (Marie Curie won one and shared another with her husband). Pauling's name is probably best known among the general public through his advocacy, backed by personal example, of large doses of ascorbic acid (vitamin C) as a dietary supplement to promote general health and prevent (or at least reduce the severity of) such ailments as the common cold and cancer. Indeed, Albert Einstein and Linus Pauling are probably the only scientists in our century whose names are known to every radio listener, television viewer, or newspaper reader.

## EARLY YEARS

Pauling was the first child of Herman Pauling, son of German immigrants, and Lucy Isabelle (Darling) Pauling, descended from pre-revolutionary Irish stock. There were two younger daughters: Pauline Darling (born 1902) and Lucile (born 1904). Herman Pauling worked for a time as a traveling salesman for a medical supply company and moved in 1905 to Condon, Oregon, where he opened his own drugstore. It was in this new boom town in the arid country east of the coastal range that Pauling had his first schooling. He learned to read early and started to devour books. In 1910

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the family moved back to Portland, where his father wrote a letter to *The Oregonian,* a local newspaper, asking for advice about suitable reading matter for his nine-year-old son, who had already read the Bible and Darwin's theory of evolution. We do not know the replies, but Pauling later confessed that one of his favorites was the *Encyclopaedia Britannica.* Soon tragedy struck. In June of that year Herman Pauling died after a sudden illness, probably a perforated stomach ulcer with attendant peritonitis, leaving his family in a situation with which the young mother could not adequately cope.

Linus did well at school. He collected insects and minerals and read omnivorously. He made up his mind to become a chemist in 1914, when a fellow student, Lloyd A. Jeffress, showed him some chemical experiments he had set up at home. With the reluctant approval of his mother he left school in 1917 without a diploma and entered Oregon Agricultural College at Corvallis as a chemical engineering major, but after two years his mother wanted him to leave college to earn money for the support of the family. He must have impressed his teachers, for in 1919, after a summer working as a road-paving inspector for the State of Oregon, he was offered a full-time post as instructor in qualitative analysis in the chemistry department. The eighteen-year-old teacher felt the need to read current chemical journals and came across the recently published papers of Gilbert Newton Lewis and Irving Langmuir on the electronic structure of molecules. Having understood the new ideas, the “boy professor" introduced them to his elders by giving a seminar on the nature of the chemical bond. Thus was sparked the "strong desire to understand the physical and chemical properties of substances in relation to the structure of the atoms and molecules of which they are

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composed," which determined the course of Pauling's long life.

The following year Pauling resumed his student status and graduated in 1922 with a B.Sc. degree. In his final year he was given another opportunity to teach, this time an introductory chemistry course for young women students of home economics. This new teaching episode also had important consequences for his future. One of the students was Ava Helen Miller, who became his wife in a marriage that lasted almost sixty years.

## PASADENA

Pauling came to the California Institute of Technology as a graduate student in 1922 and remained there for more than forty years. He chose Caltech because he could obtain a doctorate there in three years (Harvard required six) and because Arthur Amos Noyes offered him a modest stipend as part-time instructor. It was a fortunate choice both for Pauling and for Caltech. As he wrote towards the end of his life, "Years later ... I realized that there was no place in the world in 1922 that would have prepared me in a better way for my career as a scientist" (1994). When he arrived the newly established institute consisted largely of the hopes of its three founders, the astronomer George Ellery Hale, the physicist Robert A. Millikan, and the physical chemist Arthur Amos Noyes. There were three buildings and eighteen faculty members. When he left, Caltech had developed into one of the major centers of scientific research in the world. In chemistry Pauling was the prime mover in this development. Indeed, for many young chemists of my generation, Caltech meant Pauling.

Pauling's doctoral work was on the determination of crystal structures by X-ray diffraction analysis under the direction of Roscoe Gilkey Dickinson (1894-1945), who had obtained

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his Ph.D. only two years earlier (he was the first person to receive a Ph.D. from Caltech). By a happy chance, Ralph W. G. Wyckoff (1897-1994), one of the pioneers of X-ray analysis, had spent the year before Pauling's arrival at Caltech and had taught Dickinson the method of using Laue photographic data (white radiation, stationary crystal; a method that fell into disuse but has newly been revived in connection with rapid data collection with synchrotron radiation sources). Wyckoff taught Dickinson, and Dickinson taught Pauling, who soon succeeded in determining the crystal structures of the mineral molybdenite $MoS_2$ (Dickinson and Pauling, 1923) and the intermetallic compound MgSn (1923). By the time he graduated in 1925 he had published twelve papers, most on inorganic crystal structures, but including one with Peter Debye (1884-1966) on dilute ionic solutions (Debye and Pauling, 1925) and one with Richard Tolman (1881-1948) on the entropy of supercooled liquids at 0 K (Pauling and Tolman, 1925). Pauling had already made up for his lack of formal training in physics and mathematics. He was familiar with the quantum theory of Planck and Bohr and was ready for the conceptual revolution that was soon to take place in Europe. Noyes obtained one of the newly established Guggenheim fellowships for the rising star and sent him and his young wife off to the Institute of Theoretical Physics, directed by Arnold Sommerfeld (1868-1951), in Munich.

They arrived in April 1926, just as the Bohr-Sommerfeld model was being displaced by the "new" quantum mechanics. It was an exciting time, and Pauling knew he was lucky to be there at one of the centers. He concentrated on learning as much as he could about the new theoretical physics at Sommerfeld's institute. Pauling had been regarded, and probably also regarded himself, as intellectually outstanding among his fellow students at Oregon and even at Caltech;

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however, he must have become aware of his limitations during his stay in Europe. The new theories were being made by men of his own generation. Wolfgang Pauli (1900-58), Werner Heisenberg (1901-76), and Paul Dirac (1902-84) were all born within a year of Pauling and were more than a match for him in physical insight, mathematical ability, and philosophical depth. Pauling was not an outstanding theoretical physicist and was probably not particularly interested in problems such as the deep interpretation of quantum mechanics or the philosophical implications of the uncertainty principle. On the other hand, he was the only chemist at Sommerfeld's institute and saw at once that the new physics was destined to provide the theoretical basis for understanding the structure and behavior of molecules.

The year in Europe was to have a decisive influence on Pauling's scientific development. In addition to Munich, he visited Copenhagen in the spring of 1927 and then spent the summer in Zurich. In Copenhagen it was not Bohr but Samuel A. Goudsmit (1902-78) who influenced Pauling (they later collaborated in writing *The Structure of Line Spectra,* New York: McGraw-Hill, 1930), and in Zurich it was neither Debye nor Schrödinger but the two young assistants, Walter Heitler (1904-81) and Fritz London (1900-54), who were working on their quantum-mechanical model of the hydrogen molecule in which the two electrons are imagined to "exchange" their roles in the wave function—an example of the "resonance" concept that Pauling was soon to exploit so successfully.

One immediate result of the stay in Munich was Pauling's (1927) first paper in the *Proceedings of the Royal Society of London,* submitted by Sommerfeld himself. Pauling was eager to apply the new wave mechanics to calculate properties of many-electron atoms and he found a way of doing

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this by using hydrogen-like single-electron wave functions for the outer electrons with effective nuclear charges based on empirical screening constants for the inner electrons.

## THE NATURE OF THE CHEMICAL BOND

In 1927 Pauling returned to Caltech as assistant professor of theoretical chemistry. The next twelve years produced the remarkable series of papers that established his worldwide reputation. His abilities were quickly recognized through promotions (to associate professor, 1929; full professor, 1931), through awards (Langmuir Prize, 1931), through election to the National Academy of Sciences (1933), and through visiting lectureships, especially the Baker lectureship at Cornell in 1937-38. Through his writings and lectures, Pauling established himself as the founder and master of what might be called structural chemistry—a new way of looking at molecules and crystals.

Pauling's way was first to establish a solid and extensive collection of data. By means of X-ray crystallography, gasphase electron diffraction (installed after Pauling's 1930 visit to Europe, where he learned about Hermann Mark's pioneering studies), and infrared, Raman, and ultraviolet spectroscopy, interatomic distances and angles were established for hundreds of crystals and molecules. Thermochemical information was already available. The first task of theory, as Pauling saw it, was to provide a basis to explain the known metric and energetic facts about molecules, and only then to lead to prediction of new facts. At this stage of his development Pauling was attracting many talented co-workers, undergraduates, graduate students, and postdoctoral fellows, and their names read like a Who's Who in the structural chemistry of the period: J. H. Sturdivant, J. L. Hoard, J. Sherman, L. O. Brockway, D. M. Yost, G. W. Wheland, M. L. Huggins, L. E. Sutton, E. B. Wilson, S. H. Bauer, C. D.

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Coryell, V. Schomaker, and others. Here are the major achievements.

Pauling's ionic radii: Once the structures of simple inorganic crystals began to be established it was soon seen that the observed interatomic distances were consistent with approximate additivity of characteristic radii associated with the various cations and anions. Among the several sets that have been proposed, Pauling's are not merely designed to reproduce the observations but, typical for him, are derived from a mixture of approximate quantum mechanics (using screening constants) and experimental data. His values, derived almost seventy years ago, are still in common use, and the same can be said for the sets of covalent radii and nonbonded (van de Waals) radii that he introduced.

Pauling's rules: Whereas simple ionic substances, such as the alkali halides, are limited in the types of crystal structure they can adopt, the possibilities open to more complex substances, such as mica, $KA1_3Si_3O_{10}(OH)_2$, may appear to be immense. Pauling (1929) formulated a set of rules about the stability of such structures, which proved enormously successful in testing the correctness of proposed structures and in predicting unknown ones. As Pauling himself remarked, these rules are neither rigorous in their derivation nor universal in their application; they were obtained in part by induction from known structures and in part from theoretical considerations. His second rule states essentially that electrostatic lines of force stretch only between nearest neighbors. In the meantime, as structural knowledge has accumulated, this rule has been modified by various authors to relate bond strengths to interatomic distances, but it seems fair to say that it is still the basis for the systematic description of inorganic structures. W. L. Bragg, who may

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have felt somewhat beaten to the post by the publication of these rules, wrote (1937): "The rule (the second one) appears simple, but it is surprising what rigorous conditions it imposes upon the geometrical configuration of a silicate... To sum up, these rules are the basis for the stereochemistry of minerals."

Quantum chemistry: In 1927 Ø. Burrau solved the Schrödinger equation for the hydrogen molecule ion H2+ in elliptic coordinates and obtained values for the interatomic distance and bonding energy in good agreement with experiment. Burrau's wave function fails, however, to yield much physical insight into the stability of the system. Soon afterwards, Pauling (1928) pointed out that although an approximate perturbation treatment would not provide any new information, it would be useful to know how well it performed: "For perturbation methods can be applied to many systems for which the wave equation cannot be accurately solved .... ." Pauling first showed that the classical interaction of a ground state hydrogen atom and a proton is repulsive at all distances. However, if the electron is not localized on one of the atoms, and the wave function is taken as a linear combination of the two ground state atomic wave functions, then the interaction energy has a pronounced minimum at a distance of about 2 a.u. This was the first example of what has come to be known as the method of Linear Combination of Atomic Orbitals (LCAO). For the hydrogen-molecule ion, the LCAO dissociation energy is only about 60% of the correct value, but the model provides insight into the source of the bonding and can easily be extended to more complex systems. In fact, the LCAO method is the basis of modern molecular orbital theory.

A few months earlier Heitler and London had published their calculation for the hydrogen molecule. This was too

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complicated for an exact solution, and their method also rested on a perturbation model, a combination of atomic wave functions in which the two electrons, with opposite spins, change places. More generally, the energy of the electron-pair bond could now be attributed to "the resonance energy corresponding to the interchange of the two electrons between the two atomic orbitals." As developed by Pauling and independently by John C. Slater (1900-76), the Heitler-London-Slater-Pauling (HLSP) or Valence Bond model associates each conventional covalent bond with an electron pair in a localized orbital and then considers all ways in which these electrons can "exchange."

Much has been made of Pauling's preference for Valence Bond (VB) theory over Molecular Orbital (MO) theory. The latter, as developed by Fritz Hund (born 1896), Erich Hückel (1896-1980), and Robert S. Mulliken (1896-1986), works in terms of orbitals extended over the entire molecule, orders these orbitals according to their estimated energies, and assigns two electrons with opposite spin to each of the bonding orbitals. Electronic excited states correspond to promotion of one or more electrons from bonding to antibonding orbitals. Nowadays, MO theory has proved itself more amenable to computer calculations for multicenter molecules, but in the early days, when only hand calculations were possible, it was largely a matter of taste. The main appeal of the MO model was then to spectroscopists. Chemists, in general, were less comfortable with the idea of pouring electrons into a ready-made framework of nuclei. It was more appealing to build molecules up from individual atoms linked by electron-pair bonds. The VB picture was more easily related to the chemist's conventional structural formulas. Both models are, of course, drastic simplifications, and it was soon recognized that when appropriate correction terms are added and the proper transformations

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are made they become equivalent. In particular, the MO method in its simplest form ignores electron-electron interactions, while the VB method overestimates them.

Pauling was fully acquainted with early MO theory—there is at least one important paper (Wheland and Pauling, 1935) on the theory of aromatic substitution. But he clearly preferred his own simplified versions of VB theory and soon became a master of combining them with the empirical facts of chemistry. A remarkable series of papers entitled "The Nature of the Chemical Bond" formed the basis for his later book with the same title. In the very first paper Pauling (1931) set out his program of developing simple quantum mechanical treatments to provide information about "the relative strengths of bonds formed by different atoms, the angles between bonds, free rotation, or lack of free rotation about bond axes, the relation between the quantum numbers of bonding electrons and the number and spatial arrangements of bonds, and so on. A complete theory of the magnetic moments of molecules and complex ions is also developed, and it is shown that for many compounds involving elements of the transition group this theory together with the rules of electron pair bonds leads to a unique assignment of electron structures as well as a definite determination of the type of bonds involved." To a large extent Pauling developed his own language to describe his new concepts, and of the many new terms introduced, three seem indelibly associated with his name: hybridization, resonance, and eletronegativity.

Only the first of these truly originates from him. In the first paper of the series Pauling took up the idea of spatially directed bonds. By a generalization of the Heitler-London model for hydrogen, a normal chemical bond can be associated with the spin pairing of two electrons, one from each of the two atoms. While an *s* orbital is spherically symmetri

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cal, other atomic orbitals have characteristic shapes and angular distributions. It was not difficult to explain the angular structure of the water molecule $H_2O$ and the pyramidal structure of ammonia $H_3N$. But the quadrivalency of carbon was a problem. From its ground state ($1s^2 2s^2 2p^2$) carbon ought to be divalent; from the excited state *($1s^2 2s^1 2p^3$)* one might expect three mutually perpendicular bonds and a fourth weaker bond (using the *s* orbital) in some direction or other. As a chemist Pauling knew that there must be a way of combining the *s* and p functions to obtain four equivalent orbitals directed to the vertices of a tetrahedron. Atomic orbitals can be expressed as products of a radial and an angular part. Pauling solved the problem by simply ignoring the former. The desired tetrahedral orbitals are then easily obtained as linear combinations of the angular functions. Pauling called these hybrid orbitals and described the procedure as hybridization. Other combinations yield three orbitals at 120° angles in a plane (trigonal hybrids) or two at 180° (digonal hybrids). With the inclusion of *d* orbitals other combinations become possible. In his later years Pauling stated that he considered the hybridization concept to be his most important contribution to chemistry (Kauffman and Kauffman, 1996).

Resonance: In attempting to explain the quantum-mechanical exchange phenomenon responsible for the stability of the chemical bond, Heitler and London had used a classical analogy originally due to Heisenberg. In quantum mechanics a frequency $v = E/h$ can be associated with every system with energy *E.* Two noninteracting hydrogen atoms are thus comparable to two classical systems both vibrating with the same frequency v, for example, two pendulums. Interaction between the two atoms is analogous to coupling between the pendulums, known as resonance. When coupled

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the two pendulums no longer vibrate with the same frequency as before but make a joint vibration with frequencies v + Av and *v* -Av, where Av depends on the coupling. Going back to quantum mechanics, it is as if the system now has two different energies, one higher and one lower than before. Heitler and London interpreted the combination frequency Av as the frequency of exchange of spin directions.

Pauling first used the term resonance more or less as a synonym for electron exchange, in the Heitler-London sense, but he went on to think of the actual molecule as “resonating" between two or more valence-bond structures, and hence lowering its energy below the most stable of these. Thus, by resonating between two Kekule structures the benzene molecule is more stable than these extremes, and the additional stability can be attributed to "resonance energy." Through his resonance concept Pauling reconciled the chemist's structural formulas with simplified quantum mechanics, thereby extending the realm of applicability of these formulas, and he proceeded to reinterpret large areas of chemistry with it.

In the mid-years of the century resonance theory was taken up with enthusiasm by teachers and students; it seemed to be the key to understanding chemistry. Since then, its appeal has declined. It has now a slightly old-fashioned connotation. Certainly, it had some failures. Resonance theory would lead one to expect that cyclobutadiene should be more stable as a symmetric square structure than as a rectangular one with alternating long and short bonds, whereas the contrary is true. (It seems ironic that in the 1935 classic *Introduction to Quantum Mechanics* by Pauling and E. Bright Wilson, Jr., qualitative MO theory was applied to only one example, four atoms in a square. In contrast to the Valence Bond method, which gave a typical "resonance energy" to

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this system, the MO model gave none. Of course, cyclobutadiene was then still only a synthetic chemist's dream.) Similarly, it does not explain the stability of the cyclopentadienyl anion compared with the corresponding cation; in these and other cases simple molecular orbital theory provided immediate and correct answers. In the index of a modern textbook on physical chemistry "resonance" is likely to appear only in an entry such as "resonance, nuclear magnetic." It does not fare much better in textbooks on inorganic and organic chemistry; a few pages on resonance formalism are usually followed by a more extensive account of simple molecular orbital theory.

Electronegativity, the third concept associated with Pauling's name, is still going strong. It emerged from his concept of partially ionic bonds. The energy of a bond can be considered as the sum of two contributions-a covalent part and an ionic part. The thermochemical energy of a bond D(A-B) between atoms A and B is, in general, greater than the arithmetic mean of the energies D(A—A) and D (B—B) of the homonuclear molecules. Pauling attributed the extra energy A(A —B) to ionic resonance and found he could assign values XA, etc., to the elements such that A(A—B) is approximately proportional to $(XA - XB)^2$. The x values form a scale, the electronegativity scale, in which fluorine with x = 4 is the most electronegative element, cesium with x = 0.7 the least. Apart from providing a basis for estimating bond energies of heteropolar bonds, these x values can also be used to estimate the dipole moment and ionic character of bonds. Other electronegativity scales have been proposed by several authors, but Pauling's is still the most widely used—it is the easiest to remember. According to Pauling, electronegativity is the power of an atom *in a molecule* to attract electrons to itself. It therefore differs from the elec

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tron affinity of the free atom although the two run roughly parallel. Many other interpretations have been proposed.

These and many other topics were collected and summarized in the book based on Pauling's Baker lectures, *The Nature of the Chemical Bond,* probably the most influential book on chemistry this century. In my opinion the 1940 second edition is the best; the 1939 edition was short-lived, and the 1960 edition, although it contains much more material, did not evoke the same feeling of illumination as the earlier ones.

Like so many others, I first encountered Pauling through this book, which I discovered sometime in my second year as an undergraduate at Glasgow University. It came as a revelation. Setting out to offer an introduction to modern structural chemistry, it explained how the structures and energies of molecules could be discussed in terms of a few simple principles. The essential first step in understanding chemical phenomena was to establish the atomic arrangements in the substances of interest. To try to understand chemical reactivity without this information or with dubious structural information was a waste of time. This was just what I needed to help me make up my mind that my future was to be in structural chemistry.

## PAULING AND MOLECULAR BIOLOGY

*The Nature of the Chemical Bond* marks perhaps the culmination of Pauling's contributions to chemical bonding theory. There were achievements to follow, notably an important paper (1947) on the structure of metals, but the interest in chemical bonding was being modified into an interest into the structure and function of biological molecules. There are intimations of this in the chapter on hydrogen bonds. Pauling was one of the first to spell out its importance for biomolecules:

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> Because of its small bond energy and the small activation energy involved in its formation and rupture, the hydrogen bond is especially suited to play a part in reactions occurring at normal temperatures. It has been recognized that hydrogen bonds restrain protein molecules to their native configurations, and I believe that as the methods of structural chemistry are further applied to physiological problems it will be found that the significance of the hydrogen bond for physiology is greater than that of any other single structural feature.

Like many of his comments it seems so obvious, almost a truism, but it was not obvious then. Essentially the same idea had been expressed in Mirsky and Pauling (1936), but hydrogen bonds are not even mentioned, for example, in Bernal's (1939) article on the structure of proteins.

Two remarkable observations from 1948 deserve to be mentioned here. One is a forerunner of the 1953 Watson-Crick DNA double-helix structure and explains what had not yet been discovered (1948,1;1976):

> The detailed mechanism by means of which a gene or a virus molecule produces replicas of itself is not yet known. In general the use of a gene or a virus as a template would lead to the formation of a molecule not with identical structure but with complementary structure .... If the structure that serves as a template (the gene or virus molecule) consists of, say, two parts, which are themselves complementary in structure, then each of these parts can serve as the mold for the production of a replica of the other part, and the complex of two complementary parts thus can serve as the mold for the production of duplicates of itself.

And in the same vein, although nothing whatsoever was known about the structure of enzymes, the other (1948,2) announced what became clear to biochemists in general only many years later:

> I think that enzymes are molecules that are complementary in structure to the activated complexes of the reactions that they catalyse, that is, to the molecular configuration that is intermediate between the reacting substances and the products of reaction for these catalysed processes. The attraction of the enzyme molecule for the activated complex would thus lead to a

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decrease in its energy, and hence to a decrease in the energy of activation of the reaction, and to an increase in the rate of the reaction.

The message seems to have laid in oblivion until well after "transition-state binding" had become popular; it is not mentioned, for example, in Jencks's classic work (1969) on enzyme catalysis.

