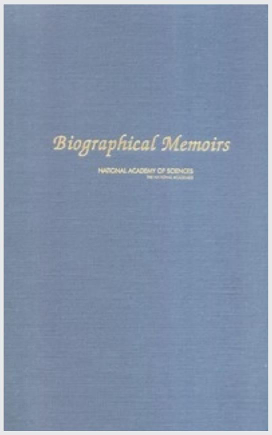

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PREFACE

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.

JOHN I. BRAUMAN
Home Secretary

Biographical Memoirs

VOLUME 87

Lars V. Ahlfors

LARS VALERIAN AHLFORS

April 18, 1907–October 11, 1996

BY FREDERICK GEHRING

PERSONAL AND PROFESSIONAL HISTORY

LARS AHLFORS WAS BORN in Helsinki, Finland, on April 18, 1907. His father, Axel Ahlfors, was a professor of mechanical engineering at the Institute of Technology in Helsinki. His mother, Sievä Helander, died at Lars's birth. As a newborn Lars was sent to the Åland Islands to be taken care of by two aunts. He returned to his father's home in Helsinki by the age of three.

At the time of Lars's early childhood, Finland was under Russian sovereignty but with a certain degree of autonomy, and civil servants, including professors, were able to enjoy a fairly high standard of living. Unfortunately, all this changed radically during World War I, the Russian Revolution, and the Finnish civil war that followed. There was very little food in 1918, and Lars's father was briefly imprisoned by the Red Guard.

For historical reasons the inhabitants of Finland are divided into those who have Finnish or Swedish as their mother tongue. The Ahlfors family was Swedish speaking, so Lars attended a private school where all classes were taught in Swedish. He commented that the teaching of mathematics was mediocre, but credited the school with helping him become almost fluent in Finnish, German, and English, and less so in French. A sister of Lars was two years ahead of him in school. Hence Lars was able to learn high school mathematics by doing her homework and by means of clandestine visits to his father's engineering library. Indeed, the teachers eventually relied on Lars to provide problems for the class.

Lars spent a summer vacation as a young student with a Finnish-speaking family to improve his knowledge of Finnish. He had hoped to pay for his keep that summer by teaching mathematics, but having found no takers, he earned his board by giving lessons to the children of the family in the German language and playing the cello.

In 1924 Lars Ahlfors entered the University of Helsinki, where his teachers were two internationally known mathematicians, Ernst Lindelöf and Rolf Nevanlinna. At that time the university was still run on the system of one professor for each subject. Lindelöf was the professor for mathematics. There were no graduate courses then, and all advanced reading was done under the supervision of Lindelöf. Lars remembered well the Saturday mornings when he had to visit Lindelöf in his home at 8 a.m. to be praised or scolded—as the case may have been.

In the spring of 1928 Lars completed all examinations for the master's degree, and in the fall of the same year he accompanied Nevanlinna to Zürich. Nevanlinna had been invited to the Eidgenösische Technische Hochschule in Zürich for a year to fill Hermann Weyl's chair while Weyl was on leave of absence. Lindelöf persuaded Lars's father to let his son accompany Nevanlinna to Zürich. Later Lars emphasized the importance of this visit to Zürich: "I found myself suddenly transported from the periphery to the center of Europe."

Nevanlinna's course of lectures at Zürich was Lars's first exposure to contemporary function theory, and he became "addicted" to complex analysis. Among other things, Nevanlinna introduced the class to a 21-year-old conjecture made by the French mathematician Denjoy on the asymptotic values of an entire function, namely that an entire function of order k can have at most $2k$ finite asymptotic values. Lars created a sensation in the mathematical world when he found a different way to approach this problem and obtained a beautiful proof of this conjecture by means of conformal mapping (1929). His name became known to all those working in this area and, as he remarked later, "my future was made." Lars returned to Finland and undertook his first teaching assignment as lecturer at Åbo Akademi, the Swedish-language university in Turku. At the same time he began work on his thesis, which he defended in the spring of 1930.

During 1930-1932 Lars made several trips to continental Europe, including a longer stay in Paris supported by a fellowship from the Rockefeller Foundation. In 1933 he returned to Helsinki as an adjunct professor at the University of Helsinki. That same year he married Erna Lehnert, an Austrian who with her parents had first settled in Sweden and then in Finland. Half a century later, in the preface to his *Collected Papers,* Lars wrote, "This was the happiest and most important event in my life."

In 1935 Lars was first offered and then accepted a three-year appointment at Harvard University as a visiting lecturer. One year later, at the quadriennal International Congress of Mathematicians in Oslo, he was awarded a Fields Medal, the equivalent for mathematicians of a Nobel Prize.

During the 1924 International Congress of Mathematicians in Toronto, the president of the congress, Professor J. C. Fields of Canada, had proposed that two gold medals be awarded at each International Congress of Mathematicians "for outstanding achievements in mathematics." The winners were to be selected by an international jury. The 1932 Congress in Zürich had approved Fields's proposal and named the medals after Fields, who had died just before the 1932 Congress. The first two Fields Medals were then awarded to Lars Ahlfors and Jesse Douglas of the United States in 1936. That he was to be awarded a Fields Medal came as a complete surprise for Lars. Indeed, he was told officially that he was to receive this honor only hours before the formal award ceremony.

Lars Ahlfors's appointment at Harvard was for three years. He and his wife had found life in Cambridge very rewarding and they had to decide in 1938 whether to stay at Harvard or return to Finland, where he had been offered a professorship at the University of Helsinki. In the end, patriotic feelings and loyalty to his teachers drew them back to Helsinki, where they had a happy year.

Unfortunately, World War II broke out in 1939, and Lars's wife and two children were evacuated and found refuge with relatives in Sweden. Helsinki was bombarded, the university was closed for lack of male students, but otherwise life went on. Lars was never called to military duty because of an earlier physical condition, and it is quite ironic that one of his best papers was written while he was in an air raid shelter.

Soon after the winter war the Ahlfors family was able to return to Helsinki and resume a seemingly normal life. However, politics in Finland took an unfortunate turn, and, when Hitler attacked the Soviet Union in 1941, Finland was his ally. When the Russians were able to repulse the Germans, they intensified the war in Finland with foreseeable results. The Finnish-Russian war ended with a separate armistice in September 1944, and Finland was forced to expel the German troops that had been stationed there.

The war continued off and on until 1944, and Lars felt he could not remain in Finland without sacrificing his research. He therefore accepted an invitation to the University of Zürich and, after a very difficult journey, joined their faculty in the spring of 1945. He and his wife found the postwar era a difficult time for strangers to take root in Switzerland. Hence in 1946 Lars was delighted to accept an invitation to return to Harvard, where he remained for the rest of his career.

We consider next excerpted descriptions of three aspects of Ahlfors's research by Robert Osserman, Irwin Kra, and the present author, respectively, which appear in Volume 45 of the *Notices of the American Mathematical Society*.

CONFORMAL GEOMETRY

BY ROBERT OSSERMAN

There are two directions in which one can pursue the relations between Riemann surfaces and Riemannian manifolds. First, if a two-dimensional Riemannian manifold is given, then not only lengths but also angles are well defined, so that it inherits a conformal structure. Furthermore, there always exist local isothermal coordinates, which are local conformal maps from the plane into the surface. The set of all such local maps forms a complex structure for the manifolds, which can then be thought of as a Riemann surface. One then has all of complex function theory to bring to bear in studying the geometry of the surface. The most notable successes of this approach have been in the study of minimal surfaces, as exemplified in the contributions to that subject made by some of the leading function theorists of the nineteenth century: Riemann, Weierstrass, and Schwarz.

In the other direction, given a Riemann surface one can consider those metrics on the surface that induce the given conformal structure. By the Koebe uniformization theorem, such metrics always exist. In fact, for "classical Riemann surfaces" of the sort originally considered by Riemann, which are branched covering surfaces of the plane, there is the natural Euclidean metric obtained by pulling back the standard metric on the plane under the projection map. One can also consider the Riemann surface to lie over the Riemann sphere and to lift the spherical metric to the surface. Both metrics prove very useful for obtaining information about the complex structure of the surface.

For simply connected Riemann surfaces the Koebe uniformization theorem tells us that they are all conformally equivalent to the sphere, the plane, or the unit disk. Since the first case is distinguished from the other two by the topological property of compactness, the interesting question concerning complex structure is deciding in the noncompact case whether a given surface is conformally the plane or the disk, which became known as the parabolic and hyperbolic cases, respectively. In 1932 Andreas Speiser formulated the "problem of type," which was to find criteria that could be applied to various classes of Riemann surfaces to decide whether a given one was parabolic or hyperbolic. That problem and variants of it became a central focus of Ahlfors's work for several decades. He started by obtaining conditions for a branched surface to be of parabolic type in terms of the number of branch points within a given distance of a fixed point on the surface, first using the Euclidean metric and later realizing that a much better result could be obtained from the spherical metric. But perhaps his main insight was that one could give a necessary and sufficient condition by looking at the totality of all conformal materics on the surface.

The problem of type may be viewed as a special case of the general problem of finding conformal invariants. There one has some class of topologically defined objects, such as a simply or doubly connected domain or a simply connected domain with boundary and four distinguished points on the boundary, and one seeks to define quantities that determine when two topologically equivalent configurations are conformally equivalent. One example is the “extremal length” of a family of curves in a domain, which is defined by a minimax expression in terms of all conformal metrics on the domain and is thereby automatically a conformal invariant. Ahlfors and Beurling, first independently and then jointly, developed this idea into a very useful tool that has since found many further applications.

From the first, Ahlfors viewed classical results like Picard’s theorem and Bloch’s theorem as special cases of the problem of type, in which such conditions as the projection of a Riemann surface omitting a certain number of points would imply that the surface was hyperbolic and hence could not be the image of a function defined in the whole plane. He felt that the Nevanlinna theory should also fit into that framework. Finally, in 1935 he produced one of his most important papers, in which he used the idea of specially constructed conformal metrics (or “mass distributions” in the terminology of that paper) to give his own geometric version of Nevanlinna theory. When he received his Fields Medal the following year, Carathéodory remarked that it was hard to say which was more surprising: that Nevanlinna could develop his entire theory without the geometric picture to go with it or that Ahlfors could condense the whole theory into 14 pages.

Not satisfied that he had yet got to the heart of Nevanlinna theory from a geometric point of view, Ahlfors went on to present two further versions of the theory. The first, also from 1935, was one of his masterpieces: the theory of covering surfaces (1935). The guiding intuition of the paper is this: If a meromorphic function is given, then the fundamental quantities studied in Nevanlinna theory, such as the counting function, determined by the number of points inside a disk of given radius where the function assumes a given value, and the Nevanlinna characteristic function, measuring the growth of the function, can be reinterpreted as properties of the Riemann surface of the image of the function, viewed as a covering surface of the Riemann sphere. The counting function, for example, just tells how many points in the part of the image surface given by the image of the disk lie over the given point on the sphere. The exhaustion of the plane by disks of increasing radii is replaced by an exhaustion of the image surface. Ahlfors succeeded in showing that by using a combination of metric and topological arguments (the metric being that of the sphere and its lift to the covering surface), one can not only recover basically all of standard Nevanlinna theory but that—quite astonishingly—the essential parts of the theory all extend to a far wider class of functions than the very rigid special case of meromorphic functions, namely, to functions that Ahlfors calls "quasiconformal"; in this theory the smoothness requirements may be almost entirely dropped, and asymptotically, images of small circles—rather than having to be circles—can be arbitrary ellipses as long as the ratio of the radii remains uniformly bounded.

Of his three geometric versions of Nevanlinna theory, Ahlfors has described the one on covering surfaces as a "much more radical departure from Nevanlinna's own methods" and as "the most original of the three papers," which is certainly the case. (According to Carathéodory that paper was singled out in the decision of the selection committee to award the Fields Medal to Ahlfors.) Nevertheless, the last of the three, published two years later in 1937, was destined to be probably at least as influential (1937,1). Here the goal was to apply the methods of differential geometry to the study of covering surfaces. The paper is basically a symphony on the theme of Gauss-Bonnet. The explicit relation between topology and total curvature of a surface, now called the "Gauss-Bonnet theorem," had not been around all that long at that time, perhaps first appearing in Blaschke's 1921 *Vorlesungen über Differentialgeometrie.*

It occurred to Ahlfors that if one hoped to develop a higher-dimensional version of Nevanlinna theory, it might be useful to have a higher dimensional Gauss-Bonnet formula, a fact that he mentioned to André Weil in 1939, as Weil recounts in his collected works. When Weil spent the year 1941-1942 at Haverford, where Allendoerfer was teaching, he heard of Allendoerfer's proof of the higher-dimensional Gauss-Bonnet theorem; and remembering Ahlfors's suggestion, he worked with Allendoerfer on their joint paper, proving the generalized Gauss-Bonnet theorem for a general class of manifolds that need not be embedded. That in turn led to Chern's famous intrinsic proof of the general Gauss-Bonnet theorem. As for Ahlfors's idea of adapting the method to obtain a higher-dimensional Nevanlinna theory, that had to wait until the paper by Bott and Chern in 1963.

The year following his Gauss-Bonnet Nevanlinna theory paper, Ahlfors published a deceptively short and unassuming paper called "An Extension of Schwarz's Lemma" (1938). The main theorem and its proof take up less than a page. That is followed by two brief statements of more general versions of the theorem and then four pages of applications. Initially it was the applications that received the most attention and that Ahlfors was most pleased with, since they were anything but a straightforward consequence of the main theorem. That is particularly true of the second application, which gives a new proof of Bloch's theorem in a remarkably precise form. Bloch's theorem states that there is a uniform constant *B* such that every function analytic in the unit disk, and normalized so that its derivative at the origin has modulus 1, must map some subdomain of the unit disk one-to-one conformally onto a disk of radius B. The largest such constant B is known as Bloch's constant. In 1937 Ahlfors and Grunsky published a paper giving an upper bound for B that they conjectured to be the exact value for this constant (1937,2). The conjectured extremal function maps the unit disk onto a Riemann surface with simple branch points in every sheet over the lattice formed by the vertices obtained by repeated reflection over the sides of an equilateral triangle, where the center of the unit disk maps onto the center of one of the triangles. One obtains the map by taking three circles orthogonal to the boundary of the unit disk that form an equilateral triangle centered at the origin with 30° angles and mapping the interior of that triangle onto the interior of a Euclidean equilateral triangle. Under repeated reflections one gets a map of the entire unit disk onto the surface described. Ahlfors and Grunsky write down an explicit expression for the function of that description with the right normalization at the origin and thereby get the size of the largest circular disk in the image, which is just the circumscribed circle of one of the equilateral triangles whose vertices are the branch points of the image surface. The size of that circle turns out to be approximately .472.

The above mentioned extension for the Schwarz lemma yielded the lower bound $\sqrt{3}/4 = .433...$ for the Bloch constant, a bound that has been improved by only .0001 in the intervening 67 years.

When his collected papers were published in 1982, Ahlfors commented that this particular paper "has more substance than I was aware of." He also said, "Without applications my lemma would have been too lightweight for publication." It is a lucky thing for posterity that he found applications that he considered up to his standard, since it would have been a major loss for us not to have the published version of the Ahlfors-Schwarz lemma. As elegant and important as his applications were, I believe that they have long ago been dwarfed by the impact of the lemma itself, which has proved its value in countless other applications and has served as the underlying insight and model for vast parts of modern complex manifold theory, including Kobayashi's introduction of the metric that now bears his name and Griffith's geometric approach to higher dimensional Nevanlinna theory. It demonstrates perhaps more strikingly than anywhere else the power that Ahlfors was able to derive from his unique skill in melding the complex analysis of Riemann surfaces with the metric approach of Riemannian geometry.

KLEINIAN GROUPS

BY IRWIN KRA

Of the many significant contributions of Lars Ahlfors to the modern theory of Kleinian groups, I will discuss only two, which are closely related: the Ahlfors finiteness theory and the use of Eichler cohomology as a tool for proving this and related results. Both originated in the seminal paper (1964).

For the purposes of this note, a Kleinian group G will always be finitely generated, nonelementary, and of the second kind. Thus, G consists of Möbius transformations, a subgroup of PSL$(2,C)$, and it acts discontinuously on a nonempty maximal open set $\Omega \subset C \cup \infty$, the region of discontinuity of G, whose complement Λ in $C \cup \infty$, is an uncountable perfect nowhere dense subset of the Riemann sphere.

In the early 1960s not much was known about Kleinian groups. Around the beginning of this century, Poincaré suggested a program for studying discrete subgroups of PSL(2,C); Poincaré's program was based on the fact that PSL(2,R) acts on the upper half plane H^2, a model for hyperbolic 2-space. The quotient of H^2 by a discrete subgroup (a Fuchsian group) of PSL(2,R) is a 2-dimensional orbifold (a Riemann surface with some "marked" points). By analogy PSL(2,C) acts on H^3, hyperbolic 3-space, and the quotient of H^3 by a torsion free discrete subgroup of PSL(2,C) is a 3-dimensional hyperbolic manifold. The study of subgroups of PSL(2,C) was successful because of its connection to classical function theory and to 2-dimensional topology and geometry, about which a lot was known, including the uniformization theorem classifying all simply connected Riemann surfaces. Poincaré's program was to take advantage of the connection of PSL(2,C) to 3-dimensional topology and geometry to study groups of Möbius transformations. However, in 1965 very little was known about hyperbolic 3-manifolds. Research in the field seemed to be stuck and going nowhere. Ahlfors completely ignored Poincaré's program and took a different route to prove the finiteness theorem. He used complex analytic methods and his result described the Riemann surfaces that can be represented by a Kleinian group. About 15 years later in the mid-1970s, as a result of the fundamental contributions of W. Thurston (1982), 3-dimensional topology came to the forefront in the study of Kleinian groups.

The history of Ahlfors's work on Kleinian groups is also part of a remarkable collaboration between Lars Ahlfors and Lipman Bers. Although they coauthored only one paper (1960), their work and the work of many of their students was intertwined. See, for example, Kra (1996). Ahlfors's finiteness theorem says that the ordinary set Ω of a finitely generated Kleinian group G factored by the action of the group is an orbifold of finite type, finitely many "marked" points and compactifiable as an orbifold by adding a finite number of points.

Bers (1965) reproved an equivalent known result in the Fuchsian case, a much simpler case to handle. The finiteness theorem for PSL(2,R) had been known for a long time, and Bers reproved it using modern methods, Eichler cohomology. Bers constructs Eichler cohomology classes from analytic potentials by integrating cusp forms sufficiently many times, using methods developed by Eichler (1957) for number theory.

Ahlfors generalized Bers's method to a much wider class of subgroups of PSL(2,C). This generalization was completely nontrivial. It required the passage from holomorphic potentials to smooth potentials. This involved a conceptual jump forward—a construction of Eichler cohomology classes via an integral operator, producing a conjugate linear map α that assigns an Eichler cohomology class $\alpha(\phi)$ to a bounded holomorphic q-form ϕ for the group G. In addition, there appeared a very difficult technical obstacle that Ahlfors had to surmount to prove the injectivity of α. To do so Ahlfors introduced a mollifier, a function used to construct an approximate identity. Ahlfors worked only with the case $q = 2$. Using a modified Cauchy kernel, he constructed a potential for $\lambda^{2-2q}\ \bar{\phi}$ a continuous function on C whose $\bar{z}$ derivative is $\lambda^{2-2q}\ \bar{\phi}$, where is λ a weight function.

In his proof of the finiteness theorem (1964), Ahlfors omitted the case of infinitely many thrice-punctured spheres appearing in Ω/G. Such surfaces admit no moduli deformations and alternatively carry no nontrivial integrable quadratic differentials. This case was covered in subsequent papers of Bers, Greenberg, and Ahlfors. Ahlfors also initially limited his work to quadratic differentials, in part because this case and the abelian case are the only ones with geometric significance. Perhaps more significantly, it was Ahlfors's style to make the pioneering contributions to a field and leave plenty of room for others to continue in the same area. In this particular case much remained to be done.

Bers saw that if he studied the more general case of q-differentials, he would be able to improve on the results of Ahlfors and get quantitative versions of the finiteness theorem that have become known as the Bers area theorems. The first of these (Bers, 1967) implies that if G is generated by N-motions, then

$$Area\ (\Omega/G) \leq 4\pi\ (N-1).$$

Since the minimal area of a hyperbolic orbifold is $\pi/21$, Bers's area theorem gives an upper bound on the number of connected Riemann surfaces represented by a non-elementary Kleinian groups as $84(N-1)$. Ahlfors lowered that bound to $18(N-1)$ (1968). Even after some important work of Abikoff, there is still no satisfactory bound on the number of surfaces that a Kleinian group represents, especially if one insists on using only 2-dimensional methods. Bers's paper (1967) also showed that the thrice-punctured spheres issue can be resolved "without new ideas." It, together with Ahlfors's discoveries on Kleinian groups, led 15 years later to work on the vanishing of Poincaré and relative Poincaré series.

The so-called measure zero problem first surfaced during a conference at Tulane in 1965, the first of a series of periodic meetings, roughly every four years, of researchers in fields related to the mathematical interests of Lars Ahlfors and Lipman Bers. In his 1964 paper Ahlfors remarked that perhaps of greater interest than the theorems he had been able to prove were the ones he was not able to prove. First of these was the assertion that the limit set of a finitely generated Kleinian group has two-dimensional Lebesgue measure zero. This has become known as the Ahlfors measure zero conjecture. It is still unsolved, although important work on it has been done by Ahlfors, Maskit, Thurston, Sullivan, and Bonahan. In some sense the problem has been solved for analysts by Sullivan (1980), who showed that a non-trivial deformation of a finitely generated Kleinian group cannot be supported entirely on its limit set; topologists are still interested in the measure zero problem. Formulae in Ahlfors's attempt (1982) to establish the measure zero conjecture led Sullivan to prove a finiteness theorem on the number of maximal conjugacy classes of purely parabolic subgroups of a Kleinian group.

The measure zero problem not only opened up a new industry in the Kleinian groups "industrial park," it also revived the connection with 3-dimensional topology following the fundamental work of Marden and Thurston. It showed that Poincaré was not all wrong when he thought we could study Kleinian groups by 3-dimensional methods.

The second theorem that Ahlfors had wanted to establish in his 1964 paper can be rephrased in today's language to say that a finitely generated Kleinian groups is geometrically finite. A counter-example was later produced by Greenberg.

QUASICONFORMAL MAPPINGS

BY FREDERICK GEHRING

In 1982 Birkhäuser Boston published two volumes of Lars Ahlfors's collected papers and his fascinating commentaries on them. Volume 2 contains 43 articles. Twenty-one of these are directly concerned with quasiconformal mappings and Teichmüller spaces, 12 with Kleinian groups, and 10 with topics in geometric function theory. This distribution illustrates the dominant role that quasiconformal mappings played in this part of Ahlfors's work. Moreover, quasiconformal mappings played a key role in several other papers, for example, the important finiteness theory for Kleinian groups. For this reason I have chosen quasiconformal mappings as the subject of the final part of this survey. In particular, I will consider four papers on this subject that have had great impact on contemporary analysis.

ON QUASICONFORMAL MAPPINGS (1954)

In his commentary on this paper Ahlfors wrote, "It had become increasingly evident that Teichmüller's ideas would profoundly influence analysis and especially the theory of functions of one complex variable. . . . The foundations of the theory were not commensurate with the loftiness of Teichmüller's vision, and I thought it was time to reexamine the basic concepts." The quasiconformal mappings considered by Grötzsch and Teichmüller were assumed to be continuously differentiable except for isolated points or small exceptional sets. Teichmüller's theorem concerned the nature of the quasiconformal mappings between two Riemann surfaces S and S' with minimum maximal dilatation. This and the fact that any useful theory that generalizes conformal mappings should have compactness and reflection properties led Ahlfors to formulate a geometric definition that was free of all *a priori* smoothness hypotheses.

A *quadrilateral* Q is a Jordan domain Q with four distinguished boundary points. The *conformal modulus* of Q, denoted by $mod(Q)$, is defined as the side ratio of any conformally equivalent rectangle R. Grötzsch showed that if $f: D \to D'$ is K-quasiconformal in the classical sense, then

$$\frac{1}{K}\ mod(Q) \le mod(f(Q)) \le K\ mod(Q) \qquad (1)$$

for each quadrilateral $Q \subset D$. Ahlfors used this inequality to define his new class of quasiconformal mappings: a homeomorphism $f : D \to D'$ is K-quasiconformal if (1) holds for each quadrilateral $Q \subset D$. Ahlfors then established all of the basic properties of conformal mappings for this general class of homeomorphisms, including a uniform Hölder estimate, a reflection principle, a compactness property, and an analogue of the Hurwitz theorem. He did all of this in nine pages. The major part of this article was, of course, concerned with a statement, several interpretations, and a proof of Teichmüller's theorem.

In his commentary on this paper Ahlfors modestly wrote, "My paper has serious shortcomings, but it has nevertheless been very influential and has led to a resurgence of interest in quasiconformal mappings and Teichmüller theory."

This is an understatement. Ahlfors's exposition made Teichmüller's ideas accessible to the mathematical world and resulted in a flurry of activity and research in the area by scientists from several different fields, including analysis, topology, algebraic geometry, and even physics.

Next Ahlfors's geometric approach to quasiconformal mappings stimulated analysts to study this class of mappings in the plane, in higher-dimensional Euclidean spaces, and now in arbitrary metric spaces. His inspired idea to drop all analytic hypotheses eventually led to striking applications of these mappings in other parts of complex analysis such as discontinuous groups, classical function theory, complex iteration, as well as in other fields of mathematics, including harmonic analysis, partial differential equations, differential geometry, and topology.

THE BOUNDARY CORRESPONDENCE UNDER QUASICONFORMAL MAPPINGS (1956)

In the previous paper Ahlfors proved that a quasiconformal mapping $f : D \to D'$ between Jordan domains has a homeomorphic extension to their closures. A classical theorem due to F. and M. Riesz implies that the induced boundary correspondence ϕ is absolutely continuous with respect to linear measure whenever ∂D and $\partial D'$ are rectifiable and f is conformal. Mathematicians asked whether this conclusion holds when f is K – quasiconformal.

By composing f with a pair of conformal mappings, one can reduce the problem to the case where $D = D' = H$, where H is the upper half plane and $\phi(\infty) = \infty$. Next if x and t are real with $t > 0$ and if Q is the quadrilateral with vertices at $x - t$, x, $x + t$, ∞, then $mod(Q) = 1$ and inequality (1) implies that

$$\frac{1}{\lambda} \le \frac{\phi(\chi+t)-\phi(\chi)}{\phi(\chi)-\phi(\chi-t)} \le \lambda \qquad (2)$$

where $\lambda = \lambda(K)$. Inequality (2) is a quasisymmetry condition that many thought would imply that ϕ is absolutely continuous.

In 1956 Ahlfors and Beurling published the paper cited above in which they exhibit for each $K > 1$ a K – quasiconformal mapping $f : H \to H$ for which the boundary correspondence $\phi : \partial \mathrm{H} \to \partial H$ is completely singular. The importance of this example was, however, overshadowed by the authors' main theorem, which showed that inequality (2) characterizes the boundary correspondences induced by quasiconformal self-mappings of H. The sufficiency part consisted in showing that the remarkable formula

$$f(z) = \frac{1}{2y}\int_0^y [\phi(x+t)+\phi(x-t)]dt + \frac{1}{2y}\int_0^y [\phi(x+t)+\phi(x-t)]dt \qquad (3)$$

yields a K – *qua*siconformal self-mapping of H with $K = K(\lambda)$ whenever ϕ satisfies (2). Moreover, f is a *hyperbolic quasi-*

isometry of H, a fact that turns out to have many important consequences.

In 1962 it was observed that a quasiconformal self-mapping of the n-dimensional upper half space H^n induces a quasiconformal self-mapping of the $(n-1)$–dimensional boundary plane ∂H^n. This fact, for the case $n = 3$, was an important step in the original proof of Mostow's important rigidity theorem. It was then natural to ask if every quasiconformal self-mapping f of ∂H^n admits a quasiconformal extension to H^n. This question was eventually answered in the affirmative by Ahlfors in 1963 for the case when $n = 3$, by Carleson in 1972 when $n = 4$, and by Tukia-Väisälä in 1982 for all $n \geq 3$.

RIEMANN'S MAPPING THEOREM FOR VARIABLE METRICS (1960)

If $f: D \to D$ is K – quasiconformal according to (1), then f is differentiable with $f_z \neq 0$ a.e in D and

$$\mu_f = \frac{f_{\bar{z}}}{f_z} \tag{4}$$

is measurable with

$$\left|\mu_f\right| \leq k = \frac{K-1}{K+1} < 1 \tag{5}$$

a.e. in D. The *complex dilatation* μ_f determines f uniquely up to post composition with a conformal mapping. The main result of this article, joint with Lipman Bers, states that for any function μ that is measurable with $|\mu| \leq k < 1$ a.e. in D, there exists a K – quasiconformal mapping f of D that has μ as its complex dilatation. Moreover, if f is suitably normalized, then f depends holomorphically on μ.

The above result, known by many as the "measurable Riemann mapping theorem," has proved to be an enormously effective tool in analysis. It is a cornerstone for the study of Teichmüller space, it was the key for settling several outstanding questions of classical function theory including Sullivan's solution of the Fatou-Julia problem on wandering domains, and it currently plays a major role in the study of iteration of rational functions.

Unfortunately, as we all know, important theorems in mathematics sometimes become definitions. This theorem may already have become a verb in complex dynamics. For at a plenary lecture at the International Congress of Mathematicians in 1986 a distinguished French mathematician Adrien Douady was heard to explain that "before mating two polynomials, one must first Ahlfors-Bers the structure."

QUASICONFORMAL REFLECTIONS (1963)

A Jordan curve C is said to be a *quasicircle* if it is the image of a circle or line under a quasiconformal self-mapping of the extended complex plane. A domain D is a *quasidisk* if ∂D is a quasicircle. Quasicircles can be very wild curves. Indeed, for each constant $0 < a < 2$ there exists a quasicircle C with Hausdorff dimension at least a.

Nevertheless, the first theorem of this elegant paper contains the following remarkable characterization for this class of curves. A Jordan curve C is a quasicircle if and only if there exists a constant b such that

$$|z_1 - z_2| \leq b|z_1 - z_3| \tag{6}$$

for each ordered triple of points $z_1, z_2, z_3 \in C$. The proof for the sufficiency of (6) depends on the fact that the function in (3) is a hyperbolic quasi-isometry.

The fact that quasicircles admit such a simple geometric description is one of the reasons why these curves play an important role in many different areas of analysis. Inequality (6) is universally known as the "Ahlfors condition" and many regard it as the best way to define the notion of a quasicircle.

The second main result of this paper asserts that the set S of Schwarzian derivatives

$$S_f = \left(\frac{f''}{f'}\right)' - \frac{1}{2}\left(\frac{f''}{f'}\right)^2 \tag{7}$$

of conformal mappings f that map the upper half plane H onto a quasidisk D is an open subset of the Banach space of holomorphic functions ϕ with norm

$$\|\Phi\|_H = \sup_H |\Phi(z)|^2 y^2 \tag{8}$$

This fact is the key step in proving that the Bers universal Teichmüller space $T(1)$ is the interior of S with the topology induced by the norm (8). It also led to another surprising connection between quasiconformal mappings and classical function theory, namely, that a simply connected domain D is a quasidisk if and only if each function f, analytic with small Schwarzian derivative S_f in D, is injective. Here the size of S_f is measured by

$$\|S_f\| = \sup_D |S_f(z)|^2 \rho_D(z)^{-2} \tag{9}$$

where ρ_D is the hyperbolic metric in D.

Lars's beautiful paper on quasiconformal reflections pointed out how these mappings occur naturally in many different areas of mathematics. Lars lectured on this material at the Forschungs Institute at Oberwolfach, Germany, in 1963. He, Professor Olli Lehto of the University of Helsinki, and I were to speak the same morning. After several hours of socializing and wine the evening before, Olli and I tried to excuse ourselves so that we could get some sleep before our talks. We were told by Lars that that "was a very silly idea indeed" and that it would be far better to relax and drink with the pleasant company. The next day Lars's talk went extremely well and he was asked if he believed that staying up late always improved his lectures. "I am not sure," he replied, "but at least they always sound better to me!"

Quasiconformal mappings appear first under this name in Ahlfors's 1935 paper on covering surfaces, the famous paper for which he received the Fields Medal in 1936. In discussing Lars's work, Carathédory said that this article opened a completely new chapter in analysis, one that could be called "metric topology." In a commentary on this article Lars wrote, "Little did I know at the time what an important role quasiconformal mappings would come to play in my own work."

Lars's geometric approach to quasiconformal mappings stimulated their development in higher dimensional Euclidean space and recently in general spaces, such as the Heisenberg and Carnot groups. The fact that this class is so natural and flexible has led to striking applications to Kleinian groups, classical function theory, complex dynamics, and to other parts of mathematics, including harmonic analysis, differential geometry, elasticity, and topology.

The class of quasiconformal mappings offers a stripped-down picture of the geometric essentials of complex function theory and, as such, admits applications of these ideas to many other parts of analysis and geometry. They constitute just one illustration of the profound and lasting effect that the deep, central, and seminal character of Lars's research has had on the face of mathematics.

FINAL REMARKS

In addition to the Fields Medal he received in 1936, Lars was awarded an International Prize from the Wihuri

Foundation of Finland in 1968, an award also known as the Sibelius Prize when given to composers.

He was honored with the Wolf Prize in Jerusalem in 1981. In describing the mathematical achievements of the prizewinner, the jury ended its report with the following final comment: “Everyone working today in complex analysis is in some sense a student of Ahlfors.”

At each International Congress of Mathematicians it is customary for the host country to nominate its most prestigious mathematician as honorary president of the congress. This congress was held in the United States in 1986, 50 years after Lars had been awarded the Fields Medal in Oslo. At this time Lars was asked by his American colleagues to serve as the honorary president of this meeting in recognition of his achievements in research.

Lars was very generous with his time and always glad to talk with and advise young students and mathematicians. He has also had a powerful and lasting influence in his field through the aid and direction he gave to the 24 doctoral students who wrote theses under his direction as well as to at least an equal number of postdoctoral visitors and many close associates who benefited greatly from the opportunity to consult with him on problems of common interest. At a conference held in Storrs, Connecticut, to commemorate his seventy-fifth birthday, Lars remarked that “retirement is wonderful. I can’t perish any more, so I don’t have to publish!” The upshot of his remark was that he could now devote himself full time to understanding the new developments in his field without the pressure of having to write them up. At 75 Lars was devoting himself to learning the newest ideas in the subject!

Lars died in Pittsfield, Massachusetts, in October 1996. He was survived by his wife, Erna, and three daughters, Cynthia, Vanessa, and Caroline. A son, Christopher, died in infancy.

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Photo by David E. Almond

Gabriel A. Almond

GABRIEL A. ALMOND

January 12, 1911–December 25, 2002

BY SIDNEY VERBA, LUCIAN PYE, AND HEINZ EULAU

WITH THE PASSING OF Gabriel Almond on December 25, 2002, shortly before what would have been his ninety-second birthday, the profession of political science lost one of its most talented, creative, disciplined, influential, and widely respected members. At the time of his death, Almond, a professor emeritus at Stanford University, was still actively involved in a number of research projects and remained vitally interested in public affairs.

Gabriel A. Almond was born in 1911 in Rock Island, Illinois, and was raised in Chicago, the son of a rabbi. Though he lived a secular life, his religious background can be seen in many ways, from his frequent references to biblical events and biblical themes to the deep moral commitments that infused his work. His last work, finished just before his death, was on religious fundamentalism.

THE CHICAGO YEARS: 1928-1938

Throughout his scholarly life, it was Almond's good fortune to be, as he put it, in the right place at the right time—a pattern of luck that began in his undergraduate and graduate years at the University of Chicago. By the middle of the 1920s, under the leadership of Charles E. Merriam, the Chicago Department of Political Science had become the creative center of a behavior-oriented and interdisciplinary movement in political science, a movement that later spread through the entire discipline in the two decades after World War II. Merriam surrounded himself with superior students who became his colleagues and would translate their mentor's message into novel theoretical and methodological approaches to the study of politics. Best known among them still today are Harold F. Gosnell and Harold D. Lasswell. Their influence on Almond is unmistakable in his post-World War II work on the role of public opinion in the making of American foreign policy, on the psychological appeal of communism, and on his masterful and influential study—in collaboration with Sidney Verba—of the "civic culture" in five nations.

Almond's intellectually rewarding career began in 1928 with his entry as an undergraduate to the University of Chicago, where he encountered a faculty that was working at the discipline's research frontiers as well as a cohort of bright fellow graduate students who became innovators in different fields of specialization and leaders in the profession.

In his senior year Almond took Lasswell's course on "Non-Rational Factors in Political Behavior" and, clearly under Lasswell's guidance to judge from its voluble title, wrote a senior thesis on "Developmental and Equilibrium Analysis of Balancing Power Processes." He also collaborated with Lasswell in a joint study of people on public relief. The study, a truly pioneering work, was based on a sample of case records as well as personal interviews with relief clients that Almond conducted while working as a casework aide in the Stockyards district of the Unemployment Relief Service. It led to Almond's first published article (with Lasswell) in *The American Political Science Review* for August 1934, under the title "Aggressive Behavior by Clients Toward Public Relief Administrators: A Configurative Analysis." This first publication exemplifies Almond's lifetime work as a social scientist: concern for real people and real societies, with all their problems, potentialities, and conflicts; the skill to observe and study them systematically through carefully gathered data; and the skill to make sense of the data through more general and abstract theorizing. Many years later he described the translation of everyday events at work into systematic social science data: "As I sat there day after day writing complaints on three-by-five slips of paper, it occurred to me that I was witnessing human behavior, and that perhaps it was interesting and researchable" (2002, pp. 2-4).

Lasswell also encouraged Almond's Ph.D. dissertation on the elite of New York City, one of his mentor's interests. Of his New York adventure Almond once recalled, "I went to New York . . . bringing my University of Chicago culture with me. . . . Making contacts with the New York City elite . . . presented some problems. . . . I had, in some sense, to give false credentials to get invited to a dinner or a social occasion as a graduate student working for a Ph.D., and what I really was interested in was . . . seeing at first hand what their [the elite's] attitudes and their values were." His good intention to be a "participant observer" could not be sustained. "I just couldn't take it [like tea with Emily Post, he often recounted in good humor] and at the same time do a full day's work at the New York Public Library."[1]

The story of Almond's tribulations as a Ph.D. thesis writer has a unique aftermath. While he successfully defended the dissertation and received the degree in 1938, the work was not published until 60 years later, under the title *Plutocracy and Politics in New York* City (1998). The reason for this enervating postponement was that when in 1944 Almond included a number of chapters on the psychological aspects of wealth, Professor Merriam refused to recommend its publication, concerned about offending some of the major

New York donors to the University of Chicago. As Almond has ruefully written, including the chapters "would have given me the claim of being a political psychologist as well as a political sociologist."[2]

With the Ph.D. baton in his briefcase, Almond joined the faculty of Brooklyn College in 1938, at a time when jobs in academe were difficult to come by. He later remembered the "boredom" of having to teach five sections of the conventional course in American government for 15 hours per week. He remained at Brooklyn until World War II, which rescued him by bringing him to Washington for government service.

THE WAR YEARS: 1941-1946

Wartime Washington was a beehive of social scientists, and Almond became one of the hundreds of bees who found themselves in the dozen or so agencies that were in need of "intelligence." The demand for intelligence as a governmental function on a large scale was something radically new. That the Chicagoans would be in the forefront of the social scientists arriving in Washington should not come as a surprise, and the nation's capital became something of a replica writ large of the interdisciplinary movement that had been nursed at the University of Chicago. Once again, Harold Lasswell was for many, whether from Chicago or elsewhere, a kind of advance man who facilitated their migration into the new agencies. Through Lasswell's intervention Almond obtained a job in the bureau of intelligence within the Office of Facts and Figures (later the Office of War Information). Lasswell, as Almond recounts, thought of the bureau as "a really major research effort, both here and abroad, that would guide American information and activity. . . . In particular, he wanted to have a monitoring of the media in the country and abroad. He wanted to have a regular surveying of opinion and attitudes relating to the war."[3] Though the agency's emphasis shifted from informed social science research to easily available news reports as sources of intelligence, Almond continued to work for the reduced operation. His job was to help in setting up a content analysis code. Almond also headed a small unit assigned to collect information about Germany, Italy, and occupied Europe. "Beginning with a knowledge of German, I began to think of myself as a European specialist, and as a comparativist during these middle years of the war." While, from the point of view of his interdisciplinary education and orientation, Almond once again found himself in the right place at the right time, he seems to have considered his government experience as not rewarding. "I can't say," he told an interviewer, "that our morale, as contributing to the war effort, was particularly high."[4]

Much more exciting and rewarding was his work in postwar Germany for the U.S. Strategic Bombing Survey. The major purpose was to study, by way of survey research, the effect of strategic bombing on the population's attitudes and behavior. The Almond team's special assignment was to retrieve documents dealing with the air war and interrogating police and Gestapo officials but also survivors of the German resistance. In this connection Almond came to be in contact with American social scientists, especially the scholars who were experimenting with and applying probability sampling in survey research. Some of them had come from the National Opinion Research Center; others later migrated to the University of Michigan and formed the Survey Research Center. Once again, Almond had come to be at the right place at the right time; he later referred to this unusual experience as "a form of postdoctoral training."

RETURN TO THE ACADEMY: 1947-1963

Almond was appointed to the professorate at Yale in 1946, where he also became a member of the Institute of International Studies, one of the first of such research groups in the country with an interdisciplinary orientation. Once again, he found himself in an intellectually stimulating environment. His first major book, *The American People and Foreign Policy*, published in 1950, quickly established him as a leading practitioner of a behavioral political science. Immediate evidence of the work's importance came when the journal *World Politics*, then only in its second year, asked a well-known social psychologist to review it and gave him the unusual space of more than 10 pages for doing it. One of the study's major themes is the periodic swings of American public opinion toward international affairs—from idealistic to cynical attitudes, from a support for withdrawal to support for intervention, from optimism to pessimism. Much influenced by the then current attempt to explain politics and society in psychosocial terms, but also distancing himself from the then fashionable but nebulous notion of "national character," Almond formulated the concept of mood. By mood Almond meant a rather pliable and formless reaction to an ambiguous context that was particularly pronounced in foreign affairs. He argued, however, that the pervasive and destructive nature of mood swings, especially among the lower social strata, which feel powerless, is offset by attentive publics among elites. Attentive publics is another then novel concept that Almond introduced into discourse about the relationship between public opinion and public policy formation.

When the institute moved to Princeton in 1950, Almond, now tenured, followed. About this time began his longtime and deep commitment to the interdisciplinary activities of the Social Science Research Council that launched on a national scale what had begun in Chicago as the behavioral movement in political science. Quite apart from this involvement (treated below), his own research of the early 1950s culminated in the innovative *Appeals of Communism*, published in 1954; a book that remains, even today, a masterful treatment of the topic. Based on a wide range of data—opinion polls conducted in this country and abroad, depth interviews with former communists, and content analysis of relevant documents—the study employed whatever methodologies and relevant theories were available at the time, securing for Almond the recognition of having been one of the first practitioners of political psychology, long before it had become a field of study in its own right. Almond remained at Princeton until 1959, when he moved back to Yale, and from there, four years later, to Stanford, where as chair from 1964 to 1969 he effectively rejuvenated an old-fashioned Department of Political Science.

TOWARD A COMPARATIVE POLITICS: 1951-1963

With the coming of the 1950s, Almond would again be the right person at the right place at the right time. It was a time of much ferment in the social sciences, especially his own home discipline of political science. The major foundations—Carnegie, Rockefeller, and Ford—had become aware of the need for social science research and for the training of social scientists. The Social Science Research Council, then headed by the political scientist Pendleton Herring, became a major agency for promoting new developments in the social (now increasingly named "behavioral") sciences. In the fall of 1953 the Political Behavior Committee of the Social Science Research Council, under the leadership of David Truman and Pendleton Herring, asked Gabriel Almond to organize a new SSRC committee to work on bringing the behavioral approach to the study of comparative politics. At that time the subfield of comparative politics was limited largely to the study of the major Western European states with an emphasis on constitutional and structural/institutional arrangements. Gabriel quickly organized the new Committee on Comparative Politics with a double mandate: first, to mobilize all the powers of the modern social sciences—including, in particular, the insights and findings of sociology, anthropology, and psychology—for the comparative study of political systems; and second, to expand the range of comparative analysis to include the non-Western world, in particular, the new states just emerging from colonial rule. A majority of the members of the initial Committee on Comparative Politics were specialists on the newly independent states and such non-Western countries as Japan, Turkey, and Iran.

By the summer of 1955 the committee had organized its first workshop, which examined the role of leadership in the political development of the postcolonial states. Almond recognized early on that among academics there was a great deal of untapped energy and specialized knowledge that could be brought together at relatively low cost to produce significant advances in the discipline. Although Gabriel was foremost an intellectual theorist and research scholar, he was also a man of action who had a keen sense of the state of the discipline and what organizational measures were likely to be most productive.

In addition to recruiting volunteer scholars, Almond sought additional foundation funds for a competitive program of grants to individuals for fieldwork. That effort supported 24 recipients, representing six disciplines, and produced research in 21 countries. The grants make possible such noteworthy studies as Edward Banfield's *The Moral Basis of a Backward Society*; Samuel H. Beer's *British Politics in a*

Collectivist Age; Seymour M. Lipset's *Political Man*; Fred Riggs's *Thailand: The Modernization of a Bureaucratic Polity*; and Myron Weiner's *The Politics of Scarcity*.

It soon became apparent that a proliferation of ad hoc area-oriented studies would not produce the accumulation of knowledge expected of a science. At the beginning Almond suspected that comparative politics would benefit greatly by following the experience of American politics, which had achieved a breakthrough by focusing on the role of interest groups. However, there needed to be a more solid theoretical foundation for the analysis of political development. Building on the earlier social theorists who analyzed social change during the initial industrial revolution in Europe and on Talcott Parsons and Edward Shils's new work, *Toward a Theory of Action*, Gabriel crafted a heuristic theory for analyzing total political systems. He posited that all political systems consisted of a set of specific functions that could be performed by the same or different structures in different settings. This structural-functional formulation was the basis for *The Politics of the Developing Areas* (1960), which he edited with James S. Coleman. Gabriel did not insist upon a rigid application of his theoretical formulation, but rather encouraged others to use what they found most useful. Thus, the approach, in a loose way, provided the basis of one of the committee's most noteworthy projects, the nine-volume series of "Studies in Political Development" published by the Princeton University Press. Each volume examined political development from a different perspective, such as communications, bureaucracy, political parties, political culture, and the historical sequences of a set of general crises in development.

The committee produced more than 300 reports, ranging from books to articles and unpublished memoranda. It organized 23 conferences and cosponsored 6 others. It conducted 5 summer workshops. In all its activities it involved some 270 scholars, with nearly 50 from foreign countries.

Gabriel had the extraordinary ability to recognize how people with different skills and area specializations, working with different concepts and theories, could still be brought together to produce a more general contribution to knowledge. He significantly advanced comparative studies through his ability to devise multiple models and to conceptualize typologies that would highlight significant factors for explaining differences among systems. He was thus able to bring order to the otherwise confusing world of political realities. As an intellectual leader he also had a remarkable instinct for judging when the stage was right for setting out in new directions. In the meetings of the committee he would tolerantly listen to the group discussion and then intercede to make first a general intellectual point, but then a proposal for action. He provided the leadership that fundamentally changed the character of comparative politics.

What is perhaps Almond's best known book, *The Civic Culture* (1963, with Sidney Verba), appeared during this period and had a significant impact on the comparative study of democracy. It was one of the first large-scale cross-national survey studies, and it examined the cultural roots of democracy in five nations. It opened the new field of comparative surveys and represented one of the first attempts to study cultural factors systematically in comparative politics. *The Civic Culture* spawned much additional research, some written to replicate it, some to present alternative positions, and some that went beyond it.

THE 1970S AT STANFORD

Almond's view of political change and development was broad and encompassing. In *The Politics of the Developing Areas* he proposed a broad analytical framework for identifying the basic institutions and processes of social change; in *The Civic Culture* he used quantitative empirical analysis to consider the cultural components of democracy. In the 1970s he worked with a group of students at Stanford on an even broader approach. In *Crisis, Choice, and Change* (1973), Almond and his collaborators considered the role of leadership and strategic choice in political change. They turned to history, using seven historical accounts to consider the relative applicability of various approaches to political explanation. As Almond put it later, "We took . . . four distinctively different approaches to development explanation and . . . tried to use them . . . in historical contexts, not so much to generate a theory from these case studies . . . but as a demonstration of how these distinctive approaches fitted in together and had to be used together to get an adequate historical explanation on the historical outcome." As he put it, his work had now gone beyond an earlier focus on the social and psychological variables that explained the input side of politics to consider the performance of political systems—their productivity.[5] This expansive view of political explanation was carried over to his well-known textbook with G. Bingham Powell (1978), a standard work that has gone through numerous editions.

THE YEARS OF RETIREMENT

Crisis, Choice, and Change was completed at about the time of Almond's retirement from Stanford in 1976. In the oral history interview with Richard Brody at about that time, he described this comprehensive view of comparative politics as representing a "sense of closure as far as my own career is concerned." But his career was far from closed. In retirement Almond remained an active scholar and member of the discipline, rarely missing the annual meeting of the American Political Science Association. His attention turned to two main topics: the state of the political science discipline and a study of the role of religious fundamentalism in political life.

In a number of articles, brought together in *A Discipline Divided* (1990), Almond deplored the divisions in political science. What he believed to have been a more unified, though pluralistic, discipline was now—to use the phrase that became standard in the field to describe the unease he and others felt—seated at "separate tables," unable and unwilling to collaborate. He described the discipline as divided into two tendencies: "those who view the discipline as a hard science—formal, mathematical, statistical, experimental—dedicated to the accumulation of tested 'covering laws,' and those who are less sanguine and more eclectic, who view all scholarly methods, the scientific ones as well as the softer historical, philosophical, and legal ones, as appropriate and useful." Almond identified with the second school, because he thought that the "qualities of human culture and behavior" were not explicable by hard and fixed laws (1990, p. 7). It was not so much that he rejected a scientific approach; rather that he wanted a political science that was open to many approaches, a political science that was empirical and whose conclusions were open to testing and falsification. His objection was to premature closure in the name of overarching theories. Rational actor theory was his prime example of the latter. To Almond, politics was too important and too complicated to be encompassed in any particular approach; he wanted us all around the same table arguing it out.

Gabriel spent a large part of his retirement as a leader of a large-scale project on fundamentalisms sponsored by the American Academy of Arts and Sciences. The project took a very broad view of what is one of the more important religious and political phenomena of our day, fundamentalist religion. The project has the Almond stamp on it. It brought together numerous scholars, specialists in one religion or culture, to consider the more general subject of fundamentalist religions—just as Almond had brought together, many years earlier, numerous specialists to study the comparative politics of development. Almond and others provided an overarching framework within which comparisons could be made, but not one that obliterated the particularities of the many religions studied. The result was a massive outpouring of scholarship: 75 research papers and 5 volumes. The project culminated in an overview volume, *Strong Religions* (2003), authored by Almond, R. Scott Appleby, and Emmanuel Sivan. The book considers the role of fundamental religion most broadly, from its social roots to its political consequences. It does not simplify and reduce all to a single pattern, but allows one to see beyond the particularities of each of the forms of fundamentalism.

The book also reflects Almond's lifetime interest in religion and its role in social and political life. He was a student of the Old Testament and often cited its lessons in a modern context. His last paper, finished just before his death, was on "Foreign Policy and the Theology of Ancient Israel." Almond's early work with the Committee on Comparative Politics had been within the framework of modernization theory and its focus on the secularization of the world, but he had never abandoned his belief in the importance of religion.

Few scholars have had as broad and sustained an impact on political science. Almond's first publication was his article in *The American Political Science Review*, with Harold Lasswell, in 1934 on bureaucratic encounters in welfare offices. His last, *Strong Religions*, appeared shortly after his death in 2003. Seven decades of creativity is a record few scholars attain. The article with Lasswell represented an innovative approach to citizen encounters with government, looking at the social and psychological micro-interactions of citizens face-to-face with officials. It was an approach that would be followed in many later works. And the article was about one of the most important substantive issues of that Depression era: how government provides assistance to its needy citizens. The last book, on fundamentalism, is on one of the most important substantive issues of the beginning of the twenty-first century. And it too will provide a template for further research in this important area.

It is fitting that in 2002, a year before his death, Almond published a collection of essays, *Ventures in Political Science: Narratives and Reflections*. At an age when many a scholar might collect a life's work of papers as a way of summarizing a productive career—and Gabriel's was surely productive during seven decades—he produced a set of insightful and relevant essays mostly written in his eighties.

Almond straddles Isaiah Berlin's famous distinction. He is neither a fox that knows many things nor a hedgehog that knows one big thing; rather he is a person who knows many big things. Almond was a producer of large-scale typologies and approaches who never abandoned close empirical work; a generalizer who accepted the variety of particular nations and cultures; an early user of quantitative approaches who never abandoned history. Some of Almond's schemas have been modified or replaced by others. Almond welcomed changes and modifications to his work, and assumed that others would move beyond it.

Seven decades of productivity: a long life, and a fruitful life. Gabriel Almond died on Christmas Day 2002, just before his ninety-second birthday. He was surrounded by his family at their annual reunion in Asilomar.

NOTE

1. American Political Science Association, Oral History Project. Interview with Richard Brody, 1976.
2. *Ibid.*
3. *Ibid.*
4. *Ibid.*
5. *Ibid.*

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Photograph by John Heinsinger

THOMAS FOXEN ANDERSON

February 7, 1911–August 11, 1991

BY ROBERT P. PERRY

THOMAS F. ("TOM") ANDERSON was internationally known for his pioneering use of the electron microscope to study viruses and bacteria. His ability to master the instrument in its early stages of development, his invention of an ingenious method for specimen preservation, and his acute perspicacity in interpreting his observations resulted in pictures of historic importance. These included the first micrographs to clearly show infectious viruses attaching to and reproducing in their bacterial hosts and elegant detailed images of male (donor) bacteria transferring genetic information to female recipients. His research achievements helped elucidate several important mechanistic principles of virus-host interactions.

Tom was born in Manitowoc, Wisconsin. His father, Anton Oliver, the first of seven children, was born on a farm in central Wisconsin, which his parents had homesteaded shortly after they arrived from Norway. Anton graduated from high school in the nearby village of Amherst and studied electrical engineering for two years at Lawrence College in Appleton before joining the Navy and serving as the chief electrician on the battleship USS *Texas*. After his honorable discharge from the Navy, Anton married Mabel Foxen, a young woman from Amherst, who was also of Norwegian descent. They settled in the beautiful little town of Manitowoc on the western shore of Lake Michigan and had two children: Tom and his younger brother, Norman.

Anton organized and built the Oslo Power and Light Company, which supplied electricity to power lines connecting the many small towns and farms of Manitowoc County. He also established the Anderson Electric Company to wire subscribers' buildings and to sell and repair electrical fixtures and appliances. As a result, Tom grew up not only with electrical toys but also with some knowledge of generators and motors, power lines, and transformers.

Tom's mother was an accomplished pianist. After graduation from Amherst High School, she studied music for two years at the Lutheran Seminary in Red Wing, Minnesota. Tom loved to hear her play the piano, and he also enjoyed music on the Andersons' radio, one of the first in the town of Manitowoc. Although Tom's paternal grandfather and maternal grandmother died shortly before his birth, their surviving spouses married each other and provided him and his brother, Norman, with a single set of double-loving and -spoiling grandparents. Their many children and grandchildren constituted an extended family, which provided Tom with a very pleasant and memorable childhood.

Tom became acquainted with bacterial diseases at a very young age. In this pre-antibiotic era, his brother developed a chronic mastoiditis, and sadly, when Tom was only nine years old, his mother died of tuberculosis after a long illness. Fortunately, his father's second wife, Edna Halvorsen, took over the care of the children as if they were her own. Because of Norman's illness, Anton sold his holdings in Manitowoc and searched for a climate that might be more beneficial to his son's health. The Andersons tried four locations: Tampa, Florida (1923-1924); Amherst, Wisconsin

(1924-1925); Rockford, Illinois (1925-1926); and Glendale, California, where they finally settled.

Scientists are often asked about aspects of their early education that might have stimulated them toward a career in science. In Tom Anderson's case, it may have been a botany course at Rockford High School taught by Miss Agnes Brown. As described in an autobiographical essay by Anderson, Miss Brown, although physically handicapped, guided her students on many field trips to neighboring fields and woods to collect specimens that would later be dissected and studied with compound light microscopes. As he viewed the intricate architecture of plant tissues and cells, Tom gained an early insight into how the great variety and specificity of biological structures appear to steadily increase as one examines them in ever-increasing detail. Under Miss Brown's tutelage, textbook concepts like cells and chromosomes became real objects once he had seen them.

Tom's second and third scientific loves were chemistry and physics. Excellent courses at Glendale High School and a chemistry set at home effectively developed these interests. After graduation, he successfully passed the entrance examination at the California Institute of Technology, and began his studies there in 1928. At this time the departments of physics and chemistry, led by Robert A. Millikan and A. A. Noyes, were well established with distinguished faculties. Biology was just getting started under the guidance of the geneticist T. H. Morgan. The courses were demanding, but Tom worked hard and received excellent grades. In his view the most rewarding courses were those that revealed scientific principles. Less attractive to him were those that required excessive memorization or dealt with abstract formalisms. He gravitated toward physical chemistry and was introduced to scientific research by the inorganic chemist Don M. Yost. In a senior project with another undergraduate, Folke Skoog, Tom determined the free energy of formation of iodine monobromide in carbon tetrachloride solutions. The data, which confirmed and extended earlier work, resulted in Tom's first publication, a paper in the *Journal of the American Chemical Society*.

After receiving his B.S. degree in 1932, Tom spent a year in Kasmir Fajan's Physikalisch-Chemisches Institut in Munich. This laboratory was concerned with the determination of refractive indices of various substances to the highest possible degree of precision. With Peter Wulff, Tom used an inexpensive spectrograph equipped with aluminum-coated mirrors to measure the dispersion of cesium chloride crystals in the ultraviolet. The interpretation of his data was made according to old classical theories of refraction developed by Lorentz, rather than the newer quantum mechanics, which had not yet effectively penetrated the thinking of the Munich group. When Tom returned to Caltech, he was persuaded to give a seminar on this research before an audience that included Linus Pauling. About midway through the seminar, Pauling commandeered the blackboard and, much to Tom's chagrin, sketched out the quantum theory of mole refractions. When he had finally finished, Tom continued his rigid presentation, erasing everything that Pauling had written and causing the audience to roar with laughter. Although this episode was alarming at the time, it obviously had a lasting effect on Tom's development into a mature scientist.