Both of these prescient statements depend on the concept of complementarity, which arose out of Pauling's early work on proteins and antibodies. This started because, in the search for funding during the depression, Pauling obtained a grant from Warren Weaver, director of the Rockefeller Foundation Natural Science Division, but only for research in life sciences. With his knowledge of inorganic structural chemistry, hemoglobin was the first target, and within a few months he solved an important problem. By magnetic susceptibility measurements it was shown that, whereas hemoglobin contains four unpaired electrons per heme and the oxygen molecule contains two, oxyhemoglobin (and also carbonmonoxyhemoglobin) contains none (Pauling and Coryell, 1936). This result showed that in oxygenated blood, the 02 molecule is attached to the iron atom of hemoglobin by a covalent bond-that it was not just a matter of oxygen being somehow dissolved in the protein. Magnetic susceptibility measurements could also yield equilibrium constants and rates for many reactions involving addition of molecules and ions to ferro- and ferrihemoglobin. It is interesting that Pauling had introduced the magnetic susceptibility technique at Caltech in connection with the prediction and identification of the superoxide radical anion, a molecule whose biological significance was recognized only many years later (1979).

In 1936 Alfred E. Mirsky (1900-74) and Pauling published a paper on protein denaturation, which was known to be a

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two-stage process, one under mild conditions partially reversible, the other irreversible. Pauling associated the first stage with the breaking and reformation of hydrogen bonds, the second with the breaking of covalent bonds. The native protein was pictured as follows: "The molecule consists of one polypeptide chain which continues without interruption throughout the molecule (or, in certain cases, of two or more such chains); this chain is folded into a uniquely defined configuration in which it is held by hydrogen bonds

.... The importance of the hydrogen bond in protein structure can hardly be overemphasized." Loss of the native conformation destroys the characteristic properties of the protein. From the entropy difference between the native and denatured forms of trypsin, about 1020 conformations were estimated to be accessible to the denatured protein molecule. On heating, or if the pH of the solution was near the isoelectric point of the protein, unfolded segments of acidic or basic side-chains would get entangled with one another, fastening molecules together, and ultimately leading to the formation of a coagulum. This was perhaps the first modern theory of native and denatured proteins.

Complementariness enters the picture in 1940, when Max Delbrück (1906-81) and Pauling published their refutation of a proposal of Pascal Jordan, according to which a quantum-mechanical stabilizing interaction between identical or nearly identical molecules might influence biological molecular synthesis in such a way as to favor the formation of molecular replicas in the living cell. After dismissing this proposal the authors went on to say that complementariness, not identity, should be given primary consideration. They continued:

> The case might occur in which the two complementary structures happened to be identical; however, in this case also the stability of the com

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> plex of two molecules would be due to their complementariness rather than their identity. When speculating about possible mechanisms of autocatalysis it would therefore seem to be most rational from the point of view of the structural chemist to analyze the conditions under which complementariness and identity might coincide.

The use of the word "complementariness" instead of the more usual "complementarity" is striking. According to Delbrück, his only role in the publication, apart from suggesting a few minor changes, was to have drawn Pauling's attention to Jordan's proposal, and it seems quite likely that "complementariness" was one of these minor changes, introduced in order to avoid the epistemological connotations that Delbrück associated with "complementarity" in Bohr's sense.

By this time Pauling was thinking about antibodies. In 1936 he had met Karl Landsteiner (1868-1943), discoverer of the human blood groups and instrumental in establishing immunology as a branch of science. According to Pauling (1976), Landsteiner asked him how he would explain the specificity of interaction of antibodies and antigens, to which he replied that he could not. The question set Pauling thinking about the problem, and it was not long before he had a theory (1940) that guided his research on antibodies for years to come. Eventually, it turned out to be wrong, or at least only half right.

The correct part was that the specificity of antibodies for a particular antigen is based on complementarity: "Atoms and groups which form the surface of the antigen attract certain complementary parts of the globulin chain and repel other parts." The wrong part was his assumption "that all antibody molecules contain the same polypeptide chains as normal globulin and differ from normal globulin only in the configuration of the chain." Pauling was clearly not too happy about this assumption, which he adopted only be

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cause of his inability "to formulate a reasonable mechanism whereby the order of amino-acid residues would be determined by the antigen." He could not know then about the genetic basis of amino-acid sequence. So he was right about how antibodies work and wrong about how they are produced. It was still a long time before a better theory emerged, based not on instruction but on selection, and involving hypervariable regions of the amino-acid chain and shuffling genes. In retrospect then it is not surprising that Pauling's immunochemistry program, carried out mainly by his Caltech collaborator Dan Campbell, never achieved the successes he had hoped for. During World War II there was a brief flurry of excitement when they claimed to have made "artificial antibodies" from normal globulins, but the claim proved to be ill founded and was soon retracted.

In 1941 Pauling's intense work schedule was temporarily stemmed when he was diagnosed as having Bright's disease, regarded then by many doctors as incurable. Under the treatment of Dr. Thomas Addis, he slowly recovered. Addis, a controversial figure, put Pauling on a low-protein, salt-free diet, which was effective in healing the damaged kidneys. After about six months Pauling was more or less back to normal, but he kept to Addis's diet for many years afterwards. Pearl Harbor brought further distractions when Pauling's energies were diverted to war work, mainly on rocket propellants and in the search for artificial antibodies. Earlier he had used the paramagnetism of oxygen to design and develop an oxygen meter for use in submarines.

By the end of the war Pauling felt well enough to travel abroad again. In late 1947 he came as Eastman visiting professor with his family to England, where he gave lectures to packed audiences in Oxford and elsewhere, received medals, and suffered from the climate. In 1948, confined to bed with a cold, he began thinking again about a problem

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that had briefly occupied him a decade earlier—the structure of a-keratin. By this time, thanks to the X-ray crystallographic work of Robert B. Corey and his associates, the detailed structures of several amino acids and simple peptides were known, and although the interatomic distances and angles did not differ much from the values derived earlier by resonance arguments, Pauling could now take them as facts rather than suppositions—especially the planarity of the amide group. With the help of paper models he then set himself the problem of taking a polypeptide chain, rotating round the two single bonds but keeping the peptide groups planar, repeating with the same rotation angles from one peptide group to the next, and searching for a helical structure in which each N-H group makes a hydrogen bond with the carbonyl oxygen of another residue. He found two such structures, one of which also fulfilled the condition of tight packing down the central hole. The structure in question repeated after 18 residues in 5 turns at a distance of 27 Å hence 5.4 Å per turn, whereas X-ray photographs of a-keratin seemed to show that the repeat distance was 5.1 Å. The discrepancy could not be removed by minor adjustments to the model and was large enough for Pauling to put the problem aside (1996).

It was taken up again after his return to Pasadena, with the help of Corey and a young visiting professor, Herman Branson, who checked details of the model and searched for alternatives, but without coming up with anything really new. Then came a paper from the Cavendish Laboratory by Bragg, Kendrew, and Perutz (1950), who described several possible helical structures for a-keratin, all unacceptable in Pauling's view because they allowed rotation about the C—N bond of the amide group. This paper provoked Pauling to publish his ideas in a series of papers that described the now famous a-helix (essentially the one modeled in Oxford

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with 3.7 residues per turn), the so-called y-helix (disfavored on energetic grounds), and the parallel and anti-parallel pleated sheets with extended polypeptide chains (Pauling and Corey, 1950;1951,1,2. Pauling, Corey, and Branson, 1951). By this time X-ray photographs of synthetic polypeptides had clarified the apparent discrepancy concerning the repeat distance along the helix; it was 5.4 Å after all. Max Perutz has vividly described his consternation on first reading Pauling's proposed structure and how he managed to corroborate it by observing the 1.5 Å reflection corresponding to the step distance along the a-helix, which everyone had missed until then (Perutz, 1987).

Very soon evidence began to accumulate that the a-helix is indeed one of the main structural features and that the two pleated sheet structures are also important elements of the secondary structure of globular proteins. Just as a few rules concerning the regular repetition of simple structural units had sufficed twenty years earlier to successfully predict the structures of minerals, now a few simple principles derived from structural chemistry were enough to predict the main structural features of proteins.

Pauling's next essay in model building was not so successful. In the summer of 1952 he learned about the Hershey-Chase experiment proving that genetic information was carried not by protein but by DNA, deoxyribonucleic acid, a polynucleotide. Pauling felt it should be possible to decipher the structure of this substance by model building along lines similar to those in the protein work. The available X-ray diffraction patterns showed a strong reflection at about 3.4 Å, but nothing much else. Having convinced himself that a two-stranded helical structure would yield too low a density, he went on to the assumption of a three-stranded helical structure held together by hydrogen bonds between the phosphate groups of different strands-that is, the struc

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ture rested on the tacit assumption that the phosphodiester groups were protonated! They were closely packed about the axis of the helix with the pentose residues surrounding them and the purine and pyrimidine groups projecting radially outward. When this structure was presented at a seminar, Verner Schomaker is credited with the remark, “If that were the structure of DNA, it would explode!" Nevertheless, the structure was published (Pauling and Corey, 1953), a pre-publication copy having been sent to Cambridge, where it stimulated Watson and Crick into their final spurt, culminating in their base-paired structure, which was immediately acclaimed as correct by everyone who saw it —including Pauling. The Watson-Crick structure conformed to the self-complementarity principle that Pauling had enunciated many years earlier and then apparently forgotten.

Much has been written about this spectacular failure. Why was his model-building approach so successful with the polypeptides and so unsuccessful (in his hands) with DNA? First was the time factor. Pauling had thought about polypeptide structures for more than a decade before he risked publishing his conclusions; he thought only for a few months about DNA.

Secondly, the available information: for the polypeptide problem, precise metrical and stereochemical data for amino acids and simple peptides, mostly from Pauling's own laboratory, were at hand; for DNA almost nothing was known about the detailed structures of the monomers or oligomers. The X-ray photographs available to Pauling were obtained from degraded DNA specimens and were essentially noninformative (they were later recognized to be derived from mixtures of the A and B forms of DNA), and he made a bad mistake in neglecting the high water content of the DNA specimens in his density calculations.

Yet Watson and Crick succeeded with Pauling's methods

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where Pauling failed. There is no doubt in my mind that *if* Pauling had had access to Rosalind Franklin's X-ray photographs, he would immediately have drawn the same conclusion as Crick did, namely, that the molecule possesses a twofold axis of symmetry, thus pointing to two chains running in opposite directions and definitely excluding a three chain structure. Then there were Chargaff's data about base ratios; Pauling later admitted that he had known about these but had forgotten. It seems clear that Pauling was in a hurry to publish, although, according to Peter Pauling's entertaining account twenty years later (P. Pauling, 1973), he never felt he was in any sense "in a race." Finally, as described in the next section, he was by this time under severe harassment from the FBI and other agencies for his political views and activities. This must have taken up much of his mental and emotional energies during these months.

Pauling's standing as a founder of molecular biology rests partly on his identification of sickle-cell anemia, a hereditary disease, as a molecular disease—the first to be recognized as such. The red blood cells in the venous systems of sufferers adopt sickle shapes which tend to block small blood vessels, causing distressing symptoms, whereas the cells in the more oxygenated arterial blood have the normal flattened disc shape. When, towards the end of the war, Pauling heard about this it occurred to him that it could be due to the presence of hemoglobin molecules with a different amino-acid sequence from normal. The abnormal molecules, but not the normal ones, could contain self-complementary patches such as to lead to end-to-end aggregation into long rods that twist the blood cells out of shape. Oxygenation could cause a conformational change to block these sticky patches. It took several years to confirm the essential correctness of what was no more than an intuitive guess. In the preliminary studies attempts to identify any difference be

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tween the hemoglobins of normal and sickle-cell blood were unsuccessful, but with the advent of electrophoresis it could be shown that molecules of sickle-cell and normal hemoglobin moved at different rates in the electric field; the two molecules have different isoelectric points and must indeed be different (Pauling, Itano, Singer, and Wells, 1949). When, much later, it became possible to determine the amino-acid sequence in a protein, sickle-cell hemoglobin was found to contain valine instead of glutamic acid at position 6 of the two P chains. A single change in a single gene is responsible for the disease.

A decade later the further study of mutations in hemoglobin led to yet another fundamental contribution to molecular biology—the concept of the "molecular clock" in evolution (Zuckerkandl and Pauling, 1962). By this time, amino-acid sequencing of proteins had become standard. Hemoglobins obtained from humans, gorillas, horses, and other animals were analyzed. From paleontological evidence the common ancestor of man and horse lived somewhere around 130 million years ago. The a-chains of horse and human hemoglobin contain about 150 amino acids and differ by about 18 amino-acid substitutions, that is, about 9 evolutionary effective mutations for each of the chains, or about one per 14 million years. On this basis the differences between gorilla and human hemoglobin (two substitutions in the *a*- and one in the P-chain) suggest a relatively recent divergence between the species, on the order of only 10 million years. On the other hand, differences between the hemoglobin *a*- and 5-chains of several animals suggest divergence from a common chain ancestor about 600 million years ago, in the pre-Cambrian, before the apparent onset of vertebrate evolution. From this work it became clear that comparison of protein sequences (now replaced by comparison of DNA sequences) is a powerful source of

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information about the origin of species. Evolution of organisms is bound with the evolution of molecules.

## POLITICAL ACTIVISM

By 1954, when Pauling was awarded the Nobel Prize in chemistry for his "research into the nature of the chemical bond and its application to the elucidation of the structure of complex substances," he was famous not only as a scientist; he was also a well known public figure, at least in the United States. Although he was not connected in any way either with the Manhattan Project or the Radiation Laboratory, his wartime research on antibodies and rocket propellants brought him into government advisory agencies such as the Office of Scientific Research and Development (OSRD) under Vannevar Bush and earned him the Presidential Medal for Merit, the highest civilian honor in the United States, awarded by President Truman in 1948. A few years later he was being vilified in the local and national press, being cited for "un-American activities," being denied the possibility to travel outside the United States, and his government research contracts were being terminated. How did this happen?

Almost immediately after August 1945 Pauling became concerned with the implications of the atomic age for international relations and the necessity for controls. His lectures and writings on this subject soon attracted the attention of the FBI and other government agencies. Far from being intimidated by these attentions, he began, with the encouragement of his wife, Ava Helen, to take a more active stance. He signed petitions, joined organizations (such as the Emergency Committee of Atomic Scientists, headed by Albert Einstein, and the American Civil Liberties Union), protested against the loyalty oaths demanded of public em

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ployees, and spoke eloquently against the development of nuclear weapons.

In the McCarthy era and especially during the Korean War this was enough to make him suspect as a security risk. Pauling was invited to lecture at a Royal Society meeting on protein structure to be held in London in May 1952. In February his application for a passport was refused because his proposed travel "would not be in the best interests of the United States." Renewed applications up to the end of April met with renewed refusals. A few hours before the start of the meeting Pauling telegraphed his regrets to London. I was present when the news came that Pauling had not been granted a passport and was therefore unable to attend. It was a grave disappointment, for we had all looked forward to Pauling's presence at the meeting, and there was also a feeling of outrage. The action of the State Department was seen as an insult not only to Pauling and The Royal Society, but to the scientific community at large. Pauling was certainly not the only U.S. citizen whose right to travel was denied by the State Department, but the incident provoked such widespread criticism that it probably helped lead to a reexamination and ultimate change in the State Department's policy. Later that year Pauling was permitted to travel to France and England (where he did not see Rosalind Franklin's X-ray diffraction photographs of DNA!) and the following summer he was again in Europe (where he did see the Watson-Crick DNA structure). This freedom to travel was bought at the cost of temporary, self-imposed political restraint, and was in any case a fragile privilege which he lost again a few months later, when he spoke out in defense of J. Robert Oppenheimer.

In March 1954, following the Bikini Atoll explosion of a "dirty" thermonuclear superbomb, Pauling was in the news again when he began to call attention to the worldwide

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danger of radioactive fallout in the atmosphere. In the summer his renewed application for a passport was again turned down, but in November, when his Nobel Prize was announced, the State Department found itself in a public relations dilemma. The fuss created by Pauling's absence in London in 1952 would be nothing compared with the international outcry that could be imagined if Pauling were refused permission to travel to attend the Nobel Prize ceremony. So Pauling went to Stockholm, where he was a tremendous success, and followed this by visits to Israel, India, Thailand, and Japan. Everywhere—outside his own country—he was welcomed with enthusiasm, not only for his scientific accomplishments but even more for his political stance.

In the United States, too, the public was becoming increasingly concerned about radioactive fallout, not only from American tests but also from ever more powerful Soviet nuclear explosions. Increasing levels of strontium 90 and carbon 14 made newspaper headlines. Pauling claimed that the increased level of radioactive isotopes in the atmosphere was a danger not only to the living but also to future generations. The spokesmen on the Atomic Energy Commission countered that, although radiation might be harmful, it was not harmful in the doses produced by the tests and that Pauling vastly exaggerated the dangers. In fact, all the estimates were tentative at best, but since the Atomic Energy Commission was responsible both for developing nuclear weapons and for monitoring the associated health hazards, its estimates were probably no more objective that those who demanded a stop to the tests. Andrei Sakharov (1990) estimated that every one-megaton test cost about 10,000 human lives.

In January 1958 Pauling, together with his wife, was instrumental in collecting thousands of signatures from scientists all over the world for a petition to end nuclear bomb

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testing, which was presented to Dag Hammarskjold, secretary general of the United Nations. A few months later the Soviet Union called for an immediate halt to nuclear testing, and in October, after more tests by both sides that added markedly to world concern about fallout, talks began in Geneva to discuss details of a possible test ban. During the talks there was an informal moratorium on testing by the Soviet Union, the United States, and the United Kingdom. In the meantime, Pauling's book *No More War!* was published.

In 1960 the Senate Internal Security Subcommittee (SISS) headed by Senator Thomas Dodd issued a subpoena to Pauling to answer questions about Communist infiltration of the campaign against nuclear testing. At Pauling's request the hearings were open and they soon turned into a public relations fiasco for Dodd and the SISS. This was partly because the members of the SISS had not done their homework and partly because it gave Pauling the excuse to lecture them about elementary civic rights and duties: "The circulation of petitions is an important part of our democratic process. If it is abolished or inhibited, it would be a step towards a police state." By this time public opinion was mostly on Pauling's side, but the whole affair must have been experienced by him as an emotional strain—and a tremendous waste of his time and energy.

In 1961 there was a new petition, an "Appeal to Stop the Spread of Nuclear Weapons," again presented to the United Nations, and he also helped to organize the Oslo conference on the dangers raised by the proliferation of nuclear weapons. But in September there was a new spate of Soviet tests of even more powerful bombs—fifty within a couple of months—and in March 1963 President Kennedy announced that the United States would also resume testing. This time the tests did not last long; they were stopped in the sum

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mer, when new proposals were made to forbid atmospheric tests while permitting underground tests. In August both sides signed a treaty to ban all tests in the atmosphere, in outer space, and under the sea. The treaty went into effect on October 10 and the following day Pauling was awarded the Nobel Peace Prize for 1962.

At the present time, especially in the aftermath of the Chernobyl disaster, the cultural climate has changed so much that this short account of atomic politics until 1963 must strike younger readers as almost inconceivable. In the summer of 1996, when France exploded some "nuclear devices" several hundred meters underground below a remote atoll in the South Pacific, there was an international outcry of protest by governments, the press, and the public. Forty years ago, when tons of radioactive material were being spewed into the atmosphere by test after test, there was no such outcry, at least not in the United States and the Soviet Union, the two countries most responsible for the pollution. One can assume that the majority of people believed the tests were necessary. Small groups of people organized protest marches, but there were no social structures in these nuclear states to resist the continuation of testing and the spread of atomic weapons. Pauling was one of the few who consistently spoke against the dangers of atmospheric testing, against the spread of nuclear weapons, for efficient control of such weapons, and for a more rational approach to solve international conflicts. These sentiments found a ready ear in the non-nuclear countries, and eventually public opinion in the United States also swung in his direction. Whether he had any effect in the Soviet Union is another matter; he is not mentioned in Sakharov's (1990) autobiography.

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## APOSTLE OF VITAMIN C

A few days after the news of the Nobel Peace Prize Pauling announced that he was leaving Caltech to become a member of the Center for the Study of Democratic Institutions in Santa Barbara. He was disappointed with the lukewarm reaction of the administration and some of his colleagues. Perhaps he had intended to move anyway. In the mid-1950s he had become interested in phenylketonuria (mental deficiency due to inability to metabolize phenylalanine) as a further example of a molecular disease arising from the lack of a specific enzyme. At about this time he was also developing his theory that xenon acts as anesthetic because it forms crystalline polyhedral hydrates; microcrystals of such hydrates in the brain could interfere with the electric oscillations associated with consciousness (1961). He obtained a $450,000 grant from the Ford Foundation to study the molecular basis of mental disease and turned his laboratories more and more away from traditional chemistry, not to the unanimous approval of his colleagues. In 1958 he resigned from his position as department chairman, a position he had held for more than twenty years, and found himself under pressure to give up research space to a new generation of researchers. In these years of intense political activity and world travel he was in any case spending less and less time with his own research group and in keeping up with new developments in chemistry. When he left Caltech he vanished without a trace. In the 1963-64 annual report of the chemistry department his name appears in the list of professors with more honors and degrees than anyone else; in the corresponding report a year later his name has disappeared.

The next few years were not the happiest in Pauling's life. Not only did he sever his connection with Caltech, he

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resigned from the American Chemical Society as well. The move to Santa Barbara was not a success. He turned to theoretical physics, but his close-packed spheron theory of the atomic nucleus met with little acceptance. He became engaged in actual and threatened libel suits. He moved briefly to the University of California at San Diego (1967-69) and then on to Stanford University (1969-72), where he was closer to his ranch at Big Sur, but he had no stable position in which to continue his planned research into "orthomolecular" psychiatric therapy. Meanwhile, he was deeply unhappy about the American involvement in Vietnam and about American politics in general.

One consolation was that after passing his sixty-fifth birthday Pauling's health took a sudden turn for the better. Thanks to Dr. Addis's unconventional low-protein diet, he had recovered well from the kidney disease that had laid him low in his forties, but he had always suffered from severe colds several times a year. In 1966, following a suggestion from Dr. Irwin Stone, the Paulings began to take three grams of ascorbic acid per day each. Almost immediately they felt livelier and healthier. Over the next few years the colds that had plagued him all his life became less severe and less frequent. This experience made Pauling a believer in the health benefits of large daily amounts of vitamin C. It was not long before he was enthusiastically promulgating this belief in lectures and writings, which, not too surprisingly, brought on him the displeasure of the American medical establishment. After all, the then recommended daily allowance (RDA) of vitamin C was 45 mg; it was well known that there was no known cure for the common cold, and, in particular, previous studies had shown conclusively that vitamin C had no effect. Nevertheless, the NAS Subcommittee on Laboratory Animal Nutrition was then recommending daily intakes around 100 times that of the human RDA

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(adjusted for body weight) to keep laboratory primates in optimal health.

In his 1970 book *Vitamin C and the Common Cold,* Pauling gave evolutionary arguments why much larger amounts of vitamin C than the RDA may be conducive to optimal health. He cited studies supporting its efficacy in preventing colds or at least in lessening their severity. He criticized studies that claimed the opposite and he argued that since vitamin C is not a drug but a nutrient there is no reason why a large daily intake should be harmful. Pauling's arguments did not win the approval of the medical profession but they caught on with the general public. The book rapidly became a best seller. As a result, in America and later in other countries, millions of people have been persuaded that a daily intake of 1-2 g of ascorbic acid has a beneficial effect on health and well being, essentially agreeing with Pauling that “we may make use of ascorbic acid for improving health in the ways indicated by experience, even though a detailed understanding of the mechanisms of its action has not yet been obtained."

One result of the book was a collaboration with a Scottish surgeon, Ewan Cameron, from Vale of Leven, who had observed beneficial effects of high doses of vitamin C in treating terminal cancer patients. Cameron thought that vitamin C might be involved in strengthening the intracellular mucopolysaccharide hyaluronic acid by helping to inhibit the action of the enzyme hyaluronidase produced by invasive cancerous cells. A paper by Cameron and Pauling (1973) advocating vitamin C therapy in cancer was submitted to the *Proceedings of the National Academy of Sciences* (PNAS), which, in an unprecedented move, rejected the paper (it was then published in the specialist journal *Oncology).* During the next few years Cameron continued his trials. Since a double-blind trial was ethically unacceptable, he compared

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results obtained with one hundred ascorbate-treated terminal patients and one thousand other cases, ten controls for each patient, matched as closely as possible, and found that the ascorbate-treated patients lived longer and felt better subjectively. A paper describing these results was eventually published in PNAS (Cameron and Pauling, 1976) but only after long arguments with referees. The Cameron-Pauling collaboration culminated in their 1979 book *Cancer and Vitamin C,* which was again more popular with the public than the medical profession, which continued to regard claims about the effectiveness of vitamin C in treating or preventing cancer as quackery. But by this time several important changes had occurred in Pauling's life.

At Stanford Pauling's demands for more laboratory space for his orthomolecular medicine studies had been turned down. A solution was found by a younger colleague, Arthur B. Robinson, who had left a tenured position at San Diego to work with Pauling at Stanford. Instead of working in cramped quarters at the university they would set up their own research institute nearby. A building was rented, initial financial help was forthcoming, and the Institute for Orthomolecular Medicine was founded in 1973. Once the initial funding ran out the institute found itself in financial straits. Soon it was renamed the Linus Pauling Institute of Science and Medicine with Pauling as president. By this change, it was hoped, fund-raising possibilities would be improved—a hope that proved illusory. Since Pauling was frequently away on travels and in any case disliked administration, Robinson took over in 1975, but the fiscal problems of the institute dragged on for several years until support began to be provided by private foundations and individual donations.

Personal and scientific difficulties between Robinson and Pauling led to Robinson's dismissal in 1979 and to lawsuits that dragged on for years. Meanwhile, Pauling continued to

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defend his unorthodox views and became once again a controversial figure, regarded by some as a crackpot, by others as a sage. In 1986 he wrote another popular book *How to Live Longer and Feel Better,* which, based on his own experiences, gave advice about how to cope with aging.

In July 1976 Ava Helen underwent surgery for stomach cancer. Instead of post-operative chemotherapy or radiation treatment she adopted vitamin C therapy to the tune of 10 g per day. She was soon well enough to accompany Pauling on his various travels, but she finally succumbed five years later in December 1981. Pauling continued to travel, appear on television, write, and receive honors—his energy seemed unabated. When quasi-crystals with forbidden fivefold symmetry were discovered in 1984 Pauling took a contrary position and argued that the fivefold symmetry seen in Al/Mn alloys resulted merely from twinning of cubic crystallites (1985). He was probably wrong, but the resulting controversy was nevertheless useful in forcing the proponents of quasi-crystals to seek better evidence for their view.

He even became reconciled with Caltech, where his eighty-fifth and ninetieth birthdays were marked by special symposia in his honor. In 1991 he was diagnosed with cancer. Surgery brought temporary relief, and megadoses of vitamin C kept up his spirits. He spent his last months at the ranch at Big Sur and died there on August 19, 1994.

In the meantime, the medical establishment is no longer so totally dismissive of Pauling's views about possible therapeutic benefits of vitamin C on the common cold and on cancer. A recent review of several studies concludes that although supplemental vitamin C does not decrease the incidence of the common cold it does diminish the duration and severity of symptoms (Hemila, 1992). This review also states that the level of vitamin C intake derived from a

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normal or balanced diet may be insufficient for optimal body function and that the substance is safe even in large amounts.

The connection between vitamin C and cancer has also become a respectable topic of discussion. It was the subject of a conference organized by the National Cancer Institute in Washington, D.C., in 1990. Vitamins C and E (and other anti-oxidants) inhibit the endogenous formation of N-nitroso compounds in animals and humans (Bartsch, Ohshima, and Pignatelli, 1988). Such compounds are known to be carcinogenic in animals. Conclusive proof that they are dangerous at the levels naturally present in man is lacking, but the evidence seems suggestive. Thus, although the effectiveness of vitamin C in treating cancers may still be debatable, there is good reason to believe that it has at least an important preventative role.

The final word about the effect of large doses of vitamin C on health has still to be said. If you have a full, healthy diet rich with fruit, grains, and fresh vegetables, then you probably do not need supplemental vitamins and minerals. But in the modern world many people have, and may even prefer, an unhealthy diet. For them vitamin supplements are probably beneficial. After all, Pauling not only recommended large doses of vitamin C but also advised people to stop smoking, eat less, and cut down on sucrose.

## PAULING THE MAN

Pauling lived a long and productive life. As scientist, through his writings and personal impact, he influenced several generations of chemists and biologists. As political activist he challenged the political and military establishment of the United States and helped to change them. As health crusader he took on the medical establishment and persuaded millions of people to eat supplemental vitamins.

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He could be very persuasive indeed. His lectures were spellbinding, and he had a characteristically simple and direct literary style.

I remember his lectures at Oxford in early 1948. The lecture hall was too small to hold all who wished to attend; there was standing room only. He told those of us who had never studied electrostatics to go home and read Sir James Jeans's book on that subject before coming to his lectures on chemical bonding. I had never studied electrostatics but I stayed, spellbound. I had never heard anyone quite like him, with his jokes, relaxed manner, seraphic smile, slide-rule calculations, and spontaneous flow of ideas (only much later did I realize that much of that apparent spontaneity was carefully studied). He had great histrionic skills.

Vain? Conceited? Pauling was certainly aware of his own intellectual superiority, but he could be patient in dealing with the slowness of the slow witted. On the whole he was fairly tolerant of young, insecure seminar speakers, although, as I remember, he could also be intimidating at times. I am referring here to Pauling in middle age; I am told he became more intolerant in his later years. Political harassment during and after the McCarthy era must have taken its toll. Ambitious? Self-centered? Undoubtedly. Without these traits he would not have been able to accomplish as much as he did. But he often had a merry twinkle in his eyes and could be very charming, both as a public personality and in private.

In personal matters he kept most people at a distance. I believe he was basically rather shy. When he talked about science or politics or anything that caught his interest there was no stopping him. He read widely and was extremely knowledgeable in many areas-a result of having pored over the *Encyclopaedia Britannica* in his youth? In conversation one sometimes sensed a faraway look in his eyes; one felt

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that he was already thinking about something else. Probably he was, and, indeed, he was a formidable thinker, both at the problem-solving level and about fundamentals. With his prodigious memory he could call up facts and derivations, what so-and-so had written in 1928, the unit cell dimensions of an obscure mineral, the standard heat of formation of ethane; and he had a remarkable capacity to visualize complex three-dimensional structures. I once asked him why he had never discussed the application of group theory to problems of chemical bonding. "Jack," he replied, "if you need group theory to solve that sort of problem then you're in the wrong line of business."

In addition to his Nobel Prizes Pauling was awarded dozens of honors and distinctions, including honorary doctorates from Oregon State College, Brooklyn Polytechnic Institute, Reed College, and the Universities of Chicago, Princeton, Yale, Cambridge, London, Oxford, Paris, Toulouse, Montepellier, Lyon, Liege, Humboldt (Berlin), Melbourne, York (Toronto), New Brunswick, and Warsaw. His election to membership in the National Academy of Sciences, Royal Society of London, Academie Frangaise des Sciences, and Akademiya Nauk SSR may be specially mentioned.

His name will be remembered as long as there is a science of chemistry.

I HAVE LEARNED MUCH about Pauling's life from the excellent biography by Tom Hager (1995) and am grateful for information and advice from many friends and colleagues, among them David Craig, Durward W. J. Cruickshank, Albert Eschenmoser, Edgar Heilbronner, Barclay and Linda Pauling Kamb, Paul Kleihues, Alan Mackay, Peter J. Pauling, Alexander Rich,John D. Roberts, and Verner Schomaker.

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# CARL PFAFFMANN

*May 27, 1913-April 16, 1994*

BY LORRIN A. RIGGS

A RHODES SCHOLAR is chosen for scholastic abilities, for such qualities as truthfulness, courage, kindliness, and devotion to duty; for moral force of character and instincts to lead and to take an interest in others; and for physical vigor and sportsmanship. Carl Pfaffmann was awarded a Rhodes scholarship while he was a graduate student at Brown University. Already he had shown the qualities listed above. He had graduated from Brown *magna cum laude,* with honors in psychology; he had played saxophone in orchestras and dance bands; he had been involved with the swimming team; and his scholarship had earned him the respect of Leonard Carmichael, then chairman of the psychology department, who had invited him to join the teaching and research activities of the laboratory. From this beginning Carl went on to a career in which his all-around talents took him to a position of world leadership in his chosen academic field of the chemical senses of taste and smell.

Carl Pfaffmann was born in Brooklyn, New York, on May 27, 1913. All four of his grandparents had emigrated from Germany and lived in New York City as their families grew up. Carl's parents, Charles and Anna Pfaffmann, had to work hard and never went to high school. By the time Carl

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was born they had achieved sufficient success to purchase a home on Long Island and to give him the opportunity to attain the education they never had. Carl, in turn, did well enough in school to qualify for rapid advancement and to graduate from high school with honor grades at the age of sixteen.

Next came college at Brown. As a freshman Carl undertook an ambitious schedule of earning tuition money, participating in sports and concert tours, and working hard at his studies. During his sophomore year he discovered the academic field that was to dominate his whole future career. This came about because of a young and enthusiastic professor, who in Carl's words (1989) “had been recruited from Princeton to head and modernize the development of a psychology department at Brown." Listening to lectures and observing experiments performed by Carmichael persuaded Carl that here was an exciting new field for his own future career. Furthermore, on consultation with Carmichael, he found that a good opportunity to get personally involved was to enroll in the honors program. Carmichael suggested, in fact, that Carl do experiments and write an honors thesis on the subject of human taste sensitivity. This subject was selected because relatively little was known about it in comparison, say, with vision or hearing. It appealed to Carl's pioneering spirit that he could open up new findings in an uncharted field.

On graduating from Brown, Carl considered various options for continuing work on the chemical senses. He chose to stay at Brown because Carmichael had been successful in building up a graduate program and had offered him a teaching assistantship. Furthermore, he could go with a research assistantship to the nearby laboratory of Herbert Jasper, who was doing pioneering work with the recording of human brain waves. Jasper's electrophysiological record

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ing equipment provided Carl with the opportunity to record nerve impulses in animals in response to tactile stimuli and chemical solutions. This research was reported in a 1935 publication with Jasper. Carmichael, who was also working with Jasper at the time, was so impressed by Carl's research that he urged him to apply for a Rhodes scholarship. The Rhodes award was made in 1935, and Carl spent the next two years at Oxford University. By this time he had convinced himself that a necessary part of his career goal was to do research on the neurophysiology of taste; but because he had no formal training in physiology, he devoted the two years at Oxford to the achievement of a B.A. degree in that subject.

Carl's third and fourth years of the Rhodes program were devoted to research for a Ph.D. degree from Cambridge University. This was in the famous physiological laboratory of Lord Adrian, and the only degree requirement was for an original thesis. Carl set for himself the ambitious goal of recording the responses of taste nerve fibers in the cat. The aim was to "crack the code" of how different types of taste quality were mediated by the receptors and afferent nerve fibers from the tongue.

Going into this program of research, Carl was familiar with the psychologists' current classification of basic taste components as sweet, sour, salty, and bitter. He thus anticipated that, if he could successfully isolate individual sensory fibers from the cat's tongue, each fiber would be found to respond deferentially to sugar, acid, salt, or quinine; but his initial attempts to isolate single taste fibers were unsuccessful. The lingual nerve of the cat was made up primarily of nontaste-sensitive fibers whose responses "drowned out" those that responded to chemical stimuli; but persistent efforts at dissection, together with a judicious use of newly developed recording techniques, finally resulted in the iso

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lation of responses in single units responsive to chemical stimulation. Then came the surprising result: every fiber responded to acid; some of them to acid plus salt, some to acid plus quinine, and some to acid alone. No response to sugar was ever found. The conclusion Carl drew from this pioneering work was that the coding for taste discrimination must be based, not on sensory units (each of which is tuned to a particular type of stimulus) but on an across-fiber pattern of response that the brain uses to identify specific tastes. Thus, acid would be identified by the cat if all of its taste fibers were active: bitter, if only the fibers sensitive to acid and bitter were involved; salt would be signaled by the acid-salt fibers. The absence of a sweet-sensitive system was later shown to be characteristic of the cat; some taste fibers in monkeys and other animals, presumably including humans, are strongly affected by sweet substances. Furthermore, responses to sugar have been shown to be more prevalent in the glosso-pharyngeal and petrosal nerves than in the chorda tympani, from which the original recordings were made.

Carl submitted his dissertation on taste nerve responses to his mentor at Cambridge, but Adrian declined to read it, saying that Carl should first give it to the examining committee and defend it himself against possible criticisms. Adrian, it seems, was insistent that the dissertation be judged as the original work of the candidate, not modified by any ideas of the thesis adviser. Is this too lofty an ideal to be maintained in our present-day evaluations of a dissertation?

The clouds of World War II were gathering in Europe as Carl completed his work at Cambridge. At this point an opportunity for postdoctoral research became available to him back in the States. This was at the Johnson Foundation for Medical Physics at the University of Pennsylvania. The invitation to Carl was from Detlev Bronk, who had estab

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lished the foundation after doing research at Cambridge with Adrian. The year of 1939-40 in Bronk's laboratory gave Carl additional experience in the recording of nerve impulses from single fibers. Carl was the third psychologist to have been at the Johnson Foundation, the earlier ones having been Clarence Graham and Lorrin Riggs.

In 1939 Carl married Hortense Louise Brooks, whom he had met at Oxford. They had three children, Ellen Anne, Charles Brooks, and William Sage.

In 1940 Carl received an appointment to the faculty of Brown University. By this time Walter Hunter had succeeded Leonard Carmichael as head of the psychology department. Hunter's goal was to build up a center for physiological and experimental psychology, and other appointments went to Graham, Lindsley, and Riggs. World War II intervened, and all of us, including Hunter, had to devote our efforts to various military causes. Carl was commissioned an officer of the U.S. Naval Reserve, advancing to the rank of Commander in 1945. His service included experiments on visual factors related to aircraft landings.

At the war's end Carl was able to embark on a professional career of research, teaching, and administration. First at Brown University (1945-65) and then at Rockefeller University (1965-94) he pursued the problem he had uncovered during his graduate study: how can we identify and discriminate the basic taste components of sweet, sour, salty, and bitter given that each of the individual sensory units responds to more than one type of stimulus? The answer gradually emerged from a prodigious amount of research by Carl and his teams of co-workers, as well as by colleagues working in this field throughout the world.

When Carl returned to Brown after the war he found it to be a stimulating environment. Hunter, who had been a leader among the psychologists working on military prob

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lems, had now turned his efforts to securing grant funds, laboratory facilities, and professional staff for the Department of Psychology. Realizing that the department was relatively small, and housed in old frame houses, he aimed to concentrate its efforts on sensory and physiological psychology. Thus, Carl was given a generous allotment of space for his research on the chemical senses.

On the teaching side, also, the mood at Brown was one of enthusiasm and expansion. Students returning from military service were unusually mature and dedicated to academic achievement. Supported by grants for their tuition under the G. I. Bill, they enrolled in record numbers. This in turn had the effect that the faculty and staff could be expanded. Graduate students could be supported by teaching and research assistantships. Faculty members were solicited by national agencies, notably the Office of Naval Research, for the purpose of underwriting projects of their own choosing in the areas of sensory and physiological psychology. This support meant that faculty members could build up their laboratory facilities, hire secretarial help, pay their assistants, and avail themselves of funds for travel to professional meetings. Under these favorable conditions research teams were formed with members of the faculty, graduate students, postdoctorals, and scientists from other institutions.

Carl found that he could design an effective experimental program at Brown, making use of these favorable surroundings. During the twenty-one years of his tenure at Brown he published thirty-eight research articles, seven chapters in books, fourteen abstracts of reports at meetings, four articles on the methodology of teaching, five articles for encyclopedias, and three biographical articles. His research, much of it conducted jointly with colleagues, graduate students, and postdoctoral fellows, involved a variety of

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methodologies employed in the study of taste. These included neurophysiological recordings from gustatory nerves and observations of the feeding behavior of experimental animals. Human subjects made psychophysical determinations of sensitivity and qualities of experiences of taste and smell.

Among his research collaborators at various times were Abbott, Bare, Bartoshuk, Benjamin, Carpenter, Erickson, Frank, Frommer, Hagstrom, Halpern, Hockman, Johnston, MacLeod, Matthews, McBurney, McCutcheon, Morrison, Mozell, Nord, Oakley, and Pierrel. Most of them lived on or near the Brown campus, as did the Pfaffmanns. Carl and his wife Louise were active in campus activities. Every year they hosted a Christmas party, mainly for the psychology department.

These were happy years for all of us. First Walter Hunter (1946-56) and then Harold Schlosberg (1956-66) were benevolent chairmen of the department, making sure that each member could attain his own goals without competition with the others. Neither Carl nor I was pressured into becoming the department chairman. One sad time was when we heard that the Pfaffmann's first son Charles had died in an aircraft accident.

A major expansion of our facilities at Brown occurred in 1957-58 with the construction of the Hunter Laboratory of Psychology. Carl was able to move his experiments into specially designed basement rooms, where he and his team of co-workers could be together and share their equipment and plans. In another portion of the basement a vision research unit was established under my leadership. The two of us worked together harmoniously; vision was a field more highly developed while the study of the chemical senses was more of a pioneering effort in which new findings could be uncovered by a relatively small number of workers.

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During his years at Brown, Carl first turned his attention to neurophysiological experiments in which he and his colleagues explored the roles of taste fibers they found in portions of the seventh, ninth, and tenth cranial nerves. Their single-fiber recording from the chorda tympani, for example, gave beautiful records in which the impulse frequency increased progressively in response to higher and higher concentrations of the stimulating solution. Thus, the neural coding for stimulus magnitude was shown to be similar to that which had been established for the senses of vision, hearing, and touch. The coding for taste quality, however, turned out to be much more complex. Each single fiber did respond most vigorously to stimulation of the tongue by a particular chemical substance, but the same fiber might respond, albeit less vigorously, to other substances as well. Some fibers had a high sensitivity for two or more types of stimuli. These results on the rat confirmed and extended the findings in Carl's original thesis on the cat. Detailed studies by Beidler and others showed that even individual sensory cells exhibited this multiple sensitivity. Thus, it was concluded that an animal's ability to distinguish the various types of chemical solution must depend on the brain. The brain centers must be able to analyze out the separate taste qualities from the barrage of lingual nerve impulses. This is the theory that Carl called the "across-fiber patterning" hypothesis.

Further experiments by Carl and his students revealed that certain chemical substances had similar abilities to arouse taste nerve responses. This generalization resembled the stimulus similarity reported by human tasters for certain classes of chemical substances. This resulted in an adaptation in which the neural responses to that substance were diminished or eliminated. Related substances then showed a "cross-adaptation" in which there was also a diminished

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response. This type of generalization occurs also in vision and other senses with human and animal subjects. Carl's student Erickson developed a quantitative analysis of the combinations of neural response that led to judgments of taste quality.

Stimulus deprivation experiments also were conducted by Carl, initially with his student Bare. They found that, while cravings exist for salt or sugar in animals deprived of these substances, there is no evidence that this results from any diminution of taste sensitivity. More generally, it can be said that food preferences depend on central as well as receptoral factors. Experiments have been performed in which lesions in the hypothalamus and other regions have altered the feeding behavior of animals. Human food intake is facilitated by "taste enhancers" and other aspects of the environment. Genetic factors are shown dramatically in the case of "taste-blind" subjects, who lack the ability possessed by most individuals to experience bitterness of a test substance, phenyl thiocarbamide.

Further psychophysical experiments revealed a spatial summation on the tongue, such that small areas required strong solutions for their arousal. Moreover, good taste discriminations required that large tongue areas be covered by the stimulating solutions. Saliva played a role also; in some cases a prior rinse with distilled water rendered the tongue much more sensitive.

With all this enterprise devoted to the sense of taste, Carl was not unmindful of the importance of smell. He always included olfaction in his writings on the chemical senses, and he did conduct some experiments in olfaction with colleagues and students at Brown. For practical reasons, however, he devoted a major part of his research to the taste experiments, leaving to others the task of analyzing the principal facts and interpretations of olfaction. His ex

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pertise in the evaluations of taste and smell led to his occasional involvement in setting up panels of tasters for the food industry. One of Carl's most important articles, "The Pleasures of Sensation" (1960), brought out the predominant role of the chemical senses in the hedonic side of human experience.