Tom resumed his research with Don Yost and studied the Raman spectra of various inorganic compounds, using vibrational frequencies to determine their thermodynamic constants. For his dissertation research, he showed how isotopes affected vibrational frequencies in boron compounds and in deuterium gas. For deuterium, in addition to effects on vibrational and rotational frequencies, the nuclear spin affected selection rules without influencing the force constants between atoms. All told, five publications resulted from this research.

After receiving his Ph.D. degree in 1936, Tom went to the University of Chicago to work with William D. Harkins on the properties of surface films. With a very simple and inexpensive film balance, he studied films formed by the combination of calcium ions with fatty acids and by cytochrome C monolayers. Decades later cytochrome C surface films were used by Kleinschmidt and Zahn to coat nucleic acids and give them sufficient contrast to be visible in the electron microscope.

Although brief, the year in the highly competitive Harkins laboratory provided Tom with other lessons for an aspiring scientist, which he related in his autobiographical essay. One was that claims of priority for discoveries based on unpublished data sequestered in old lab notebooks are unacceptable. An investigator should either have the courage to publish the best possible interpretation of the data and be prepared to suffer criticism if wrong or relinquish any future claims of priority. Another lesson stemmed from Harkins's habit of riding herd on his assistants and postdocs by daily inquisitional visits to their lab benches and by creating temporary outcasts among the group. Such a tense atmosphere would not foster creativity in people of Tom's temperament. For obvious reasons, he was anxious to change venues for his postdoctoral training. The opportunity came in the summer of 1937 when he was offered a position at the University of Wisconsin, where he would investigate the effects of ultraviolet light on yeast cells with B. M. Duggar and later serve as a laboratory assistant in Farrington Daniels's physical chemistry course.

The most pleasant part of Tom's Chicago experience was his meeting and falling in love with his future wife, Wilma Fay Ecton. Wilma, who came from Kansas City, Missouri, was at the University of Chicago studying for a career as a lawyer and a judge. Tom and Wilma met at the International House, a popular place on campus for dining and social activities. Their courtship continued after Tom's move to Wisconsin, and they were married in North Kansas City on December 28, 1937. They later had two children, Thomas Foxen Jr. in 1942 and Jessie Dale in 1946.

A crucial event in Anderson's career occurred in 1940 when he was awarded a fellowship, funded by RCA and administered by the National Research Council, to help explore biological applications of the electron microscope. Invented in Germany in the 1930s and later developed independently at the RCA Laboratories, the electron microscope greatly improved one's ability to peer into the world of very tiny objects. Entities that had heretofore remained invisible when magnified a thousandfold by the light microscope suddenly could be seen at magnifications of 10,000 to 50,000. The potential impact of this instrument on biology was enormous. The fellowship was directed by a committee of prominent biologists from all over the United States. The committee, which was chaired by Stuart Mudd, a bacteriologist from the University of Pennsylvania, probably selected Anderson because of his strong background in physics and chemistry and his biological research experience in the Duggar laboratory. Working at an intense pace in the RCA laboratory of Vladimir Zworkin in Camden, New Jersey, Tom collaborated with a steady stream of microbiologists, embryologists, and geneticists, who were all eager to visualize their favorite specimens in a totally new way.

The Camden laboratory had three electron microscopes. One instrument, designated as EMA, was designed by Ladislaus Marton, a Belgian, who had constructed and used an earlier model in Brussels in the mid 1930s. The EMA was difficult to use because it required frequent cleaning to remove contamination of the vacuum system. A second instrument, EMB, developed with the help of James Hillier, an electronics engineer from Toronto, was easier to use and became the prototype for RCA's first commercial electron microscope. The third microscope was an experimental high-voltage instrument. Tom's initial observations were made with the EMA. He switched to the EMB when it became available in July 1940, and he carried out some experiments with the high-voltage instrument after it was put into operation in mid-1941.

Everything that Tom and his collaborators looked at was novel in those days. He and Harry Morton observed that the reduction of potassium tellurite occurred within the cells of *Corynebacterium diphtheriae*. Tom, Stuart Mudd, Katherine Polevitzky, and Leslie Chambers were able to visualize for the first time flagella and the details of cell wall structures of various bacilli. He and Wendell Stanley were able to measure directly the sizes and shapes of various plant virus particles, and, in so doing, confirm the dimensions previously inferred from diffusion, ultracentrifugation, and flow birefringence measurements of viral suspensions. Importantly, the electron microscope had given these investigators the power to actually see as individual objects things that had been only mental concepts. Shades of Tom's early experience in Ms. Brown's botany class!

Other notable observations included the combination of antibodies with specific viral or flagellar antigens and the beautiful stereoscopic pictures of insect structures obtained with A. Glenn Richards Jr. One paper, published with Richards in 1942, describes the iridescent wing scales of blue morpho butterflies, where the optical path length between layers on the scales was found to be one half the wavelength of the blue light that is selectively reflected from them. This paper has the distinction of being cited 53 times in scientific literature published after 1991, a remarkable durability for research carried out a half-century earlier. In fact, such long-term durability, which testifies to the solidity of experimental observations and the deep insight of the interpretation of such observations, is a salient feature of Tom's research. Sixteen of his papers published more than 30 years ago continue to be cited in the current literature.

Tom supplied three important ingredients to the collaborative projects. First, he pursued these projects with great enthusiasm. He was willing to spend long and irregular hours working with his collaborators. Second, he had a thorough knowledge of the physical and chemical principles that governed the performance of the instrument and the quality of the specimen preparations. Such knowledge enabled him to make judicious adjustments of parameters and conditions that could spell the difference between success and failure in revealing fragile structures. Third, his easy-going personality, his calm unflappable demeanor, and his highly logical and orderly approach to problems maintained tranquility in the laboratory, thereby greatly enhancing the productivity of his numerous and diverse projects. As a result, some 31 papers resulted from his two years' work as an NRC-RCA fellow.

One of Tom's most exciting discoveries during this period was made in late 1941 and early 1942 when he, Salvador Luria, and a little later Max Delbrück looked at preparations of the viruses that infect bacteria, the bacteriophages. Studying a variety of phage strains active on the bacterium *Escherichia coli*, they observed uniform sperm-shaped objects with distinct head and tail structures. The initial observations with Luria showed that different strains had different morphologies, indicating that there are multiple families of bacteriophages rather than a single type as had previously been believed. Helmut Ruska, working concurrently in Germany with an electron microscope designed by his brother, Ernst, observed similar structures, although he was unable to distinguish clearly the phage from bacterial debris or to examine pure preparations of different phage strains. Unfortunately, World War II prevented Anderson and Ruska from having any open discussion of their results.

A more detailed study was made in the summer of 1942 at the Marine Biological Laboratory in Woods Hole, Massachusetts, where RCA had installed an EMB so that it could be seen by the visiting biologists. Together with Luria and Delbrück, Tom examined the infectivity and growth of phage α, later know as T1, and γ, later known as T2, each of which has a characteristic shape and size. Their micrographs clearly demonstrated the adsorption of virus on the host bacterium and, after a predicted time, the lysis of the host with the liberation of virus particles of only the infecting type. Thus, the phage "bred" true morphologically through each round of infection. These very important observations were contrary to a popular notion that bacteria harbored phage precursors that are converted to mature viruses upon infection. This provided the first compelling evidence that phages were not specified by genes of their hosts, but rather that they probably had genes of their own.

When his NRC-RCA Fellowship expired in September 1942, Tom decided that of the many fields that had been opened by the electron microscope, the study of bacteriophages offered the most interest and excitement. Thus began a lifelong commitment to phage research. He took a position in the Johnson Foundation for Medical Biophysics, then directed by Detlev W. Bronk, where Leslie A. Chambers had recently obtained an EMB for his studies of microbial pathogens. In addition to his phage research, Tom collaborated with Chambers, Mudd, and others on studies of pathogenic organisms, such as rickettsia and pneumococcus. During the next decade, Tom made several important discoveries, several of which, as he recounted later, were serendipitous. One such discovery was made when he was investigating the effects of ultraviolet light on the virus-host complex. For these experiments he had to use a UV-transparent minimal medium rather than the nutrient broth that was routinely used for the phage studies. He noted that, although the plating efficiency of T2 phage in this medium was normal, that of phages T4 and T6 was very low. Tracking down the explanation of this unexpected result, he found that the T4 and T6 phages would not attach to their host unless activated by an aromatic amino acid cofactor like L-tryptophan, which was present in the nutrient broth but not in the minimal medium. The cofactor phenomenon represented the first directly observed example of allosterism, for, as it was later shown, these cofactors cause the phage's long tail fibers to be released from the tail sheath so that the connectors on their tips can engage receptors on the surface of the host.

In 1946 Tom was appointed to the Penn faculty as an assistant professor of biophysics. He was promoted to associate professor in 1950. During this period, his pursuit of the cofactor phenomenon led to another serendipitous discovery, namely, the release of DNA from phage heads by osmotic shock. In experiments designed to determine how activation of T4 by tryptophan depends on salt concentration, Tom noted that the phage was inactivated if it was incubated in NaCl at a concentration greater than 2M and then rapidly diluted into a solution of low osmotic pressure. Following up this initial finding, he observed the empty heads of the osmotically shocked phage and the greatly increased viscosity of the disrupted preparations, indicating a loss of DNA. This was confirmed by Roger Herriott's chemical analysis, and, therefore, it could be concluded that DNA is required for the phage's infectivity.

According to Tom, one of the best ideas that he ever had was that of the critical point method for drying specimens for the electron microscope. It was obvious to him early on that most biological specimens were flattened by surface tension forces when dried in air on standard electron microscope grids. He was especially anxious to eliminate these surface tension artifacts, which seemed to be responsible for his uncertainty as to whether bacteriophage attached to their hosts by their heads or their tails. The extant electron micrographs could support either view. Exploiting his background in physical chemistry, he posed the key question: Given a material immersed in a liquid, how can one transfer it to a gas or vacuum without having a phase boundary, with its attendant surface tension, pass through it? The answer seemed obvious to him: Eliminate the phase boundary by raising the temperature of the ensemble above the critical point of the liquid, thus converting the liquid to a gas. Then let the gas escape at the higher temperature, which will leave the specimen high and dry. Because water, the liquid that specimens are usually immersed in, has a critical temperature of 374°C, which would likely destroy most biological material, he cleverly devised a procedure to replace the water with liquid carbon dioxide, which has a critical temperature of only 31°C, by stepwise passage of the specimen through series of miscible liquids. Using inexpensive components, he constructed an apparatus to prepare specimens by this method and quickly answered his quandary about phage attachment. The phages adsorb to receptive host cells by the tips of their tails. Tom presented beautiful stereoscopic pictures of phages and other biological material prepared by this method at the First Congress of Electron Microscopy meeting, held in the charming amphitheater of the Jardin des Plantes in Paris in 1952. His audience was stunned by the excellent quality of these pictures, and, appropriately, they accorded him their highest praise.

A few years later, Tom took a sabbatical leave to work at the Institut Pasteur in Paris with André Lwoff, Francois Jacob, and Elie Wollman on bacterial conjugation, a phenomenon originally described by Joshua Lederberg. Assembling a critical point apparatus from components that he brought with him, Tom was able to make highly detailed stereoscopic electron micrographs of pairs of mating bacteria connected by a narrow tube through which DNA could pass from the male to the female strain. These vivid pictures and their interpretation were published in a 1957 paper by Anderson, Wollman, and Jacob in the *Annales de L'Institut Pasteur*. This paper was a paragon of clarity, elegant experimentation, and incisive analysis. The pictures have become the classic illustrations of bacterial conjugation in scientific textbooks. An amusing popularization of this work occurred when one of the pictures was used to illustrate a story in the magazine *Paris Match*, which was titled simply "La Vie." The picture caption was "Un Accouplement de Bacterie."

The 18-month stay in Paris, which was supported by prestigious fellowship awards to Tom from the Fulbright Scholarship Fund and the Guggenheim Foundation, was certainly a highlight in the lives of the Anderson family. The rich cultural experience, the challenge of a foreign language, and the vibrant scientific atmosphere of the Pasteur Institute all combined to make this a memorable experience for Tom, Wilma, and their two children.

In the latter part of his stay in Paris, Tom decided to investigate the recombination between male and female genes that occurs in the zygote after conjugation. With a light microscope and a micromanipulator, he devised a system to isolate the individual progeny of the zygote through successive cell divisions. With this system, he and Lwoff's technician, R. René Mazé, were able to follow the pedigrees of more than a score of exconjugants. The results were surprising and confusing. In contrast to the male exconjugants, which divided regularly after separating from the females, the female exconjugants (the zygotic progeny) divided erratically and exhibited a diverse array of morphological abnormalities, which in some cases led to eventual death. At that time, the lack of knowledge of the yet-to-be discovered episomal plasmids and the poor understanding of genetic recombination mechanisms prevented Tom from providing a reasonable explanation of these strange results. Nevertheless, he decided to publish them, adhering to the adage of Albert Einstein that is engraved near his statue on the grounds of the National Academies: "The right to search for truth implies also a duty; one must not conceal any part of what one has recognized to be true."

In 1957 Tom returned to the Johnson Foundation and the University of Pennsylvania and in 1958 was promoted to professor of biology. It was during this period that I first met him. I came to the Johnson Foundation for postdoctoral training with Britton Chance and was mainly involved in projects dealing with mitochondria and respiration. In a study with synchronized populations of *E. coli*, I wanted to verify the degree of synchrony by examining the bacteria with the electron microscope at various stages of the cell division cycle. At this time, Tom had a very simple microscope that he put at my disposal. Although this microscope was perfectly adequate for my purposes, it did not have sufficiently high resolution for Tom to take advantage of the powerful new negative staining technique, which had recently been developed. The opportunity to obtain such a microscope came when he was offered a senior position at the Institute for Cancer Research (ICR) and a chance to initiate electron microscope studies at that institution. Located in northeast Philadelphia, the ICR not only had very pleasant surroundings but also a firm commitment to basic research in biology, a tradition established by its first director, Stanley Reimann, and carried on by his successor, Timothy Talbot. Tom happily accepted the offer, which also came with a substantial increase in salary. He joined ICR in 1958 and maintained his affiliation with Penn as an adjunct professor in both the biophysics and biology departments.

As I was completing my research at the Johnson Foundation, I received an American Cancer Society Fellowship to take additional postdoctoral training in a world-renowned laboratory of cell biology in Brussels, Belgium. Before I departed for Brussels in January 1959, Tom inquired whether I might like to join him in his new laboratory when I returned to the States. This was an attractive offer, because it was unlikely that I would be able to find an equivalent academic or research position in this country while living abroad. After a few months of deliberation, I decided to accept the offer and to begin work at the ICR in the summer of 1960. This turned out to be one of the best decisions that I ever made.

Tom's generosity and support were evident from the moment my wife and I and our two small children arrived at the Philadelphia airport, somewhat haggard after traveling for more than 25 hours because of extended flight delays. He drove us to his home, where we were put up for the night and allowed to get some much-needed rest. At this time the Andersons were still living near the university, but they would soon be moving to a lovely split-level home in Fox Chase. The next day he brought us to a comfortably furnished home near the ICR, which had been rented for our temporary use until we could find something more permanent. Tom and Wilma even made sure that we had the necessary groceries and household supplies. This was certainly a warm welcome, which grew into a lifelong friendship between the Anderson and Perry families.

In his new lab at the ICR, Tom was using his state-of-the-art Siemens microscope to investigate the fine structures of phages. In one study, carried out with Nobuto Yamamoto, a young research associate from Japan, an interesting phenomenon termed "genomic masking" was discovered. These studies involved a temperate phage that infects the bacterium *Salmonella typhimurium*. At a low frequency, bacteria infected with phage P22 produced, in addition to the P22 progeny, a variant form with a morphologically distinct tail structure. It turned out that the variant was the result of an exchange between a latent capsid-encoding gene in the bacterial genome and the normal capsid gene of the infecting phage. This observation was one of the earliest examples of such genetic exchange.

Throughout the 1960s, the Anderson laboratory continued to be at the forefront of the bacteriophage field. As the techniques for specimen preparation were perfected, including thin sectioning combined with negative staining, finer and finer ultrastructure could be visualized. Tom was fascinated by the symmetry properties of the viral structures, particularly the connection between the icosahedral phage heads, which have fivefold symmetry and the phage tails, which have hexagonal symmetry. He wrote thoughtfully about this interesting relationship, which he considered to be one of nature's mysteries. In a notable study Tom and Manfred Bayer, a research associate from Germany, described in exquisite detail the surface structure of *E. coli*. This study revealed membrane patches that were later found to be sites of viral attachment. In another elegant series of experiments, he and his graduate student, Lee D. Simon, presented some superb electron micrographs showing T2 and T4 phages in the process of infecting their hosts. In these pictures one could visualize changes in the shape of the delicate tail fibers, repositioning of the short tail pins, and contraction of the tail sheath. One could also see structural changes in the tail base plate and the needle through which DNA is injected into the bacterium. These extraordinarily detailed pictures have graced the pages of many textbooks.

In these research projects Tom usually gave his young collaborators leeway to work independently and to follow their own instincts as much as they desired. As a mentor, he was accessible for discussions of results, exchanges of ideas, and suggestions based on his sound knowledge of physical principles. He played a major role in the write-ups of the experiments, insisting that they be logically presented and critically interpreted. Between 1960 and 1977 Tom had four graduate students and six research associates, some of whom were later appointed to the ICR staff. I did not directly participate in experiments with Tom but rather decided to follow up some exciting experiments with eukaryotic cells that I had initiated in Brussels. Nevertheless, Tom was very supportive of my research. He gave me adequate space in his lab, provided me with a technical assistant, and initially even shared with me some financial support from his National Science Foundation grant. As he did with his collaborators, he also helped me by cogent discussions of my research and by critical reviews of my manuscripts.

In addition to his phage research, Tom was also very busy on other fronts. His reputation as an electron microscope virtuoso led several researchers to seek his collaboration in projects with various animal viruses. He was a member of the Council and the Executive Board of the Biophysical Society and served as its president in 1965. He also served as president of the International Federation of Electron Microscope Societies and hosted the international congress that was held in Philadelphia in 1962. This was an enormous job that consumed an inordinate amount of his time. In addition, he chaired the U.S. National Committee of the International Union for Pure and Applied Biophysics from 1965 to 1969 and served on the editorial boards of several journals. A more complete list of his professional commitments is given at the end of this memoir. In between all these activities, Tom found time to write several insightful reviews dealing with the structural and genetic properties of bacterial viruses and the electron microscopy of microorganisms.

Tom continued his research until the mid-1970s. From 1977 to 1983 he directed the postdoctoral training program in basic research at Fox Chase. Although he officially retired in 1983, he maintained an active presence at the ICR for several years. After his retirement he had the luxury to spend more time painting. Tom, an outstanding watercolor artist, created many beautiful landscape paintings that exhibited a remarkable use of perspective and subtle applications of shimmering light and shadows. He also enjoyed playing golf with friends, former colleagues, and especially with his brother, Norman, when the brothers and their wives took winter vacations in Florida. After a series of strokes, Tom died on August 11, 1991.

Anderson received numerous awards in recognition of his scientific achievements. He was elected to the National Academy of Sciences in 1964 and served as chairman of its Genetics Section from 1985 to 1988. He was elected president of the Electron Microscope Society of America in 1955 and received its Distinguished Award in 1978. He also received the Pasteur Institute's Silver Medal in 1957 and was elected an honorary member of the German and French electron microscope societies.

Tom Anderson had exceptionally keen powers of observation and a remarkable ability for logically sound reasoning. He would frequently cut through to the core of problems, asking critical questions that would expose gaps and flaws in current concepts. A desire to answer these clearly framed questions often provided the impetus for the design of new experiments or the invention of more powerful methodology. He firmly believed that serendipity played a major role in scientific discovery, requiring only that the experimenter be prepared to accept an unexpected result with an open mind and then resolve to eventually provide a cogent explanation for it. He once wrote, “Nature is trying to tell us something, the investigator’s goal is to get the message.” Tom was generous with his time and concerns for other people’s problems. His high ideals and ethical standards were greatly admired by all who knew him.

I OBTAINED A substantial amount of personal information from two autobiographical essays: “Some Personal Memories of Research,” published in the *Annual Review of Microbiology* in 1975, and “Reflections on Phage Genetics,” published in the *Annual Review of Genetics* in 1981. I obtained additional information from an article by John H. Reisner, “A Glimpse of the Anderson Papers,” published in the *Electron Microscope Society Bulletin*, vol. 22, pp. 50-58, and from several conversations with Wilma E. Anderson.

HONORS AND DISTINCTIONS

Deutsche Gesselschaft für Elektronenmikroskopie
Société Francaise de Microscopie Electronique (Honorary)
Society of General Physiologists
American Association for the Advancement of Science
Sigma Xi
Electron Microscope Society of America (President, 1955)
Biophysical Society (President, 1965)
American Society of Naturalists
American Society of Microbiology
International Federation of Electron Microscope Societies
(President, 1959-1963)

Biophysical Society (Member, Council and Executive Board, 1959-1965)
Associate Editor of *Virology* (1960-1966)
Member of the Editorial Board of *Bacteriological Reviews* (1967-1969)
National Academy of Sciences
U.S. National Committee, International Union for Pure and Applied Biophysics (Chairman, 1965-1969)
International Union for Pure and Applied Biophysics, Member, Executive Committee of the Commission on Subcellular Biophysics (1971-1977)
International Union for Microbiology, Member, Committee on Nomenclature of Bacteriophages (1968-1971)
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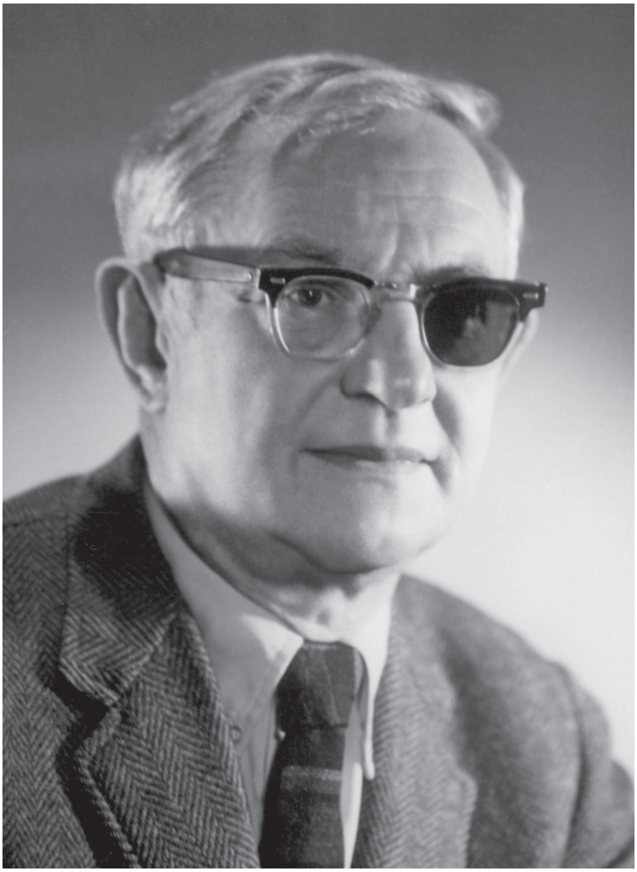

JULIUS AXELROD

May 30, 1912–29 December 29, 2004

BY SOLOMON H. SNYDER

On the morning of Wednesday, December 29, 2004, I was visiting the offices of the *Proceedings of the National Academy of Sciences* in Washington, D.C., handling some editorial chores and staring repeatedly at my watch. Whenever in Washington for a morning meeting, I would try to get away early to visit either my father, who lives in nearby Rockville, Maryland, or my mentor Julie Axelrod, who lived only a few miles away from my father's home. I had decided that day to call Julie for a lunch date when my cell phone rang with a message from my secretary that Julie had died early that morning. He had evidently arisen from bed and collapsed of a heart attack. Till the day of his death, Julie had been alert, visiting his office at the National Institutes of Health several times a week to keep up with the literature and chat with colleagues. He had even flown to Wisconsin to visit his grandchildren just a week prior to his death. In keeping with Jewish tradition, Julie's funeral was held two days following his death. In keeping with Julie's own resistance to fuss and religious dogma, no rabbi was present nor were there any eulogies. Instead, a few longtime friends, including a high school classmate, provided warm reminiscences.

Simplicity and absence of pomp epitomized the life and scientific style of Julius Axelrod, arguably the greatest molecular pharmacologist of the modern era of drug research. His contributions were recognized in 1970 by the award of the Nobel Prize in physiology or medicine. Yet, Julie's scientific success story was most improbable for an individual who spent a major portion of his professional life as a technician without a Ph.D.

Julie's parents, Isadore and Molly, immigrated from Polish Galicia to the United States, where they met and resided in the lower East Side of Manhattan. Julie was born in a coldwater flat at 415 East Houston Street. His father, a basket maker, sold to grocers in lower Manhattan from a horse and wagon. Saturdays were a special treat for Julie, who could accompany his father and get a chance to drive. Though his family was not religious, Julie's parents were part of the East European Jewish immigrant culture so that Julie spoke Yiddish and attended religious school every day after emerging in late afternoon from a public school building that had been built during the Civil War era. His high school, Seward Park, subsequently became well known for its show business graduates, Tony Curtis, Walter Matthau, and Zero Mostel, but not for any intellectual tradition. Julie long maintained that his "real education" came from voracious reading of volumes from nearby Hamilton Fish Park Library.

Julie began college at New York University but ran out of money and transferred in 1930 to the City College of New York, which was tuition-free. Many poor New York citizens similarly attended CCNY, with seven Nobel laureates emerging in the middle to late twentieth century. Traveling an hour each way to college on the subway and maintaining a time-consuming job to support himself, Julie was not an outstanding student. Upon graduation, he applied to several medical schools but was not accepted by any, in part because of the widespread Jewish quotas of those days.

When he graduated in 1933, the United States was in the midst of the Great Depression, and Julie was lucky to obtain a position in a laboratory at New York University paying $25 a month. In 1935, when the laboratory lost its funding, he obtained a position in the Laboratory of Industrial Hygiene, a nonprofit unit set up by the New York Health Department to evaluate vitamin supplements. Julie remained employed by that laboratory from 1935 to 1946. At the same time he enrolled in night courses at New York University, from which he obtained a master's degree in 1942.

RESEARCH WITH BERNARD BRODIE

In early 1946 Julie's laboratory was approached by the Institute for the Study of Analgesic and Sedative Drugs to help determine why the major nonaspirin analgesics, acetanilide and phenacetin, caused methemoglobinemia. In this condition toxic chemicals oxidize the ferrous iron of hemoglobin to the ferric form that cannot carry oxygen. It was recommended to Julie that he seek advice from Dr. Bernard Brodie, then at the Goldwater Memorial Hospital, a New York University division, who was an authority on drug metabolism. In their first meeting in February 1946, Julie and Brodie spoke for several hours about strategies to solve the problem, and Julie was invited to do some of the experiments in Brodie's laboratory. At that time no one knew anything about the metabolism of acetanilide, but an examination of its structure suggested that it might be converted to the dye aniline, already known to elicit methemoglobinemia. Julie developed a simple, sensitive, and specific assay for aniline in urine and plasma. Such elegant methodology, a hallmark of all of Julie's subsequent research, was born at that time. A direct relationship between blood levels of aniline and methemoglobin was soon evident.

As negligible levels of acetanilide were detected in the urine, it seemed likely that the drug was metabolized to other substances. Within a few weeks Julie discovered that the major metabolic product of acetanilide is N-acetyl-p-aminophenol. He showed that this substance has analgesic activity and does not cause methemoglobinemia. Julie's first publication thus concluded, "the results are compatible with the assumption that acetanilide exerts its action mainly through N-acetyl-p-aminophenol. The latter compound administered orally was not attended by the formation of methemoglobinemia. It is possible therefore that it might have distinct advantage over acetanilide as an analgesic." Soon thereafter Julie and Brodie applied similar methodology to evaluate the metabolism of phenacetin, one of the active ingredients in what for years was the most popular headache remedy in the United States: APC (aspirin-phenacetin-caffeine). Phenacetin was also converted to N-acetyl-p-aminophenol as its active ingredient with p-phenetidine being responsible for methemoglobinemia.

These studies comprised Julie's first scientific publications, all in the *Journal of Pharmacology and Experimental Therapeutics.* They were soon recognized as among the most elegant and important drug metabolism studies of the modern era. N-acetyl-p-aminophenol, better known as acetaminophen, was subsequently marketed by the McNeil Drug Company as Tylenol. Neither Brodie nor Axelrod received any royalties, as it never occurred to either of the two to patent their discovery.

Julie remained with Brodie and participated in numerous drug metabolism studies. When James Shannon, director of the Goldwater Memorial Laboratories, was chosen in 1949 to head the recently established National Heart Institute in

Bethesda, Maryland, Brodie brought Julie with him to the NIH along with a number of talented Goldwater staff: Sidney Udenfriend, Thomas Kennedy, Robert Bowman, and Robert Berliner. In the early 1950s at the National Heart Institute, Julie and Brodie elucidated the metabolism of a number of drugs, including pioneering investigations of caffeine disposition. Then, working on his own, Julie began studying the metabolism of sympathomimetic amines, such as amphetamine and ephedrine. He discovered a variety of metabolic pathways, including hydroxylation, demethylation, deamination, and conjugation. These studies led to a major breakthrough, Julie's discovery of the liver's microsomal drug-metabolizing enzymes.

Julie was so impressed with the complete metabolism of amphetamines in animals that he decided to seek enzymes that might degrade the drug. Lacking expertise in this area, he obtained the assistance of Gordon Tompkins and in early 1953 was able to demonstrate that in liver homogenates, amphetamine could be degraded but only if cofactors such as NAD, NADP, and ATP were added. In assessing the role of various subcellular fractions, Julie noted that enzyme activity required a combination of the microsomal fraction (obtained when one centrifuges supernatant fractions of mitochondria at 100,000 times gravity) and cytoplasmic preparations. By selectively heating subcellular fractions, he soon discovered that the enzyme was located in a microsomal fraction and that the cytoplasm provided a critical cofactor. With the assistance of Bernard Horecker, Julie discovered the cofactor to be NADPH. Though nominally only a technician, Julie was able to publish these papers as sole author. Subsequently other workers in the Brodie group showed that the drug-metabolizing system, now known as the cytochrome-P450 mono-oxygenases, metabolizes a wide range of drugs.

CATECHOLAMINE METABOLISM

Julie was more than 40 years old when he discovered the drug-metabolizing enzyme system and still did not have a Ph.D. His friends urged him to go to graduate school, but the logistics of supporting a wife and two young sons seemed to preclude further educational endeavors. Finally, Julie spoke with Paul K. Smith, chair of pharmacology at George Washington University, who agreed that Julie's already published papers could constitute his thesis. Since he had a master's degree already, course requirements were relatively minor. Hence, Julie was able to devote only a single year to graduate school, taking courses and preparing for the comprehensive examinations. In 1955, at 42 years old, he received his Ph.D.

About this time Seymour Kety, the scientific director of the National Institute of Mental Health, set up the Laboratory of Clinical Science under the neurophysiologist-psychiatrist Edward Evarts with a mandate to set up multiple sections with a long-term target to understand schizophrenia. Evarts offered Julie the opportunity to head the Section on Pharmacology, which initially consisted only of Julie himself.

In 1956 Kety presented to the NIMH staff a highly publicized and provocative publication by the Canadian psychiatrists Abram Hoffer and Humphrey Osmond purportedly showing that epinephrine, the hormone of the adrenal medulla, was transformed in the blood of schizophrenics but not of normals to an oxidized, pink-colored substance called adrenochrome. Julie attempted to identify an enzyme that might transform epinephrine into adrenochrome but was unsuccessful. Then, in April 1957, he noted an abstract in *Federation Proceedings* (Federation of American Societies for Experimental Biology) by Marvin Armstrong and Armand McMillan reporting in the urine of patients with pheochromocytomas, catecholamine-secreting tumors, a new metabolic product of norepinephrine or epinephrine, 3-methoxy-4-hydroxymandelic acid, also called vanillymandelic acid (VMA). In VMA a methyl group had been added to one of the two hydroxyl constituents of the catechol ring. Julie wondered whether he might be able to find an enzyme that carries out this process. Biological methylation had only recently been discovered by Giulio Cantoni, an NIMH scientist, to be mediated via a single universal methyl donor, S-adenosylmethionine (SAM). As SAM was precious, Julie in his initial experiments made do with adding to liver extracts the amino acid methionine and ATP, which should generate SAM, and observed a rapid disappearance of added epinephrine. When he finally obtained a sample of SAM, it worked like a dream.

A number of Julie's important advances in catecholamine metabolism depended heavily on identification of key metabolic products. The collaborative atmosphere of the NIH provided Julie with the assistance that facilitated many of its discoveries. The distinguished organic chemist Bernhard Witkop and his associate Siro Senoh provided the chemical synthetic efforts enabling Julie to show that his enzyme did indeed methylate epinephrine and norepinephrine to form, respectively metanephrine and normetanephrine. With Irwin Kopin he elucidated the relative roles of COMT and monoamine oxidase in human catecholamine disposition. Another member of Witkop's group who attained great distinction in his own right, John Daly, was also a key collaborator. Julie dubbed the enzyme catechol-O-methyltransferase (COMT), because it was clearly capable of adding methyl groups to all catechols, not just catecholamines.

The discovery of COMT revolutionized research into catecholamine metabolism. It was already known that catecholamines—such as dopamine, epinephrine, or norepinephrine—were metabolized by monoamine oxidase. Julie rapidly established how the two metabolic pathways interface, resulting in VMA, the final common product identified by Armstrong and McMillan. VMA measurements in the urine became the standard method for diagnosing pheochromocytomas, epinephrine-secreting adrenal tumors that cause hypertension.

With the availability of SAM radiolabeled in the methyl group, Julie proceeded to identify a variety of important methylating enzymes, such as the enzyme that methylates and inactivates histamine and the enzyme that converts norepinephrine to epinephrine in the adrenal gland and elsewhere. One of the most fascinating of these discoveries is worth recounting, because it illustrates Julie's approach to the discovery process. Eager to seek new methylating enzymes, Julie incubated offbeat tissues with radiolabeled SAM, extracted the products into an organic solvent that he could readily evaporate to dryness, and then conducted paper chromatography to isolate and identify the radiolabeled methylated product. In the case of the pituitary gland, he found an enzymatic activity whose product was volatile, because whenever he would evaporate the organic solvent to a small volume for chromatography, the radiolabel vanished. His collaborator John Daly added a derivatizing agent to stabilize the product and discovered that Julie had found an enzyme that methylates water to form methanol. Subsequently other investigators showed that the enzyme, renamed protein carboxylmethyl transferase, methylates proteins on the carboxyl groups of glutamyl and aspartyl residues, an important regulatory step. The methylated carboxyl is a labile ester that undergoes hydrolysis with water giving rise to methanol.

CATECHOLAMINE UPTAKE

In 1970 Julie shared the Nobel Prize in physiology or medicine with Ulf von Euler and Sir Bernard Katz "for their discoveries concerning the humoral transmitters in the nerve terminals and the mechanism for their storage, release and inactivation." Julie's portion of the prize clearly was for his discovery that the synaptic actions of norepinephrine are terminated by reuptake into the nerve ending that had released it. This mechanism for neurotransmitter inactivation is now regarded as the most frequent means for terminating synaptic transmission. At the time that Julie began this work it was thought that neurotransmitters were inactivated by metabolic alterations as was well known for the first characterized neurotransmitter acetylcholine, which is inactivated by the enzyme acetylcholinesterase. Enzymatic degradation of norepinephrine by COMT or monoamine oxidase was assumed to serve as the inactivating mechanism, but inhibiting these enzymes did not terminate the effects of injected epinephrine or norepinephrine.

Julie was not initially investigating those questions. Rather, Seymour Kety was continuing his efforts to examine the validity of the Hoffer-Osmond hypothesis that schizophrenics convert epinephrine to adrenochrome. He obtained custom-prepared tritium-labeled preparations of epinephrine and norepinephrine and provided samples to Julie to do with what he wished. Julie decided to investigate catecholamine disposition simply by injecting these substances into animals. With Hans Weil-Malherbe he administered [^{3}H]epinephrine to cats and found it concentrating and persisting in sympathetically enriched tissues such as the heart, spleen, salivary, and adrenal glands. With Gordon Whitby, he obtained similar findings in rats with [^{3}H]norepinephrine. Particularly crucial experiments were carried out with a visiting scientist, George Hertting, who lesioned the sympathetic nerves by unilaterally removing the superior cervical ganglion of a cat and observed a loss of norepinephrine accumulation into the salivary gland and eye muscles of the lesioned side. Based on these data, Julie postulated that uptake into the sympathetic nerves accounted for inactivation of norepinephrine.

Julie soon explored actions of drugs and discovered that cocaine, amphetamine, and other sympathomimetic amines blocked this uptake process. Inhibition of norepinephrine uptake could explain the ability of drugs to potentiate the effects of sympathetic nerve stimulation, providing compelling evidence that reuptake is the physiologic mode for neurotransmitter inactivation. Julie also found that the major tricyclic antidepressants inhibited the uptake of norepinephrine into the heart. The blood brain barrier precluded studies of the brain in initial experiments. When Jacques Glowinski and Leslie Iversen joined Julie as postdoctoral fellows, they used a technique developed by Glowinski for introducing [^{3}H]norepinephrine directly into the brain via injections into the lateral ventricle, dissecting various brain regions and monitoring accumulated norepinephrine. Glowinski showed that the ability of a variety of antidepressants to inhibit norepinephrine accumulation in the brain paralleled their antidepressant efficacy. It was proposed that this means of potentiating the effects of norepinephrine is responsible for antidepressant actions. We now know that inhibition of the uptake of serotonin as well as norepinephrine participates in antidepressant efficacy. To this day, inhibition of amine uptake remains the accepted mechanism of action of tricyclic antidepressants and has driven the development of several generations of novel antidepressants during the succeeding 40 years.

The above description highlights only a few of Julie's scientific contributions. In the interest of brevity I haven't mentioned his research on the pineal gland, which opened up an entire new field. When the pineal-gland hormone melatonin was discovered by Aaron Lerner to be 5-methoxy-N-acetylserotonin, Julie identified the methylating enzyme that generates melatonin. With Richard Wurtman he then showed that melatonin is the active principle of the pineal gland, which mediates organismic influences of light, presaging a vast body of work establishing melatonin as a regulator of sleep and circadian rhythms.

JULIE'S STYLE

Julie's extraordinary creativity as a researcher was virtually unparalleled. Thus, understanding Julie's "magic" might provide insight into what makes for innovative discovery in science. All his students echo a few common themes. One was Julie's skill at getting to the heart of complex problems. He had less formal training in complex areas than most scientists, and his eyes would glaze over when he was presented with convoluted intellectual schemes involving multiple equations. He eschewed statistics and often said, "If the difference between the two groups is so small that you need a statistical test to prove its significance, then it might not be a very important difference." In speaking to the scientific historian Robert Kanigel, Julie commented, "I don't like to do complex experiments. I'm not a complicated person. . . . Picasso makes a single line, but it takes a lot of time and thought."

Similarly, Julie did not like complicated theoretical schemes in which one predicts the results of an elaborate series of experimental studies. Rather, he felt that the data should tell you where to go next. "Just follow your nose." He was irritated by scientists who spend weeks or months in the library developing a project, remarking to Kanigel, "You don't learn anything by thinking about what to do . . . just by going into the lab and doing it."

In the biomedical sciences researchers work through their students so that mentoring is key to discovery. Julie was an ideal mentor, teaching by gradations of positive reinforcement. Even if a student had a result that was only modestly interesting, Julie would provide encouragement and brainstorm with the student trying to find gold in the dross. I remember many occasions when I felt my results were not even worth showing to him, but Julie was able to find promising hints in what seemed to be drab data.

Julie also conveyed an infectious exhilaration in the discovery process. Research for Julie was genuine fun, and almost all his students came away from time in his lab with the same attitude. I remember vividly a lecture he was giving soon after receiving the Nobel Prize. He opened the talk by saying, "It seems that all these speaking invitations are a conspiracy to get me out of the lab. I find it hard to imagine that I am paid a good salary for doing things that are so much fun that I'd work in the lab for no pay." When a critical experiment was completed, he was often so excited that standing in front of the liquid scintillation counter, which records radioactivity levels, he almost jumped up and down using body English to accelerate or decelerate the accumulation of radioactive counts, depending on what result he was seeking.

Despite his skills as a mentor Julie always saw himself as a bench scientist. Because of his own many years as a technician, Julie felt uncomfortable if he was not himself carrying out an experiment. Each day he arrived in the laboratory at 8:15 a.m. and by 8:30 was incubating a set of test tubes. He generally completed his experiment by noon and then joined one of the "boys" for lunch. He religiously spent an hour each day after lunch in the library keeping up with current literature and then devoted the remainder of the afternoon to working on manuscripts together with students. His style of manuscript preparation differed markedly from most senior scientists. Instead of the student preparing a draft that the mentor would then review, Julie and the student would sit together and review the data and its significance, whereupon Julie would write out the manuscript by hand, incorporating the student's input. Julie was a superb scientific writer, presenting his story in simple declarative sentences and providing no more discussion than was warranted by the data. He hated elaborate pretense and so was irritated when scientists wrote that the rats were "sacrificed." He admonished, "There aren't any altars in our laboratory; we just kill the rats."

Julie was a quiet, self-effacing, mild-mannered individual who rarely was angered. Though he and his wife did not socialize much with students, he was always empathic, inquiring about events in our lives and immensely helpful in our career decisions.

Julie married Sally Taub in 1938. She had grown up in the same lower East Side environment as Julie and worked for many years as a second grade school teacher. She died in 1992 from complications of diabetes. Their elder son, Paul, is a professor of anthropology at Rippon College in Wisconsin, where he lives with his wife, Michelle, and their children, Sonya and Sander. Their younger son, Fred, is a forestry consultant in Wisconsin; he and his wife, Johanna, had two children, Julia a student at Bethel College in St. Paul, Minnesota, and Nathan, who died in 2004. Julie was immensely devoted to his children and grandchildren and frequently traveled to Wisconsin to visit them.

Julie's humility is best conveyed by his own statement: "I soon learned that it did not require a great brain to do original research. One must be highly motivated, exercise good judgment, have intelligence, imagination, determination and a little luck." Then Julie got to the heart of the matter, commenting, "One of the most important qualities in doing research, I found, was to ask the right questions at the right time. I learned that it takes the same effort to work on an important problem as on a pedestrian or trivial one. When opportunities came, I made the right choices."

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Courtesy of the Harvard University News Office, Cambridge, Massachusetts

Elso S. Barghoorn

ELSO STERRENBERG BARGHOORN JR.

June 30, 1915–January 27, 1984

BY LYNN MARGULIS AND ANDREW H. KNOLL

ELSO BARGHOORN, PALEONTOLOGIST and polymath, extended knowledge of the fossil record of life back some 2 billion years to the Archean eon. When he began his career, all life, for most scientists, was classified as either plant or animal. When he first launched investigations of the fossil record, most agreed that plants and animals did not appear until the Cambrian Period. At that time, in early 1940s, the Precambrian-Cambrian boundary was set some 650 million years ago. Since animals depend for sustenance on plants, by logic plants, with their capacity for photosynthesis, must have preceded (i.e., evolved prior to) the evolution of animals. Indeed, by the same logic, the origin of life was assumed to be equivalent to the origin of plants. Precambrian sedimentary rocks were known to exist prior to the Cambrian Period (now set from 541 to 490 million years ago), but none had yielded definitive evidence of animals or plants. Many thought that the origin of life was so entirely improbable that it had taken eons for life to originate (i.e., the long stretch of time from Earth's origin to the explosion of fossils at the base of the Cambrian was required). The dramatic discontinuity between the abundantly fossiliferous Cambrian sediments in Wales and in the Grand Canyon, for example, and the barren igneous and sedimentary deposits prior to the Cambrian had even puzzled Charles Darwin and his predecessors before 1859. The scientific consensus in any event was that the abrupt appearance and rapid diversification of trilobites, brachiopods, sclerites, and other abundant well-preserved remains of life implied a sudden appearance of Cambrian fossil animals. This discontinuity at the Precambrian boundary was considered "the most vexing riddle of paleontology" (Fischer, 1965).

The discoveries of Barghoorn, more than any other single scientist, generated a revolution in these views: He entirely altered the way early evolution is perceived by organic chemists, biologists, Earth scientists, and now even the literate public.

At the end of 1950 Robert B. Shrock, at a meeting of the Geological Society of America, gave to Elso Barghoorn some black rock samples that he had received from Stanley Tyler, a geology professor at the University of Wisconsin. Iron ore was the object of Tyler's research. The importance of the iron supply for the Gary, Indiana, steel mills that supplied Detroit's automobile industry was inestimable. The vast sedimentary deposits in Michigan, Minnesota, and Ontario, the source of the ore, had led many geologists, including Tyler, to intense study of the region. Tyler began petrographic investigation (i.e., to study rocks associated with the ore by light microscopy). For observation of rocks collected from a 20-foot-deep test pit, he made standard 0.03-millimeter petrographic thin sections and mounted them on glass microscope slides. Because the sides of the pit had collapsed, no rocks from further depths could be accessed. Tyler suspected he had discovered coal. He approached Shrock in search of a paleobotanical expert. All three geologists—Shrock, Tyler, and Barghoorn—recognized that accurate identification of coal was potentially extremely important. Tyler wanted advice about his shiny black samples from the shale pit because they so closely resembled Pennsylvanian coal. The samples were from a formation called "the Michigamme slate" in the Iron River district of Michigan. Of course,the economic incentive was to find new sources of coal.

As both Tyler and Barghoorn knew, the earliest well-documented extensive coal deposits were far, far younger. The Michigan rocks had been already well dated as Upper Huronian in age. They were approximately 1.9 billion years old, whereas those, replete with plant fossils, from the Carboniferous of Pennsylvania were only 350 million years in age!

In December 1951, in a letter to Dr. Barghoorn (the professors did not yet address each other by the first name), Tyler wrote, "I am sending you under separate cover a thin section of the Michigamme shale associated with the coal, which I believe contains a fairly large quantity of the amber material . . . [Microscopic examination] . . . shows considerable differentiation in the amber material which I presume may be algal cell structure." Hence, Tyler was on to a discovery of "fossil plants" more than 1.5 billion years younger than the accepted date for plants in the paleontological literature! At Barghoorn's request Tyler sent results of an analysis "of the putative coal as determined in the coal research laboratories of Jones and Laughlin Steel Corporation." At first Tyler cautiously wrote "coal" in quotation marks, but after later analysis, including the "amber material," he omitted the quotes. Results showed the black shiny samples to be 79.9 percent carbon, 16.3 percent sulfur with an ignition temperature of 755°F. Both scientists, in full agreement, dropped their skepticism and their quotation marks. They indeed had discovered Precambrian coal! Thin sections were further analyzed at Harvard by Barghoorn. No doubt that much of the "orange-yellow material" (i.e., the amber-colored rock) was mineral, probably "iron oxide [hematite and goethite] and not organic in origin," wrote Tyler to Barghoorn. By early 1952 Barghoorn begged Tyler's pardon for a delay in his response. Barghoorn had been distracted by a request from Near East archaeologists that he honored when "a batch of charcoal was visited upon me" from a hearth site. Barghoorn told Tyler that the charcoal was of great interest because it came from "the oldest known community site yet discovered." "After working with it [the Near East charcoal] briefly I'm not certain whether pre Cambrian coal or charcoal is the more difficult material to handle!"

This correspondence, which as we will see, led directly to the discovery of the world's oldest fossils at the time, demonstrates Barghoorn's qualities of mind. Barghoorn was always captivated by the scientific effort to reconstruct the early history of life. He was easily distractible but only by serious scientists with questions of the highest scientific significance. He worked indefatigably, but only on a one-to-one basis with those whom he respected. He kept things whole: He recorded in correspondence and in his many notebooks wise speculation about the significance of his data along with the data itself. In the same letter to Tyler (January 15, 1952), he wrote, "I have attempted to determine the possible origin of that concentric fan-like appearance, in reflected light, on the coal specimens. There is one existing blue green alga which could conceivably make a similar pattern . . . *Rivularia.* It is entirely conceivable that our coal is composed almost solely of the remains of this colonial alga. I seriously doubt if the discrete patterns on the coal could be other than biological in origin."

The new rocks, including cherts that Tyler sent Barghoorn from Canada, were far better samples than the polished black coal from Michigan. From certain obscure localities rock samples were collected by Tyler and his colleagues.

Among these from two widely separate sites in Ontario (Frustration Bay and Schrieber Beach) were the rocks that yielded spectacular fossil remains. Thin sections of these black cherts became microbial Rosetta stones in the search for ancient life. Cherts, an opal-like material technically called "cryptocrystalline quartz," preserved with incredible detail an abundance of microorganisms. Although tricky to find in the first place, great quantities of black chert had to be painstakingly processed. However, when found the prodigious samples contained up to 1.5 million microfossils per cubic centimeter. The trickiness was because most black cherts in the region, even those quite close to the lucky optimal samples, when thin-sectioned contained no fossils at all. In June 1953 Tyler sent Barghoorn a slide of chert "containing crystals of carbonate and considerable organic matter." The rocks were from the upper members of the Gunflint Iron Formation. The specimen had been taken on the shore of Lake Superior about 30 miles east of Port Arthur, Ontario. "The black carbonaceous material certainly looks like organic matter to me." The collection of beautifully preserved enigmatic microfossils at this site were soon added to those of the basal Gunflint Iron Formation from Schreiber Beach. Tyler wrote, "A second slide from the algal chert [is from] the Schreiber locality. This is a lulu. Filaments are extremely abundant and there are many rounded bodies which may be spores—occasionally the rounded bodies appear to be attached to a filament." Hasty sketches annotated Tyler's letters. In spite of quizzical suggestions by Tyler that the microfossils might be radiolaria or jellyfish medusa, "fancy algal spores" or "actinomycetes" (a kind of filamentous bacterium), Barghoorn opted for an interpretation as algae. He mentioned his conversations with Professor William ("Cap") Weston, Harvard professor and the most experienced fungal expert of his generation.

Today we can see in these letters the first descriptions of *Kakebekia, Gunflintia, Huronospora, Leptoteichus,* and a host of other named genera. Even today by no means have all the Gunflint microfossils been described and identified to the general satisfaction of geologists and biologists. But most now agree that these beautifully preserved organic forms are primarily fossils of bacteria, both cyanobacteria (formerly called blue-green algae) and iron-oxidizing bacteria. Whatever the species names are, the importance and consequence for science is indisputable. The Tyler-Barghoorn discovery of abundant and diverse 2-billion-year-old microfossils associated with iron ores began the study of Precambrian life.

Robert Shrock wrote on December, 14, 1953, to Tyler, “Dear Stan: I spent last Friday afternoon with Elso Barghoorn and had a wonderful time looking at the flora which you have unearthed in the Pre-Cambrian. It would be a tremendous understatement to say that I was thrilled, I was fully prepared for what I saw after what you and Elso have been telling me for the past several years, but even with such preparation I was amazed at the excellence of preservation of such tiny plants.” Shrock goes on to urge publication “not only in the United States but elsewhere” and continues, “I think your discovery is of such great importance that announcement of it should be made in all the common languages and in the biological as well as paleontological journals.” The Gunflint microbiota was documented by publication in 1954 with a short joint paper that announced the discovery.

Subsequent work on the rocks of the Swaziland system of South Africa led Barghoorn to extend the Gunflint-style analysis to even older former sediments. The nearly 3.5-billion-year-old cherts and shales from the Barberton Mountainland preserve a tractable, if scrappy signature of life. Barghoorn, his colleagues, and especially his students went on to document life's evolutionary record from these earliest roots to the Cambrian explosion of animal diversity, some 85 percent of life's recorded history. They helped provide the geochronological scaffolding for environmental and molecular evolution. Indeed, they founded a new formal paleontological field, "Precambrian Paleobiology."

Elso Barghoorn was born on June 30, 1918, in New York City, or as he liked to put it, remembering wistfully the rural character of his first home: in "Queens Village on Long Island." During his boyhood the Barghoorn family moved to Dayton, Ohio, where Elso's interest in both rural life and natural history blossomed. Long summer hours spent curled in the crotch of a tree with books about science or exploration set Elso on his course toward discovery. The teenaged Barghoorn developed reputations as both fine athlete and budding scholar, and upon completion of high school he enrolled in nearby Miami University. Elso would complete an honors degree in botany in 1937 but not before departing for a number of jobs, including a stint as deckhand on a Great Lakes freighter.

Later in 1937 Elso began graduate study at Harvard, supported first by an Anna C. Ames Fellowship and then by an Austin Teaching Fellowship. In a pattern that persisted his entire life Barghoorn plunged immediately into a variety of research studies. Elso's thesis, completed in 1941 under the tutelage of the preeminent plant anatomist I. W. Bailey, elucidated the anatomy, development, and taxonomic variation of vascular rays in woody plants. The papers that issued from this work remained standard references for decades. At the same time, Elso's work with paleobotanist William Darrah kindled a lifelong love of fossils. Barghoorn's first paleobotanical publication was on the plant *Hornea,* preserved in the Rhynie Chert, which Elso renamed *Horneophyton.*

Inspired by the same Professor William ("Cap") Weston who had helped analyze the Gunflint chert, Elso also became fascinated by the decay of wood by fungi. Among other things he demonstrated to an initially skeptical community that cellulolytic fungi, admittedly a very few species, live in the ocean as well as on land. Some even form little mushrooms under water on driftwood. These three topics—plants, fossils, and fungal decay—would be intertwined in Elso's work for more than 40 years.

Several months of research at the Atkins Botanical Garden in Cienfuegos, Cuba, introduced Elso to another enduring interest: tropical botany. Tropical environments and fungal decay found common focus when World War II began. Called in 1943 from his first faculty position at Amherst College, Elso reported to Barro Colorado Island in Panama, where he spent the duration of the war conducting research on a problem that plagued Allied troops in the Pacific theater: fungal degradation of canvas tents, clothing, food, and even optical equipment where lenses were mounted with organic glues.

After the war, Elso returned to Massachusetts, where he was appointed instructor at Amherst College. However, in 1946 he returned to Harvard University. He began as an assistant professor but was soon promoted to associate professor (in 1949). From 1955 until his death he held a position of full professor of biology. In 1973 he was honored with a Harvard chair. He became the Fisher Professor of Natural History. The distinguished botanist Richard A. Fisher had founded the Harvard Forest School of Forestry at the turn of the twentieth century and had set up an experimental forest reserve some 70 miles away from the Cambridge campus in (central) Petersham, Massachusetts.

In the decade that followed the war Elso published important papers in Carboniferous paleobotany, the early geological record of flowering plants, fungal decay, and even marine mycology and archaeology (analyses of timbers used in the pre-Columbian Boylston Street Fishweir, Boston, and study of the colonial iron works at Saugus, Massachusetts). But he also made two paleontological discoveries (or to be strictly correct, one rediscovery and one new discovery) that were to play key roles in his subsequent research life. First, Elso rediscovered and began the systematic study of Oligocene-Miocene plant fossils preserved in a small outcropping of lignite near Brandon, Vermont. As documented by two generations of graduate students, wood, pollen, fruits, seeds, and rare flowers from Brandon provide a unique yet extensive and unusually well-preserved view of Tertiary vegetation in northeastern North America.

But it was the beautifully preserved fossils of the Gunflint that captured the attention of scientists well beyond the world of paleontology. In the decade of work culminating in their *Science* paper (1965) on the Gunflint "plants," Tyler and Barghoorn nearly quadrupled the known history of life. They set the stage for the new and distinctive field. At first "Precambrian Paleobiology" expanded slowly, but Barghoorn's research in the 1960s, along with that of graduate student J. William Schopf, placed this discipline on a firm footing (Schopf, 1983; Schopf and Klein, 1992). The publications on the Gunflint biota (1963, 1965) were followed in short order by the discovery of exceptionally well-preserved microfossils in cherts of the ca. 800-million-year-old Bitter Springs Formation, Australia, and possible microfossils in nearly 3.5-billion-year-old cherts of the Onverwacht Group, South Africa. At the same time, Elso and his colleagues expanded paleobiological research on Precambrian rocks to include biogeochemistry as well as the fossil morphologies traditionally studied by paleontologists (Knoll, 2003). Today, fossils and geochemical data are routinely integrated in accounts of Earth's early biological and environmental history.

When he died, in 1984, Barghoorn left the beginnings of this new and solid "science of Precambrian life." In his long and productive career Elso never wavered in his pursuit of evidence and his enthusiasm for important and accessible scientific problems: the origin of life, the early stages of evolution, origin of coal, silicification of fungi and of wood, the formation of the atmosphere, the origin of Earth and its land forms and water bodies, and the origin and fossil record of seeds and flowers. Nor did he fail to help colleagues and students who shared his interests. Although he was shy and avoided purely social events and meetings, he enjoyed intense personal scientific relations with many of his nearly 89 coauthors. His eclectic tastes are revealed by perusal of the thesis projects of his nearly two dozen Ph.D. and undergraduate honors research students. The first two, Alfred Traverse (1949) and William Spackman (1951), and one of his last, Bruce Tiffney (1977), dealt with the Brandon Lignite. Between these came projects on topics as diverse as the origin of amber, the formation of petrified wood, the origins of maize, Pleistocene vegetation in the Panamanian rain forest, the early diversification of flowering plants, and of course, the Precambrian fossil record.