Brown University has always been an institution where undergraduate teaching is considered a duty and an art. Carl entered enthusiastically into a seminar program that President Wriston had introduced under the heading of "The Identification and Criticism of Ideas." To quote from Carl's own account (1984):

> I agreed to handle the second semester, which began with reading Freud's original lectures on psychoanalysis (in English edition, 1952), followed by the modern behavioral extensions and reinterpretations thereof. This meant that I personally had a great deal of new reading and preparation to do, both ahead of time and during the second semester.

Carl also undertook, with his colleague Harold Schlosberg, development of an undergraduate teaching laboratory in which students obtained firsthand practice in the methods of experimental psychology. They were also taken on visits to nearby mental hospitals and clinics. The success of the whole enterprise led to an expansion of the faculty and graduate teaching assistantships during the 1950s.

By the end of the 1950s Carl's reputation had spread far beyond his own university. He was elected in 1959 to the National Academy of Sciences[1] and soon became chairman of its Assembly of Behavioral and Social Sciences. He became a delegate to the Academy's counterpart societies and laboratories in the Soviet Union. He was elected President of the Eastern Psychological Association and of the Division of Experimental Psychology of the American Psychological Association. In 1959, together with Lloyd Beidler

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and Yngve Zotterman, he founded the International Symposium of Olfaction and Taste, which still meets every three years with the International Congress of Physiology.

In 1965 Carl was chosen by President Bronk to become a vice-president and professor of the Rockefeller Institute for Medical Research. Carl was given the challenge to build up the biobehavioral sciences. This move was part of a plan to broaden the institute, with a view to attaining the status of a university. Bronk's other moves in that direction included new appointments in physics, mathematics, and philosophy.

Responding to the challenge, Carl first persuaded Neal Miller to leave his very prestigious chair at Yale and join the faculty at Rockefeller. Next he recruited two other distinguished psychologists, William Estes from Stanford and George Miller from Harvard. At the same time a joint program of the New York Zoological Society enabled the Rockefeller Institute to attract Donald Griffin from Cornell and Peter Marler from Berkeley to establish a program of research in animal behavior. The official name change to Rockefeller University occurred in 1967, and the biobehavior group continued to thrive after the retirement of Bronk and the subsequent (1968-78) administration of Frederick Seitz as president.

Neal Miller extended his research and training activities to include a unit of the New York City Hospital on Roosevelt Island. He and his students and colleagues demonstrated the success of behavioral training techniques for the rehabilitation of patients with neurological disorders. Griffin and Marler established a field station for animal behavior research on property given to the university in Milbrook, New York. There they pursued basic research on genetic and learned influences on patterns of singing behavior in birds.

Carl's own laboratory at Rockefeller became a world-class

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center for research on the chemical senses. Despite his administrative duties Carl devoted much of his time to working with postdoctoral students and junior faculty members. Among these were Bernard, Coutreras, Costanzo, Frank, Grill, Herness, Leonard, Meredith, Norgren, Nowliss, Pfaff, Rails, Scott, Singer, Smith, and J. and S. Travers. He was called on also to contribute many articles and summary chapters, and he gave lectures at international meetings.

Coding of taste quality continued to be a major concern in Carl's Laboratory of Physiological Psychology. Work on the specificity of monkey taste neurons by one of his students, Marion Frank, led him to reevaluate the across-fiber patterning hypothesis. The monkey experiments revealed that each taste neuron was typically activated most strongly by one particular stimulus. Other stimuli might also be effective but to a lesser degree. This led Carl to conclude that the test stimulus gave a “clue to their labeled-line significance, that is, their primary quality." He felt that this view of the neurophysiological data could be seen as consistent with the psychophysical finding of primary taste qualities.

Carl's earlier interest in the hedonic importance of the chemical senses was given substantial foundation in new discoveries of taste fiber projections into the limbic system. These studies were accompanied by behavioral measures of feeding behavior and sexual activity that were conditioned by specific taste and smell stimuli in the same experimental animals.

Experiments with human subjects made use of taste sensations elicited by electrical or chemical stimulation of spots on the tongue. Other studies involved individual differences among normal subjects and subjects with a specific taste disorder.

Carl experienced a considerable disappointment after the retirement of President Seitz in 1978 and before his own

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retirement in 1983. Administrative support for his program was cut back. As a result, Estes left Rockefeller in 1979 to go to Harvard, and George Miller left the same year to go to Princeton. Neal Miller stayed on for two more years until his retirement, and Carl himself retired two years after that.

Carl's successor as head of the laboratory was Donald Pfaff, who had been a member of Carl's group for many years. Under a new name, Laboratory of Neurobiology and Behavior, the group continued to study the biological bases for behavior; but, in line with national and international trends in this field, they identified themselves with the newly expanding organizations of neuroscience, rather than the traditional ones in physiological and comparative psychology.

Carl's autobiography, written after his retirement, gives a retrospective view of his accomplishments. He gave enthusiastic credit to his early mentors and later colleagues who had an influence on his career. Among these were Carmichael, Adrian, Bronk, Zotterman, Bujas, and Beidler. He expressed particular satisfaction in his "family" of graduate students and postdoctorals in the laboratories at Brown and Rockefeller. They gathered to celebrate his seventieth birthday at the annual meeting of the Association of Chemoreception Sciences in 1983. He gave approval to a new program for clinical taste and smell research centers organized with advice from his former students Bruce Halpern and Max Mozell. One of these centers was cofounded by one of his former students, Linda Bartoshuk, and is now directed by another, Marion Frank. Another center, emphasizing research in olfaction, is operated by Max Mozell. Carl never became caught up in any theoretical framework for his accomplishments. He was satisfied with the empirical discovery of new facts, and a good part of his life was devoted to the estab

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lishment and administration of research facilities for that purpose.

The last few years of Carl's life were spent at his home in Killingworth, Connecticut. In his late seventies he suffered irritating and sometimes painful skin ailments. He was also afflicted with a rare condition called Ramsey-Hunt syndrome in which there is a deterioration of some of the cranial nerves. One aspect of his own symptoms was particularly intriguing to Carl: Ramsey-Hunt was known to affect taste nerves. He therefore called upon his former student and co-worker Linda Bartoshuk to make a psychophysical study of these effects. Shortly after this he suffered a stroke that ended his life.

In looking back over Carl's career I am reminded again of the attributes of a Rhodes scholar. His superb scholarship was accompanied by an ability to inspire and direct the activities of his students, and he was warmly respected by his colleagues. He was a devoted husband to Louise throughout their marriage of fifty-five years and a loving father to their children. The discoveries and writings based on his pioneering dissertation at Cambridge blossomed into the present huge corpus of literature on the chemical senses.

IT WAS A PRIVILEGE to be associated with Carl at Brown University, where our laboratories were adjacent to one another from 1945 to 1965. In describing earlier and later events of his career I have relied heavily on his 1989 autobiography. I am particularly indebted to his former student and colleague Linda Bartoshuk for supplying me with publications and for helpful comments on the present memoir.

## NOTE

1. It is remarkable that membership in the National Academy of Sciences was attained by ten out of the relatively small group of psychologists at Brown during the Pfaffmann years.

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# EDWARD SAPIR

*January 26, 1884-February 4, 1939*

BY REGNA DARNELL AND JUDITH T. IRVINE

AMONG THE ANTHROPOLOGISTS trained by Franz Boas in the early decades of the twentieth century Edward Sapir alone was regularly acknowledged by his peers as a genius. The only professionally trained linguist among Boas's students, and gifted with intuitive insight into grammatical patterning and historical relationships of linguistic families, Sapir contributed seminally to general linguistic theory, Amerindian linguistics, and Indo-European linguistics. He also made important anthropological contributions in ethnology, culture theory, and cultural psychology. A prolific fieldworker as well as theorist, Sapir recorded for posterity thirty-nine different Amerindian languages, often working with the last living speaker. Alongside his linguistic investigations he gathered ethnographic information and transcribed indigenous-language folklore texts. He was a humanist as well as linguist and anthropologist, composing music and publishing poetry and literary criticism. For his successors in a range of disciplines he continues to exemplify the study of meaning and expressive form across linguistic and cultural boundaries.

## EARLY LIFE AND EDUCATION

Although Sapir was born in Lauenberg, Pomerania

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(Prussia), in what is now Lebork, Poland, his parents, Jacob David and Eva Seagal Sapir, were Lithuanian Jews. Sapir undoubtedly learned German as a child, but the language of his home was Yiddish; he read Hebrew with his father, a cantor, beginning when he was seven or eight. Jacob Sapir preferred music to theology, however, and the family's daily life was not intensely orthodox in religious observance.

The family moved several times during Sapir's early childhood. He began kindergarten in Liverpool, England, while Jacob preceded his wife and children to America, obtaining a position in Richmond, Virginia, in 1890. Shortly after the move to the United States Sapir's younger brother Max died of typhoid, and Jacob's career declined through a series of short-lived appointments. The family took root on the Lower East Side of New York City when Edward was ten. Eva Sapir ran a small notions shop to support herself and her remaining son; she and Jacob divorced sometime after 1910.

When Sapir was fourteen he won a Pulitzer scholarship for four years at the prestigious Horace Mann High School. He declined it in favor of a local high school and used the scholarship for his undergraduate education at Columbia University. He was one of the bright stars among the immigrant children of the city, and higher education was his prize.

Entering Columbia in 1901, Sapir concentrated on Germanic philology while gaining formal training in Indo-European linguistics. He received his B.A. in German in 1904, having taken only three years to complete the four-year program. In 1905 he received his M.A., also in German. He took two more years of courses in anthropology and German, receiving his Ph.D. in anthropology in 1909 with a dissertation on the Takelma language of southwestern Oregon.

Languages were Sapir's forte from the beginning. Since

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Columbia had no department of linguistics as such, Germanics was the field of choice for a student interested in linguistic science. After Sapir met Franz Boas, however, he was inspired by the urgency of the need to record endangered Amerindian languages before they were lost forever. To apply the methods of comparative Indo-European to unwritten aboriginal languages was, for him, an obvious step. His interest in linguistic theory went far beyond that of Boas, a self-taught linguist who acknowledged his pupil's intellectual leadership in linguistics while Sapir was still a graduate student.

The transition from Germanics to anthropology was a smooth one. Sapir's M.A. thesis on Herder's theory of the origin of language, by including Eskimo examples, already reflected the influence of Boas. At this time, as in later years, Sapir defended the functional equivalence of all human languages, explicitly including those of "primitive" peoples. But his real apprenticeship as a field linguist, in the anthropological tradition, began in 1905 when Boas sent him to the Yakima Reservation in Washington to work on Wasco and Wishram Chinook. There were many languages begging for description. In 1906 Sapir returned to the field for his dissertation research, working on Takelma and Chasta Costa at Siletz Reservation in Oregon.

## PROFESSIONAL EMPLOYMENT

Sapir's first professional appointment, in 1907, was as a research assistant at the University of California, Berkeley, where fellow Boas student Alfred Kroeber had a mandate to map the enormous cultural and linguistic diversity of the state. In a single year Sapir studied three dialects of Yana and worked briefly on Kato. But Kroeber was more interested in surface description that would classify related languages than in the careful grammatical analysis Sapir thought

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should produce a dictionary, grammar, and texts for each language studied. California did not continue the appointment after its first year.

Sapir moved to the University of Pennsylvania in 1908 to take up a Harrison fellowship, which involved teaching as well as research through the University Museum. With his ethnologist colleague Frank Speck, another former Boas student, Sapir worked on Catawba. In the 1909 field season Sapir and his student John Alden Mason began fieldwork with Uintah Ute in Utah. They planned a long-term study of Ute language and culture, but their project was not funded by the museum.

Remaining in Philadelphia in 1910, Sapir began studying Southern Paiute, a language closely related to Ute, with Tony Tillohash, a student from the nearby Carlisle Indian School. It was a fortunate collaboration: Tillohash's ability to analyze his native language meshed with Sapir's intuitions to produce what has sometimes been called the most beautiful grammatical description ever written of an Amerindian language. Sapir worked briefly on Hopi with another Carlisle student but abandoned it in favor of his work with Tillohash, choosing the ideal linguistic informant over the language as such.

At the age of twenty-six Sapir obtained a plum position in the expanding Boasian network of professional anthropology in North America. He served from 1910 to 1925 as the first chief ethnologist of the Division of Anthropology in the Geological Survey of Canada, Department of Mines. As Canada's paramount anthropologist he quickly developed a research and publication program and a national museum focusing on the aboriginal peoples of the dominion.

With a wide research field to cover Sapir hired several Boas-trained researchers and alternated his own research

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program between intensive work with Nootka on Vancouver Island and survey fieldwork among a variety of northeastern languages spoken within easy range of Ottawa. Although he was able to make only two field trips to the Nootka area before the First World War dried up research funds and administrative responsibility made summer fieldwork more difficult, Sapir worked with speakers of various northwest coast languages when their speakers visited Ottawa on tribal business. In 1922, a few years after the end of the war, he was able to return to a brief stint of in situ fieldwork for a study of Sarcee, an Athabaskan language, in Alberta. The following year he pursued the Athabaskan research with Kutchin and Ingalik, Athabaskan languages of northern Canada, since some speakers of these languages happened to be living not far away at Camp Red Cloud, Pennsylvania.

The Canadian work was interrupted only once, when Kroeber invited Sapir back to California to work with a “wild" Indian, the last speaker of Yahi, a Yana language. Using his knowledge of other Yana varieties studied years before, Sapir spent the summer of 1915 recording Ishi's unique knowledge of his language and culture.

The later Ottawa years were depressing ones, on personal as well as professional grounds. Sapir was a pacifist during the First World War and keenly felt his position as an immigrant to North America. Florence Delson Sapir, whom he had married in 1910, suffered from a series of mental and physical ailments until her death of a lung abscess in 1924. Sapir's mother came to help with the three children. As for his research activities, even after the end of the war the research portion of the division's work did not recover enough funding to restore its original grandeur.

These were years of intense introspection for Sapir. He wrote poetry and literary criticism, dabbled in psychology, and composed music. Largely prevented from carrying out

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new fieldwork and increasingly frustrated by his inability to write up his accumulated materials on the myriad languages he already had studied in the field, he turned to more general linguistic questions and to the theory of culture, society, and the individual. Although his own intellectual activities had lost none of their vigor, he felt isolated in Ottawa and lamented the absence of a university affiliation with the chance to train his own students.

In 1925 Sapir was called to the University of Chicago, which had already assembled a stellar faculty. His appointment was to a joint Department of Sociology and Anthropology, which split in 1929. Since the "Chicago School" of sociology was the most prestigious and professional variety of social science in North America at the time, the new position placed Sapir at the center of a network of interdisciplinary scholarship, much of it sponsored by the Rockefeller Foundation. After the years of perceived isolation in Ottawa, Sapir thrived on the intellectual excitement of Chicago in the late 1920s. He eagerly joined the interdisciplinary conference circuit, becoming the man of words who enabled colleagues from sociology and psychology/psychiatry to understand the common links of their work. His collaboration with interactional psychiatrist Harry Stack Sullivan and political scientist Harold D. Lasswell is particularly notable. Because he was teaching in the social sciences Sapir found himself thinking a great deal about culture, psychology, and social science methodology. Still, in this period he did not abandon his linguistic work, even managing to make field trips to study Navajo and Hupa.

It was shortly after his arrival in Chicago that Sapir renewed an acquaintance with Jean McClenaghan, then a social work student on a practicum at the Chicago Institute for Juvenile Research. The couple were married in 1927 and had two children.

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In 1931 Sapir followed Rockefeller funding to Yale University. As Sterling professor of anthropology and linguistics he was expected to bring interdisciplinary research to the Graduate Division of the university, heading a new department of anthropology and drawing social science research together into a single coherent research program. With colleagues at the Institute of Human Relations he was to offer a seminar on "the impact of culture on personality," supported by the Rockefeller Foundation. He was also to serve in a newly independent graduate department of linguistics. For the first time he found intellectually congenial colleagues in linguistic theory and Indo-European studies. A cadre of his Chicago graduate students in linguistics moved to Yale with him, constituting the first Yale school of linguistics (the second one coalesced around Leonard Bloomfield in the 1940s).

These utopian plans were undermined by local academic politics, especially by vested interests in sociology, by the economic effects of the Depression, and by currents of anti-Semitism at Yale. Sapir was overextended and unhappy. Outside Yale he continued with his interdisciplinary activities; within it he focused on his own teaching in anthropology and linguistics.

In 1937, while teaching at the Linguistic Society of America Summer Institute at Ann Arbor, Michigan, Sapir suffered his first heart attack. A sabbatical trip to China in 1937-38 had to be cancelled because of his health. Although he returned to teaching in the fall of 1938, he had not recovered his strength. He died early in 1939 at the age of fifty-five.

## LINGUISTIC METHOD AND THEORY

Sapir's first synthetic works were part of the formalization of the Boasian paradigm. In 1916 his *Time Perspective in*

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*Aboriginal American Culture: A Study in Method* laid out the method of historical inference implicit in the Boasian reconstruction of the history of cultures and languages. (At the time, direct archaeological evidence of American prehistory was scanty, and there were no consistent standards for its interpretation until the Pecos Conference a full decade later; indirect evidence, such as might be provided by linguistics and ethnology, was therefore crucial.) Drawing on linguistic examples from a remarkable range of cases, Sapir in *Time Perspective* distinguished methodologically between the properties of language and culture for historical reconstruction. Sound change in language, unlike the other parts of culture, he argued, retained traces of the past historical relationships of languages. In consequence, genetic relationships could be discerned and distinguished from other kinds of relationships by the application of methods used in Indo-European historical linguistics, even in the absence of written records. Sapir's treatise remained the ethnologist's guide to historical method for a generation and still repays careful attention to the forms of his logic.

In 1921 Sapir published *Language: An Introduction to the Study of Speech,* the only book he completed during his lifetime. He included written and unwritten languages on an equal footing, marvelling at the precision and beauty of grammatical forms and structural typologies. This was Sapir the linguist writing at his most lyrical and persuasive. The book was directed at an educated general audience, but its broad canvas and penetrating vision of linguistic form, as well as its treatment of specific topics, have greatly influenced professional linguists ever since. The discussion of "drift," for example, remains fundamental to linguistic theory about processes of language change.

Also in 1921 Sapir published a one-page summary of his six-unit classification of American Indian languages, based

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on a paper read to the American Association for the Advancement of Science. Although the 1929 version of this classification is better known and is accompanied by considerable justification, including a medial classification of twenty-three units acceptable even to conservatives among Amerindian linguists, the 1921 version was essentially complete. It was based on the comparative work Sapir and his colleagues had done over the past two decades. Although Sapir himself saw the classification as a series of working hypotheses, many anthropologists promptly reified its categories, latching onto the six-unit classification as an easy guide to tribal relationships.

The most daring of the proposals made by Sapir in this period involved linking Athabaskan to Haida and Tlingit to form Na-dene and then linking Na-dene, largely on the basis of its tonal structure, to Sino-Tibetan. By the 1930's, however, when Sapir moved to Yale, his colleagues in linguistics were skeptical of such speculative large-scale genetic hypotheses, and the anthropologists were no longer in dire need of historical models from linguistics (if only because of the emergence of reliable dating methods in prehistoric archaeology). During the Yale years Sapir paid less attention to the six-unit classification, returning instead to linguistic theory and to specific linguistic problems both within and beyond the Americanist field, including studies in African, Semitic, and Indo-European linguistics.

Some of Sapir's most famous contributions to linguistic theory lie in phonology, the study of sound systems. In 1925 the inaugural issue of *Language-the* journal of the Linguistic Society of America, of which Sapir was a crucial founder-carried his paper, "Sound Patterns in Language," which defined the concept of the phoneme in terms of significant relationships among sounds, rather than their objective qualities. In 1933 he followed up this pattern-ori

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ented argument in discussing the phoneme's "psychological reality," that is, the intuitions of Amerindian language speakers for their native language's phonological system. The level of generalization implicit in Sapir's distinction between phonetics and phonology in these papers, which revolutionized American linguistics, was derived from fieldwork with aboriginal languages independently of parallel work on phonemic models by the Prague School of linguists in Europe. A late (1938) paper of Sapir's on glottalized continuants pursued these phonological themes and is significant for its use of evidence from Amerindian languages alongside Indo-European data.

Sapir is also especially noted for his dynamic conception of grammar. His analysis of the grammar of Southern Paiute, together with his student Stanley Newman's grammar of Yokuts, stand as exemplars of the "process grammar," an important though discontinuous precursor of contemporary generative theories. What intervened was the school of linguistics associated with Leonard Bloomfield, Sapir's younger colleague at Yale. Sapir's conception of grammatical process and his interest in the study of meaning as integral to the theory of grammar contrast sharply with the work of the Bloomfieldians.

Sapir's discussions of the role of meaning in grammatical form and the relationships of these to the use of language in formulating and conveying ideas have been taken as his contribution to what is often called the Sapir-Whorf hypothesis. In fact the hypothesis was developed largely by his student Benjamin Lee Whorf after his mentor's death. But there are certainly intimations in Sapir's own writing of the way in which habitual thought might be influenced, if not determined, by linguistic structures.

There is almost no important topic in linguistics or its allied disciplines to which Sapir did not contribute. Some

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of his interests, it is true, no longer command widespread scholarly attention, such as the construction of an international language. Others, such as his work on sociolinguistic variation in Yana, have been rediscovered by modern scholars who emphasize these topics more than Sapir himself did. Taken as a whole, however, the range of Sapir's concerns significantly shaped the outlines of American linguistics for later generations.

## SAPIR AS THEORETICIAN OF CULTURE

Although Sapir's reputation in the decades following his death has rested more upon his contributions to linguistics than upon his role in cultural anthropology, during his lifetime he was known as an important ethnologist and cultural theorist as well. In 1916 after the publication of *Time Perspective—an* essay that includes explorations of the diachronic implications of ethnological phenomena, on analogy with language—he embarked on a consideration of theoretical problems in the concept of culture. These interests were to occupy him increasingly during the rest of his professional life. His 1917 debate with Kroeber on the "superorganic," a debate in which Sapir challenged Kroeber's assumptions about anthropological epistemology and the role of individual achievement and experience in cultural systems, was only the first of many discussions of these themes.

Sapir's conception of culture and anthropological method was always influenced by his work in linguistics. Language was, for him, the cultural phenomenon par excellence. It offered the prime example of cultural difference and cultural systematicity; it provided the ethnographer with the terminological key to native concepts; and it suggested to its speakers the configurations of readily expressible ideas. But Sapir's thinking about culture drew significantly, as well, on his interests in psychology and psychiatry, especially Jung's

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writings on personality, Koffka's Gestalt psychology, and Sullivan's interactional psychotherapy.

One of the problems that most interested Sapir was the tension between the anthropologist's concern with abstracting cultural patterns from observable behavior and the individual participant's personal biography and subjective experience. In contrast to many other anthropologists of the time Sapir emphasized intracultural variability, disagreement, and individual agency. He distinguished carefully between, on the one hand, subjective meanings and experience, and, on the other, the public symbols and social conventions prescribing the forms a person's behavior takes. Although much interested in the relationships between culture and personality, Sapir criticized approaches which, in his view, failed to distinguish collective and individual levels of analysis, confusing conventional patterns of behavior with the personality patterns of actual individuals. Late in his life, influenced by his collaboration with Harry Stack Sullivan, Sapir began to look to the analysis of social interaction as the locus of cultural dynamics.

Sapir's writings on culture have sometimes been seen as falling into an extreme methodological individualism, but this view distorts his position. He was equally interested in cultural configurations and in the ways an individual's experience is dependent on social setting. The problem was how a theory of culture could accommodate both its individual and its social sides. Since his peers, Kroeber especially, seemed to give priority to the social, Sapir's writings often emphasized the individual.

During his lifetime his contribution to cultural theory took the form of a series of essays. Although he planned to write a book on "the psychology of culture," based on his graduate lecture course of that title and the Rockefeller seminar at Yale, he did not live to complete it. A manu

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script for the book was finally reconstructed posthumously from students' lecture notes and published in 1993.

## CONTINUING REPUTATION

Sapir's scholarly reputation is easily documented in the official honors accorded him. His positions at Chicago and Yale, an honorary degree from Columbia, elections to the presidencies of the American Anthropological Association and the Linguistic Society of America, his membership in the National Academy of Sciences, the memorial volume (originally planned as a festschrift) published shortly after his death, and many other honors are evidence of the scholarly esteem in which his colleagues held him. Their respect was also personal. As his student David Mandelbaum wrote in an obituary of Sapir published in 1941, "He was more than an inspired scholar, he was an inspiring person. Listening to him was a lucid adventure in the field of ideas; one came forth exhilarated, more than oneself. . . . An eminent psychiatrist recently remarked that Sapir was an intoxicating man. That he was."

Yet despite the force of his personality and the importance of his contributions, there is no "Sapir school" in either of the major disciplines to which his work was foundational. No single one of Sapir's students pursued all of the disciplines or topics that consumed his interests over the course of his career. His untimely death in 1939 left them without a mentor at a time when a world depression and then a world war took priority over scholarly concerns. After the war there were many changes in the academic scene. As linguistics became an autonomous discipline its ties to anthropology weakened in a number of ways. Not all anthropologists were expected to be linguists some of the time, and linguists were doing specialized work beyond the capabilities of scholars lacking very intensive training. An

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thropology, meanwhile, expanded both in geographical area and in size of the profession. Amerindian studies could no longer be seen as the core of anthropology as they had been for the first half of the century. In the late 1940s and 1950s the "culture and personality" school associated with the work of Margaret Mead and Ruth Benedict in anthropology and the structuralist school of Leonard Bloomfield and his students in linguistics took positions opposed to Sapir's and temporarily dominated the fields in which he had principally worked.

Nonetheless, the continuities were there, and they have emerged in the responsiveness to Sapir common among students of the students of his students. In recent years scholars in both linguistics and anthropology have rediscovered the continuing relevance of his work. The centenary of Sapir's birth in 1984 produced a spate of Sapir scholarship, including several conferences and collections of papers, a biography by Regna Darnell, a reprinting of David Mandelbaum's (1949) *Selected Writings of Edward Sapir* in paperback, a reconstruction of *The Psychology of Culture* by Judith T. Irvine, and a plan by Mouton de Gruyter to publish a definitive collected works in sixteen volumes (six of which have now appeared) under the general editorship of Sapir's third son Philip.

There is probably no North American linguist or anthropologist today who does not respect, even revere, the name of Edward Sapir. He set a standard for the integration of disciplines—linguistics, anthropology, psychology, and the humanities. He wrote grammars of process rather than static formalism. He treasured the study of meaning and the myriad forms in which it could be expressed. His concept of human nature and communication, which included primary research in Amerindian, African, Indo-European, and Semitic languages, was sufficiently broad to encompass any and all

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human languages. These are ideas and approaches which have come full circle in the half century since Sapir's death.

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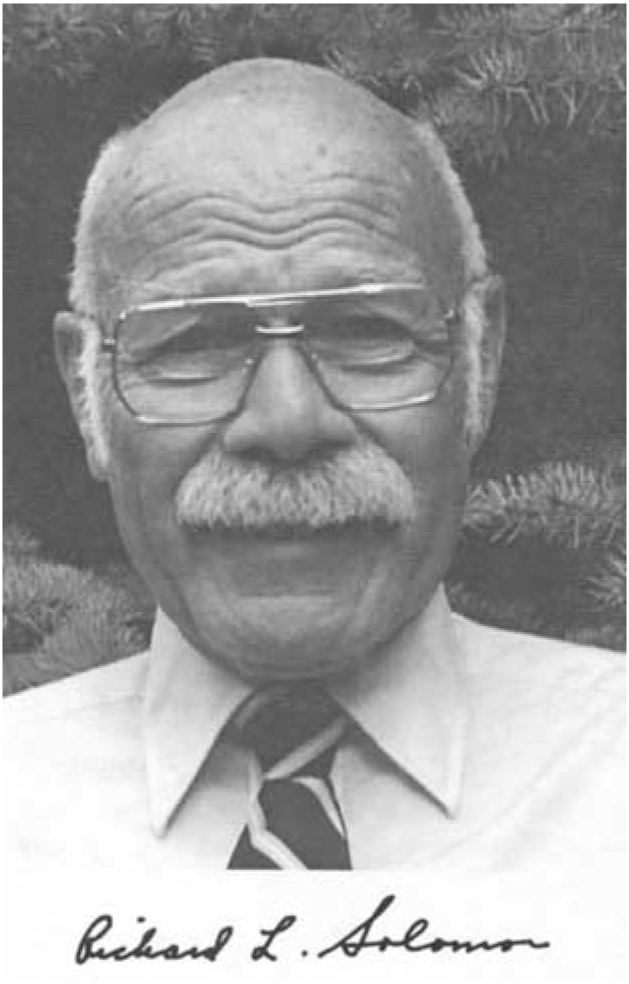


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# RICHARD LESTER SOLOMON

*October 2, 1918-October 12, 1995*

BY ROBERT A. RESCORLA

RICHARD LESTER SOLOMON was the complete university professor. He cared deeply about the creation and evaluation of ideas. He loved the process of sharing these ideas with his colleagues and students. And he glowed with enthusiasm when he had the opportunity to foster the development of ideas in others. Dick Solomon was an experimental psychologist whose research interests ranged broadly around the theme of learning and motivation. He made major contributions to many areas, but he is especially known for his work on avoidance learning and opponent-process theories of motivation. For that work he received a wide assortment of awards and honors. Of equal importance, especially to him, he trained a whole generation of research psychologists, literally populating an important subfield with most of its leaders. Most importantly of all, he was a warm and supportive person, whose affection and wisdom strengthened every person and institution with which he had contact.

Dick was born in Boston in 1918, into a family whose mother had high moral values and whose father was a hard driving CPA. He described his family life as orderly and intense, with an emphasis on manners, achievement, and

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personal responsibility. The home environment emphasized the importance of reading and debate.

He attended public schools in Newton and Brookline, graduating with a spotty grade record marked by high grades from teachers he liked and low grades from those he disliked. One especially well-liked teacher, Tyler B. Kepner, demanded analytic thinking in the context of teaching United States history. It was Kepner's encouragement, and his high recommendation, that was critical to Dick's applying to college and matriculating at Brown.

Although his high school interests had tended more towards the humanities, Dick was drawn to economics and psychology at Brown, eventually completing a joint major. He carried out an undergraduate honors thesis directed by Joseph McV. Hunt, which earned him a summa cum laude degree in 1940. His eventual decision to focus on psychology was heavily influenced by the quality of the members of the Brown psychology department at that time, people such as Walter Hunter, Harold Schlosberg, Donald Lindsley, Carl Pfaffmann, and Lorrin Riggs.

Dick elected to remain at Brown for his graduate training, working in the laboratory of Harold Schlosberg. His graduate career was interrupted by the Second World War, during which he served as a research psychologist in the Office of Scientific Research and Development. There he worked on perceptual-motor systems for the defensive weapons systems of the B-29 bomber. At the end of the war Dick returned to Brown where he received his Ph.D. in 1947.

In 1947 Dick took up an assistant professorship in social relations at Harvard. He remained at Harvard, becoming an associate professor in 1950 and a full professor in 1957. In 1960 he was recruited by Bob Bush to the newly emerging psychology department at the University of Pennsylvania. At Penn he became the first James M. Skinner Univer

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sity Professor of Psychology. Dick retired from that department in 1984.

Reflecting the times in which he was trained, Dick Solomon had wide ranging basic research interests within experimental psychology. But two themes run through this remarkably diverse research career: a repeated concern with improving the sophistication of experimental designs and a consistent desire that the research be brought to bear on applied psychological problems.

Much of Dick's earliest work dealt with the so-called “new look" in perception. In the late 1940s and early 1950s it was popular to suppose that personal motivational variables might produce distortions leading both to nonveridical perception of such object dimensions as size and to reductions in the likelihood of seeing unpleasant events. In the midst of a field full of injudicious claims based on uncertain methodology, Dick conducted careful systematic experiments exposing clear parametric relations. As a result of these experiments, many of the less cautious claims were put to rest.

Beginning in the early 1950s, Dick began the work for which he is perhaps best known, the systematic study of avoidance learning in dog subjects. Avoidance learning was a hot topic at that time, in part because of the puzzle about what maintained the behavior once it was acquired. In a typical experiment a dog was placed in a two-compartment shuttlebox. Its task was to jump a barrier in order to cross to the other side. A warning signal, such as a tone or light or the raising of a door separating the chambers, alerted the animal that it had a short period, such as ten seconds, in which to cross to the other side. Failure to cross within that period resulted in the application of electric shock to the grid floor of the chamber; that shock could only be terminated by crossing. However, crossing during the warn

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ing period avoided the shock altogether. Dogs readily learned such a task and would reliably execute the jumping response trial after trial without shock, once it was learned. For many, the puzzle was that the animal appeared to be rewarded by the failure of some event to occur.

Characteristically, there were three aspects to Dick's approach to this work. The first was careful parametric investigation of the determinants of avoidance learning. In an era of demonstration experiments, Dick and his students collected some of the first really systematic data on avoidance learning. The second was the development of a theoretical framework which would account for the behavior in all its richness. For this he turned to the two-process theory which was being developed by Miller, Mowrer, and others. He saw, perhaps more clearly that anyone else, that avoidance learning was the product of two learning processes: a classical conditioning process in which the warning signal became aversive by virtue of developing a Pavlovian association with the shock and an instrumental learning process in which the animal's jumping response was rewarded by the removal of that aversive warning signal. That theory remains even today as the core part of current explanations of avoidance behavior. Thirdly, Dick realized the important clinical applications of avoidance behavior, and its extreme resistance to being eliminated, for the understanding of such human pathologies such as phobias.

Out of this work on traumatic avoidance learning grew three other threads of Dick's work. The first was the development, with Lucy Turner, of the so-called transfer paradigm. In the course of their analysis of the Pavlovian basis of avoidance behavior, they developed a paradigm which has proved to be of immense power in the analysis of associative learning. They found that after dogs had been trained to make an avoidance response to one warning signal, other

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signals which had simple Pavlovian pairings with shock would also produce the avoidance behavior. Of special interest to Dick was the fact that this latter learning occurred even when the signal was paired with shock at a time when the animal was fully immobilized by curare. That transfer paradigm remains one of the major tools used today to identify Pavlovian and instrumental associations. In the course of developing that paradigm, Dick was extremely influential in helping the field work through one of its core distinctions, that between Pavlovian conditioning and instrumental learning. Second, while conducting transfer experiments, students in Dick's laboratory discovered the phenomenon of "learned helplessness" in which an animal that receives uncontrollable shocks subsequently has difficulty learning to avoid those shocks when given the chance. Again, the analysis involved careful parametric work, construction of theory, and attention to clinical application. Third, the study of avoidance naturally led to Dick's interest in a paradigm which is in some ways its complement, punishment. During the 1950s and 1960s a combination of political, scientific, and social attitudes conspired to popularize the view that punishment was an ineffective way to suppress behavior. Dick correctly saw that this was an absurd position and said so in his 1963 presidential address to the Eastern Psychological Association. The impact of that address was immense, leading many laboratories to take up the systematic investigation of punishment, greatly expanding our understanding.

Dick's final theoretical contribution was the development of a broad ranging theory of motivation, called the opponent-process theory. Building on ideas from perception, he developed a framework within which to examine strong emotional effects in terms of their initial consequences for the organism and the reactions that the organism gener

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ates to counter those consequences. This theory proved to have vast integrative power, bringing together ideas about such powerful human emotions as fear, love, and hope. In Dick's hands it also provided the means of understanding some important psychological aspects of drug addiction, participation in sports, and thrill-seeking of various sorts.

This array of important scientific work earned Dick just about every prize and honor that psychology has to offer. He was awarded the Distinguished Scientific Contribution award of the American Psychological Association and the Howard Crosby Medal of the Society of Experimental Psychologists. He was elected to the American Academy of Arts and Sciences and, in 1968, to the National Academy of Sciences. He held such honorific offices as president of the Experimental Division of the American Psychological Association, president of the Eastern Psychological Association, and chairman of the Governing Board of the Psychonomic Society. The University of Pennsylvania honored him as one of its University Professors, and Brown University bestowed on him an honorary doctorate. Because of wide respect others had for his thinking, he was asked to edit the field's most prestigious journal, the *Psychological Review.*

Influential as Dick was as a researcher, he was even more influential as a teacher and mentor. He had a huge educational impact on students at all levels. Undergraduates flocked to his classes, attracted by his enthusiastic and articulate lecture style. He was one of those teachers students remember decades later. His training of graduate students is legendary. Both at Harvard and Penn he attracted the brightest and best graduate students and gave them a training which made them the leaders in the field of elementary learning processes. In 1983 many of the thirty-two students he trained, together with colleagues he influenced at Penn and Harvard, gathered in a two-day celebration of Dick's

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career. Uniformly, they recalled the combination of intellectual excitement and personal support which Dick conveyed. Every one of them spoke of Dick's commitment to fostering their intellectual growth and helping them to become independent thinkers and scientists. Dick had a way of creating a setting, providing resources, subtly affecting your thinking, and then standing back while you grew.

Each student had a story about how Dick had placed his students' careers first, often potentially sacrificing his own. My own experience is typical. While I was a graduate student, Dick and I were writing what we both knew would be an important theoretical paper on two-process theory. As we handed the drafts back and forth, something peculiar kept happening: the order of authorship kept changing. I would give him a draft with him as the (proper) first author and when it came back from him my own name was placed first. Thinking that it was a clerical error, I told Dick that he needed to speak to his secretary so that she got it right. I still recall his telling me, "She does have it right. I have plenty of publications and an established career, but you are just beginning. You need the authorship much more than I." It was this attitude that resulted in dozens of publications coming out of Dick's lab without his name ever being listed as an author. So unusual was his generosity that his grant applications had to have a separate section listing the publications of his students that resulted from earlier funding; his own bibliography reflected only a small portion of the work.

Anyone who passed through the University of Pennsylvania psychology department in the 1960s, 1970s, and 1980s heard of Dick's research seminar, the weekly meeting of his students. This exciting discussion was frequently attended by graduate students and faculty from other labs. It formed the core of the graduate education for dozens of psycholo

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gists. The interactions were broad-ranging and the arguments often heated. But no matter what the topic, Dick had a way of finding the essential and good ideas in what everyone said.

Just as the field honored Dick for his research contributions, it acknowledged his educational role. He was awarded Sigma Xi's Montie A. Ferst Award for ". .. notable contributions to motivation and encouragement of research through teaching" and the American Psychological Foundation's Award for Distinguished Teaching. He is one of the few people to have won the American Psychological Association's primary awards both for distinguished teaching and distinguished scientific contribution.

Dick served as a role model not only for his students but also for dozens of professional colleagues. Although he never accepted a major administrative position, he was the acknowledged intellectual and moral leader of the Penn psychology department. His commitment to high intellectual standards, combined with his fondness for others and his gentlemanly manner, made his opinion the most valued in any discussion of policy. The tone of civility that he established allowed even the most potentially explosive of issues to be debated openly and frankly. I never knew anyone to attribute to Dick any motives other than the good of the department and the science.

When Dick retired in 1984, he moved to North Conway, New Hampshire. There he continued to pursue vigorously his outdoor interests in hiking and canoeing. He also continued his role as a mentor, actively encouraging the members of the White Mountain Miler's, a local running club in which his wife Maggie was active. When I visited him, I would frequently be taken aside by members of the community to be told of his wonderful contributions to their lives. When he died in 1995, over two hundred people from

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the community attended the memorial service. With the exceptions of Maggie, his daughters, Janet and Elizabeth, and his brother, David, those present knew little of his scientific contributions. But they had been touched by the same qualities of personal warmth, enthusiasm, and supportiveness that had so guided his professional research and teaching career. In 1996 the University of Pennsylvania renamed its psychology building as the Richard L. Solomon Laboratory of Experimental Psychology. This will memorialize his scientific contributions. But no building name can capture his human qualities.

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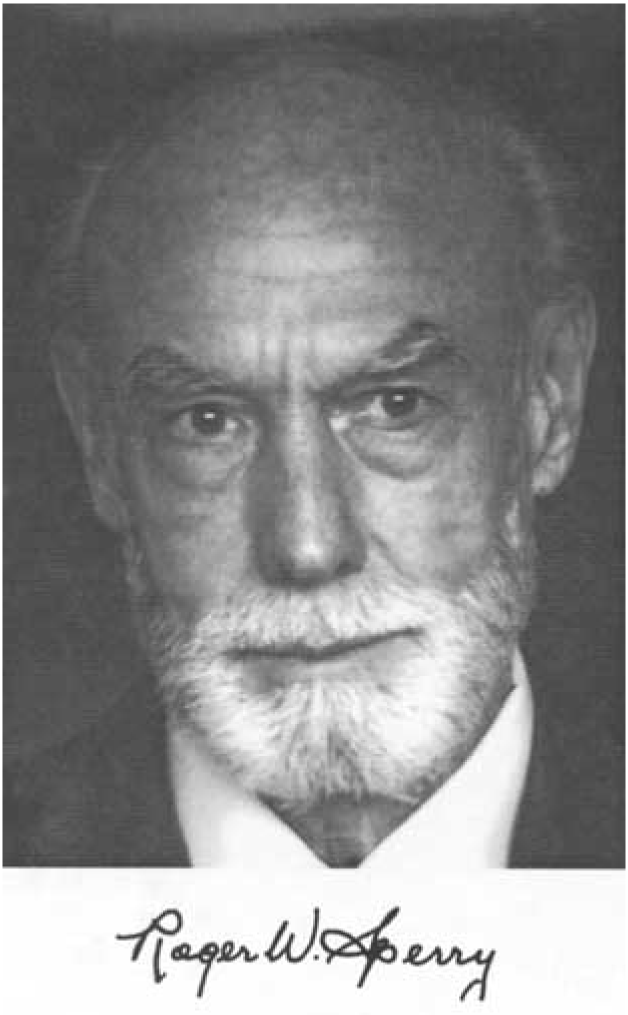


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# ROGER WOLCOTT SPERRY

*August 20, 1913-April 17, 1994*

BY THEODORE J. VONEIDA

WHERE HERE DOES behavior come from? What is the purpose VV of consciousness?”

Questions such as these, which appeared on the first page of Sperry's class notes in a freshman psychology course at Oberlin College, represent an accurate preview of a career that included major contributions to fundamental issues in neurobiology, psychology, and philosophy. Indeed, his first paper, published in the *Journal of General Psychology* in 1939, entitled "Action Current Study in Movement Coordination," begins: "The objective psychologist, hoping to get at the physiological side of behavior, is apt to plunge immediately into neurology trying to correlate brain activity with modes of experience," and continues, setting the stage for much that was to follow: "The result in many cases only accentuates the gap between the total experience as studied by the psychologist and neural activity as analyzed by the neurologist."

Roger Sperry was born in Hartford, Connecticut, and spent his early years on a nearby farm, where he developed a lifelong interest in nature. After the death of his father, the family moved to West Hartford, where he attended high school and established an all-state record in the javelin throw.

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Sperry was accepted to Oberlin College under a full academic scholarship, earning his board by waiting tables. He maintained an active interest in sports and was elected captain of the varsity basketball team, while receiving varsity letters as well in baseball and track. As an undergraduate he attended R. H. Stetson's course, "Introduction to Psychology." It was during a lecture by Stetson in this course that Sperry got the idea for a paper he published some twenty years later entitled "On the Neural Basis of the Conditioned Response" (1955). This short paper carries powerful theoretical implications for those interested in central nervous pathways in conditioned learning. Sperry remained at Oberlin College, in Stetson's laboratory, through 1937, when he received his M.A. in psychology.

Sperry took his Ph.D. in Zoology from the University of Chicago in 1941, under the tutelage of the renowned neuroscientist Paul Weiss. During that period, in addition to developing highly skilled neurosurgical techniques, he made the first of what was to become a number of successful challenges to existing concepts related to neuronal specificity and brain circuitry. In a series of carefully controlled and clearly written publications between 1941 and 1946, Sperry conclusively demonstrated that the rat's motor system was "hard wired" and unmodifiable (following transplants) by training. This work clearly established that the basic circuitry of the mammalian central nervous system is largely hard wired for specific functions and seriously challenged Weiss's "resonance principle" and "impulse specificity theory."

These studies were to have an impact on human neurosurgery as well. From 1942 to 1945, during his military service with the Office of Scientific Research and Development, Nerve Injury Project, Sperry's work, along with that of Weiss and others, resulted in a major change in the sur

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gical management of nerve-damaged soldiers. It was common practice until that time to surgically transplant nerves to antagonistic muscle groups and then to subject the recipient to intense retraining, with the goal of regaining normal function. The demonstration that the basic structure of the mammalian central nervous system is hard wired, and unmodifiable by training, resulted in significant modifications of treatment protocols.