As early as the 1960s, while deeply involved in the study of Precambrian rocks and their modem analogues, Barghoorn reflected on the extraterrestrial implications of his discoveries. Although one of us (L.M.) was the chair of the National Academy of Sciences' Committee on Planetary Biology and Chemical Evolution (1977-1980), Barghoorn was the great intellectual force behind its report; the 80-page pamphlet published in 1981 by the Space Science Board *(Origin and Evolution of Life—Implications for the Planets: A Scientific Strategy for the 1980's)* became the unsung charter for today's astrobiology initiative. Behind the scenes Barghoorn's scientific acumen, his ability to see Earth as a whole planet through time and space, to recognize the adequacy as well as the limitations of research tools such as gas chromatography, mass spectroscopy, and electron microscopy for specific tasks coupled with his clear prose style and genuine humility make this report his lasting nearly anonymous contribution to world-class science. He was the master teacher of the committee members for all of the chapters. These include: Overall Goals and Recommendations (for "Chemical Evolution and Planetary Biology"); Global Ecology; Chemical Evolution: Distribution and Formation of Biologically Important Elements; Chemical Evolution: Early Earth Prior to Life; and Early Evolution of Life. Appendix B of this document concluded that "there is no evidence for current life at Viking sites." Besides his immense contribution to the National Academy of Sciences' Space Science Board exploration strategy, as exemplified by this document, Barghoorn was an advisor to NASA. He helped develop a strategy for biological aspects of the exploration of the entire solar system and sought, but did not find, signs of life in the returned *Apollo* lunar samples and in the Orgeuil meteorite that landed in France in the nineteenth century. His long-standing interest in the red neighbor, especially the *Viking* missions to Mars, would have been prelude to Elso's fascination with the data currently streaming from the NASA rovers *Spirit* and *Opportunity.*

In many ways Elso's life followed his science. A keen practical botanist, Elso reveled in the vegetables and exotic plants he grew at his farm and in the greenhouse. At his rural home and abroad Elso also read the night sky and described the clouds, the aurora borealis, and the evening light. His notebooks contain far more extensive commentary on weather and local climate than they do about people. He admitted to one of us (L.M.) that from childhood he was afraid of thunderstorms; when L.M.'s family lived in

Newton and the phone rang after 10 p.m. on a lightening-ridden Saturday night, it would be Elso calling from Carlisle, ostensibly to make a date to meet in the lab or to check on a missing reference in a joint article. Only after quite a number of such calls did it become clear that he wanted company when the storm was furious, especially in his last two years after his wife, Dorothy, died. Not surprisingly, Elso also loved to travel—not to the castles and museums of Europe but to the ends of the Earth, where natural treasures abound.

Elso married Margaret Alden MacLeod in 1941. One son, Steven, followed in his father's footsteps into paleontology, but in vertebrate paleontology rather than in paleobotany. Tragically, a second son died as a young adult. This marriage ended in divorce. Neither Barghoorn's second marriage to Teresa Joan LaCroix, in 1953, nor his last and lasting marriage to Dorothy Dellmer Osgood, in 1964, resulted in more children. His final marriage ended with Dorothy's untimely death by suicide in 1982. A native of Brookline, Massachusetts, Dorothy had graduated from the geology and geography department of Mt. Holyoke College. She was his knowledgeable companion in the laboratory and the office for nearly two decades. Not only did she breed dogs and raise Morgan horses on their land in Carlisle, northwest of Boston, but she also enjoyed cordial and helpful relations with Elso's students, colleagues, and myriad visitors until the end of her life.

Elso Barghoorn received many honors during his productive career, including a New York Botanical Garden award for "outstanding contributions to fundamental aspects of botany" in 1966, the Botanical Society of America's Certificate of Merit in 1968, the Hayden Memorial Award of the Academy of Natural Sciences in Philadelphia in 1968, and the Charles D. Walcott Award of the National Academy of

Sciences in 1972. His election to the National Academy of Sciences came in 1967.

We now know that all life is not "either animal or plant" and in fact plants and animals, closely related to each other, reside on two relatively short branches of a greater microbe-dominated tree of life. We also know that life has left an interpretable fossil record in some of our planet's oldest rocks. We begin to have the tools to search for comparable signatures of life in sediments deposited on other planets. Elso Barghoorn's contribution was quietly enormous, and his death at home at age 68 deprived us of an intellectual giant in our lives.

Barghoorn published more than 122 scientific research papers from 1938 until 1982. A book contract from Lewis Thomas's Commonwealth Book Fund committee sat on his desk from 1980 until he died. The committee had urged him to write up his scientific life and to encourage him they even sponsored a superb typist (Geraldine Kline) to transcribe his correspondence as well as his laboratory and field notebooks. The offer included the committee's usual $50,000 advance limited to carefully chosen active research scientists for an acceptable two-page book proposal. Although he certainly could have used the money, and he wrote well and with ease, he refused to sign. He claimed that never, in any agreement with anyone, had he failed to deliver a publishable manuscript in good form and on time. Never either had he dared to write a book. He simply would not sign in good conscience any commitment he felt he could not deliver. Even given the extraordinary generosity of the Commonwealth Book Fund committee and even after one of us (L.M.) helped to prepare a book outline to his specifications, his reluctance to submit a proposal was permanent.

We are left with his magnificent published work in the primary literature. Here we have chosen 25 representative papers with great difficulty, as Elso S. Barghoorn was an excellent writer and a profound scholar, who widely traveled and documented his observations with great care. As a generous teacher he gently accepted only the highest standards of scholarship from his students and colleagues. The papers chosen here indicate the range of interest and breadth of coauthorship over his long, productive life. His full contribution, including the Commonwealth Book Fund typescripts (in possession of L.M.) and the rest of his writings and correspondence banked in the archives of the Harvard University Library, are worthy of further attention by historians of twentieth-century science.

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HERMAN SAMUEL BLOCH

June 15, 1912–June 16, 1990

BY JAMES P. SHOFFNER

Herman Bloch was born in Chicago to Aaron and Esther Bloch, who were immigrants from the Ukraine. He attended Senn High School on Chicago's north side, graduating in 1929 with high honors. According to his principal, his grade point average of 97.41 was the highest ever achieved by a Senn graduate. On the basis of this outstanding record he was granted a full-tuition scholarship to the University of Chicago, where he graduated Phi Beta Kappa in 1933, with a degree in chemistry and physics. He continued at the University of Chicago for graduate school, receiving his doctorate in organic chemistry in 1936, with Prof. Julius Stieglitz as his thesis advisor.

After receiving his doctorate he joined Universal Oil Products Co., a research company specializing in the development of refining processes and products for the petroleum industry. In 1940 he married Elaine Judith Kahn, also a University of Chicago graduate. Herman and Judith had three children, Aaron, Janet L., and Merry D., of whom I will say more later.

Herman joined Universal Oil Products (UOP) during a period when the petroleum industry—while not exactly in its infancy—certainly was not that far advanced technologically. Most of its products were developed by thermal cracking of crude petroleum to produce straight-run gasoline, with tetraethyl lead being added to boost the octane number to a value suitable for smooth running in the internal combustion engines of that period. The petrochemicals industry was virtually nonexistent. The company's R&D activities were carried out in a Chicago suburb, at the Riverside laboratory, which had been established in 1921 for the purpose of applying the latest developments in science and technology toward the manufacture of gasoline to serve the growing automobile market. To that end a staff of outstanding scientists and engineers had been assembled at Riverside. Herman joined this group as a young research chemist in 1936, immediately after receiving his doctorate.

The research laboratories were moved to Des Plaines, another Chicago suburb, in 1956. After going through a period of diversifications and acquisitions in the 1960s and 1970s, UOP was subsequently acquired by Signal Companies, which merged in the early 1980s with Allied Company to form Allied Signal. In 1988 the refining and petrochemicals process unit was spun off and combined with the catalysts and absorbents unit of Union Carbide to form a new "UOP," jointly owned by Allied Signal and Union Carbide. In 1999, with the purchase of Union Carbide by Dow Chemical and the merger of Allied Signal and Honeywell, UOP became a joint venture between Honeywell and Dow Chemical. With all of these changes it is amazing how little has changed in terms of the UOP mission and core business. Processes developed by Herman and his colleagues are still very important to the business and survive, with modifications and updates to keep pace with current technology, to the present day.

Much of the history of both the Riverside and Des Plaines labs is given in a historical account of UOP published in 1994, the eightieth anniversary of the company's founding.[1]

The Riverside lab was designated as a National Historic Chemical Landmark by the American Chemical Society in 1995.[2] Bloch's research contributions are highlighted in the history as well as the commemorative brochure that accompanied the landmark designation.[3]

It gives me great pleasure to author this biographical memoir of Herman, who was my mentor, friend, and fellow chemist.

RESEARCH AT UNIVERSAL OIL PRODUCTS COMPANY

When Herman joined UOP, the study of reaction mechanisms as a subfield of organic chemistry was underway. In their attempt to make better gasoline and improve the yield of gasoline from a barrel of crude oil, UOP chemists were at the forefront of studies on fundamental hydrocarbon chemistry. There was a rather general consensus among traditional organic chemists that while the chemistry of olefins and aromatic hydrocarbons was interesting and important, the chemistry of paraffins was not, and hardly worthy of much effort. Indeed, the very name "paraffin" means "little affinity," or unreactive. When Herman arrived at UOP, he became a part of an effort to modify paraffinic hydrocarbons to make better gasoline using catalytic chemistry rather than the predominantly thermal processes prevalent at that time.

A fundamental achievement of UOP research scientists was their understanding of the role of acid catalysis in initiating the full range of hydrocarbon reactions leading to the formation of a higher grade and yield of gasoline. This represented one of the first examples of the use of what was then called carbonium ion chemistry—now called carbocation chemistry—in a practical way to form useful products. This was extremely important during World War II, when the use of acid-catalyzed reactions of hydrocarbons led to gasolines having a much higher octane number. The gasolines obtained from these processes were far superior to those being produced by the Axis powers and resulted in Allied pilots having a substantial performance advantage over their German counterparts. This achievement has been cited by some observers as one of the keys to victory during the Battle of Britain.

Herman carried out fundamental research studies on the full range of reactions of hydrocarbons that could be used to make a higher quality and quantity of gasoline, conducting research on classic organic reactions, such as isomerization, polymerization, cyclization, dehydrocyclization, and dehydrogenation—all of which can be initiated by formation of a carbocation, or in some cases a carbocation radical.

A further development that arose from these studies was an appreciation of the fact that surface acidity could catalyze reactions in a similar manner to solution acidity. Among UOP scientists who carried out critical work on solution acidity that supported this analogy were Louis Schmerling and Herman Pines, both of whom went on to receive American Chemical Society awards for their work. UOP scientists pioneered in the development of catalysts whose surfaces had been modified to increase acidity by the addition of trace amounts of noble metals. This understanding eventually led Bloch's colleague, Vladimir Haensel, to develop a catalyst that contained a relatively small amount of platinum on alumina as a reforming catalyst. The term "reform" refers to those processes by which the structures of hydrocarbon molecules are rearranged to form new molecules that are more highly branched and more aromatic. The reformed molecules give gasoline that has a much higher octane number and runs much smoother in internal combustion engines. The use of the platinum-on-alumina catalyst, further modified by halide ions, gave significant improvements in gasoline yield, as well as higher octane gasoline. This process was patented, trademarked, and licensed as "platforming," or platinum reforming.

Even more important, from the standpoint of basic organic chemistry and process chemistry, platforming led to the formation of large amounts of aromatic hydrocarbons. Prior to this development the main source of these basic organic chemicals was the coal tar that was a by-product of the coke and natural gas industry. The products from coal tar were of uncertain quantity and quality. Herman Bloch led the way in the development of a separation process for separating aromatics from nonaromatics by selective solvent extraction, which was trademarked as Udex. A single platforming unit, attached to a Udex unit, produced more aromatics during a relatively short period of time than were obtained from coal during the entire period of World War II. If left in the gasoline fraction, the aromatics led to significant improvements in the overall octane ratings, as mentioned earlier. When separated from nonaromatics into the individual C6-C9 aromatics, they became intermediates for the plastics, textile, and synthetic fibers industries. These industries were virtually nonexistent before the advent of a reliable source of chemical intermediates from petroleum. It should also be noted that the significance and importance of the entire petrochemicals industry was greatly enhanced by the operation of the platforming process and the separation processes. Herman's role in the development of a process for this separation was crucial to the utilization of aromatic hydrocarbons as commodity chemicals and the establishment of robust petrochemical and polymer-based industries.

Immediately after World War II there was significant growth in the synthetic detergents industry. Most of these detergents were based on sulfonated alkybenzene derivatives. The problem with these detergents resided in the nature of the alkyl chain(s). Because these alkyl chains were highly branched, the detergents could not be completely degraded by bacteria. Thus, when laundry wash water was discharged into streams, the detergent molecules were not degraded and retained their detergent properties. The resulting buildup of these waste detergent molecules caused severe foaming in lakes and streams. The solution to this problem seemed obvious: Use linear alkyl benzensulfonates as detergents instead of the branched-chain derivatives. The problem was that the long-chain linear olefins necessary for the formation of the desired alkylbenzenes were not readily available.

Although the process leading to the appropriate alkylbenzene derivatives seems straightforward in retrospect, it was anything but that in execution. There was no readily available stock of linear olefins for benzene alkylation. Fortunately, the UOP Molex process that was developed to separate low-octane-number linear paraffins from naphtha in order to increase the octane number of the paraffinic components of gasoline could be modified to work with higher-molecular-weight alkyl mixtures. Using this modified process, linear paraffins were separated from branched paraffins. The naphtha remaining could be further isomerized to give more linear paraffins and the separation cycle repeated to give more linear paraffins. Eventually the total hydrocarbon feedstock could be converted to a mixture rich in linear paraffins of the desired chain length.

The linear paraffins could be selectively dehydrogenated by a process, developed by Bloch and his associates, called the Pacol process, to give the desired straight chain primary olefins. Herman was also involved in the development of the process by which benzene was alkylated with linear olefins to produce alkylbenzenes. The alkylbenzenes were then sulfonated to produce biodegradable detergents. The process was described and illustrated with a full-page story in the Technology section of the American Chemical Society's weekly newsmagazine, *Chemical & Engineering News.*[4]

Although the LAB (linear alkyl benzene) process was introduced nearly 40 years ago, it is still one of the major UOP technologies. Even though the process has been continuously updated and improved—they are now on the fifth generation of catalysts—the essential details of the original process are still in place. As it is described today in UOP trade literature, it still consists of the basic steps as listed below.

1. Hydrotreated kerosene or other paraffinic feedstocks are fed to a molecular sieve unit for separation of normal from branched paraffins (UOP Molex Process). The naphthas and branched paraffins are eventually all converted to normal paraffins by a combination of isomerization/separation steps.
2. The normal paraffins in the C-10–C-14 range are dehydrogenated to give the corresponding linear olefins. This step is known as the Pacol Process.
3. The linear olefins are then used to alkylate benzene in an acid-catalyzed process to form the LAB. This process now uses a solid acid catalyst.

As of today 85 percent of the world's supply of LAB detergents are made using this technology. There are 25 to 30 plants located all over the world. Since its development as a UOP process, it is estimated to have contributed more than $500 million to UOP revenues.

It seems that for much of his professional career Herman was focused on finding a solution to a serious environmental problem. For the most part this was coincidental, and an observation can now be made retrospectively. It just so happened that because the petroleum and petrochemicals industries produced such a large proportion of widely used consumer products, it was inevitable that waste products and by-products would also be produced in significant quantities. It is one of the characteristics of technology that problems caused by its use can usually be solved most effectively by the application of additional technology. But I also have absolutely no doubt that working on solutions to these problems had a powerful appeal to Herman's sense of stewardship for the world in which we live and all who live in it. He truly did believe that we ought to work to make the world a better place and that chemists have a special role to play in bringing this about.

So, when the exhaust gas from automobile engines was found to be a major contributor to smog and that a major component of exhaust gas was unburned hydrocarbons, it was natural that Herman would consider the possibility of using UOP catalysts in a post-engine device for the cleanup of engine exhaust. Herman led the team that focused on the development of such a catalytic device. Of course, this involved much more than the technical aspects of using science and technology to develop a product. The automobile industry had to be convinced that it would have to take this step. In order to meet exhaust emission standards of the Clean Air Act, the industry finally accepted the idea that it had to accept a retrofit developed by someone else for their automobiles. Today every automobile produced throughout the world has to have a catalytic exhaust device in order to meet clean-air standards. Without these devices, life would be a whole lot more uncomfortable and unhealthy for the world's citizens.

Herman received many awards for his research achievements. He was honored with the Eugene Houdry Award in Applied Catalysis from the Catalysis Society in 1973; Kokes Lecturer, Johns Hopkins University in 1975; E. V. Murphee Award in Industrial and Engineering Chemistry from the

American Chemical Society in 1975; the IR-100 Award from Industrial Research Magazine; the Robert A. Welch Lecturer in Chemistry in 1975; and elected to National Academy of Sciences in 1975.

Earlier I mentioned that when Herman came to Universal Oil Products, he joined a research group at the Riverside laboratory that was conducting fundamental research on the chemistry of hydrocarbons. On November 15, 1995, the laboratory was designated as a National Historic Chemical Landmark by the American Chemical Society. Bloch and his accomplishments were cited among those responsible for the landmark designation.

It is important to note that in addition to his achievements and contributions as an outstanding research scientist, Herman was also a very skilled and dedicated manager and leader of research groups. Most of his career predated the time of dual career ladders. Under the dual career system it was possible to choose between being a research manager or research investigator as a career path. Herman was very productive in both roles and progressed steadily after joining UOP as research chemist in 1936. He subsequently became research group leader (in 1939), coordinator (in 1945), deputy director of refining research (in 1955), associate director of process research (in 1961), associate director of research (in 1964), and director of catalysis research (in 1973), a position from which he retired in 1977.

AMERICAN CHEMICAL SOCIETY

Throughout Herman's career he was actively engaged in and involved with the American Chemical Society. He supported the society through membership in the Chicago Section and the Division of Petroleum Chemistry, subscribing to its major journals and abstracts, attending society meetings and serving as an officer, and presenting papers at meetings and in journals. To be more specific, he served as chair of the Chicago Section, councilor from the section, member of the board of the society for nine years, chair of the board for five years (the only person ever to do so for that length of time), and chair of several local and national committees.

He led the board of directors during a time of significant change in the public affairs focus of the American Chemical Society, as well as that of other scientific and professional societies. Historically, the only involvement of these societies with governmental bodies and public agencies had occurred when the government was in some kind of crisis and needed the special scientific and technical knowledge that society members could contribute. They would then be invited to give their expertise during the period of the emergency. Most often this occurred during wartime. This posture began to change in the 1960s and 1970s. Herman was a leader in this change of direction. One result was that Herman, as board chair, presented testimony that represented the ACS position on pending toxic substances legislation before the Subcommittee on Consumer Protection and Finance of the Committee on Interstate and Foreign Commerce of the U.S. House of Representative. He presented testimony in 1975, and the landmark toxic substances control legislation was passed in 1976. The legislation has been critical ever since to the nation's environmental protection program.

While Herman was a member of the ACS Council in the 1950s, he became very troubled by the fact that when meetings of the society were held in Southern cities, the society abided by the laws and customs of the region, and minority members were not allowed to stay in hotels or meet in function rooms where committee meetings were held. He, with other like-minded councilors, presented a resolution that attempted to prohibit the society from holding meetings in cities that required the society to enforce segregated practices. Although the motion was tabled, the message was heard and no future meetings were scheduled in Southern cities until civil rights laws were passed to eliminate segregation in public accommodations.

It is not surprising that Herman took a stand for what was a very unfair and insensitive practice toward the society's minority members. Not only did it go against the very strong grounding of his faith in the tenets of Judaism, it violated his sense of fair play. In addition, the practice of such acts of discrimination directly affected friends and colleagues with whom Herman had strong personal relationships. They were members with him in the Chicago Section, the society, as well as other professional organizations. I remember vividly when Herman told me this story many years later. You just knew he felt it was one of his finest hours as a professional and as a human being. As he shared it with me, I could feel the sense of pride that he felt, and I, too, felt the same pride. I was elated that he shared the moment with me.

As a dedicated and committed chemist, he gave a significant amount of time and effort to the American Chemical Society, first and foremost. But he also was a member of the American Institute of Chemists, the Catalysis Society, the Chicago Chemists Club, American Association for the Advancement of Science, Society of Chemical Industry, and the Illinois and New York academies of science. He was a founding member of the Chicago Chemists Club and was also very active in the local chapter of the American Institute of Chemists.

CIVIC AND COMMUNITY CONTRIBUTIONS

During his entire life Herman felt a strong obligation and duty to be involved in the support and uplift of his community. This led to his answering the call to serve in those organizations that are the heart and soul of any community. For example, during a lifetime of commitment he served as a member of the Cook County Housing Authority for many years, serving as chair in 1971. (Chicago is located in Cook County.) During his tenure he fought for fair housing for all citizens of the county, without regards to race, ethnicity, or religion. This was a period during which adequate housing for all citizens was an issue that was hotly debated, and certainly not universally accepted. He served on the Human Relations Commission of the city of Skokie, Illinois—where he made his home—for six years. During his tenure he authored the first fair housing legislation for Skokie. He also served as a trustee of the library board for many years. For his civic work he received many community service awards, including the University of Chicago's Distinguished Alumni Award for Public Service. For the State of Illinois he served as chair of the physical sciences section of the Illinois Board of Higher Education.

HUSBAND, FATHER, MENTOR

Herman was much beloved by all who knew him. He and his wife, Judith, were married for 50 years, from 1940 until his death in 1990. With each of his children he had very special relationships. His son, Aaron N., received his B.S. from Yale before going on to get his Ph.D. in chemical physics from the University of Chicago, working with Prof. Stuart Rice. Aaron went on to have a very distinguished career in industry, serving as a principal scientist with the Exxon Corporation for many years, followed by an academic career as vice-provost for Columbia University for three years. He served for four years as provost for the University of Buffalo until his untimely death in 1995.

His daughter Janet also attended the University of Chicago as an undergraduate. She went on to do graduate work at

Harvard, where she received an M.S., and returned to the University of Chicago for her Ph.D. in Russian history. She is presently a professor of history at the University of Miami (Florida). His daughter Merry Bloch Jones did her undergraduate work at Cornell and received an M.A. from the Annenberg School for Communication at the University of Pennsylvania. She is a popular author with many books to her credit.

Given the special relationship that the Bloch family has with the University of Chicago, it was not surprising that Mrs. Bloch chose to offer the chemistry department at the university the opportunity to host an annual lecture in Herman's memory that would honor an industrial scientist. Thanks to a generous contribution from UOP and contributions from his many friends, the lectureship was established, with the first presentation being made in 1992. In addition to honoring Herman's memory, the award has a two-fold purpose. First, the award is intended to recognize the achievements of chemists and chemical engineers who made outstanding contributions to science while working in industry, as did Herman Bloch; second, the award is meant to promote creative dialog between academic and industrial researchers. There is no doubt that the lecture has succeeded in achieving its stated purposes. As a result of the award, outstanding industrial researchers have the opportunity to visit the University of Chicago annually and share their research with faculty, students, and invited guests.

Herman was a father figure and mentor for all who worked for him and for other young men and women whom he met outside of work in his professional and civic duties. He was first and foremost a very gentle man, with a great sense of humor. I can remember going to him on many occasions with a problem—sometimes technical, sometimes about life. His approach was always the same. He would lean back, reflect for a while, perhaps ask a question for clarity, and then he would give advice that was not necessarily a direct answer to the question you had asked, but he had put you on the path that would lead you to your own understanding and solution. He always would make sure that you did the critical thinking so that you could claim ownership of whatever the outcome might be.

There is another term, used frequently today, that also can be applied to what Herman meant to many who knew him: role model. People observe you and want to be "like you." In his professional capacity as a research leader at UOP, as chair of the board of the American Chemical Society, and as a community leader, he was much admired and emulated. He touched and affected lives far beyond those with whom he worked on a daily basis, and his legacy will live on in his technical achievements, in the lives of those who admired and were inspired by him, and the life of service that he gave to his society, community, profession, and nation.

THIS BIOGRAPHICAL MEMOIR for Herman Bloch would not have been possible without help and encouragement from Mrs. Judith Bloch, who shared many personal documents and information about her beloved husband and family life. I have truly admired her spirit and dedication in establishing the Herman Bloch Lectureship, which continues to contribute to Herman's legacy as an industrial scientist. Thanks very much to Bipin Vora for the information on biogradable detergents, Tamo Imai for general information about the period during which he worked for Bloch, and to George Lester for assembling much of the material that served as the basis for this memoir, as well as continued consultation and encouragement. Finally, I would be remiss if I failed to mention Shirley Cornelious, Jo Ann Boston, and Kay Kim of the UOP Technical Information Center. Their help and support in locating information was crucial to bringing this effort to a successful conclusion.

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American Institute of Chemists, honor scroll, 1957; honorary membership, 1964; Chemical Pioneer Award, 1980
Brotherhood Award, North Shore Human Relations Council, 1966
Chicago Section American Chemical Society, Distinguished Service Award, 1989
Ernest J. Houdry Award in Applied Catalysis, Catalysis Society, 1971
IR-100 Award, 1973
American Chemical Society, E. V. Murprhee Award in Industrial and Engineering Chemistry, 1974
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National Academy of Sciences, elected 1975
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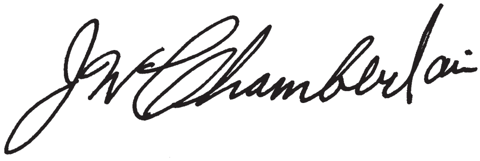
Kitt Peak National Observatory photo



JOSEPH W. CHAMBERLAIN

August 24, 1928–April 14, 2004

BY DONALD M. HUNTEN

JOSEPH W. CHAMBERLAIN was the son of a country doctor, and it was assumed that he, as well as his older brother Gilbert, would also become doctors. His first laboratory experience in comparative anatomy as a college freshman convinced him to switch to physics and then astronomy. His first job after obtaining his doctorate was at the Air Force Cambridge Research center and involved the study of the Earth's upper atmosphere through spectroscopic observations of the aurora and the faint emissions from the night sky, called airglow. Interpretations of these data led to studies of the upper atmosphere from which they came, and then led to work on the interplanetary medium and the atmospheres of the other planets. Shortly after the dawn of the space age, he left the University of Chicago for the recently formed Kitt Peak National Observatory, taking the opportunity to initiate a program of sounding rockets and to assemble a group of observers and theorists who could, and did, become interested in studies of the upper atmosphere and atmospheres of other planets. Although Chamberlain really preferred to continue his own scientific work, his leadership helped this group (of which the present writer was a member) to expand into observations with telescopes and rockets and then to become involved in NASA's planetary program. After a dozen years of administrative work, including two years at the Lunar Science Institute, he returned to the academic life and later retired back to Tucson. In his last seven years he was crippled by the effects of a stroke. Former members of his group continued to have the weekly lunches that he had organized, and he participated until his last year, when he became too weak. He and his wife, Marilyn, were enthusiastic golfers and connoisseurs of opera and orchestral music; they raised three children.

Joseph Chamberlain was born in 1928 in Boonville, Missouri, and lived his entire childhood in New Franklin, Missouri, a town of about 1,200 people located three miles from Boonville. In high school he was active in several extra-curricular pursuits, especially basketball, band (where he played first-chair cornet), and the high school newspaper, where he wrote a weekly sports column. It was here that he first developed an interest in writing as a creative activity in itself. He attended the University of Missouri, quickly switching (as mentioned above) from pre-medicine to physics, finishing with an A.M. in June 1949, and then transferring to the Astronomy Department at the University of Michigan. He was married just before entering that university, on September 10, 1949, to Marilyn Jean Roesler, daughter of a Milwaukee dentist, whom he had met two years previously, when she attended Stephens College in Columbia, Missouri. His research professor and thesis advisor was Lawrence Aller, and the chairman of the department, Leo Goldberg, was also an exemplary instructor and advisor.

Chamberlain's first publication dealt with the atmospheres of certain stars that exhibited peculiar A-type spectra and that had been previously interpreted qualitatively as being deficient (compared with "normal" stars) in their hydrogen abundance. The quantitative spectral analysis that he did with Professor Aller indicated that the opposite was true;

the stars were cooler than implied by their incorrect A-type classification and actually were F-type. This paper turned out to be the first one clearly exhibiting large metallic deficiencies in certain Population II stars, and it helped open up a rather major field of investigation of cosmic abundances and their relationship to stellar evolution. Of course, the real interpretation was done by Lawrence Aller; at that time Chamberlain was merely a graduate student, learning as he assisted Aller with the technical details.

Chamberlain went to work with the Air Force Cambridge Research Center, Geophysics Research Directorate, in the Boston area, in December 1951, about six months before actually receiving his Ph.D. Much of the writing of the thesis and final analysis of results were done while he lived in Brookline. There he developed an interest in aurora and airglow, through his affiliation with Norman J. Oliver. Under Oliver's supervision he made two brief wintertime expeditions to Thule, Greenland, which is very near the north geomagnetic pole, where he obtained spectra of the airglow over the polar cap (i.e., inside the auroral zone). Their most significant result was that the spectrum emitted by the OH radical at high latitudes exhibited a considerably higher spectroscopic temperature than it did at middle latitudes. While they made no attempt to explain the effect at the time, this was apparently the first indication that the mesopause region (at a height of about 90 km) over the polar cap was warmer in winter than the same region at middle latitudes, a result that has since been verified by other means and explained as a consequence of the dynamical circulation of the upper atmosphere.

Part of his job at the Air Force Research Center was the monitoring of three contracts at universities. This work introduced him to Ralph Nicholls at the University of Western Ontario; the present writer; A. Vallance Jones at the University of Saskatchewan; and Aden B. Meinel at Chicago's Yerkes Observatory. When he later expressed a desire to work with Meinel, it was arranged by Oliver that, starting in July 1953, he would spend some six months with him at Yerkes Observatory in Williams Bay, Wisconsin. At the end of the six months, Meinel offered him a job working on the contract that he had previously monitored, and he accepted with alacrity, remaining then at Yerkes for nine productive and enjoyable years.

The Chamberlains's first born, Joy Anne, arrived in February 1953, a few months before they moved to Wisconsin. Two sons, David Wyan and Jeffrey Scott, were born in Wisconsin in 1956 and 1957, respectively.

At Yerkes Observatory he was influenced not only by Meinel, before he left for Arizona to take charge of the development of the Kitt Peak National Observatory, but also by Professors Bengt Strömgren, then the director of Yerkes; G. P. Kuiper, who later became the director; and perhaps most of all by S. Chandrasekhar.