During Sperry's postdoctoral years with Karl S. Lashley at Harvard and at the Yerkes Laboratories of Primate Biology in Orange Park, Florida, he continued the work on neuronal specificity that he had begun as a doctoral student and initiated a new series of studies on the role of electrical fields in neocortical functioning. It was also during this period that he performed a series of brilliant experiments involving the rotation of eyes in amphibians. The optic nerves were sectioned and the eyes rotated 180 degrees. The question was whether vision would be normal after regeneration or would the animal forever view the world as "upside down" and right-left reversed. Should the latter prove to be the case, it would mean that the nerves were somehow "guided" back to their original sites of termination. Restoration of normal vision (i.e., "seeing" the world in a "rightside-up" orientation) would mean that the regenerating nerves had terminated in *new* sites, quite different from the original ones. The answer was unequivocal. The animals reacted as though the world was upside down and reversed from right to left. Furthermore, no amount of training could change the response. These studies, which provided strong evidence for nerve guidance by “intricate chemical codes under genetic control" (1963) culminated in Sperry's chemoaffinity theory (1951).

Sperry later confirmed anatomically his behavioral studies with amphibia in a series of papers published between

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1952 and 1964, on nerve-muscle and retino-tectal regeneration in fish. These experiments laid the foundation for many of our present-day views about neuronal specificity in brain development. While a number of recent studies have challenged the chemoaffinity theory, it still stands as "one of the most profound insights in developmental neurobiology.[1] Thus, through an ingenious combination of behavioral and anatomic approaches, Sperry related the functional interconnection of neuronal elements to developmental principles of differentiation, cellular interaction, cytochemistry, and genetics. It was primarily this work, begun as a predoctoral student in 1938 and pursued through the early 1960s that led to his election to the National Academy of Sciences in 1960.

Sperry's reason for choosing Lashley as a postdoctoral mentor is not entirely clear, but reflects his interest in Lashley's principle of equipotentiality. Sperry was uncomfortable with the idea that electrical fields or waves acting in a volume conductor were critical for neocortical processing. His first study to challenge this concept was published in 1947. Here he demonstrated that motor coordination in monkeys remained virtually unaffected after multiple transections of sensorimotor cortex. Later, in a series of papers with Miner, Myers, and Zartman, he confirmed this point by demonstrating that neither subpial slicing, the insertion of numerous short-circuiting tantalum wires, or insulating mica plates into the cortex had any adverse effect on cortical function. These studies demonstrated that perception depends on vertically oriented afferent and efferent cortical axons, predating Mountcastle's discovery of vertically oriented cortical columns. Sperry's premise was based on his keen understanding of neuroanatomy and neurophysiology, including the work of Santiago Ramon y Cajal and Lorente de Nó, both of whom had demonstrated the

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importance of radial cortical connections to cortical function. In these few carefully conducted experiments, Sperry once again upset two major theories of brain function—the gestalt electric field theory of perception and the reduplicated interference pattern hypothesis. Indeed, when the renowned neuroembryologist Viktor Hamburger presented Sperry with the Ralph Gerard Award from the Society of Neuroscience in 1979, he proclaimed: "I know of nobody else who has disposed of cherished ideas of both his doctoral and his postdoctoral sponsor, both at that time the acknowledged leaders in their fields."

It was during this postdoctoral period that Sperry began thinking about the functions of the corpus callosum. The function of this "great cerebral commissure," which represents the major set of connections between the two cerebral hemispheres, had remained a mystery to neurobiologists. Some even joked about it, possibly out of embarrassment, for very little was known of its function at the time. Lashley, for example, is said to have remarked that its major function may be to mediate epileptic seizure activity from one hemisphere to the other; Warren McCulloch quipped that it may simply be there to keep the two hemispheres from falling into each other. The mystery of the corpus callosum continued to absorb Sperry, and shortly after moving to the Department of Anatomy at the University of Chicago in 1946, he began to examine this problem.

Sperry remained at Chicago through 1953, during which time several momentous events took place in his life. On December 28, 1949, he and Norma Deupree were married in Wichita, Kansas. Norma was to become his lifelong collaborator and mother of two children, Glenn Tad and Jan Hope. In 1949 Sperry contracted tuberculosis from a monkey he had been dissecting in order to obtain tissues for nerve transplants. The diagnosis was made during a rou

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tine physical examination, the initial results of which declared him in excellent health (Norma Sperry, personal communication, 1995). Chest X rays and further tests, however, confirmed the diagnosis, and the Sperrys began a sabbatical leave at Saranac Lake in upstate New York, for a period of rest and recovery. Norma relates that while there was very little rest, there was a great deal of fishing, swimming, hiking, and writing. In six months Sperry was given a clean bill of health, and he and Norma spent the remainder of his sabbatical year at the Marine Biology Laboratory in Coral Gables, Florida.

Sperry returned to Chicago and became associate professor of psychology in 1952, a position he held concurrently with his position as section chief of neurological diseases and blindness at the National Institutes of Health. The first published description of his studies on callosal function appeared as an abstract in 1953, in collaboration with his doctoral student, Ronald Myers. Plans to move to Bethesda, Maryland, were postponed by a delay in building construction at NIH, during which time Sperry was offered the prestigious Hixon Professorship of Psychobiology at the California Institute of Technology, a position he began in 1954.

During the next four decades, a very large number of students and visiting scholars were to study in Sperry's laboratory. I first met Roger during the summer of 1958, when I spent several months with him as a visiting graduate student from Professor Marcus Singer's laboratory at Cornell. Singer and Sperry generously shared the cost of my visit, and I was able to take my wife and young daughter along for a summer that was to have a profound effect on my career. Sperry was, among other things, an outstanding neuroanatomist, and we hit it off immediately. My work on central nervous substrates of conditioned learning began during the summer of 1958 and continued in his labora

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tory through September 1962. My work continues to this day and remains strongly influenced by the impact of Sperry's thinking.

Sperry was a great teacher, but not in the conventional method of lecturing to students about factual material. His style involved one-on-one discussions, exchanging ideas, and providing insightful critiques of proposals. He once told me, during a discussion about a research idea, that I should "write it up, as if you have completed the study." I was rather surprised by this, but he went on to explain that by writing the Introduction, I would be forced to not only critically review the literature but also consolidate my ideas. "Materials and Methods" would tell me exactly what I would need to carry it out, and "you pretty well know that the results will turn out one way or another, so you should write it up both ways." "Finally," he said “the Discussion section will assist you in critiquing your results, whatever they are. By the time you get that done, you will know whether it's worthwhile to embark upon the study." Then he said with a broad grin: "And you will already have the paper written."

Sperry's laboratory in the Division of Biology at Caltech also became a center for many new studies on nerve regeneration in fish and amphibia, reinforcing his earlier work on chemoaffinity and genetic control as major factors in neural development. His interests in learning began to take full form during the early years of this period, and in 1955 he published a short provocative paper on the nature of the conditioned response, in which he emphasized the role of transitory facilitatory motor sets and "perceptual expectancy" that continues to have a profound effect on work in this area. It was also at Caltech where Sperry began to develop, along with a growing number of graduate students, postdoctoral fellows, and visiting scientists, his "split-brain"

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experiments, in which the two brain halves are separated by midline section of forebrain and midbrain commissues. These studies elegantly elucidated some of the major functions of the corpus callosum in interhemispheric memory transfer and eye-hand coordination. Restriction of sensory input to one brain half in commissurotomized animals was shown to limit the learning of various tasks to that hemisphere; the opposite side was capable of learning but remained naive to those tasks until trained. Learning curves for each hemisphere were virtually identical; it was as if two separate brains were housed within a single cranial vault.

A large number of experiments were carried out by Sperry and his students during the late 1950s and early 1960s, all based on the possibilities suggested by the split-brain preparation. Sperry was very generous about sharing authorship. He insisted on being second or third author on much of the work published with his students. When I once suggested that he should be first author on a study that we had worked on together, he said he would prefer that I be sole author, but if I felt that it might help to have his name on the paper, he would be second author. I remained sole author on most of the work I performed in his laboratory because Sperry felt that it would help my own career more that way. This was very typical of his attitude toward authorship when he felt that another had done the bulk of the work in the area, even though he had made important contributions to it. He was a fair and generous person in all of his interactions with others.

In 1960 Dr. Joseph Bogen, who had been doing research in the Biology Division at Caltech, suggested that the split-brain work might be extended to humans because earlier studies by Van Wagenen, Akelitis, and others had suggested that commissurotomy was efficacious in the treatment of epilepsy. Commissurotomy was known to have little effect

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on general levels of intelligence and motor coordination, and it was felt that this operation might not only reduce seizures but also prevent their propagation, with little or no severe side effects. The opportunity arose in 1962, when a World War II veteran with progressively worsening seizures (up to twenty per day), underwent a callosalectomy by Drs. Philip Vogel and Joseph Bogen. The operation was successful, and there was a dramatic reduction in the number and severity of the patient's seizures.

Sperry, along with Bogen and Sperry's graduate students, Colwyn Trevarthen and Michael Gazzaniga, then began a series of tests directed at understanding the effects of commissurotomy on human perception, speech, and motor control. The work on humans allowed investigators to compare cognitive abilities between the two separated brain halves, demonstrating differences theretofore unrecognized. The left brain half, for example, was found to be superior to the right in tasks involving analytical, sequential, and linguistic processing; the right performed better in wholistic, parallel, and spatial abilities.

For the next twenty years the work of Sperry and his collaborators revolutionized our understanding of brain function. They elucidated the unique capabilities of each hemisphere and demonstrated that the combined effect of bihemispheric activity amounted to more than the simple additive effects of the two separate hemispheres. Sperry's brilliant studies on the functional specialization of the cerebral hemispheres won him a share of the 1981 Nobel prize for physiology or medicine.

Far from resting on his laurels, Sperry left others to continue the examination of right-hemisphere/left-hemisphere functions and moved forward to explore the emergence of consciousness from the unified brain. His first major paper on the topic of the mind and consciousness appeared in

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1965 and was only the beginning of many more to follow over the next thirty years. He had actually broached this issue as early as 1959 as part of a discussion at a Josiah Macy conference on the central nervous system and behavior, where he stated:

> I have never been entirely satisfied with the materialistic or behavioristic thesis that a complete explanation of brain function is possible in purely objective terms with no reference whatever to subjective experience; i.e., that in scientific analysis we can confidently and advantageously disregard the subjective properties of the brain process. I do not mean we should abandon the objective approach or repeat the errors of the earlier introspective era. It is just that I find it difficult to believe that the sensations and other subjective experiences *per se* serve no function, have no operational value and no place in our working models of the brain.

In his 1965 paper entitled "Mind, Brain and Humanist Values," Sperry proposed that subjective experience plays a principal role in brain function. He posited that behaviorism and reductionism must both be replaced by a new concept of consciousness, based on the ideas of emergence and downward causation. The concept of emergence, according to Sperry, "occurs whenever the interaction between 2 or more entities, be they subparticles, atoms or molecules, creates a new entity with new laws and properties formerly nonexistent in the universe." He notes the parallel with quantum physics in which "interactions among subatomic particles result in emergent properties which in no way resemble the particles from which they arose." It is important to emphasize that Sperry did *not* see this as dualism, which treats the mind as a separate entity outside the brain that is capable of existing independently of it. Nor did he accept the term "psychophysical interaction," suggested by Popper and Eccles in 1977. Sperry pointed out in "Holding Course Amid Shifting Paradigms" (1994) that the erroneous classification of this conception is probably based on an earlier

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terminology in which "mentalism" was equated with dualism. He describes his reasons for retaining the term mentalism in preference to Bunge's (1977) "emergent materialism" or Natsoulas's (1987) "physical monism," emphasizing that this new form of mentalism must be viewed as a "quite different intermediate position which is monistic, not dualistic."

Thus, consciousness, in Sperry's view, while generated by and dependent on neural activity, is nonetheless separate from it. Consciousness emerges from the activity of cerebral networks as an independent entity. This newly emerged property, which we call "mind" or "consciousness," continually feeds back to the central nervous system, resulting in a highly dynamic process of emergence, feedback (downward causation), newly emergent states, further feedback, and so forth. Reducing consciousness to its separate components obliterates the emergent phenomenon of "mind" with all its great power and uniqueness.

Sperry elevated this concept of emergence from the individual to the global level, stating that "the new paradigm affirms that the world we live in is driven not solely by mindless physical forces but, more crucially, by subjective human values. Human values become the underlying key to world change" (1972). He contended that this view, integrating macro- and microdeterminism with the causal reality of mental states is a more valid foundation for all science, not just psychology, with "endless humanistic implications for philosophy, religion and human values (1993). By introducing the issue of human values, Sperry moved beyond the specifics of mind and consciousness to urge that these very unique and powerful forces be directed toward improving and preserving the quality of life on our planet, rather than the reverse. He made a strong appeal, especially to his scientific colleagues, to turn their efforts

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toward these goals. His message began, finally, to be heard by the scientific community. In response, and under the able leadership of his long-term friend and colleague, Dr. Rita Levi-Montalcini, an international conference was convened at the University of Trieste in November 1992 to discuss these ideas in greater detail. The plan was to work toward the creation of a strong statement of human duties, generated by the scientific community, but speaking to every "mind" willing to listen. This might represent a corollary to the United Nations's Declaration of Human Rights. The first meeting of the group, which, unfortunately, Sperry. was not able to attend, included ten Nobel laureates and numerous others, representing such widespread disciplines as neurobiology, chemistry, physics, economics, and theology. After much discussion, a draft version of "The Magna Carta of Human Duties" was generated, with an agreement to continue discussion the following year. In November 1993 a near-final draft was completed, and after circulation to all participating members for ratification, the final version, entitled, "A Declaration of Human Duties," was agreed on in 1994. The document was forwarded to the United Nations, where it is presently under review and consideration.

A second series of conferences inspired by Sperry's ideas on the mind and human values was organized by Professor Kaoro Yamaguchi. The long-term goal of these conferences (there have been four to date), held on the island of Awaji, is to work toward establishing an International Network University of the Green World, dedicated to the continuing study of human values.

Sperry's thinking about subjective experience, consciousness, the mind, and human values makes a powerful plea for a new scientific examination of ethics in the workings of consciousness. These ideas were crystallized in his paper 'The impact and promise of the cognitive revolution" (1993),

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which I had the honor of delivering for him at the centennial meeting of the American Psychological Association. It was his great hope and sincere belief that if we humans could simply be persuaded to put our collective minds together and use the enormous emergent powers that they are capable of generating, we would not merely improve the quality of life on the planet, we would ensure our very survival.

Finally, on a personal note, Roger and I remained close friends and correspondents from the time I left Caltech until his death in 1994. My wife and I were frequent recipients of the Sperrys' warm hospitality, and I last visited him in October of 1993. Roger, Norma, and I enjoyed an evening and breakfast together at Sperry's home in Pasadena, discussing the forthcoming Trieste and Japan conferences on human values. During the many years of our friendship, I came to appreciate his quiet, thoughtful manner and to respect his insightful comments, high ethical standards, deep love of science, and wry sense of humor. Though a rather private person, preferring the quiet beauty of remote places to large crowds, he was known during the early 1960s for his delightful parties, with good conversation, dancing, and his special "split-brain" punch. His interests were seemingly unlimited. Along with Norma and his two children, he searched for giant ammonites and dinosaur bones in the Southwest. The family also shared numerous adventures in Baja, California, camping on remote beaches and fishing from a 12-foot rubber boat with homemade lures. On one occasion he hooked a 14-foot marlin, which towed the boat for a considerable distance. He instructed Norma to 'just keep snapping pictures." She did and took a prize-quality photo of the entire fish in midair, with foaming water flying in all directions. I asked him later what happened to the fish. "I cut it loose, of course," he said quietly, looking straight

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into my eyes. "What in the world would I have ever done with a 14-foot marlin?" My wife and I recently purchased the Sperrys' 1986 camper truck with extra-wide tires and plan similar trips to Baja. We continue to discover ingenious little space-saving devices in the camper, ranging from refrigerator doorguards to fold-out shelves; wonderful reminders of the quiet, practical man who installed them. Roger was also a highly talented sculptor, artist, and ceramicist. The Sperry home is filled with busts of his family, paintings, and other items attesting to Roger's combined artistic and scientific creativity.

This brief review is inadequate to describe Roger Wolcott Sperry's multiple talents and inspired contributions to science, art, and philosophy. I have tried to provide a few examples of the far-reaching, global effects that emerged from the synaptic interactions, transmitters, and circuitry of his brain. No doubt these were quite similar to most other brains. But the *mind* that emerged from those interactions was truly unique, for it not only stimulated and inspired his students, colleagues, and friends, it has stirred as well the minds of thousands of others and will continue to excite and inspire new thinking from generations of minds yet to emerge.

Scientist, teacher, philosopher, humanist—Roger Sperry has left us a rich legacy of ideas and a challenge to foster the emergence of new understandings of human capabilities and responsibilities.

I WANT TO THANK Norma Sperry, not only for her help in providing personal information about Roger, but also for reading and commenting on drafts of this memoir.

## NOTE

1. W. A. Harris and C. E. Holt. From tags to rags: chemoaffinity

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# DEWITT STETTEN, JR.

*May 31, 1909-August 28, 1990*

BY J. EDWIN SEEGMILLER

DEWITT STETTEN, JR., made significant contributions to science as a biochemist, as an unusually talented mentor in the training of young scientists, and as an inspiring and dedicated administrator of educational and research programs. Through his pioneering use of heavy isotopes as labels of various molecules, Stetten was one of the first investigators to obtain quantitative assessment of the dynamic relationship of interconversions between various molecules in intact biological systems. He extended these studies to include assessment of aberrations of relevant metabolic pathways in human diseases, including the first demonstration of an impaired synthesis of fatty acids in patients with diabetes and the causes of hyperuricemia in patients with gouty arthritis. In addition, Stetten's standards of integrity, his contagious excitement about science and life, his honesty, his warm personal concern for his friends, and his pervasive humor are well remembered by his many scientific associates and personal friends.

## EARLY LIFE AND SCHOOLING

DeWitt Stetten,Jr., was born in New York City on May 31, 1909, to DeWitt Stetten, Sr., a prominent young surgeon,

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and his wife, Magdalena Ernst Stetten. A German nursemaid caring for his older sister Margaret disliked the name DeWitt and so she gave Stetten the affectionate name of “Haensel" and Margaret the German contraction "Gretel." Thus, Stetten was known as "Hans" to all visitors, and the name persisted among family and friends throughout his life.

As children, Stetten and his sister attended the Horace Mann School, an experimental school associated with Columbia University in New York City. While still a young boy in the Horace Mann Boy's School, Stetten's parents sought to help him overcome his excessive shyness by arranging for him to receive special instruction in performing magic from one of his teachers who had this as a hobby. He then entertained family and friends, as well as audiences on steamships during family vacation travels to and from Europe, as an amateur magician. The same teacher also taught a woodworking class at school—thus started two of Hans's lifelong hobbies.

Another hobby was his construction at age 12 of a radio receiver using a galena crystal as a detector and a coil of wire wrapped around a wooden rolling pin as a tuner—all from instructions he found in the journal *QST,* the official publication of the American Radio Relay League. Stetten then progressively increased the performance of succeeding radios over the years as vacuum tubes became available. His resulting hobby of electronics was helpful to him later when he participated in the construction of two mass spectrometers used at different times in his research career.

Stetten received an A.B. degree magna cum laude from Harvard College in 1930, along with membership in Phi Beta Kappa. Although he knew by this time that his first love was biochemistry, he was encouraged by his surgeon father as well as by his tutor and mentor at Harvard, Frank

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Fremont-Smith, to attend medical school before embarking on a laboratory career. He therefore received his M.D. in 1934 from the College of Physicians and Surgeons of Columbia University.

## EARLY RESEARCH TRAINING

After his internship and residency at Bellevue Hospital in New York City, Hans was convinced that, even with the best of medical care then available, the effectiveness of treatment was severely restricted by lack of knowledge of the normal human body and the changes induced in it by each disease process. Some years earlier Hans had spent part of a summer vacation in the laboratory of Dr. Rudolf Schoenheimer in Freiburg, Germany, and had become fascinated with Schoenheimer's brilliant mind and his approach to science. Since Dr. Schoenheimer had recently arrived on the faculty at Columbia University as a refugee from Germany, Stetten selected him as his mentor for his Ph.D. degree in biochemistry, which was awarded in 1940. For his dissertation, published with Schoenheimer, Stetten used the then newly developed technique of isotopic labeling with deuterium to follow the biological conversion of labeled palmitic acid to stearic and palmitoleic acid and the conversion of aliphatic alcohols to fatty acids in intact rats, published in two papers in *the Journal of Biological Chemistry.* He then continued his research by studying the origins of the extra fat in various types of fatty liver using 15N labeling to show the dynamic relationships of choline, ethanolamine, and related compounds, as well as the role of lipotrophic methyl groups in the synthesis of choline in rats. It was in 1941 that he married fellow graduate student Marjorie Roloff, known as Marney, and thus began both a scientific and a domestic partnership of over four decades, until her sudden death from a heart attack in 1983. They had four chil

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dren—Gail, Nancy, Mary, and George. A year later at Woods Hole, Massachusetts, where their two families had spent many summer vacations as friends, Stetten married Jane Lazarow, widow of Dr. Arnold Lazarow.

## ACCOMPLISHMENTS IN BIOMEDICAL RESEARCH

Stetten was a superb teacher with a contagious enthusiasm for scientific research. From 1938 to 1947 he taught biochemistry and continued his research at Columbia University. One of his early students in research was Juan Salcedo, who on returning to his homeland eliminated the nutritional disease beri beri from Bataan province and subsequently held many high scientific offices in the Philippines. During a subsequent two years of teaching and research at Harvard University's Peter Bent Brigham Hospital, Stetten taught a course on the clinical aspects of biochemistry. One of his students was an undergraduate, Gordon Tompkins, who credited Stetten's course as being the decisive event contributing to his own decision to make a career in biochemistry and medicine, eventually becoming chief of the Laboratory of Molecular Biology at the National Institute of Arthritis and Metabolic Diseases, from which he became a professor of biochemistry and biophysics at the University of California, San Francisco. Stetten's research on gouty arthritis began in Boston with his use of heavy isotope labeling of uric acid to determine the pool size and turnover of uric acid in gout.

In 1948 Stetten moved back to New York for his appointment as chief of the Division of Nutrition and Physiology of the Public Health Research Institute of the City of New York. He described this six-year period as one of the most productive times of his life. Among the bright young physicians attracted to his laboratory as a postdoctoral fellow was James Wyngaarden, later a director of the National Insti

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tutes of Health. Among the achievements of this general period was Stetten's demonstration of the marked expansion above normal of the miscible pool of uric acid in gouty humans and the role of overproduction of uric acid as the cause of hyperuricemia in at least a portion of gouty patients. For this and other major contributions of his laboratory during this period, Stetten was honored with election to the National Academy of Sciences.

In 1954 Stetten was appointed associate director in charge of the intramural program of research at the National Institute of Arthritis and Metabolic Diseases of NIH. During his eight years in this position, he recruited a number of outstanding young scientists to the program, including Marshall W. Nirenberg, who became the intramural program's first Nobel laureate for his identification of the DNA code for specific amino acids. Stetten was also coauthor of the first two editions of *Principles of Biochemistry* with Abraham White, Phillip Handler, and Emil L. Smith, which was designed for graduate and medical students and widely adopted as the standard textbook of biochemistry. The authors spent summer vacations together in Woods Hole, Massachusetts, for the book's development and revisions. Stetten felt strongly the importance of formal teaching programs as an important symbiotic component of an optimal research environment. He therefore worked with other NIH researchers of like minds to aid in the establishment of the Foundation for Education in the Sciences, a nongovernmental teaching institution located adjacent to the NIH campus, and later served as its president.

## DEAN OF RUTGERS UNIVERSITY MEDICAL SCHOOL

In 1962 Stetten left NIH to serve as the founding dean of the Rutgers University School of Medicine soon to be launched in Nutley, New Jersey. He used his considerable

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skills as a negotiator to help establish a first-class medical school, complete with a strong program, excellent faculty, and a handsome medical sciences building. After much hard work on Stetten's part, the two-year medical school was up and running and the four-year medical school was ready to be launched; however, the state legislature decided to separate the medical school from the university in order to integrate it with the recently acquired Seton Hall Medical School. The merger was opposed by faculty, students, and administrators alike, but they were overruled by the legislature. When the merger took place, Stetten tendered his resignation.

## NIH ADMINISTRATION

Stetten returned to Bethesda, Maryland, in 1970 to head the National Institute of General Medical Sciences. From 1974 to 1979 he was NIH's deputy director for science, guiding the intramural research activities of a vast number of researchers. During this time he also served as chairman of the recombinant DNA advisory committee. In response to concerns in the scientific community about potential dangers in biotechnology research, Stetten's committee drafted guidelines for scientists using the new techniques.

In 1978 Stetten asked to be relieved of his duties as deputy director because of his deteriorating eyesight, caused by macular degeneration. Donald Fredrickson, then NIH director, appointed him senior scientific advisor to the director. From an office in Stone House, Stetten took up a number of new projects. He wrote a widely cited letter to the editor of the *New England Journal of Medicine* suggesting that ophthalmologists learn more about advising their visually handicapped patients on services available for the blind. Some years later the Library of Congress recognized Stetten's efforts on behalf of blind and low-vision people by asking

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him to pose for a poster promoting its Talking Books program.

At this time, in 1979, Stetten found the opportunity to do something about his long-held perception of the principal drawback facing each scientific director upon moving to the administration building: isolation from the research he hoped to foster. To counter this, Stetten established a Friday morning seminar series that became an honored forum for scientists to tell about their laboratory and clinical research activities. He also initiated the Museum of Medical Research, which now bears his name, during NIH's centennial observance. In the museum's collection is a gavel made by Stetten for NIH Director Robert Q. Marston and passed to his successors. An avid woodworker, Stetten made the gavel on his own lathe. For the head he used wood from the plane tree, found on the Aegean island of Cos and associated with Hippocrates, the father of medicine. The handle was made of all-American cherry wood. In another project, carried out in collaboration with William T. Carrigan, Stetten edited a book on the NIH intramural program, *NIH: An Account of Research in Its Laboratories and Clinics.*

## LEADERSHIP POSITIONS

Always in demand as a leader, Stetten served as chairman of the Roche Institute of Molecular Biology's Scientific Advisory Committee from 1966 to 1970. In 1971 he was president of the Foundation for Advanced Education in the Sciences and from 1977 to 1979 headed the Society for Experimental Biology and Medicine.

Long before his election to the National Academy of Sciences in 1974, Stetten served on many of the Academy's advisory committees. After his election, he served on the Academy's council, was a member at large for the Division of Medical Sciences, and served three terms on the Execu

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tive Committee. He also served two terms as a representative of the American Society of Biological Chemists.

This long and event-filled list of accomplishments cannot convey Hans Stetten's humor, his care for and facility with language, and his refusal to be slowed down in his later years even by blindness. He was an amateur magician and a professional mediator, whose ability to guide and smooth the way brought him many positions of leadership during his sixty years in the service of science.

## HONORS AND DISTINCTIONS

| EDUCATION | |
|---|---|
| 1930 | A.B., Harvard College |
| 1934 | M.D., Columbia University |
| 1940 | Ph.D. (Biochemistry), Columbia University |

| SPECIAL TRAINING OR EXPERIENCE | |
|---|---|
| 1934-37 | Intern, Resident, Bellevue Hospital, New York City |
| 1947-49 | Associate in medicine, Peter Bent Brigham Hospital, Boston |
| 1952-53 | Study Section on Nutrition and Metabolism, National Institutes of Health, Public Health Service, Bethesda, Md. |

| EMPLOYMENT | |
|---|---|
| 1938-47 | Assistant, Instructor, Assistant Professor, Department of Biochemistry, Columbia University |
| 1947-48 | Assistant Professor, Department of Biochemistry, Harvard University |
| 1948-54 | Chief, Division of Nutrition and Physiology, Public Health Research Institute of the City of New York |
| 1954-62 | Associate Director in Charge of Research and Chief, Section on Intermediary Metabolism, National Institute of Arthritis and Metabolic Diseases, National Institutes of Health, Bethesda, Md. |

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| | |
|---|---|
| 1962-70 | Dean, Rutgers University School of Medicine, and Director, Medical Center |
| 1970-74 | Director, National Institute of General Medical Science, National Institutes of Health |
| 1974-79 | Deputy Director for Science, National Institutes of Health |
| 1979-85 | Senior Scientific Advisor, Office of the Director, National Institutes of Health |

MEMBERSHIPS

American Academy of Arts and Sciences
American Association for the Advancement of Science (fellow);
Vice-President, 1962
National Academy of Sciences, 1974
American Society of Biological Chemists
Harvey Society
Phi Beta Kappa
Alpha Omega Alpha
Sigma Xi

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# JABEZ CURRY STREET

*May 5, 1906-November 7, 1989*

BY K. T. BAINBRIDGE, E. M. PURCELL, N. F. RAMSEY, AND K. STRAUCH

JABEZ CURRY STREET was a boldly innovative experimental physicist whose discoveries in cosmic rays influenced decisively the course of high-energy physics. His crucial single discovery was the clear identification of a new fundamental particle, now called the muon. His cosmic-ray research was interrupted by World War II, during which Street, as a member of the Radiation Laboratory at MIT, made major contributions to the development of RADAR and LORAN, the global radio navigation system. After his return to Harvard he added the development and use of particle beams from high-energy accelerators to his research program. As a teacher of physics he brought simplicity of concept and his enthusiasm to graduate and undergraduate students.

Street was born on May 5, 1906, in Opelika, Alabama, to Anne Dunklin and Jabez Curry Street. In 1927 he received his bachelor of science degree in electrical engineering at the Alabama Polytechnic Institute. Street worked from 1927 to 1928 at the Brooklyn Edison Power Company. In the fall of 1928 he entered the master's program in physics at the University of Virginia, where Jesse W. Beams had just arrived from Yale. Beams persuaded Street to enter the doc

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toral program and supervised his thesis, “The Fall of Potential in Electrical Discharges," which he completed in 1931.

Street then became a research fellow at the Bartol Research Laboratory in Swarthmore, Pennsylvania. Working with W. F. G. Swann and using an ion chamber for the measurement of cosmic radiation, he joined a pioneering investigation of the nature of cosmic radiation originating in outer space and its secondary radiation products. He also worked with Thomas H. Johnson, using Geiger counters and vacuum tube circuits as "telescopes" to determine that the incoming particles were deflected by the earth's magnetic field and therefore were electrically charged. Later Dr. Johnson moved their apparatus to the higher and lower latitudes of Mexico City to show there was an east-west effect, with the west intensity being greater, corresponding to positively charged particles being a dominant component of the primary cosmic radiation. Street's and Johnson's experiments were of great importance since, at the time, there was heated debate as to whether the primary cosmic rays were charged particles or uncharged photons.

Street continued his experimental research at Harvard, where he arrived as an instructor in 1932. It had become clear that the cloud chamber was key to resolving the debate on the nature of cosmic radiation; a charged particle creates a visible track in a cloud chamber, and the addition of a magnetic field allows measurement of its charge and momentum. With a series of grants from the Milton and Whiting funds and the help of his colleague Harry R. Mimno, as well as research associates and graduate students, Street designed and built a large electromagnet and cloud chamber for his research. This was a straightforward task for Street, who, like a number of physicists of his generation, benefited from his earlier training in electrical engineering. In 1934-35 Street assembled the electromagnet and cloud

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chamber, which contained absorbing sheets to study the properties of cosmic-ray showers.

The observations that Street and Edward C. Stevenson made with the cloud chamber in this new magnet convinced them that the most penetrating cosmic rays must be something entirely new to physics. Earlier experiments by B. Rossi, Street, R. H. Woodward, Stevenson, C. D. Anderson, and S. H. Neddermeyer had indicated the presence in cosmic rays of particles too penetrating to permit identifying them as electrons if the quantum theory of their interaction with matter were valid. Street and his associates with the new detector system efficiently and brilliantly conceived and executed a series of experiments that yielded two important results. First, they showed that cosmic-ray showers were in accord with quantum mechanical theories of the interaction of radiation and matter and, second, that the most penetrating cosmic rays were previously unknown positive and negative particles. In 1937 the case was clinched by Street and Stevenson's picture of a curved track from which the mass of the new particles could be deduced. It was much larger than the electron mass and smaller than the proton mass. This new particle, now called the muon, was the first discovered member of what proved to be a whole new family of elementary particles.

In the fall of 1940 Street closed down his cosmic-ray experiments to join the Radiation Laboratory at MIT, where he developed the "bootstrap," a pulser for a high-voltage magnetron. As head of the navigation division he made important contributions to the development of the global radio navigation system LORAN. His knowledge of LORAN led to an appointment at a critical time as associate director of the British branch of the Radiation Laboratory. Street proved to be an outstanding research administrator. He could listen to proposals, ask astute questions until he had a full

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understanding, and then make wise decisions as to whether a project should be pursued. On his return from Britain he assumed the position of head of the ground and ship division and remained at the Radiation laboratory until the end of the war.

In 1945 Street returned joyfully to teaching and cosmicray research at Harvard. Initially he resumed his studies of cosmic rays with his new graduate students, William Whittemore, Earle Fowler, Rodney Cool, Ann Chamberlain Birge, Robert Carter, and George Nonnemaker. They used the cosmic-ray cloud chamber at Harvard and a new cloud chamber in a deep mine for further cosmic-ray studies, including observations of the density effect for cosmic-ray muons. During the next few years as a result of the availability of new technology (and increased funding!) the search for new particles shifted from the study of cosmic rays passing through a cloud chamber to the study of the interactions of high-energy particles, which were produced with a new generation of particle accelerators and which passed through a cloud chamber or later a bubble chamber. Street played an active role in the development of the Brookhaven National Laboratory with its Cosmotron and later in the creation of the Cambridge Electron Accelerator Laboratory with its 6-GeV electron synchrotron. As founding member of the Cambridge Bubble Chamber Collaboration, Street contributed to their studies of the production and decay of many of the new particles. Street was elected to the National Academy of Sciences in 1953.

A superlative experimenter, Street was also a fine teacher. His explanations were lucid, and he emphasized the role of simple but decisive experiments. Delivered with a southern accent, his lectures had a special appeal and were greatly appreciated. With Wendell H. Furry and Edward M. Purcell he wrote a textbook that grew out of their Harvard under

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graduate teaching. Street's graduate students also felt his enthusiasm and encouragement and his respect and concern for the individual student. As one of his graduate students recalled: "With Curry we felt that we were indeed on the point, at the leading edge of the discovery of new physics. It was important, it was exciting, and a lot of the time it was fun. We were truly lucky." Another of Street's graduate students from the 1940s recalled "Professor Street's very serious dedication to general politeness to women . . . long before such consciousness-raising issues were generally observed. ... He was incensed by any carelessly made remarks. .... [There were] times when he demanded immediate and complete apology."

Curry Street and his wife Leila frequently entertained students, colleagues, visiting scientists, and friends in their Belmont home. These gatherings were memorable for delicious food, cordial and stimulating conversation, and, on occasion, live performances by a string quartet of the chamber music the Streets loved.

Street brought wisdom, fairness, and courtesy to his roles as university administrator. He served as chairman of Harvard's physics department (1955-60), acting director of the Cambridge Electron Accelerator (1962-63), and science advisor to the dean of faculty of the College of Arts and Sciences at Harvard (1966-72).

Street married Leila Tison in 1939. He was the father of a daughter, Caroline Street Trickey, an artist, homemaker, and mother of three children, of Charleston, South Carolina, and a son, Curry Tison Street, a violinist and composer, of Boston.

Once Street spoke of the changes that people undergo and display as they age. He said people simply become "more so" of what they were previously. A colleague described him as "humane and wise." These qualities marked Street through

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out his life, and to the end he was "more so." He died on November 7, 1989.

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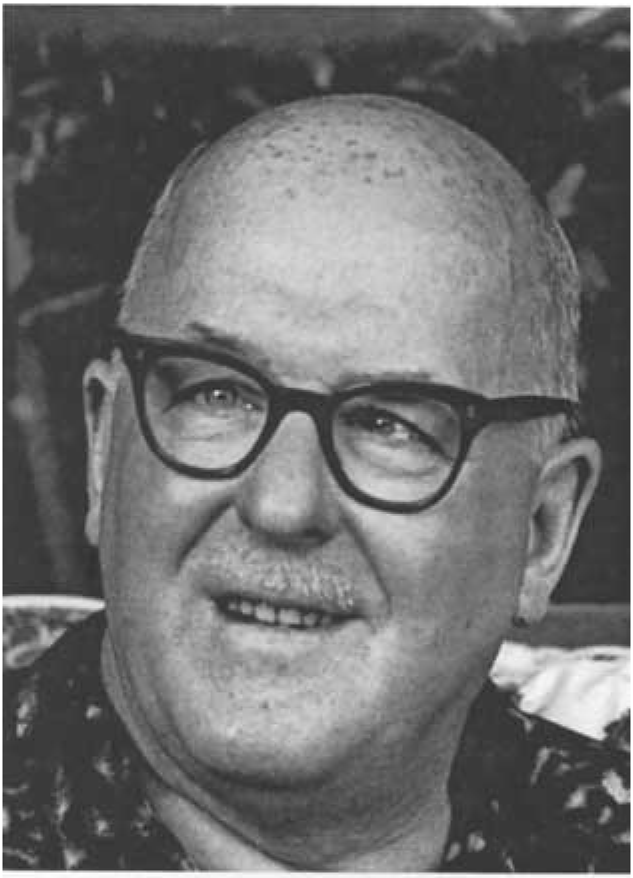

Francis J. Turner

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# FRANCIS JOHN TURNER

*April 10, 1904-December 21, 1985*

BY IRIS Y. BORG AND LIONEL E. WEISS

ON DECEMBER 21, 1985, the international geological community lost one of its most distinguished members—Francis J. Turner, professor emeritus of geology at the University of California, Berkeley. His departure affected all whose lives he had touched in various ways. Those who knew Turner only through his scientific work mourned the end of an era in the study of metamorphic and igneous rocks, a period in which he played a major role in transforming classical petrology into a modern science and through his writings, making then-current discoveries readily available to students and colleagues. Those who knew him personally, scientist and nonscientist alike, lost much more than a great geologist: they lost the company of a rare human being, unique in his warmth, generosity, and humanity. This brief memoir attempts to recall both aspects of the man—the eminent scientist and the unforgettable human being.

Francis John Turner was one of four boys born to a classics master at Auckland Grammar school in New Zealand. Turner spent more than half his life in that country. His father died young, and the children were brought up by their mother. All became successful if not distinguished pro

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fessionals, especially his older brother, a judge and president of the New Zealand Court of Appeals, who became Sir Alexander Turner. In 1930 Francis married Esme' Bentham, who together with their daughter Gillian, then Mrs. James McKercher, survived him on his death.

At the age of seventeen Frank, as he was called by family, friends, and close colleagues, matriculated at Auckland University College, winning a university senior scholarship in geology. At Auckland he earned a B.Sc. and an M.Sc. and won the von Haast Prize. His initial research interests were in the field of igneous petrology, resulting in publications coauthored with Professor J. A. Bartrum, his mentor at Auckland University. While finishing his M.Sc., Frank worked with the New Zealand Geological Survey until 1926, when he accepted a position as lecturer in the geology department at the University of Otago in Dunedin, headed by Professor W. N. Benson. It was Professor Benson who encouraged Frank to follow his own intense interest in the complex metamorphic rocks of New Zealand and who had a major influence on Frank's career. Subsequently, Frank spent many field seasons in the almost unexplored western part of the South Island of New Zealand traveling by foot, with pack horses or small boats, while doing reconnaissance mapping and sampling of the little-known metamorphic and ultrabasic rocks. Memories of those trips were the source of a lifetime of anecdotes that captivated his students and colleagues. Frank's love for the New Zealand wilderness stayed with him all his life. Throughout his long career, photographs of the fjord country of New Zealand adorned the walls of his office. His rock collections from the South Island provided the basis for many publications leading to his D.Sc., awarded in 1934 by the University of New Zealand and election to fellowship in the Royal Society of New Zealand in 1938.

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In his desire to understand the genesis of the metamorphic rocks, Frank observed and collected in the field and poured over Professor Benson's metamorphic collection from the Highlands of Scotland, where mapping and interpretation of such rocks was relatively far advanced. He read prodigiously, particularly the works of the European metamorphic petrologists and was impressed with the unified approach to metamorphic processes proposed by the Finnish petrologist P. Eskola, which in the 1930s had yet to gain wide recognition. Using Eskola's notions of metamorphic facies, Frank not only explained the progressive metamorphism he saw in New Zealand, particularly in the Otago schist, but also further advanced them to allow their applicability to metamorphic belts everywhere. These studies formed the basis of Frank's first book, *Mineralogical and Structural Evolution of Metamorphic Rocks,* published some years later as a Geological Society of America memoir (1948), a book that established Frank's worldwide reputation and profoundly influenced a generation of young geologists.

Though mainly concerned with the mineralogical and chemical properties of metamorphic rocks, Frank became intrigued with the significant relation between the macroscopic structure and the corresponding microscopic rock fabrics. In pursuit of this aspect of metamorphism, he proposed to work with Dr. Eleanora Knopf, wife of Professor Adolph Knopf of Yale University, who was introducing the techniques of the European geologists, particularly those of Professors Walter Schmidt and Bruno Sander, to the study of the microscopic structure (nowadays termed fabric or texture) of metamorphic rocks. A Sterling fellowship in 1938 allowed Frank to travel to the United States and spend a year at Yale University. At Yale he became familiar with the universal stage for the petrologic polarizing microscope, an instrument rather like a goniometer that permitted the ac

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tual crystallographic orientation of individual mineral grains in a rock to be accurately determined with respect to external coordinates. When used in conjunction with a stereographic or similar projection, the stage allowed three-dimensional mapping of the microstructure of rocks, including intra- and intercrystalline phenomena and the preferred orientations of individual minerals. Ultimately, Frank became a master of this instrument and applied it to the study of both naturally and artificially deformed rocks and minerals.

His fellowship over, Frank returned to New Zealand and taught there throughout World War II. His teaching load at Otago was inhuman by modern standards, but it had the benefit of forcing him to read even more widely in every branch of geology and to learn to synthesize and condense vast amounts of information. He developed these skills to perfection, becoming an outstanding lecturer at every levellucid, well organized, and witty, even when addressing subjects in which he was not expert. His curiosity led him far afield in geology. His graduate seminars tackled almost any subject, from chemical thermodynamics to the physics of wind-blown sand as expounded by R. A. Bagnold. In all of his lectures Frank passed on his insights and enthusiasm with amazing clarity.

Frank's year at Yale University eventually proved to be a turning point in his career. Although he had returned to New Zealand, the friendships made and the interests that were nurtured in New Haven set the stage for his return to the United States. In 1946, after being encouraged to apply for the vacant position of director of the New Zealand Geological Survey and failing to be appointed, he accepted an invitation from Chairman Howell Williams to join the faculty at the University of California in Berkeley as an associate professor. In 1948 David Griggs, whom Frank had met

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through the Knopfs when Griggs was a graduate student at Harvard, was at the Institute of Geophysics at the University of California at Los Angeles (UCLA). The two men shared a keen interest in metamorphic structures and processes. AT UCLA Griggs was engaged in the laboratory deformation of rocks and minerals and was in need of petrographic assistance in interpreting the results. Thus, a friendship was renewed, and a technically rewarding research collaboration began, which lasted until Griggs died in late 1974.

At Berkeley, Frank found a conventional faculty teaching traditional geological subjects. Its most eminent member, Andrew C. Lawson, was professor emeritus and no longer actively pursuing research. The curriculum in the Department of Geology emphasized field work, and a degree in geology required among other courses a year of field geology in the Berkeley Hills, a summer field camp in the California coast ranges, or the foothills of the Sierra Nevada, as well as a semester course in surveying. Women were discouraged from majoring in geology if not actually excluded from the major, since they were persona non grata in the required summer field course. Under Turner's influence and later his guidance as chairman (1954-59), the department attracted students and scholars from all over the world, giving it a stimulating international character. While ever conscious of the importance of field work, Turner insisted that the course requirements for the bachelor's and higher degrees be altered to accommodate students who preferred to focus on the theoretical and experimental aspects of the field. He made sure that the curriculum was changed to remain current and to include new and promising approaches and discoveries in the fields of geochemistry and tectonics. Turner taught himself the principles of thermodynamics in order to better contribute to his own specialties within the

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field of petrology and began his collaboration with Professor John Verhoogen in the writing of an advanced text on igneous and metamorphic petrology.