When Meinel left Yerkes Observatory, Chamberlain had been appointed assistant professor, and he took charge of the Air Force contract. Working with him on the program in laboratory work was C. Y. Fan. And in 1957 Lloyd B. Wallace came to Yerkes to do aurora-airglow spectroscopy on the contract, having just completed a doctorate in astronomy at the University of Michigan. Thus began many years of a most enjoyable collaboration and friendship.

In March 1960, upon Kuiper's departure from Yerkes Observatory, W. W. Morgan was appointed director, and he selected Chamberlain to work with him as associate director. The arrangement, which worked surprisingly well for more than two years, was that Chamberlain should handle day-to-day routine administrative matters (which were not too burdensome at Yerkes) and Morgan would deal with the larger problems. Chamberlain at that period thought of himself as associate everything: associate director, associate professor, associate editor of the *Astrophysical Journal* under Chandrasekhar's editorship. In January 1961 he was appointed professor and shortly thereafter was given a joint appointment in the Departments of Astronomy and Geophysical Sciences at the Chicago Quadrangle campus of the university.

His first book, "PAA" (*Physics of the Aurora and Airglow* [1961]) was written between July 1957 and April 1960. This classic book contains an enormous range of material and was in large part responsible for the intense respect in which the community held him rather early in his career.

Shortly thereafter he received a phone call from N. U. Mayall inviting him to head an operation in space astronomy at the Kitt Peak National Observatory (KPNO) in Tucson, Arizona. He moved to Tucson in early October 1962, and during the next two years assembled a staff of approximately 10 astronomers and an engineering organization under the direction of Russell A. Nidey, the systems manager of the Space Division at KPNO. The principal staff members who joined him in Tucson were Lloyd Wallace, from Yerkes Observatory, who became the principal experimenter in the development of the rocket astronomy program and the author of this memoir, who came from the University of Saskatchewan to take charge of the program in laboratory astrophysics. In the fall of 1964 Michael B. McElroy joined the group for a one-year postdoctoral "visit"; he had studied with Alex Dalgarno and David Bates at Queens in Belfast, and then spent a year with Joseph Hirschfelder's group in Madison. McElroy fitted in so well that he remained for six years and eventually left to become a Harvard professor.

The administrative side of science was something that Chamberlain entered with considerable trepidation, and he felt the increasing burden of it and its competition with the amount of research that he would like to do. On the other hand, the administrative accomplishment itself of assembling and keeping a fine research organization in planetary and space science was as gratifying and as enjoyable as any of his significant research. It was a close group that worked actively together and that assembled daily around the coffee pot for off-the-cuff discussions. The youthful spirit and rapport within the Space Division (later called the Planetary Sciences Division) was infectious.

At that time KPNO was organized into three scientific divisions. The functions of the other two, the Stellar Division and the Solar Division, were to build telescopes and other equipment and to provide observing time for university astronomers. The Space Division existed because Aden Meinel, the first director of KPNO, began the development of a remotely controlled telescope with the objective of learning how to control a telescope in Earth orbit. Under Nidey's management a rocket astronomy program was also initiated, with emphasis on fine pointing, which allowed the observation of preselected objects. This program had two major continuing problems: (1) Each flight was very expensive, even by rocket standards, because of the pointing controls, and (2) few guest observers could be accommodated on a budget that allowed only about three flights per year. The program and the division were in a constant battle for survival—hence the shift in title in 1967 to Planetary Sciences Division. Early flights, mostly with Wallace and Hunten as investigators, obtained important results on the day airglow from the upper atmosphere; they still required pointing of the instruments but were not as demanding as the task of orienting a telescope at a planetary or astronomical object.

There was also a project to design a small astronomical satellite, which attracted attention from NASA, and at one time it seemed that NASA would enter a cooperative program with KPNO to fund it. Eventually these plans fell through, but NASA a few years later took over the concept and did fly the satellites. The development of the remotely controlled telescope, a 50-inch aluminum mirror on Kitt Peak controlled by a computer in Tucson, was also continued. Around 1968 that project was taken away and the telescope turned over to the Stellar Division and converted for ordinary use by astronomers in the observing dome. A couple of years later the expensive rocket program was terminated.

Chamberlain was elected to the National Academy of Sciences in 1965. The next year he was invited through Gordon MacDonald's recommendation to join JASON, a group of 40 or so physicists who spend part of their summers working on applied physical problems for the government. Because much of the work was militarily oriented and classified, JASON became much maligned in the antiwar movement of the late 1960s. It was not widely known that almost all the JASONs were strongly against the war in Southeast Asia but retained their ties with the Pentagon in the hope of being able to do something useful within the system. Many of them also worked on environmental problems, including some associated with the effects of a massive exchange of nuclear weapons detonated in the upper atmosphere. Chamberlain's interest in modifications of the stratosphere and human-induced changes in the climate was largely stimulated by such work.

The major scientific interests in the division had become purely planetary, without justification of space technology. Hunten and Michael Belton increased their observing programs of the planets with the McMath solar telescope on Kitt Peak. Richard Goody of Harvard became a frequent visitor and helped convince the board of the Association of Universities for Research in Astronomy (AURA) of the quality of the research being done in the division. In February 1967 the first of what was to become an annual conference, generically called the Arizona Conference on Planetary Atmospheres, was organized. Each year a different topic was chosen, a different member of the staff served as organizer, and a somewhat different list of invitees drawn up. These meetings, held in the best season of the year for Tucson, were very successful and filled a need for planetary conferences that was not at that time being met by either the American Astronomical Society or the American Geophysical Union. In September 1970 McElroy left KPNO and in March 1971 Chamberlain left, and the division began to disintegrate. The last such conference was held in March 1971, but there was another reason for their cessation.

In 1967 a small group of planetary astronomers led by Carl Sagan petitioned the American Astronomical Society to form a division for planetary astronomy. There was a precedent in that solar astronomers had recently formed such a group. The council responded by asking Chamberlain to serve as chairman of the organizing committee, which he did. The principal task was to draft the bylaws in accord with the bylaws and constitution of the parent society, a task that was not made any easier by Martin Schwartzschild's having succeeded Al Whitford as president of the society.

In December 1968 at the American Astronomical Society meeting in Austin, a symposium on planetary atmospheres was convened, and the organizing committee met to modify the draft bylaws. After review by the council the bylaws were adopted by a mail ballot in May 1969. The first official meeting of the Division for Planetary Sciences was held at the Jack Tar Hotel in San Francisco in January 1970. Chamberlain served as chairman of the division the first year; its annual meetings are attended by a large fraction of the world's planetary astronomers.

In the spring of 1970 Chamberlain was rudely informed that the rocket engineering group was removed from his control; he immediately resigned as associate director and a few months later accepted an offer to become director of the NASA Lunar Science Institute, located beside the Johnson Space Center in Houston. Relations with its governing board were even rockier than they had been at KPNO, and after two years he took up a professorship at Rice University. He remained there until he retired as emeritus professor in 1990. He had several graduate students, and among his classes was a graduate-level one in planetary atmospheres, which was the basis of his second major text, *Theory of Planetary Atmospheres* (1978). A decade later he invited the writer of this memoir to join him in preparing a second edition, which was published in 1987.

In Houston they bought a house from Buzz Aldrin, who had been on the Apollo 11 lander with Neil Armstrong. The existing support staff at the Lunar Science Institute (LSI) was in a state of anarchy. Most of them did not have enough work to do, and office intrigue was rampant. Within a few months Chamberlain had removed the worst of the troublemakers, put out a brochure describing LSI and the resources it could provide university scientists, and organized half a dozen conferences in the manner of the Arizona conferences on different topics. The LSI also managed the review of NASA proposals for the analysis of lunar samples.

One of the sadder aspects of leaving LSI (in June 1973) was that he ceased working with Helene Thorson, who had been his assistant for more than 15 years at Yerkes Observatory, KPNO, and LSI. She remained at LSI, where she served as executive assistant to an assortment of directors that followed.

After Chamberlain arrived in Houston he was appointed adjunct professor (without salary) in the Space Physics and

Astronomy Department at Rice University. Alex Dessler had founded the department and had earlier wanted him to join it. The current chairman was Ronald Stebbings, whom Chamberlain had earlier tried to hire at KPNO to start a new scientific division in laboratory astrophysics. (Lacking enthusiastic support from the director and the AURA board he called Stebbings in London and told him the job was approved but the outlook for an expanded laboratory of the scale we both envisioned did not look good. Consequently he took an offer from Rice instead.)

One job Chamberlain undertook was the editorship of *Reviews of Geophysics and Space Physics.* He accepted because he had worked with Chandrasekhar for several years as associate editor of the *Astrophysical Journal* and because his lunar science experience gave him some familiarity with subjects and people in solid-earth geophysics.

RESEARCH ACCOMPLISHMENTS

Aurora and Airglow Spectra. In 1952 Chamberlain obtained, on an expedition to the U.S. Air Force base at Thule, Greenland, the first spectra of OH airglow from inside the auroral zone and first demonstrated substantial OH rotational temperature anomalies between polar and temperate latitudes.

In 1953-1954 he obtained at Yerkes the first airglow spectra in the near ultraviolet with sufficient dispersion (23Å/mm) to resolve the rotational structure of the bands. These spectra gave the first positive identification of these bands as the Herzberg forbidden system of O_2 and showed that the rotational temperature was less than about 200 K. The latter result placed the emitting level in the 80-100-km region, much lower than contemporary photometric studies had indicated. The exposure required for the best spectrum was 75 hours, most of the dark, moonless hours of one entire month. In 1958 he finally obtained rotationally resolved spectra in the weaker and more confused blue region. Some of the features in this region have never been satisfactorily identified. The spectrographs used in both these investigations were designed by Aden B. Meinel. The smaller one that he took to Greenland had a 13-cm-diameter Schmidt camera. The larger one, which was mounted in an observing shack on the roof of the Yerkes Observatory, had an F/0.8 flat-field Schmidt camera, 23 cm in diameter, with a two-piece mosaic grating 20 by 20 cm. There was no spectroscopic equipment for airglow work anywhere that could match the larger of Meinel's instruments except a "sister" Meinel spectrograph that was used on aurora by Alister Vallance Jones in Saskatoon. This Yerkes spectrograph was also used to obtain the profiles of H-alpha that are described below.

Theory of Hydrogen-Line Profiles in Auroral Spectra. In 1954 and 1957 Chamberlain wrote a series of three papers in which the profiles (intensity versus wavelength) of hydrogen lines were first computed for incident beams of bombarding protons. The broadened profiles, with a blueward Doppler shift indicating downward motion, had been discovered earlier by Meinel. The problem was to use the profiles seen in the magnetic zenith, the profile from the magnetic horizon, and the distribution of auroral luminosity with height to extract a self-consistent description of the energies and angular distribution of the primary auroral protons. The principal result was that the bombarding protons had to have a *wide* dispersion in their initial velocities. This was, at the time, a new concept, and it was extracted from the ground-based spectra about a year before spacecraft measurements found the Van Allen radiation with an energy dispersion. It was natural that he should have undertaken this study, because his thesis work on gaseous nebulae had considered the effect of electron collisions in producing radiation from hydrogen atoms. It was a straightforward extension to substitute collisions of H^+ and H with air molecules for collisions with free electrons.

Theory of Na "D"-Line Airglow Variations. In a series of four papers in the *Journal of Atmospheric and Terrestrial Physics* (1956, 1958) with various collaborators, he first considered the importance of multiple scattering on the twilight, night, and day airglow in the resonance transition of Na. The importance of self-absorption in the Na layer had been noted by Donahue and by Hunten. Like most astronomers with a theoretical background, Chamberlain was familiar with the theory of radiative transfer as developed by Chandrasekhar and, therefore, applied his formalism to the airglow problem.

Polarization of Resonance Radiation. In 1959 and later he published a number of analyses of polarization measurements that were unique to the geophysical literature. This work had its origins in writing his book, "PAA" (*Physics of the Aurora and Airglow* [1961]), in which he was attempting to give the theory for an observed polarization of the Na "D" emission in twilight. The standard reference was the text by Mitchell and Zemansky, which was based on Van Vleck's treatment with the old quantum theory, which can give incorrect results. Instead, he used the Weisskopf theory, which is based on Dirac's theory of radiation. He applied the theory, which gives not only polarizations but the anisotropy of the scattering phase function, to Na "D," H Ly-alpha scattering in the night sky (with J. C. Brandt), the forbidden oxygen red lines excited by electron impact, and also applied the theory to comets.

Energy Deposition by Bombarding Auroral Electrons. In 1961 in "PAA" he published the first theoretical analysis of the height distribution of energy deposited in the atmosphere by primary auroral electrons (with proper allowance for electron scattering). An electron-induced aurora is more complicated to handle than one produced by relatively heavy protons, because the electrons are readily deflected as well as slowed at each collision. The analysis indicated also the fraction of the incident electrons that would be reflected by the atmosphere and produce auroral emission in the opposite hemisphere, which was a new concept at the time. The basic equations, which are somewhat like radiative transfer equations with the added complication that the velocity of the particle varies, had been developed earlier by Harold Lewis, who was concerned with the bombardment of tissues by beta-decay electrons.

Plasma-Instability Theory of Aurora. In 1963 he published the first quantitative theory of auroral bombardment that was explicitly based on a particular plasma instability. By this time it seemed to him that the bombardment mechanism was to be found in the instabilities that were constantly plaguing plasma physicists working on the confinement of plasmas by magnetic fields. He simply watched the plasma physics literature until a likely candidate showed up—an instability developed by N. A. Krall and M. N. Rosenbluth. It was thought by them not to be important in the laboratory but seemed to him to hold promise on the geophysical scale. The key feature is an induction electric field parallel to the external magnetic field.

Solar Breeze. In two papers in 1960-1961 he challenged Eugene Parker's theoretical analysis of the solar wind, maintaining that the solutions he obtained from the basic equations were specious and that the only physical solutions were those that had decreasing velocity approaching zero at large distances. A considerable controversy raged for a few years, with neither of them allowing that the other's solution was physical. Part of the difficulty was emotional. However, another part was that in addition to the equations of motion and continuity, Parker handled the temperature with a polytropic relationship, which always yields a solar-wind (supersonic) solution at some distance (and beyond), whereas Chamberlain's second paper for the first time treated temperature with the equation of heat conduction. (Chapman had earlier used the heat conduction equation but only for a static corona.) The resolution was simply that either solution could be correct (see below). Parker's use of a temperature profile resembling the observed one had led him to the solution that is actually valid for the sun. Chamberlain's interest in this topic was due to the implications of Parker's results to the escape of planetary atmospheres.

He found that Parker's solution, if taken literally and applied to the Earth's upper atmosphere, would have produced a rate of escape of the terrestrial atmospheric gases far exceeding what was predicted by kinetic theory. Hence, he began a study of the hydrodynamic equations of motion and obtained solutions contrary to those of Parker. Much of Parker's theoretical analysis, as well as his own, were undoubtedly inspired largely by their dialogue, so that the net result was a genuine benefit to the subject. (Chamberlain's last contribution on this subject was a note in the *Astrophysical Journal* in January 1965, followed by a disagreeing note by Parker; but a subsequent paper by Parker in the same year shows that the physical conditions required for a slow "solar breeze" solution are the same as Chamberlain stated in his January 1965 note.)

Exospheres and Escape of Planetary Atmospheres. In 1963 he published a thorough exposition on the density distribution in a planetary exosphere. Elaboration of this major paper was to occupy him on and off for 15 years. The 1963 paper was a fairly complete discussion of an exosphere where atomic collisions could be ignored, except for the special case of satellite orbits. The theory had been treated most notably by Sir James Jeans, and recent contributions by Francis Johnson and Öpik and their collaborators set the scene for a treatment of densities and the velocity distribution by Liouville's theorem. Later papers in the late 1960s dealt with the correction to Jeans's escape rate due to departures from the truncated Maxwellian distribution usually assumed; in the 1970s with the radial distribution of velocities in the exosphere (H-line profiles); and with charge-exchange collisions between H^+ and H. The latter problem became important when it was realized that the background H^+ plasma was much hotter than the neutral gas. The solution was accomplished by extending the Liouville equation to a Boltzmann equation. The paper resolved a current dilemma in the escape rate of hydrogen as inferred from exospheric measurements and middle atmosphere diffusion results for hydrogen-containing compounds.

Radiative Transfer in Planetary Atmospheres. In 1956 he first showed that the variation in the strength of absorption lines in an optically thick, homogeneous planetary atmosphere increased from the crescent phase toward full phase, which is opposite to the trend for optically thin atmospheres, such as Mars's and Earth's. Observations by Kuiper analyzed in the same paper showed the absorptions by CO_2 in the Venus atmosphere to behave in just this fashion. (A footnote in that paper makes it clear that the observations were Kuiper's, the analysis Chamberlain's.) The only thing of significance to have appeared in this subject earlier was the demonstration by van de Hulst that weak absorptions in such an atmosphere would be proportional to the square root of the absorption coefficient, rather than to the first power. Nine years later Chamberlain wrote a much more thorough analysis of the Venus atmosphere, based on the earlier spectroscopic interpretation. Very soon after this, the field became alive. In 1970 he published a rather complete analytical study of spectroscopy of an optically thick, isotropically scattering atmosphere; later he extended the work to anisotropic scattering and a student (J. P. Lestrade) examined continuum scattering to identify properties of the absorber in the Venus clouds.

Influence of the Stratosphere on Climate. In 1977 he made the first proposal for a physical mechanism that could connect variations in the sun's magnetic field to terrestrial climate. On the one hand there had been observational evidence linking sunspot activity and cosmic ray bombardment to historic climatic changes. On the other hand, studies of the balance of ozone in the stratosphere due to the presence of various catalytic chemicals suggested that cosmic-ray bombardment (and hence the strength of either the geomagnetic or heliomagnetic field) could affect the concentration of one or more of these ozone-destroying catalysts. Hence, the proposed chain of events was (1) sunspot cycle governs cosmic-ray deposition in the stratosphere; (2) cosmic rays produce atomic N through ionizing and dissociating collisions; (3) N reacts to form NO and NO_2, which catalytically destroy O_3; (4) a change in O_3 affects the temperature of the surface by altering the greenhouse insulation affect; (5) the altered stratospheric temperature affects the stratospheric abundance of water vapor, which tends to be nearly saturated at the base of the stratosphere; and (6) the altered water vapor further changes the greenhouse effect in the same direction as the altered ozone abundance.

Joseph Chamberlain's colleagues and friends hold him in great respect and admiration for his creativity, integrity, and leadership.

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Ansley J Coale

ANSLEY J. COALE

November 14, 1917–November 5, 2002

BY THOMAS J. ESPENSHADE, JAMES TRUSSELL, AND CHARLES F. WESTOFF

ANSLEY JOHNSON COALE, William Church Osborne Professor of Public Affairs and professor of economics emeritus at Princeton University, died on November 5, 2002, at Pennswood retirement village in Newtown, Pennsylvania, at the age of 85. The cause was heart failure following several years with Parkinson's disease.

Coale was born in Baltimore, Maryland, on November 14, 1917. He attended public high school in Annapolis, graduating in 1934 at the age of 16. Since his College Entrance Board scores in Latin were not acceptable for admission to Princeton University (he scored only 28 percent in Virgil), he spent one year at Mercersburg Academy to correct that deficiency on a scholarship for boys from low-income families, and he matriculated at Princeton in the fall of 1935.

Coale was educated entirely at Princeton University (B.A. in 1939, M.A. in 1941, and Ph.D. in 1947) and spent his whole academic career at its Office of Population Research, serving as assistant director from 1954 to 1959, as director from 1959 to 1975, and as associate director from 1975 to 1986. He was appointed assistant professor of economics in 1947, promoted to associate professor of economics in 1954, promoted to professor of economics in 1959, and named

William Church Osborne Professor of Public Affairs in 1964. He retired from the faculty in 1986 to become senior research demographer at the Office of Population Research, a position he held until 2000. During his many years on the Princeton campus, Ansley was a familiar figure on his bicycle and on the tennis and squash courts. In June 2002 Princeton University honored Coale by naming its demographic research library the Ansley J. Coale Population Research Collection.

He served as president of the Population Association of America in 1967-1968 and as president of the International Union for the Scientific Study of Population from 1977 to 1981. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society, and he was a recipient of honorary degrees from the University of Louvain in 1979, the University of Liège in 1983, the University of Pennsylvania in 1983, and Princeton University in 1994. He was also a corresponding fellow of the British Academy. He received both the Mindel Sheps Prize in Mathematical Demography and the Irene Taueber Prize, the most prestigious prizes awarded by the Population Association of America. He was appointed by President Kennedy as the United States representative to the United Nations Population Commission and served in that post from 1961 to 1967.

He was very prolific, publishing more than 125 books and articles on a wide variety of demographic topics. He also trained and served as mentor to many students who became leaders in the field. Indeed, he was the principal advisor on more than 35 doctoral dissertations and more than 90 research papers by visiting graduate students who earned the certificate in demography offered by the Office of Population Research.

His first major influential work was *Population Growth and Economic Development in Low-Income Countries* (1958), coauthored with Edgar Hoover; the results, which showed that slowing population growth could enhance economic development, had a major impact on public policy and set the research agenda in this field. This was followed by *Regional Model Life Tables and Stable Populations* (1966), coauthored with Paul Demeny. These model life tables both established new empirical regularities and proved invaluable in the development of later techniques for estimating mortality and fertility in populations with inaccurate or incomplete data. Along with William Brass, Coale pioneered the development and use of these techniques, first explicated in *Methods of Estimating Basic Demographic Measures from Incomplete Data* (1967) and in *The Demography of Tropical Africa* (1968).

Coale was an able mathematician (he taught radar at the Massachusetts Institute of Technology during World War II as a radar officer in the U.S. Naval Reserve), and his *Growth and Structure of Human Populations* (1972) is an essential textbook for those interested in formal demography. The publication of this book is all the more remarkable since the original source materials (notes, hand-drawn figures, tables), carefully collected over the course of many years, were accidentally discarded by a new custodian who did not recognize their significance; everything had to be reconstructed from scratch. Toward the end of his career Coale became interested in the population changes in China and in understanding the fertility transition there as well as factors affecting the sex ratio at birth.

All three of the writers of this memoir have vivid memories of Ansley as a person. Ansley was the primary mentor and thesis advisor to two of us (T.J.E. and J.T.). We reflect, in turn, on those memories. One of us (T.J.E.), having attended the College of Wooster, a small liberal arts college in Ohio affiliated with the Presbyterian Church, had heard of Ansley

Coale some years before meeting him. Ansley's older son, Pete, was a classmate of mine at Wooster. Wooster College was also the alma mater of Frank Notestein, the first director of the Office of Population Research at Princeton. With these connections, I should have known I was predestined to go into demography.

But as an undergraduate I actually had little idea what demography was. I was interested in mathematics but ended up majoring in economics because several college classmates and I were determined to spend our junior year abroad, and the chair of Wooster's mathematics department convinced me that the European way of sequencing courses in mathematics was so different from the American system that I had to choose between going abroad and staying home to major in mathematics. The London School of Economics won out. Following college I enrolled in a one-year Master of Arts in Teaching program at Yale expecting to become a high school math teacher. But the Vietnam War intervened, and the draft made staying in graduate school an attractive alternative. I singled out Ph.D. programs with a concentration in mathematical economics and arrived at Princeton in the fall of 1966 still not knowing anything about demography. In fact, one of the ironies of my professional career is that I turned down a demography fellowship for graduate study at Michigan for fear that I would be committed to studying a subject that I might actually dislike.

John Isbister introduced me to Ansley during my first semester at Princeton. I wanted to meet him because I had known his son, but I was not in any of Ansley's courses. Two of my graduate school classmates (Kevin Young and Yukon Huang) tried to convince me to study demography, but the whole subject sounded rather uninteresting. During the summer following my first year in graduate school, I was a research assistant in the Department of Economics at the University of Southampton in England, assigned to work on the dwellings (or household) sector of the U.K. econometric model. I began thinking that there might be a dissertation topic here and decided, therefore, to take the demography sequence in the next academic year.

All it took was a couple of weeks in Ansley's first semester course (Survey of Population Problems) to convince me the subject matter was fascinating. What appealed to me most was Ansley's infectious enthusiasm for the subject, especially his treatment of stable population theory (his disinterest in migration or immigration is another story). The subject involved just the right amount of math, and soon the equations for the stable age distribution, birth rate, and intrinsic growth rate became as familiar as the back of my hand. Ansley also guided me through my dissertation (actually through two false starts and then a dissertation that was completed while I was doing a two-year postdoc at Berkeley under the auspices of Kingsley Davis). I'm still grateful to Ansley not only for shepherding my work long-distance but also for prodding me to consider how estimates of parental expenditures on children might be affected by alternative specifications of the underlying regression models.

Ansley had a strong competitive streak. I experienced this directly during a student-faculty squash tournament in graduate school. I was so nervous playing Ansley in the second round that I lost track of where we were in the game. After one serve Ansley pronounced, "That serve was out." And when I went to serve again, he said, "And that was your second serve!"

Ansley enjoyed a good joke. When he began teaching a radar class at Harvard during World War II, he said, "I'm sure there are many people who know more about radar than I do. But seeing none of them present . . . " And he was fond of saying that so and so was "sui generis to a fault."

There were things that could make Ansley irritable. Just the mention of Ronald Reagan (our "acting" President) would set him off. So, too, could people who used improper grammar. He would always correct someone who began a sentence, "Hopefully, it will . . ." And his frustration boiled over when he once had trouble figuring out the tip at his favorite Italian restaurant and remarked, "I'm usually infallible in such matters." Ansley kept a weight chart in his office and was proud of the fact that it seldom deviated over many years by more than a pound from a perfectly flat trend line. Being able to wear sport coats that he owned in college was another source of satisfaction. Ansley's sometimes puritanical streak extended to turning out lights at the office before he went home—a habit that was cut short after someone let out a scream when he turned off the lights in the ladies room.

The things for which I will remember Ansley are his respect for data quality, his attention to detail in his research, and rum and tonic drinks (with Bacardi's light) and a twist of lime.

The memories of another of us (J.T.) start before coming to Princeton University. After college I went to Oxford University for two years of graduate study in economics. I had no idea what I wanted to do next. In the fall of my second year Professor William Branson visited for a couple of days from Princeton. After I told him what I was interested in, he said that the economics department at Princeton would be the best place for me to finish a Ph.D., because I could study demography with Professor Ansley Coale. So I applied to Princeton and later received a letter from Ansley Coale offering me (he said) a magnificent fellowship with a $3,000 stipend if I wanted to study demography at the Office of Population Research.

I had never heard of demography, and I dutifully went to the library to look up this Ansley Coale in the card catalogue. The only book I could find was *Regional Model Life Tables and Stable Populations* by Coale and Paul Demeny, published by the Princeton University Press. This is a substantial book of 900+ pages, weighing in at 4 pounds, 12.5 ounces. It is also the world's most boring book, with only 4 pages of figures and 25 pages of text, but a staggering 875 pages of tables. Altogether there are only 14,850 words of text but 553,609 numbers. So my heart was filled with dread that I would die of boredom at an early age.

However, my fears were groundless. From my first day at Princeton, Ansley became my mentor and friend. His two-semester demography class was the starting point in my subsequent career. And he and his wife, Sue, introduced me in the first week to the Homestead Inn, their (and now my) favorite restaurant in Trenton. Ansley was simply a terrific mentor. As with more than 35 other Ph.D. students over many years at Princeton, he was my principal thesis advisor. I also had the good fortune to stay on at the Office of Population Research after finishing my dissertation, and Ansley and I continued to work together for many years, eventually publishing nine papers together.

What made Ansley such a great mentor? In part it was his infectious enthusiasm for any demographic problem or issue, with the single conspicuous exception of migration. In part it was his extraordinary brilliance and insight, but most of all, it was his integrity. Jane Menken and I were working on our theses at the same time, and we each had the experience of having to completely redo our empirical analyses when Ansley discovered a small error in our calculations. We knew, and Ansley knew, that redoing the calculations would make absolutely no qualitative difference, and only a miniscule quantitative difference, to our results. But we each knew that we had to redo the calculations, even if that took much time and effort, because Ansley would have done so. "Because Ansley would have done so" is a phrase that I have silently spoken to myself or said out loud to my students many times. Recently, Allison Hedley, a Ph.D. student at the Office of Population Research, handed in a complete draft of her thesis. A week later, she came to tell me that she had discovered that she had miscoded a handful of cases out of 6,568 in the entire dataset and so she would be rerunning all of her analyses. Imbuing that sense of integrity is Ansley's finest legacy.

Was Ansley without fault? Hardly. He could be incredibly stubborn. And he could also be controlling. In the many times he took me to dinner at the Homestead Inn, he never allowed me to order; instead he always ordered family style for the whole table. He was also extremely competitive. Often, after talking with Ansley about a problem during the day, I would work late into the night trying to solve it, knowing that if I did not, he would arrive the next morning with solution in hand. Ansley could be charmingly naive. The day that Lawrence Altman's first piece on AIDS appeared in the *New York Times*, a group of us was discussing the content. Regarding the description of a man who had had thousands of sex partners, Ansley proclaimed that the *Times* had made a typographical error by inflating the number by a factor of a thousand.

The last of us (C.F.W.) knew Ansley longest. Ansley is someone I knew well since I first arrived in Princeton in 1955 and with whom I interacted virtually every day at the Office of Population Research.