The post-World War II scientific expansion in the earth sciences stimulated a worldwide search for talent for the faculty at Berkeley in which Turner was pivotal. New members arrived from the United States, Belgium, Switzerland, England, Australia, and New Zealand. At one point, half the faculty members were foreign-born geologists and geophysicists, not one of whom had taken the Berkeley Hills field course but who were required to teach it. The department acquired the latest equipment for the study of every aspect of the earth sciences, from minerals to earthquakes. Turner was accessible to both students and faculty alike. He was never too busy to do whatever he thought appropriate to further the career of promising young geologists in the department or those he met in his travels abroad—Donald B. McIntyre, Ian S. E. Carmichael, W. S. Fyfe, R. N. Brothers, and M. S. Paterson, to name a few. Enthusiasm, excitement, and first-class research became the hallmarks of the department as its active young faculty gained international renown.

A large new building to accommodate the earth sciences became a reality. It replaced the cavernous Bacon Hall, a former library and the second oldest building on campus. Although it was a sentimental favorite, it was ill suited to house a modern science department. Along with his services to the department, Frank was always willing to serve the university as a whole. He sat on numerous administrative committees, including the Graduate Council, the Committee on Research, the Library Committee, and the Executive Committee of the College of Letters and Science.

Through all these efforts, Frank never lost passion for his own research. When queried as to what, in his opinion, was

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his most significant research, he is on record as having answered "the deformation of Yule marble," his long collaborative research with David Griggs. Griggs and his students, notably John Handin and Hugh C. Heard, conducted laboratory deformations of cylindrical samples of marble in various orientations and at various temperatures, pressures, strains, and strain rates, while Turner and his students did the interpretative petrographic analysis. These experiments led to an understanding of the various mechanisms underlying the phenomenon of preferred orientation of minerals within rocks and hence the ability to reconstruct the stress fields that prevailed during the final stages of deformation. Although many different rocks and minerals, from quartz to pyroxene, were investigated, the most important results were obtained with calcite, either as a constituent of Yule marble or as single crystals. Turner and his students demonstrated the importance of slip, twinning, and stress-induced phase transformations (e.g., in enstatite-clinoenstatite) in effecting the preferred orientations observed in both naturally and laboratory-deformed rocks. Turner's geometric analysis of the telltale clues left after several of these processes had proceeded to completion showed incredible insight. Later development of dislocation models of intracrystalline deformation in minerals coupled with the use of X-ray texture analysis and the transmission electron microscope in the study of deformed rocks depended heavily on the early discoveries of Turner and Griggs.

Turner's written legacy includes eighty technical papers and six textbooks on metamorphic, igneous, and structural petrology. All but two of the books were written with colleagues, most often from Berkeley. All bear the stamp of Turner's gift for technical writing. His ability to synthesize succinctly the volume of detail associated with complex geological processes and present it clearly in fluent prose again

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reflected his outstanding teaching skills. His constant familiarity with the latest developments in geology allowed him to provide additional insight into the interpretation of geological processes described in many historical monographs on special geological provinces that had been written before World War I (P. Eskola, P. Niggli, V. M. Goldschmidt,J. J. Sederholm, and H. Rosenbusch, among others). Integration of the new and the old into his texts was done in such a way as to remind readers that the best data and observations are not necessarily the most recent. His books were well received, especially abroad, much to his surprise; three of them went through second editions, which involved substantial updating, rewriting, and reorganization. These revisions occupied Turner after his retirement from the department in 1971.

Frank Turner received many honors during his long career. They are listed at the conclusion of this memoir. The last was the Roebling Medal, the highest honor of the Mineralogical Society of America, which was bestowed a few weeks before his death. He was not able to accept the medal personally because of his final illness, but he was deeply moved when several of his colleagues presented it to him in the hospital. His comment, as reported by H.-R. Wenk, was: "I did not do more than others, but I was always fascinated by discovering new problems along with recognizing that there are no final solutions in geology." In addition, Frank was invited to be a foreign correspondent of the Academia delle Scienze, Instituo di Bologna (1953), a corresponding member of the Geological Society of Edinburgh, a foreign member of the Geological Society of London, and a visiting fellow of Oxford University's Brasenose College (1972-73).

To a generation of geologists, his peers, colleagues, and students, Frank Turner was the most unforgettable person they ever met. He was brilliant, urbane, compassionate, and

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thoroughly eccentric. He was a familiar figure at the University of California, Berkeley, campus, walking, stick in hand, from his nearby home to the Department of Geology and Geophysics. He never learned to drive, and he was almost 50 years old before his wife Esme' bought a car and entreated a student to teach her how to drive it and how to navigate the tortuous, narrow, steep streets of the city. A ride with Esme' was a never-to-be-forgotten—or hopefully repeated—experience. But Frank was nonplused by their expeditions and took them in stride. For some reason, he eschewed all credit cards, and his pocket usually bulged with a wad of cash. The Turners were similarly unconventional in being one of the last couples of their acquaintance to own a television set.

At first glance, Frank Turner appeared to be a formidable personage—a heavy-set, balding man with a small mustache and one wild eye. One soon learned he was kind to the core and had an acute sense of humor that endeared him to all. He and Esme' regularly invited colleagues, visitors, friends, and students to their house for cocktails. To students especially it was a treat to be invited to the small Turner house with its sloping floors and to meet many of the great intellectuals of the community and distinguished visitors. Conversation inevitably turned to the French Impressionist painters, chamber music, or fine wines, subjects dear to the Turners' hearts. Frank was a marvelous raconteur and with a martini in hand usually had his guests enthralled and in gales of laughter.

IN WRITING THIS MEMOIR we used information from the following sources:

1. H.-R. Wenk. Presentation of the Roebling Medal of the Mineralogical Society of America for 1985 to Francis John Turner. *Am. Mineral.* 71 (1986):849-51.

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3. D. S. Coombs. Francis J. Turner. *Proc. R. Soc. N. Z.* 11(1986):12735.

| HONORS | |
|---|---|
| 1938 | Sterling Fellow, Yale University |
| 1950 | John Simon Guggenheim Foundation Fellow |
| 1951 | Hector Medal, Royal Society of New Zealand |
| 1956 | Member, National Academy of Sciences Fulbright Fellow to Australia |
| 1959 | John Simon Guggenheim Foundation Fellow |
| 1965 | Honorary D.Sc., University of Auckland |
| 1969 | Lyell Medal, Geological Society of London President, Mineralogical Society of America |
| 1971 | Berkeley Citation, University of California Roebling Medal, Mineralogical Society of America |

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# ERNEST GLEN WEVER

*October 16, 1902-September 4, 1991*

BY JACK VERNON

WE WERE DRIVING from Princeton, N.J., to Hibernia, N.J., when I asked Glen Wever, "What do we know about hearing in bats?" He answered, "About all we know is what Donald Griffin has written; that is, they detect and catch their prey by echo location, a term invented by Griffin. We really know nothing about their hearing ability except that it must be amazing; after all, they do with their ears what the rest of us do with our eyes." This conversation took place over forty years ago.

Glen Wever and I were driving to Hibernia to try to locate an abandoned zinc mine that we had heard was the home of hibernating bats. We were on a bat-collecting trip, the first of many to follow, from which we hoped to acquire some bats (Myotis Lucifugus, as it turned out) for experimental purposes. We planned to record the AC cochlear potentials from the bats' inner ears, which, at that time, had never been done. A filling-station attendant in Hibernia directed us to the zinc mine, where we found the entrance blocked with a heavy steel plate and a sign that read "KEEP OUT."

Left to my own devices, I think I would have obeyed the sign, but Glen said, "I think we can just manage to crawl

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under that barricade." We proceeded to do just that. One of the keynotes of Glen's life was to bypass barricades—to find ways to get around (or under) those things that stood in the way of his progress. A mere steel plate, fortunately, was not about to deter his appointed round of bat collection.

Once in the zinc mine, which had been carved from solid granite, we found it was exceptionally clean, free of any human debris and filled with cool air. Outside it was a hot July day, but inside the mine it was cool enough to require jackets and gloves. When we had walked about half a mile or so into the mine we began to see clusters of bats hanging from the ceiling. Our plan was to acquire a few bats with which to start our experiments. At this point we had little idea about the anatomy of the bat's ear and reasoned that surgical practice would be required. Upon surveying the clusters of bats Glen suggested that possibly a cluster might represent some sort of family, social or community organization, and that we should take only one bat from each cluster and thus produce as little disturbance as possible to any social organization the bats might have. That is yet another example of how Glen Wever's mind and sensitivity worked.

We returned to Princeton, and the next day began working on the bats. The first thing we discovered was that we had very few surgical tools small enough to be effective with a bat, whose total body weight was 7 grams. In our initial surgical effort I managed to drop a pair of fine pointed jeweler's forceps, which bent one tine so that it laid over the other tine. Glen looked at what I thought was now a useless tool and said, "I bet you have just made a pair of scissors adequate for bat surgery." He was correct; those bent forceps became the mainstay in our subsequent bat surgeries. Investigation of the electrophysiological aspects

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of the bat's inner ear revealed that its ear is highly and specifically tuned to 30,000 Hz, which is the primary pitch of this bat's echo location scream.

That episode with the bent forceps illustrates yet another of Glen's many positive and outstanding characteristics. If the needed tool was not available, he made it. Glen made not only tools but other things as well. For example, early on in his career he typed his own manuscripts and quickly discovered that he did not know when he was about to run off the bottom of a page, so he equipped his typewriter with a rotating wheel that would indicate the spacing of the typing according to its page location. Sometime later a typewriter salesman saw the device and shortly thereafter his company advertised the “Page Gage." Did Glen sue or demand royalties from the typewriter company? No, he did not. There was not a litigious bone in his body and thoughts of that sort simply would never occur to him. I once asked him why he did not take the typewriter company to court since it had obviously stolen his idea and was realizing a profit from it. He replied, "But the device still works just fine for me." His original need had been fulfilled and that was as far as he wished to pursue the matter.

Glen Wever's entire being was aimed at investigations and expositions of the ear. He had little or no interest in social activities or, indeed, in any activities that would detract from his investigative goals. Fortunately, for many of us those goals were aimed at the ear and hearing. His first book, *Theory of Hearing,* published in 1949 by Princeton University Press clearly lays out the investigative roadway that Glen was to travel the rest of his life. That book, by the way, was reported to be the first manuscript ever received by Princeton University Press that was totally free of error. Glen was never too busy or too distracted not to be totally accurate and totally complete.

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Glen Wever began his investigative life in the early days of electronics, when the radio was new and when it was not possible to go to an electronic supply house or catalog and purchase such things as biological amplifiers or attenuators or anything needed to conduct hearing research. Therefore, he studied electronics, taught himself, and made his own amplifiers and attenuators. Great chunky things they were, driven by automobile batteries, but they were perfect. He found that the noise floor of amplifiers could be greatly reduced by using very precise components within carefully measured tolerances. I don't know for sure, but I would bet that no present-day bioamplifier is any quieter than those made by Glen Wever so many years ago.

Early on in his work he became interested in the microanatomy of the ear as a way to compare different species and different conditions within a given specie. At about that time Stacy Guild at Johns Hopkins had perfected the thin-section celloidin-embedding technique of tissue preparation. Glen spent a month studying with Stacy Guild in order to learn the technique first hand. From that time on, animals studied in his laboratory were characterized by the electrophysiological response of the inner ear as well as the morphology of that ear.

Around 1930 Glen Wever and Charles Bray, both faculty members in the Department of Psychology at Princeton University, discovered the bioelectric signals generated in the inner ear in response to sound stimuli. That discovery started a host of investigations about the inner ear that continue to this day. The discovery of the inner ear's electric potentials is a very special story requiring special attention.

Wever and Bray initially were attempting to record from the auditory nerve of the cat when one of those happy accidents occurred. Their laboratory was in a soundproof

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room in the basement of Eno Hall. Their stimulating equipment and the animal preparation (a cat with an electrode in its VIII nerve) were set up in a dark room down the hall from the soundproof chamber in which the listener was located. Cables connected the two areas. The plan was that Glen would speak into the cat's ear while Bray would listen for the nerve responses coming from the speaker located in the soundproof chamber. Glen recalls that Bray came running out of the chamber so excited that he, Glen, could hardly understand a word he was saying. What he said was that he had heard every word Glen had said. The unexpected feature was the faithful reproduction of the human voice and not the expected neurological signals. Clearly the recording of the human voice had come from the cochlea and not from the VIII nerve on which their electrodes had been placed. This event, which was read before the National Academy of Sciences (1930) was the original recognition of the AC cochlear potential, which came to be recognized as the analog production of the inner ear in response to sound stimuli. These AC potentials of the ear also became known as the cochlear microphonic (they should have been designed the "Wever-Bray effect"), a designation that came about as the result of a misunderstanding. E. D. Adrian, a highly respected physiologist, remarked that the signals reported by Wever and Bray were probably artifacts, which he termed "microphonics," like those sounds produced in early radios when one tapped on the tubes of the radio.[1] Actually what Adrian said was, "I conclude that the effect is due to some kind of microphonic action by which vibrations produce changes in the potential between different points in the inner ear." In that same article, Lord Adrian went on to say, "But whatever its explanation, the Wever-Bray effect is certainly a remarkable phenomenon, and it may well prove to be of great importance to theories of

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hearing." Despite such statements, the term "Cochlear microphonic" has stuck and is, to this day, in common use. Wever always referred to the electrical potentials of the ear as the "AC cochlear potentials," and in his honor I have always done the same, as do most of his other students. For his work in discovering the bioelectric potentials of the ear he received the first Howard Crosby Warren Gold Medal from the Society of Experimental Psychologists in 1932.

Glen Wever was born in Benton, Illinois. He received an A.B. degree from Illinois College in 1922 and an M. A. and a Ph.D. in experimental psychology from Harvard in 1924 and 1926, respectively. His doctoral thesis was conducted under the leadership and recommendation of E. G. Boring, who, at that time, published a classic paper entitled "Auditory Theory."[2] Interestingly enough, Wever did not do his doctoral thesis in the area of audition but rather in the area of vision. It was a figure-ground investigation utilizing a Gestalt orientation. After graduation he spent a year on the faculty of the University of California at Berkeley, after which he accepted an invitation from Professor Herbert Langfeld to be an instructor in the Department of Psychology at Princeton University.

While at Berkeley, Wever had a student named Stanley Truman who needed a thesis topic, and Glen suggested that he do a figure-ground-type study in audition, wherein subjects were required to make pitch discriminations in the presence of background noise. That study was a pivotal affair for Wever, for in order to have the necessary auditory equipment he contacted Wegel and Lane of Bell Telephone Laboratories in New York, making them aware of the equipment deficiencies in his laboratory. Then when Wever moved to Princeton it was but a short fifty miles to New York to visit Wegel and Lane in person. They provided him much of the electronic equipment he needed to conduct his work.

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They provided, on "permanent loan" such things as an audio-oscillator, an audio-attenuator, a loudspeaker, and an audiometer—things Wever desperately needed for his investigations. No doubt Wegel and Lane often looked back on that era with pride, since it was they who made it possible for Glen Wever to do much of the wonderful things he did in the area of hearing.

In 1946 Dr. Julius Lempert, an otologist in New York City, invited Wever to spend one day a week with him to consider hearing problems in humans. That was the beginning of an exposure to a clinical orientation; however, it was limited to New York and did not invade the Princeton laboratory, although it was this orientation that led Wever and Merle Lawrence to extensive studies of the middle-ear mechanism.

Glen Wever remained at Princeton for the rest of his life, rising through the ranks to full professor in 1941. While there he held two distinguished endowed chairs, the first being the Dorman T. Warren Professorship from 1940 to 1950. It was because of Professor Warren that Eno Hall, the first college building in the United States to be exclusively dedicated to psychology, was constructed. The second endowed chair was the Eugene Higgins Professorship from 1950 to 1970, when he became professor emeritus.

During World War II Wever served as a consultant to the National Defense Research Council, where he suggested that sailors being considered for sonar operation be given tests predictive of musical ability. This suggestion reflects the influence of his wife Suzanne Rinehart Wever, a highly skilled musician. The use of this selection procedure, as well as improved training methods, resulted in greatly improved sonar performance. One story has it that Wever told the Navy its selection procedures for sonar operators were so

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poor that any suggestion he made would be an improvement.

In 1950 the National Institutes of Health established grants dedicated to the construction of research facilities. One such grant was awarded to Professor Wever, which resulted in the construction of the Auditory Research Laboratories at Princeton. The laboratory, built in the region of Princeton's football stadium, was soon evacuated in order to permit expansion of the stadium. The laboratories were then constructed on the north side of Princeton's Forestall Campus. The unique feature of the Forestall Laboratory was Wever's design. Each laboratory was established as a separate small building rather than being separate rooms in a single building. The concept of separate buildings provided excellent sound isolation, and, in an effort to provide electromagnetic radiation isolation, the outside wall of the internal sound chamber was lined with copper sheeting and the inside wall of its outside chamber was lined in a similar fashion. These chambers provided excellent isolation and conditions for recording the low-voltage electrophysiological signals of the auditory system.

During his lifetime Glen Wever received many awards and honors, starting in 1932 with the first award of the Howard Crosby Warren Gold Medal from the Society of Experimental Psychologists. This award was in recognition of the initial recordings of the AC cochlear potentials of the inner ear. Toward the end of his career he received the Award of Merit from the Association for Research in Otolaryngology, indicating that his contribution to science was not a one-shot affair but rather an ongoing lifetime of contributions. Other awards included the Shambaugh Prize of the Collegium Oto-Rhino-Laryngologicum, the Silver Medal of the Acoustical Society of America, and an honorary degree from the University of Michigan.

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In the 1950s Wever was appointed chairman of Princeton's psychology department, a task he found unpleasant. His first and only love was research involving the ear, and the duties of a chairman were viewed as an intrusion into his primary efforts. The politics of academe were of no interest to Glen; indeed, social interactions of any sort were of very slight interest except for those with colleagues where the exchange could be about the ear and hearing.

Wever was not a "joiner"; nevertheless, he was a member of the American Academy of Arts and Sciences, the American Psychological Association, the Society of Experimental Psychologists, the Acoustical Society of America, the American Otolaryngology Society, and the Association for Research in Otolaryngology. He rarely attended the meetings of these societies.

In 1949 Wever published *Theory of Hearing,* which became a primary source of auditory information for many generations of investigators. In 1954 he and Merle Lawrence published *Physiological Acoustics,* which proved to be another critical resource. He worked with Georg von Bekesy (the Nobel laureate) translating Bekesy's manuscript, *Experiments in Hearing,*[3] from German into English. It was in that book that Bekesy (undoubtedly thinking about Glen Wever) suggested that each scientist needs a capable enemy. He said "An enemy is willing to devote a vast amount of time and brain power to ferreting out errors both large and small, and without any compassion. The trouble is that really capable enemies are scarce, most of them are ordinary." That book by Bekesy provided yet another invaluable resource for investigators of the auditory system. Prior to that time Wever's book *Theory of Hearing* had been published. Note that he did not title it "Theories of Hearing"; it is clear that for him there was only one theory, and one has to admit to this day that Wever's theory is the most thorough treatment

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of hearing. More modern theories in this area are usually restricted and narrow in scope, dealing with limited aspects of auditory phenomena.

On May 16-18, 1982, a conference was held at Princeton University to honor Glen Wever. The conference was composed of students and colleagues who had been associated with and influenced by Glen Wever. The purpose was to say "thank you" to Glen for all he had done for so many of us. The culmination of the conference was a published volume of the presentations made at the conference.[4]

The conference and book were composed of twenty-three presentations, which ranged in topics from "Five Years of Cochlear Potentials" by Merle Lawrence to "Interpretation of the Sharply Tuned Basilar Membrane Response Observed in the Cochlea" by Shyam Khanna, to "Rate Function in Cutaneous Vibratory Perception" by Carl Sherrick, "Comparative Morphology of Stereocilia" by James Saunders, “Echo Location in Bats" by James Simmons, "Dolphin Hearing and Sound Production" by Sam Ridgeway, "The Vestibular Apparatus and Space Motion Sickness" by Donald Parker, "The Relation Between Noise and Health" by Ernest Peterson, and "Possible Physiological Correlates of Subjective Tinnitus" by Jack Vernon, to name a few. That memorial book contained twenty-one chapters, all but one written by Wever's previous students or colleagues.

In his retirement years Wever continued as a senior research psychologist at Princeton, completing two of an intended trilogy of books. The completed books were *The Reptile Ear* (1978) and *The Amphibian Ear* (1985). The third book was to be on the hearing of fish but was not completed due to health problems.

*The Amphibian Ear* provides many examples of Wever's ability to organize things and present them in an established and logical manner. He starts the book by explaining

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that the word "amphibian" means "both lives"; that is, a life above the water and in air and a life below water, which we generally consider to be impossible. The book contains a very scholarly account of amphibian characteristics, the origin of amphibia, theories of amphibian ancestry, and the function of hearing in amphibia. The experimental methods by which amphibian hearing has been investigated reveal the thoroughness with which Wever undertook tasks of this sort: (1) anatomical description of the hearing apparatus, (2) behavioral observations of the animal's acoustic responses and discriminations, and (3) electrophysiological responses of the inner ear. Most investigators would have been content with any one of these three approaches but not Glen; for him it was necessary to do the complete evaluation. The traditional view of the development of the vertebrate ear held that the course of evolution began with the fishes, extended through the amphibians to the reptiles, and then proceeded to birds and mammals. As a consequence of Wever's book, *The Amphibian Ear,* the traditional evolution view will be challenged. We will always consider it a serious loss that Wever was unable to finish his book on the hearing of fish.

Glen Wever was a dedicated scientist in the finest sense of that word, and he will be greatly missed by those of us who knew him best.

## NOTES

1. *J. Physiol.* 71(1931):28-29.
2. E. G. Boring. Auditory theory.
3. G. von Bekesy. *Experiments in Hearing.*
4. R. Fay and G. Gourevitch, eds. *Hearing and Other Senses: Presentations in Honor of E. G. Wever.* Groton, Conn.: Amphora Press, 1983.

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