Ansley had a habit of walking around the office looking for open doors and cornering people with some new idea. Several memories stick with me, especially the incredible and ingratiatingly boyish enthusiasm he had for what would sometimes turn out to be the germ of a really important idea. His chief box of tools would be a piece of paper or a blackboard on which he would depict some relationship with a scribbled graph and some illegible notations. His enthusiasm for ideas was really infectious and made for an exciting intellectual atmosphere at the Office of Population Research.

One of his teaching achievements that he was proud of was to inject an attitude of skepticism into graduate students, many of whom came from developing countries in the 1960s and 1970s. He would rejoice when a student would begin to question the accuracy of a printed number in a census or other publication.

Ansley introduced me to two activities that were to become lifetime habits for me: tennis and squash, and wine. He taught me to play, and it became my lunchtime activity for 45 years. Ansley had a highly competitive streak on the court (as well as at the bridge table) to which his wife, Sue, his two sons, and many others can attest. He also had a low tolerance for any extraneous noise while playing tennis, especially loud music emanating from loudspeakers in nearby dorms, particularly audible after he had made an error.

He also introduced me to wine. I especially remember the 1959 Beaujolais, which at the time sold for $1.29, later to be regarded as one of the great wines of the century. I could not tell the difference then but I can now; it turned into an expensive habit.

Ansley loved to argue, especially about politics. We certainly agreed on important issues, but if there were any inkling of disagreement, he would interrupt and repeat his position. If you then made the mistake of persisting in your mistaken view, he would interrupt again with an insistent “Excuse me, excuse me” (a polite way of telling you to shut up and listen to him) and with an ever-reddening complexion would begin to question your understanding of the gospel truth. And there was a lot of “gospel” in that Presbyterian conscience he acquired from his father.

He was in love with an Italian restaurant—The Homestead Inn, also known as Chick and Nello's after the two founders—in a Trenton suburb. Ansley loved that place and once confided to me, *sotto voce,* that Chick's was perhaps the best restaurant in the world! (He later denied having said that.) Ansley had learned Italian and used the staff at that restaurant to practice and to impress them (*Il capo di tutti capi!*). He and Sue spent many pleasant summers in Italy at a villa in Florence and frequently as the guest of his close friend Massimo Livi-Bacci.

Ansley was a stickler for grammar and for spelling, an obsession I shared with him that he thought was one of my really good points. I did catch him in a grammatical error once that provoked an argument, but he later sheepishly confessed that he was wrong.

There are so many other memories: the annual office picnic softball games in which we would each pitch for opposing teams; the trips we took together to the Caribbean; his accidents on his bike and especially the accident diving into the shallow pool in a Manila hotel (that resulted in his appearance with a huge Band-Aid on his face on the stage at the opening ceremony of an international population conference with President Ferdinand Marcos); and the many lovely dinners at the Coale's home. Ansley had a very important influence on my life, on the Office of Population Research, and on the field of demography.

Perhaps Coale's major scientific contribution was to our understanding of the demographic transition. He was the intellectual architect of the European Fertility Project, which examined the remarkable decline in marital fertility in Europe. Initiated in 1963, the project eventually resulted in the publication of nine major books (culminating in *The Decline of Fertility in Europe* [1986]) summarizing the changes in childbearing during a century in the 700 provinces in Europe.

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Photo by Fabian Bachrach

D S Fredrickson

DONALD SHARP FREDRICKSON

August 8, 1924–June 7, 2002

BY JAMES B. WYNGAARDEN

DONALD FREDRICKSON, eminent physician-scientist, former director of the National Institutes of Health, and first full-time president of the Howard Hughes Medical Institute, died suddenly on June 7, 2002, two months short of his seventy-eighth birthday. Fredrickson was semi-retired, living in Bethesda, Maryland, at his home of 50 years, where he swam in his back-yard pool every day. That morning his wife, Priscilla, found him floating in the pool. He could not be revived; the official cause of death was listed as drowning. Donald Fredrickson was buried in Rhijnhof, near Leyden, in the Netherlands on June 13, 2002.

Fredrickson was born and raised in Canon City, Colorado, where his father was a lawyer. He attended the University of Colorado for one year during World War II, before being transferred by the Army to the University of Michigan, where he earned a B.S. in 1946, and an M.D. with distinction in 1949.

An event in medical school profoundly changed his life. During a third-year elective he met a Dutch anesthesiologist

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who was spending a year in Ann Arbor. When Don mentioned that he was planning a summer bicycle tour with several classmates in Europe, the Dutch physician urged Don to visit his recently widowed sister in Holland. Don did so, intending to rejoin his classmates a few days later, but then he met the widow's daughter. The result was that Don canceled his bicycle tour and went to Scotland with his new friends. Two years later Don returned to Holland to marry Henriette Priscilla Dorothea Eekhof, to whom he remained devoted for the rest of his life. Don, a sparkling practitioner of the English language, in time became equally accomplished in Dutch.

Following graduation from medical school Fredrickson undertook residency and fellowship training in internal medicine under George Thorn at the Peter Bent Brigham Hospital in Boston, and then spent one year in the laboratory of Ivan Frantz, a cholesterol biochemist at the Massachusetts General Hospital. In July 1953, Don moved to the National Heart Institute (NHI), a component of the rapidly expanding National Institutes of Health (NIH) in Bethesda, Maryland, which was to be his scientific home for much of his career. For several years he worked in the laboratory of Christian Anfinsen, a protein chemist and future Nobel laureate, under whom he acquired biochemical knowledge and laboratory skills while researching lipids and lipoproteins. Don concentrated initially on cholesterol metabolism, in collaboration with Daniel Steinberg. Later he focused on structure and metabolism of plasma lipoproteins and their role in lipid transport. He and his colleagues in the Molecular Disease Branch of NHI separated apolipoproteins A, B, and C into their component parts and sequenced and characterized apolipoproteins A-II, C-1, C-II, and C-III. In the early 1960s, Fredrickson and coworkers discovered two new genetic disorders of lipids. The first they named Tangier Disease, after the island in the Chesapeake Bay where the first patient lived. This disease is characterized by lipid storage in various organs including tonsils and absence of high-density lipoproteins in plasma. Almost four decades later Tangier Disease was shown to result from a mutation in the gene for a protein that mediates cellular cholesterol efflux. The second disease they discovered was cholesteryl ester storage disease, a lysosomal enzyme disorder manifested clinically by hepatomegaly and hyperlipidemia in childhood and premature atherosclerosis.

The contribution that catapulted Fredrickson to worldwide prominence was his classification (with Levy and Lees) of lipid disorders into five categories based on their clinical characteristics and the patterns of array of plasma lipoproteins on paper electrophoresis, a relatively simple procedure widely available. This study eventually included data from over four hundred patients and their family members collected over about eight years at the Clinical Center of the NIH. The adoption of the Fredrickson Classification of the Hyperlipidemias (as it came to be known) as an international standard by the World Health Organization in 1972 focused attention of physicians around the globe on these common abnormalities. In 1970 the NHI established a national Lipid Research Clinic Program headed by Robert Levy, one purpose of which was to evaluate the effect of lipid lowering on coronary heart disease. This became the first large study to show a beneficial relationship between cholesterol lowering and cardiovascular disease.

These were exhilarating days in lipoprotein research as well as in the investigation of hereditary diseases in general, especially of the category of disorders characterized by Garrod in 1908 as *Inborn Errors of Metabolism*. The explosion of new knowledge in biochemistry and genetics led to the elucidation of the mechanisms of known hereditary diseases and the discovery of many new ones. It soon became clear that what had been thought to be a single genetic disease entity often comprised multiple variants that represented different genetic mutations affecting a single protein. In addition, many common diseases were being recognized as having important hereditary determinants. In 1960 Donald Fredrickson joined John Stanbury and James Wyngaarden in creating *The Metabolic Basis of Inherited Disease*, a comprehensive, multi-authored compendium of hereditary disorders about which there was substantial biochemical and genetic knowledge. The original editors husbanded this influential reference work through five editions over 23 years. It has since been continued under other editors, as *The Metabolic and Molecular Bases of Inherited Disease* and is now a four-volume work in its eighth edition, a veritable encyclopedia of molecular medicine.

In addition to directing a vigorous laboratory program in which he trained a number of young physicians who later became prominent figures in lipoprotein research and academic medicine, Fredrickson served in several management positions within the NHI. In 1961 he became head of the Section on Molecular Disease and simultaneously clinical director of NHI. In 1966 he was elevated to chief of the Metabolic Disease Branch and also appointed director of the National Heart Institute, a virtually full-time position involving major interactions with the extramural research community and frequent appearances before Congress on policy and budget matters affecting NHI. He relinquished the institute directorship after two years in order to spend more time in research and to serve in a less demanding administrative position as director of intramural research in the recently renamed National Heart and Lung Institute. His research and administrative accomplishments earned

Fredrickson election to both the Institute of Medicine and the National Academy of Sciences in 1973 at the age of 48.

In 1974 Fredrickson left NHLI to accept the presidency of the Institute of Medicine of the National Academy of Sciences in Washington, D.C. Nine months later he was selected by President Gerald Ford to become director of the National Institutes of Health, a position he viewed as a "cause" he could not refuse. It was a role for which he was superbly prepared by virtue of his knowledge of the institution, his management experience at several levels, and his outstanding record as a research scientist and trainer of young investigators. He was also an exemplar of how human welfare could be advanced by medical science and a gifted communicator well qualified to become its ambassador at the highest levels of government.

Fredrickson assumed the directorship of the NIH on July 1, 1975, at a time of great apprehension on the part of a number of leading scientists over the safety of recombinant DNA research. At a Gordon Conference in the summer of 1973, several scientists had presented reports on the technical ability to join together covalently DNA molecules from diverse sources to create hybrid plasmids or viruses whose biological activity was unpredictable. Following this meeting a conference-approved letter was sent to the president of the National Academy of Sciences expressing great concern over potential risks of recombinant research. The letter was published in the September 13 issue of *Science* magazine. In response, the NAS appointed a high-level committee to consider the risks involved in this research, which in its report, released in a press conference at the NAS in July 1974, recommended a moratorium on certain kinds of experiments and asked the director of NIH (at that time Dr. Robert Stone) to establish a committee to develop guidelines for work with recombinant DNA that would minimize the risks. In February 1975, the National Academy of Sciences sponsored a meeting on these questions at the Asilomar Conference Center in Pacific Grove, California, involving 90 American scientists and another 60 from 12 other countries, as well as members of the press. On the final day of the conference, by coincidence, the new NIH Recombinant DNA Molecule Program Advisory Committee held its first meeting on the other side of the country in Bethesda, Maryland. This frenetic activity among scientists concerning a new technology with unknown risks—thought negligible by some, fearsome by others—was well publicized and created considerable public apprehension.

Few directors have taken office in such a highly charged atmosphere. In 1975, two great value systems were on a collision course: those of free scientific enquiry and of environmental protection. Scientists were deeply divided over the moratorium on recombinant DNA research and the entire Asilomar conference process. Many scientists felt they had made a serious mistake by airing technical concerns before a public ill-equipped to understand the complexities that science itself was only beginning to fathom. Fredrickson immediately found himself at the center of this controversy. In his new role as advocate for the life sciences, he sought and ultimately found a solution that enabled recombinant research to continue without compromising an NIH imperative of avoiding an overt regulatory role. But for the first two years of his tenure as director, the recombinant controversy consumed at least half of his time.

Fredrickson had recruited to his staff a young psychiatrist, Joseph Perpich, who was also an attorney and who had experience as a law clerk under Judge David Bazelon and as a congressional staffer under Senator Edward Kennedy. Perpich became Fredrickson's closest advisor and assistant in dealing with the recombinant DNA controversy, a role in which he had many occasions to utilize both his legal and psychiatric training. These were tumultuous times. There were endless rounds of meetings within NIH, within the Department of Health, Education and Welfare (HEW), with other government agencies, with congressional members and staffs, with scientists from the U.S. and abroad, with concerned faculty from nonscientific disciplines, and with many alarmed citizen's groups. The Director's Advisory Committee, comprising outside academics, members from industry and the public, plus ad hoc participants including ethicists, lawyers, and university and academy officers, met to consider the panoply of issues and offer counsel on guidelines. This procedure set a model that would be repeated numerous times in succeeding years. Yet, in the end there was rarely a consensus on risk or procedure, rarely agreement between scientists and nonscientists. In this fractious setting, Fredrickson made decisions he then had to sell or defend to the secretary of HEW and the Congress. In June 1976, with the approval of the secretary, NIH released guidelines for recombinant DNA research designed to ensure that all NIH-supported recombinant research conformed to stringent safety rules and to prohibit deliberate release of organisms containing recombinant DNA into the environment. NIH also established a Recombinant DNA Advisory Committee, comprising scientists, lawyers, ethicists, and public members, whose review and approval were required before any grant application involving recombinant DNA research could be awarded. Applications were also required to have the approval of an institutional biosafety committee before submission to the NIH. Industry voluntarily pledged to observe NIH guidelines in its recombinant research. These processes succeeded in restoring public confidence in the award and oversight processes of NIH concerning recombinant DNA research.

Similar processes and review procedures were set up in other countries in which recombinant research was conducted.

Over succeeding years the guidelines have been revised toward less stringency as scientific progress and a spotless safety record warranted. The end result was that, despite the introduction of more than a dozen bills in the Congress designed to regulate recombinant DNA technology, no such law was ever passed. Fredrickson later commented, “No law, inspection force, or other external regulation can protect the public interest like responsible and responsive self governance.”

A quarter century later, many critics now believe that scientists over-reacted to imagined risks of recombinant research and that, in response, so did the public. But Fredrickson had to work with the hand he was dealt, and to his great credit he reached his decisions without negating either the freedom of scientists or the democratic process. Furthermore, the procedures he set in place to examine potential risks in recombinant DNA research have stood the test of time and are today being emulated in addressing risks of bioterrorism.

While many observers regard Fredrickson’s skillful handling of the recombinant DNA research safety controversy as his greatest contribution as director, at least one student of NIH (Bradie Metheny) considers Fredrickson’s successful defense of the general authorization of the NIH, contained in the Public Health Service Act of 1944, against attempts by Congressman Waxman to abolish that provision in favor of mandatory annual congressional authorizations, as a triumph of even greater consequence for NIH and biomedical research. However, this enervating and divisive battle, which eventually also involved the secretary and the White House, may have hastened Fredrickson’s resignation in 1981. These two controversies illustrate one of the greatest contributions an NIH director can make, namely, dissuading Congress from passing unwise legislation.

After leaving the directorship of NIH, Fredrickson became scholar-in-residence at the National Academy of Sciences. Two years later he was appointed vice president of the Howard Hughes Medical Institute (HHMI) under his early mentor George Thorn, who had been the institute's president since its formation in the 1950s. When Thorn retired from the presidency one year later, Fredrickson was elected to succeed him. He was immediately immersed in the sale of Hughes Aircraft, which the institute owned under the terms of the Hughes will. The huge proceeds of the sale of the aircraft company to General Motors provided a $5 billion endowment for the institute, and enabled the institute to relocate from Coral Gables, Florida, to Chevy Chase, Maryland (to a beautiful site suggested by Mrs. Fredrickson) and to expand its fields of research from three to five programs. Neurosciences and structural biology were added to the existing topics of genetics, immunology, and cell biology. Under Fredrickson's guidance the emphasis of research was shifted from clinical to basic investigation, and the length of support for an individual scientist was extended from several years to a potentially lifelong duration. The number of Hughes investigators was progressively expanded (and eventually grew to over three hundred, located at 70 different sites in the U.S.). A portion of Hughes resources was assigned to grants to small colleges for science teaching and research. Finally, Fredrickson negotiated a joint program with NIH, in which Hughes selected and supported 50 medical students from around the country for a year or more of full-time research in an intramural laboratory at the NIH. These sweeping changes in the character and portfolio of HHMI, which represented wise and bold uses of its new bounties, transformed the institute into a major force in world science.

Hughes awards became among the most generous and most prestigious in the life sciences. Hughes investigators have since won many distinguished prizes, including seven Nobel Prizes.

In 1987 Fredrickson resigned the presidency of the Hughes Institute, under pressure from the trustees because of financial irregularities that occurred on his watch. These events were front page news in the *Washington Post* and widely reported in national scientific journals. Yet, throughout this painful ordeal, Fredrickson remained personally composed with an almost existential detachment from the swirl around him. He returned to NIH as a Scholar at the National Library of Medicine, where he began working on two books, one on the history of the Clinical Center at NIH (which was not completed at the time of his death) and another on the recombinant DNA controversy of the mid-1970s (which was published in 2001). He also resumed clinical and research work in his old unit in the National Heart, Lung and Blood Institute. Among his patients in the Lipid Clinic were some of his original study subjects with Tangier Disease. In the late 1990s Fredrickson coauthored three research publications in *The Proceedings of the National Academy of Sciences* in which the molecular basis of the genetic defect of Tangier Disease was at last defined, almost 40 years after he described the entity. This was not only deeply satisfying to Fredrickson for personal reasons, it was also a cogent illustration of a point often made in his addresses, that the best science takes time to evolve and often awaits developments in collateral fields.

One of Fredrickson's patients during his earlier days of research on lipid diseases at NIH was the crown prince of Morocco, later King Hassan II. The two men developed a friendship that lasted throughout their lifetimes. Fredrickson served as the king's personal physician for more than 25 years, often arranging for American specialists to travel to Morocco to attend the Monarch or his family. In addition, Fredrickson gave an annual state-of-the-art address to the Moroccan Academy on research advances in the life sciences and medicine. He and Mrs. Fredrickson were also invited to the palace each year to attend the king's birthday party.

Fredrickson was an eloquent speaker and gifted writer. His sentences seemed to flow with an effortless grace, and both in casual conversation and lectern deliveries they were adorned with wit and often with sparkling allusions to classic literature and great writers. Among his favorite authors were W. H. Auden and Oscar Wilde, from whom I suspect he developed a keen appreciation of the importance of style in speech and writing. He was much sought after for keynote and commencement addresses and delivered at least 40 honorary lectureships. He received numerous professional recognitions, including 10 honorary degrees. A measure of his scientific standing is that in Garfield's "citation classics" he is recorded as the most cited physiologist in the world between 1961 and 1975. His collected writings have been deposited in the National Library of Medicine at the NIH. These include three volumes of speeches, articles and selected papers; twelve volumes of diaries relating to his years as physician to King Hassan II; other diaries kept for nine years as a director of Colange, Ltd., a private family-owned European company; twelve "Green Diaries" from the NIH directorship period; and travel summaries from 1960 onward. His was an extraordinary life of remarkable personal achievement and distinguished public service, a life lived with elan and lofty purpose. Don Fredrickson was a valued friend and colleague for 57 years. He is survived by his wife, Priscilla, and two sons, Eric and Ruric.

IN THE PREPARATION of this memoir, I was greatly aided by "In Memoriam, Donald S. Fredrickson, M.D., 1924-2002," by Antonio Gotto, Jr. (Arterioscler. Thromb. Vasc. Biol. 2002; 22:1506–08); "Genetic Engineering and Related Technologies: Scientific Progress and Public Policy," by Joseph G. Perpich (in *Biotechnology in Society, Private Initiatives and Public Oversight,* ed. Joseph G. Perpich, [New York: Pergamon Press, 1986], pp. 87–109); and *The Recombinant DNA Controversy: A Memoir. Science, Politics, and the Public Interest, 1974-1981,* by Donald S. Fredrickson (Washington, D.C.: ASM Press, 2001).

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Courtesy of the New York State Museum

JAMES HALL JR.

September 12, 1811–August 7, 1898

BY ROBERT H. DOTT JR.

JAMES HALL OF NEW YORK was North America's preeminent paleontologist and geologist of the nineteenth century. That he was a giant among early American scientists is evidenced by the facts that he was a founder of and served as president of the American Association for the Advancement of Science (1856), was a charter member of the National Academy of Sciences (1863), and was chosen to be the first president of the Geological Society of America (1889). Hall was also the best-known American geologist on the international scene in his time. As early as 1837 he was elected to membership in the Imperial Mineralogical Society of St. Petersburg. Later he was the organizing president of the International Geological Congress meetings at Buffalo, New York (1876) and at Paris (1878); he was a vice-president of the congresses at Bologna (1881) and Berlin (1885) and was honorary president of the congress at St. Petersburg (1897). Hall was elected a foreign correspondent to the Academy of Sciences of France in 1884, being its first English-speaking member. It was primarily the 13-volume *Natural*

Adapted with permission from *Encyclopedia of Geology,* pp. 194-200. New York: Elsevier, 2005.

History of New York: Palaeontology, published between 1847 and 1894, that initially brought Hall his fame; however, the broader community of geologists now remembers him more for the curious theory of mountains presented in his presidential address to the American Association for the Advancement of Science in 1857.

EARLY LIFE AND EDUCATION

Hall was born near Boston in Hingham, Massachusetts, on September 12, 1811. His parents, James Hall (Sr.) and Sousanna Dourdain Hall, had emigrated from England two years earlier, and James was their first of four children. The father became superintendent of a woolen mill at Hingham. The family was of modest means, but the young Hall was fortunate to have a gifted teacher in his public school who stimulated an interest in nature. Through his teacher, James encountered several leading members of the Boston Society of Natural History. Having developed a strong interest in science, Hall was attracted to a new college in Troy, New York, that emphasized science and employed revolutionary new approaches to learning with an active role for the student coupled with hands-on laboratory and field trip instruction. This Rensselaer Plan was developed by Amos Eaton with financial backing from his patron, Stephen van Rensselaer. Unable to afford commercial transportation, Hall walked the 200 miles to Troy. At Rensselaer he was instructed by Eaton and Ebenezer Emmons and had for classmates such geologists-to-be as Douglas Houghton, Abram Sager, Eben Horsford, and Ezra Carr. Hall graduated with honors in 1832 and undertook a tour on foot to the Helderberg Mountains in southeastern New York to collect Silurian and Devonian fossils. A job as librarian allowed him to continue at Rensselaer for another year and to earn the master of arts degree with honors (1833). He then held an assistant-ship in chemistry for several more years. In 1838 he married Sarah Aikin, the daughter of a Troy lawyer; they had two daughters and two sons. Sarah died in 1895.

THE NEW YORK SURVEY

In 1836 the New York legislature authorized a four-year geological and natural history survey; an extension of two years was later authorized. Four men—William W. Mather, Ebenezor Emmons, Timothy A. Conrad, and Lardner Vanuxem—were in charge of four respective districts, and Lewis C. Beck was mineralogist for the geological survey. Botanist John Torrey and zoologist James DeKay conducted the biological survey. James Hall was engaged to assist his former teacher, Emmons, in the Second District in northeastern New York, where Hall's first assignment was to study iron deposits in the Adirondack Mountains. A year later the districts were revised; Conrad was appointed state paleontologist, and young Hall had demonstrated such competence as to be put in charge of a new Fourth District in western New York with assistants Horsford, Carr, and George W. Boyd, all Rensselaer products. When the survey terminated in 1841, only Hall and Emmons remained in New York. Hall became state paleontologist and Emmons State agriculturalist.

Lardner Vanuxem, who had studied in France, had been instrumental in introducing to America the value of fossils for subdividing strata and correlating from place to place those of similar age based upon similar fossils. Meanwhile, Timothy Conrad had gained a reputation for studies of Cenozoic fossils of the coastal plain. Thus the survey had strength in paleontology from the start, and its staff soon developed a New York stratigraphy, the formal subdivision of successive strata, which set a precedent of naming stratigraphic divisions for geographic localities that is standard today.

Young Hall's career blossomed quickly after a monograph on the fossils and stratigraphy of the Fourth District was published in 1843. This and the other survey reports soon aroused much interest in Europe, for Paleozoic fossils and stratigraphic subdivisions were being defined there in mid-century. The name "Paleozoic Era," coined in 1838, means "ancient life" and is now known to span from approximately 540 million to 250 million years; it is subdivided into several systems, such as Cambrian and Silurian. Roderick Murchison's *Silurian System* appeared in 1839, John Phillips's *Paleozoic Series* appeared in 1840, and Joachim Barrande's monographs on lower Paleozoic fossils in Bohemia would begin appearing in 1852. The authors of these great treatises and other foreign authors began corresponding with Hall, and soon European geologists began beating a path to Albany, most notably the famous British geologist Charles Lyell during his several American visits in the 1840s. During a visit in 1846, Eduard de Verneuil, a close associate of Murchison, tried to convince Hall not to introduce the name Cambrian to the New World, rather to use only Silurian for the lowest Paleozoic strata, a reflection of a famous Murchison-Sedgwick feud then raging in Britain about which name should prevail for the oldest Paleozoic subdivision. Hall, however, was not swayed, for he was a leading exponent of the widely held nationalistic view that an American stratigraphic classification was best for America.

As geological investigations in America began to mature, stratigraphic nomenclature was becoming important, especially for comparisons among the different states. Hall and others proposed an organization to deal with such nomenclature and other mutual problems, so in 1838 in Albany the American Association of Geologists was created; the first formal meeting was held in Philadelphia in 1840. From this organization evolved in 1857 the American Association for the Advancement of Science, modeled after the British Association. Still later the Geological Society of America was spawned in 1888 from a division of the AAAS. Hall was promptly elected president.

THE ALBANY TRAINING GROUND

In 1857 Hall constructed a substantial brick laboratory building where he worked for the rest of his life. This Albany laboratory became a veritable training school for a host of young, budding geologists who would distinguish themselves in the history of American science. Although universities were beginning to offer formal instruction in geology during the mid-nineteenth century, there was practically no instruction in paleontology. So apprenticeship had to be the principal entrée into that field, and James Hall's laboratory was the place to apprentice. Among the many who profited from some association with Hall were the following:

Charles E. Beecher
Ezra S. Carr
John M. Clarke
Nelson H. Darton
Grove K. Gilbert
Ferdinand V. Hayden
Eban N. Horsford
Joseph Leidy
W. J. McGee
Fielding B. Meek
Charles S. Prosser
Carl Rominger
Charles Schuchert
Charles D. Walcott
Charles A. White
Robert P. Whitfield
Josiah D. Whitney
Charles Whittlesey
Amos H. Worthen

Hall's assistants learned more from him than just paleontology, however, for they also experienced a strong, egotistical, and irascible personality. Although his sharpest attacks were reserved for his enemies in the New York legislature, most assistants were also treated to his infamous outbursts. Besides throwing vituperative verbal daggers, he sometimes brandished menacingly either a stout cane or even a shotgun kept at the ready near his desk. Perhaps the most extreme self-righteous attack was upon James T. Foster, a school teacher in Greenbush, New York. Foster had the audacity to publish a popularized geological chart in 1849, which outraged Hall. He was so distressed that he stole aboard a New York City-bound boat and threw the entire printing of the offensive chart into the Hudson River. He had quite a time fighting the subsequent libel suit, which entangled him for several years as well as Louis Agassiz, James D. Dana, and several other notables from whom Hall solicited help in his cause.

Another celebrated example of Hall's erratic temper involved none other than British geologist Charles Lyell during his first visit to America in 1841-1842. At first, Hall and others were greatly flattered by the attentions of their famous visitor, but Lyell's insatiable grilling, which had earned him the nickname "Pump," and his copying of their geologic maps gradually provoked a reaction of resentment and fear of being preempted. In March 1842 an anonymous letter signed "Hamlet" appeared in a Boston newspaper, which charged Lyell with geological piracy. It was written by Hall after some of his compatriots criticized him for being too generous in sharing information with Lyell, especially by giving him a copy of his *Geologic Map of the Western and Middle United States,* which had not yet been published. Needless to say, this letter cast a chill upon the Association of American Geologists' meeting a month later, but the English gentleman participated as if nothing had happened. Although the charge was largely true, Hall was afterward mortified by his rash act. For once, however, he managed to mend the damage done by his intemperate action and to remain henceforth on good terms with Lyell.

Almost as legendary as his paranoiac outbursts was Hall's acquisitiveness for fossils. He stooped to every conceivable means to acquire outstanding collections. An effective technique was to flatter and invite collectors to work with him in Albany and to bring their collections. Commonly, when the apprentice moved on, however, his collection did not. Hall was a workaholic who drove himself as mercilessly as he did his assistants. He could rarely say “no” to even the most ridiculous schemes, and he ignored the entreaties of close friends—such as Joseph Henry, physicist and first secretary of the Smithsonian Institution—that he should ease his pace for the sake of his own health.

BEYOND NEW YORK

As he completed his Fourth District studies, Hall decided to see how far the New York stratigraphic classification might apply beyond his state. In 1841 he made the first of several odysseys west. With geologist David Dale Owen he made a boat trip down the Ohio River to Owen’s base at New Harmony, Indiana, and from there, he proceeded across Illinois to Missouri, Iowa, and Wisconsin. Hall was amply rewarded with evidence for extending the New York stratigraphy in a broad way across that entire region. There were some significant differences, however, which he, and perhaps only he, could recognize. For example, he found that the Paleozoic strata were much thinner to the west of New York and Pennsylvania and that there were important contrasts of the types of sedimentary rocks with more clastic, or fragmental, sediments, such as sandstones and shales, in the east and more carbonate strata (limestones and dolomites) to the west. In effect Hall had discovered the contrast between what would much later be termed the stable craton and the Appalachian orogenic or mountain belt. This trip also provided information to allow him to complete the *Geologic Map of the Middle and Western States*, which was incorporated in Hall's Fourth District report of 1843. (This was the map that Lyell had used to help prepare his own geologic map of the then United States, which was published in 1845 in *Travels in North America.*)

Hall's finances were always tenuous. He was remarkably gullible for risky ventures, and he also had his salary cut or even suspended by a frequently hostile state legislature. At least once he had to sell some of his fossil collections in order to raise money. As his reputation grew, however, opportunities for temporary outside employment helped to tide him over his New York financial droughts. These ventures also allowed him to expand his knowledge widely. One of the first such ventures took him to the Lake Superior region in 1845 to examine copper deposits for a private company. In 1847 the federal government authorized a geological survey by John W. Foster and Josiah D. Whitney to evaluate the mineral resources of northern Michigan and Wisconsin. The results were published in 1851. In 1850 Hall was engaged to provide his expertise on Paleozoic stratigraphy and paleontology for that survey. He made two brief trips to the region (1850 and 1851) from which he gained further insights into the stratigraphy of the Great Lakes region and added to his ever-growing fossil collections. Perhaps the most important result of his work for this survey, however, was the recognition of fossil reefs in the Silurian strata of southeastern Wisconsin. This was the first recognition of ancient reefs in North America, and perhaps in the world.

When asked to study fossils from western regions, which others had collected during various expeditions, he willingly obliged. He recognized the first known Mesozoic fossils collected by John C. Fremont in the 1840s. In 1853 he agreed to let his assistants Fielding B. Meek and Ferdinand V. Hayden go to the White River badlands of Nebraska Territory (now in South Dakota) to collect newly discovered Cenozoic nonmarine invertebrate and mammalian fossils. Meek, whose artistic as well as collecting skills were vital to Hall's enterprise, was glad to escape from his mentor for a few months. Eventually he extricated himself from Hall's empire by joining the new United States Geological Survey. Meek never forgave his perceived exploitation by Hall.

When Iowa decided to have a geological survey in 1855 and needed a director, the governor looked to New York, which had eclipsed all other states as well as the federal government in the caliber of its geological survey. Hall accepted the position with alacrity because his New York salary had been suspended in 1850 by an exceptionally hostile legislature. Moreover, he welcomed the opportunity to obtain and study fossils from the new state. He soon suggested Amos Dean of Albany to be the first chancellor of the University of Iowa, and he himself was identified as the first professor of geology, but apparently he never lectured there. In fact, Hall mostly directed the survey from Albany and spent little time in Iowa. Four assistants did most of the actual work. Josiah D. Whitney concentrated upon mineral resources, while Amos H. Worthen of Illinois dealt with paleontology assisted also by F. B. Meek and R. P. Whitfield. Hall knew that Worthen had the finest collection in the country of fossil crinoids (a class of echinoderms, most of which are extinct), so a condition of employment was that Hall be allowed to describe them, which he did in the Iowa survey report. Hall came to Iowa for the winter meetings of the legislature to lobby on behalf of the survey, but payment of salaries was so erratic that he had to borrow money in Albany to keep the effort going. Finally in 1859 the survey was suspended, but two volumes had appeared in 1858.

In 1857 Illinois undertook a geological survey, and Worthen was one of three applicants to direct it. Hall wrote a glowing endorsement of him, but he also supported the other two applicants. This lapse of judgment earned the hatred of all three applicants, and in the end he was denied access to the fossils collected by the survey, which was a great disappointment.

In 1856, while still engaged in work in New York, Iowa, and also paleontological consulting for the Canadian Geological Survey, Hall accepted an affiliation with Wisconsin. He joined a former Rensselaer colleague, Ezra Carr, then a chemistry professor at the University of Wisconsin, and Edward Daniels for this new effort. Hall devoted little time to the Wisconsin initiative, so Carr and Daniels were really in charge. Whitney was engaged to study the lead deposits of southwestern Wisconsin and Charles Whittlesey to study the mineral deposits of northern Wisconsin. A large volume was published in 1862, but a hostile Wisconsin legislature abruptly terminated the endeavor, because it judged the results to be insufficient. It cared only about potentially economic results, so a frustrated Hall and his assistant, Robert P. Whitfield, published Wisconsin's paleontology within a New York report in 1867 and again separately in 1871. This ingenious solution to a publication problem was typical of Hall. Much earlier he devised a scheme to circumvent a New York legislative edict to limit the number of expensive paleontological monographs simply by issuing several volumes as subdivisions of a single part of the series, resulting ultimately in 13 separate monographs—at least twice the intended limit—but numbered as only eight parts of the *Paleontology of New York.*

Hall became involved in several other state surveys to varying degrees, ranging from advising about personnel to being a consultant for paleontology or the titular head of a survey. Included were surveys of Missouri (1853 and 1871), California (1853-1856), the transcontinental railroad survey

(1853-1857), New Jersey (1854-1857), Ohio (1854-1857), Texas (1858), Mississippi (1858), Michigan (1869-1870), and Pennsylvania (1870-1875). While this list is a testimony of his prominence, Hall's contributions to these many surveys were minor except for the identification of fossils.

In 1889, at the age of 77 and while the first president of the new Geological Society of America, Hall made his last trip to the Midwest. His purpose was to obtain brachiopods by any and all means necessary for his latest project, namely, to revise the description and classification of that great group of Paleozoic fossils. Besides success in obtaining many specimens, he also met and lured to Albany a young Charles Schuchert of Cincinnati, who was destined to become his most famous protégé and ultimately a professor at Yale. The ambitious brachiopod study culminated in the last volume, Part 8, of the *Paleontology of New York*, which appeared in 1894.

During the completion of his final large paleontological monograph, Hall had his last and sweetest wrangle with New York bureaucracy. The executive secretary of the regents, which oversaw his program, had become overly zealous in trying to impose strict accounting and efficiency procedures. Such a fuss developed that the legislature had to intervene. To resolve the fracas it appointed crotchety old Hall as state paleontologist and state geologist for life with complete managerial freedom. Doubtless the legislators realized that Hall's days were numbered, and in fact he died three years later. Hall must have recalled with great satisfaction an earlier observation when a particularly vicious political enemy died suddenly that "Providence was usually on my side."

THE ORIGIN OF MOUNTAINS

Hall is most widely known for his theory of mountains, which embodied the concept of the geosyncline, a term coined not by Hall but by James D. Dana of Yale in 1873. In his 1857 presidential address to the American Association for the Advancement of Science, Hall startled his audience with a discourse on the origin of mountains rather than speaking about paleontology and stratigraphy. In stating that "the greater the accumulation, the higher will be the mountain range," he pronounced that a great thickness of strata was a prerequisite to mountain ranges composed of folded strata. Hall rejected the then-popular theories of mountains of Frenchman Elie de Beaumont and the American brothers William B. and Henry D. Rogers, who postulated catastrophic wrinkling of the crust by wavelike movements in a fluid subcrustal zone. Instead, Hall was influenced by a suggestion by J. F. W. Herschel in 1836, which anticipated the modern theory of isostasy. Herschel argued that vertical movements of the crust are caused by changes of pressure and heat at depth, which in turn respond to erosion and deposition at the Earth's surface. The vertical adjustments of gravitational equilibrium were supposed to be accommodated by a pliable subcrust. The key element for Hall was the accumulation of thick sedimentary layers, which he imagined must depress the crust and in the process become wrinkled to form the structures seen in mountain ranges, such as the familiar Appalachians. He envisioned compression of the upper layers and tension of the lower ones as subsidence occurred much as one can imagine by bending a ream of paper.

In 1859 Hall published the following in the most commonly quoted source for his theory, Part 6 of the *Paleontology of New York*: "The line of greatest depression would be along the line of greatest accumulation [that is] the course of the original transporting current. By this process of subsidence . . . the diminished width of surface above caused by this curving below, will produce wrinkles and folding of the [upper] strata. That there may be rents or fractures of the strata beneath is very probable, and into these may rush the fluid or semi-fluid matter from below, producing trapdykes, but the folding of strata seems to be a very natural and inevitable consequence of the process of subsidence" (vol. 3, pp. 70, 73).

A year earlier in the report of the Iowa Survey (1858), Hall had also emphasized the contrasts of thickness between the Appalachian region and the Midwest with detailed remarks about contrasting sedimentary rock types as well as thicknesses in various portions of the Paleozoic succession of the two regions. Here, too, he included a brief summary of his theory of mountains by stating that "the thickness of the entire series of sedimentary rocks, no matter how much disturbed or denuded, is not here great enough to produce mountain features" (vol. 1, p. 42). Clearly, he saw the excessive thickness of strata as a prerequisite for mountains.

Hall's theory attempted to explain the crumpling of strata so characteristic of mountain ranges, but it was very vague about the cause of the uplift of mountains. He simply ascribed this to continental-scale elevation of indeterminate cause, which he thought had no direct relation to the folding of strata within the mountains. Contemporaries were quick to challenge him on this point, with Dana noting that Hall had presented a nice theory of mountains with the mountains left out. Hall lamely denied that he ever intended to offer a complete theory of mountain building. His failure to publish the presidential address until 1883 may have been because of such criticisms, but, on the other hand, his first priority was always paleontology, and he knew that the essence of his theory was to appear in both the Iowa and the New York reports (as well as in an abstract in Canada) soon after his oral address.

James Hall's contribution to mountain building theory was marginal at best and was soon eclipsed by the more profound and comprehensive contraction theory of James D. Dana, which relegated thick strata to a result of mountain building processes rather than the cause. Nonetheless, Hall's emphasis upon some sort of cause-and-effect relationship between orogenic or mountain belts and very thick strata had a significant influence upon three generations of geologists, especially but not only in America. By coining the term "geosynclinal," which was later converted to the noun "geosyncline," Dana formalized Hall's demonstration that Paleozoic strata are 10 times thicker in the Appalachian mountains than in the more stable lowlands to the west (the craton).

CONCLUSIONS

Even though Hall was wrong about the cause of mountain building, he nevertheless was the first person to underscore clearly the profound stratigraphic contrasts between orogenic belts and what are now termed stable cratons. He drew attention at an early stage to large-scale stratigraphic patterns among some of the larger tectonic elements of the Earth's crust and revealed other shrewd stratigraphic insights, which were ahead of the times. By virtue of his breadth of experience in both the cratonic and orogenic regions of eastern North America, he was uniquely equipped to see such fundamental distinctions. He also made important pioneering observations about several physical sedimentary structures such as ripple marks and suggested their value for interpreting ancient sedimentary environments.

Hall was extremely productive, having some 42 books and nearly 200 articles to his name. His major monographic paleontological syntheses appeared in the 13 volumes of the *Paleontology of New York*, but he also published many shorter papers describing a single genus or group of fossils.

In addition, he contributed paleontological sections to several federal and state publications on general geology. The publications on his theory of mountains totaled only three important ones, two of which were buried as parts of larger studies.

Between his prodigious contributions to paleontology and stratigraphy as well as his theory of mountains, James Hall was justly assured of a prominent niche in the history of his science. Geology was the preeminent American science of the late nineteenth century as judged by none other than British physicist John Tyndall during a visit to the United States in the 1870s. Therefore, Hall's leadership role in the professionalization of science and his charter membership in the National Academy of Sciences assure an important niche in the history of American science in general.

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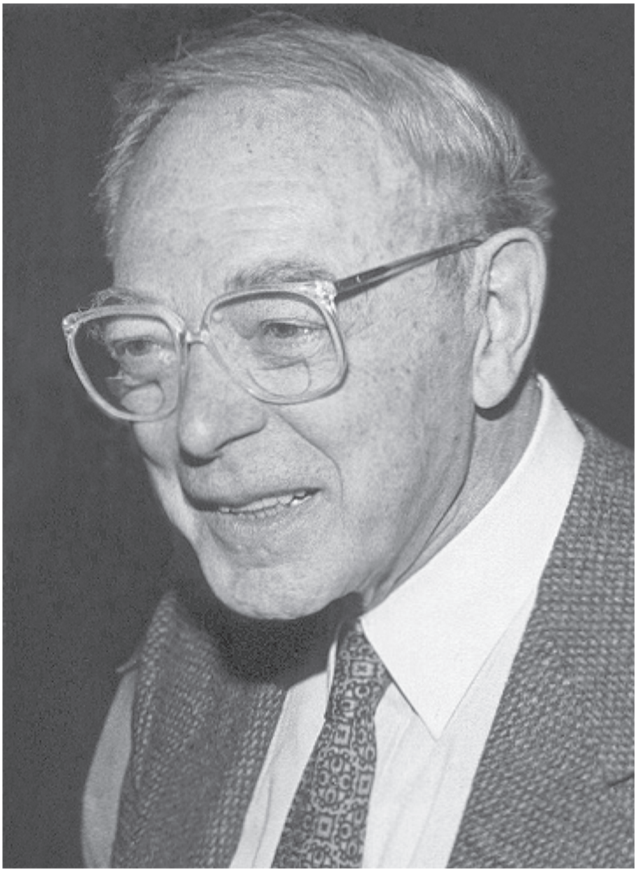

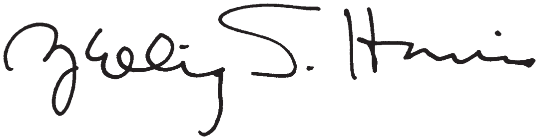

ZELLIG SABBATAI HARRIS

October 27, 1909–May 22, 1992

BY W. C. WATT

Zellig S. Harris died on May 22, 1992, midway through his eighty-second year. (The delay in memorializing him in these pages is owing to happenstance.) He was one of the half-dozen linguists, since the beginning of the serious study of language a little after 1800, whom anyone conversant with the field would label a genius. He was the first (in 1947) to adumbrate the notion that linguistics could accept the responsibility of synthesizing or "generating" the sentences of a given language (say, English), as in an algorithm or computer program, from some explicit set of rules—and in so doing he exercised a deep and abiding influence on his best-known student, Noam Chomsky; on his many other students; and on all future researchers who yearn to understand language, surely our most distinctively human attribute. Indeed, it is impossible to imagine present-day linguistics, in either its aims or accomplishments, without taking his pioneering work into account, even though the field, as is ideally true of any science, in which progress is attained by later generations' standing on the shoulders of earlier giants (as often as not after first stepping on their toes), has in part moved beyond his particular vision.

Harris spent his entire scholarly life, until his retirement in 1979, at the University of Pennsylvania. He earned all of his degrees at that institution (doctorate in 1934), and after joining its faculty became in 1946 the founding chair of what became two years later its Linguistics Department, reputedly the first in the world to be so named. He taught many, myself included, to probe deep and to respect the data; he was a merry but exacting taskmaster; he was venerated by all who knew him, surely, and by many was held in warm affection. He was quick; he was wise; he held scholarship to be a calling worthy of one's best efforts and one (as will be seen in spades below) from which the personalities of its practitioners are best held apart. Oddly, perhaps, given his expressed wish to suppress personality in science, his own individual character was strongly expressed and strongly felt. Around such a person, inevitably, legends abound. One of them concerns his reclusiveness. Few of his students had ready access to him, and I was once importuned by one of them, after he'd spent a full year at Penn, at least to point Harris out (I was able to direct his attention to the receding taillights of his aging gray Mercedes as it vanished up Walnut Street); and in my day (1959-1963) he had appointed the formidable Miss Sparagna to serve, outside his office, as a sort of Cerberus. This she did with great relish. In fact, as time went on her blinds were often drawn and the lights turned off, lending further weight, there in the gloaming, to Harris's inapproachability.

Another legend concerns his lecturing habits: Some minutes before the time allotted to close a lecture he would sometimes pause, say "And that's all," and leave the room; and on occasion—still another legend—he would, on the first fall meeting of one of his courses, ask which of us were also registered for his other two and then, having discovered that we all were, announce that they would therefore be featly combined into one. Which was fine, since all of his courses, however titled, covered a vast domain. (In my Penn graduate catalog for 1967, the year he awarded me my degree, Harris is listed as being sworn to teach “Formal Linguistics” and “Mathematical Systems in Linguistic Structure” in the fall semester and “Seminar in Linguistic Transformations” in the spring. His courses tended to merge one into the other; and the first and third of those just listed are specified in the catalog as “may be repeated for credit.” Which they were, and justly, since their contents overlapped and varied with Harris’s latest advances.)

Harris was born in North Ossetia, now a constituent republic of the Russian Federation, but was taken by his family to Philadelphia when he was but four years old. (His middle name, “Sabbatai,” set beside his brother’s first name, “Tzvee,” appears to identify the family as followers of Sabbatai Tzvee or Tsvee (1626-1676), the “False Messiah of the Caucasus.”) To my ear he had virtually no foreign accent, sounding just like any native Philadelphian (meaning that he spoke one of the half-dozen or so equally distinctive Philadelphia dialects), except that his “filled pause,” as linguists term it, rather than the usual “uh,” was something like “eh” (linguistically, a simple long /ɛ:/ with a bit of nasalization and a hint of an “h” at the end).

Unlike Chomsky he was no sailor, his physical activity being mostly confined to his working on a kibbutz in Israel many summers (his wife, Bruria Kaufman, was a professor at the Weizmann Institute there), in which purviews he was apparently known simply as “Carpenter Harris.” Prompting one to picture this great scholar, elegantly balding, slightly stooped and with thickish rimmed spectacles, astride a beam into which he was driving, with a framing hammer, a 10-penny nail. He was, as I understand it, a secular and indeed Socialist Zionist, committed to the independence of Israel (as who is not?) but less than pleased with that nation’s swerve toward theocracy.

Harris's scholarly career seems to fall into several successive phases (though he might well have denied this, since they overlapped). During his early years (in the 1930s) he devoted himself to Semitics, having been a very early analyst of the then-new Ugaritic materials; at this point he was looked upon as a quite promising Semitist. Sometime around World War II he applied himself to more general problems in linguistics, the culmination of which was the completion in 1947, with long-delayed publication in 1951, of his magisterial *Methods in Structural Linguistics* (later reprinted in paperback as just *Structural Linguistics*), which became the standard text for the next decade and more, and which cognoscenti still regard as a classic.

A little later he devoted himself to two other areas of research: computational linguistics, which was just becoming possible owing to the ready availability of computers (which had after all been invented at Penn, as ENIAC); and, above all, transformational analysis, which he had begun working on earlier in his career, an approach in which simpler sentences ("The archer shot the arrow") can be "transformed" by general rules into more complex ones ("The arrow was shot by the archer"), and vice versa.

His activity in the first of these two areas (he spearheaded development of the first truly functional computational syntactic analyzer [on a UNIVAC]) presumably arose quite naturally from his lifelong interest in analytic techniques. His work in the second eventuated from his interest in analyzing texts into simpler ones bearing the same information (a concern that never left him). And then toward the end of his life he developed a method of linguistic analysis that viewed sentences as being generated from a formally simple application of functors to their arguments (he used somewhat different terms). Such an analysis views all syntactic relationships in the same light, therefore subsuming the relationships traditionally termed “modification” (as when an adjective modifies a noun) and “predication” (in which a verb is predicated of its subject) or more broadly, “agreement” (as when “this” is pluralized to agree with “books” in “these books”) and “government” (as when “me” rather than “I” is mandated when the object of “kissed” in “Jenny kissed ___”: no “Jenny kissed I”). Then the adjective “tall,” for instance, as in the sentence “Tall men excel,” can be described as the sort of functor that turns a noun into another noun—as an FNN—while “excel” can be described as the sort of functor that turns a noun (“tall men”) into a sentence, that is, as an FNS. In “very tall men,” “very” would then be the sort of functor that turns an adjective—already an FNN—into another adjective (an “adjectival phrase”), and so it can be tagged as an FFNN(FNN). (The parentheses aren’t formally necessary, but they add perspicuity.)

In this fashion then, from just the two primitives “N” and “S,” a fully developed functor/argument analysis can aim at providing an intriguing reformulation of all the traditional parts of speech and of the phrases, clauses, and sentences they occur in. Such a formal analysis is rather rigid, and perhaps overly limited, but it does suggest a different way of viewing sentences. Suppose, for example, that one should take up the examples of “He ran up a bill” and “He ran up a hill.” These are of course very different: One can ask (transformationally) “Up what hill did he run?” but hardly “Up what bill did he run?” Then “up” in the first sentence can be represented as the sort of functor that makes a verb (“ran”) into another verb (“ran up”)—as an FFNS(FNS)—and “up” in the second sentence as the sort of functor that makes a noun (“a hill”) into a sort of adverbial, hence perhaps as an FNF(FNS[FNS]). The resulting characterization may seem a little forced—and it is—but it does present an interesting new way of viewing syntax. In short, by presenting a rigid structure derived ultimately from combinatory logic, Harris yet again showed how one kind of analysis might illuminate others.

Harris is sometimes thought of as having tried to furnish one or more "discovery procedures" that would permit the intending analyst to apply a sort of litmus test to whatever language he or she was studying, without further insight, whereupon the correct account of that language would emerge as if by magic. Not true. He always admitted that any initial linguistic analysis would depend on what are nowadays called the linguist's "intuitions"; what he aimed to provide were checks on such analyses, what could be called "confirmation procedures." All of his analytic methods were forthrightly stated to be aids to analysis but not infallible ones. This judgment holds for some of his early methodological insights, such as the one that showed how to extract, from a sequence of sound segments, such as from the consonant cluster /st/, a "phonemic long component" that those segments have in common. In the case of /st/, to continue our example, a "long component" that /s/ and /t/ have in common is "voicelessness" (the larynx isn't vibrated when making either sound). Now suppose that "voicelessness" is represented by underlining any sequence thus characterized. Then we could write /st/ as /<u>st</u>/. This would be pointless, though because /st/ consists precisely of /s/ + /t/, both of which are defined as being voiceless. However, something has still been gained, since now we can write /st/ as its voiced counterpart, /zd/, if the latter is embellished by an underline—as /<u>zd</u>/—signifying that /st/ is /zd/'s voiceless counterpart.

Seems simple, but such a move will strike the linguist as a gain, since it will have thus explicitly acknowledged the close relationship between /st/ and /zd/—they differ mainly in that /st/ is a "voiceless" variant of /zd/—and more importantly it will have made way for a generalization to the effect that "/zd/ is always voiceless when word-initial." This is easily captured by a rule, identifying the initiation of a new word as "#", on the order of "#zd→#<u>zd</u>." Such an expression is especially nice because so general a rule will automatically forbid any word (in English) from beginning with /zd/ itself, since any such initial cluster will be converted to /st/, thus capturing the fact that "stin" is a possible English word but "zdin" is not. (We ignore imports like Italiian "sdrucciolo" [Italian initial "sd" is pronounced as /zd/], a rhetorical term referring to poetic lines stressed and rhyming on the antepenult.) In this manner we could encapsulate simple facts about the English language, and facts well worth the capturing if we are to understand any language at all. (In this instance, the fact that we accept initial "st" in English but reject the initial "zd" that after all, since Italian-speakers have no trouble with it, is easily pronounced by the human mouth.)

I note in passing that the posited paired relationship between /st/ and /zd/ is not discoverable "automatically," since /st/ could also be paired with some other voiced cluster equally unable to occur initially in English, /dl/ for example (no "dlin"). But /st/ and /dl/ are related only at a rather more abstract level than /st/ and /zd/ are, a fact revealed by the sort of phonological analysis that can scarcely be thought of as "automatic."

For Harris, at least in the 1950s, such advances were made in the interest of achieving the simplest possible analytic account of the language. Later and presently, under the assumption that speakers' brains attain to a maximum of static simplicity when representing the "head-grammar" that permits them to produce language, such advances are sometimes thought of as approaches to the cognitive grammar itself. For Harris this assumption, however inviting, could not yet be firmly grounded in the psychological (much less neural) sciences. So Harris's own prejudice remained always in the interests of analysis, despite the fact that a computational grammar, such as the version he pioneered in 1959, can be put to the proof only by generating sentences from it, and despite his having said as early as 1954 that a deep analytic grammar could be viewed as "a set of instructions which generates the sentence of a language."

Information of the sort conveyed by "phonemic long components" is nowadays couched rather differently, even though the information (if not its implications) remains much the same. For example, the "long components" just discussed would now be captured in the form of a rule for English declaring in essence that any sound of the set {s,z} must be voiceless when word-initial and preceding any sound of the set {t,d} (so the set {s,z} can only be realized as member /s/ in this position), plus a rule forcing any sound of the set {t,d} to be voiceless—hence, to be realized only as /t/—when between initial /s/ and anything else. Entailing, just as in Harris's formulation, that "stin" and "strin" are possible English words while "zdin" and "zdrin" aren't. (The characterization of the set {s,z} and of the set {t,d} would in later treatments be conveyed by stating each set's members in terms of roughly the "distinctive features" they have in common.) In all, Harris's notion of "phonemic long components" was an early and persuasive presentation of the idea that entities like /s/ and /z/ could (and should) be factored into "components"—now mostly called "distinctive features"—incorporable into general rules revealing of linguistic structure; and this notion, which dates from the completion of the MS of *Methods in Structural Linguistics* in 1947, was to prove quite influential elsewhere. It had a significant rebirth within anthropology, for instance, in the analysis of kin-terms due to Romney and D'Andrade. It remains to be determined whether the notion of "phonemic long component," in which for instance initial /st/ is recognized as a unitary consonant-cluster having "voicelessness" extending over the whole, is more or less "cognitively real" than the notion of segmental-phoneme rules like those that convert first initial {s,z} into /s/ before {t,d} and the {t,d} into /t/ after initial /s/. It might well have surprised Harris to learn that his analytic devices might turn out to be superior in cognitive reality, should they do so; but science, of course, consists in large part of surprises.

As noted just above, some "analytic" approaches to language are, at least in implication, also "synthetic," in that by comprehensively revealing the inner attributes of language one may show how to synthesize or "generate" sentences adhering to that analysis. Certainly Harris's "transformational analysis" is of this sort. Once active sentences containing transitive verbs have been shown to be systematically related to passive sentences bearing the same information, as "The boy broke the toy" is related to "The toy was broken by the boy" (e.g., by a simple formula [omitting tense and ignoring many problems and complexities] on the order of "N^1 V $N^2 \leftrightarrow N^2$ be Ven by N^1") then one has indicated how passive sentences might be generated from their active counterparts. Still, some of Harris's analytic techniques are much less easily thus characterized. For instance, he once proposed a technique for determining morpheme-boundaries by examining the phoneme-sequences composing them (a "morpheme" is roughly a minimum stretch of meaningful language: the word "meaningful" consists of the three morphemes "mean," "-ing," and "-ful"). Any such technique could be "generative" only in the sense that an auditor might use it, unconsciously perhaps, to aid in the "re-generation" of a sentence he has heard; surely no speakers use it to generate their own sentences, since of course for them their morpheme boundaries are determined by what they want to say.

As to the question sometimes asked by observers outside of linguistics why "information-preserving" alterations of sentences are by almost any analyst assigned a privileged position, the answer is that while indeed speakers can produce for any sentence of the form "The cat sat on the mat" another sentence of the form "The mat sat on the cat," with no preservation of the original's information at all, such an ability is universally consigned to the periphery, on the thesis that our defining use of language is to convey information, not to play with it. Naturally there's still room for disputation regarding what is the nature of the "information" that language seems designed to convey: Is it really only "truth-value," or is more involved (e.g., point of view, point of emphasis, point of interest, or news to the auditor)? Under this rubric does "It was the *cat* that sat on the mat"—which stresses that it was the cat rather than some other creature, perhaps in denying an auditor's contrary assertion—really only "*preserve* the information" of its possibly underlying transform "The cat sat on the mat"? In this case, surely not. But such questions are still, as in Harris's own time, being debated in the scholarly literature, albeit with ever-increasing sophistication.

Harris's continuing main concern with matters of linguistic analysis of productions gleaned from actual speakers and writers could be characterized in modern terms as a concern with "performance" over "competence" (i.e., with physical evidence of what speakers and writers do over hypotheses [or mere conjectures] of what they must have inside their crania to enable them to do it). (He may have maintained some mistrust of factors lying beyond the analyst's access as available to 1930s informant techniques: "Can you say this? How about *this*?") Are transformations revealing of speakers' understanding and generating of speech? Are functors on arguments revealing of different aspects of that speech? Is a treatment of the transitional probabilities obtaining among the phonemes of a language's sentences (roughly their distinctive individual sounds) revealing of basic elements such as boundaries, such as might be used by auditors to understand speech? Then let all these methods be brought into play, that each may disclose a different aspect of language in all its performative complexity with all, perhaps, in the aggregate, revealing language as a whole. This is a fascinating view of the relation between language and its analysts, and a challenging one; and it appears to have been a view that for a while, at least, was almost Harris's alone. No longer: For nowadays some linguists hold that at least at first we parse simple sentences in a Harrisian (surface-based) manner, and then if those sentences don't compute to sense once that has happened, we apply a deeper parse, in a Chomskian manner, say, to understand them.

Like all of my fellow students, I think, I revered Zellig Harris as mentor and as resident genius; like more than a few, I had a warm affection for him, in my own case as a sort of intellectual father. This affection was only increased by my personal interactions with him, not just in his office, once regularly admitted, but also, more casually, on the streets of Philadelphia. An accident of residence—his apartment and mine were only a block or so apart—led me often to find myself afoot behind him crossing the Walnut Street Viaduct to the Penn Campus, he not infrequently in his greenish outdoorsman's jacket, with wooden toggles in the stead of buttons and, armed against Philadelphia's blustery weather, a prominent hood. As he marched along across the Schuylkill he would sometimes reach into the side pockets of this capacious garment and fish out various pieces of paper on which, presumably, he had written notes to himself on this or that linguistic point. He'd examine these without breaking pace and then, about half the time, toss them into the river. They fluttered down like the inscribed leaves that, in legend at least, some Chinese poets, uncaring of posterity, used to toss into the nearest creek. Sometimes I wondered how many potential dissertations, by us his epigones, floated down those then-noisome waters, to be swept eventually into the Delaware and then out to the Atlantic.

Some of these notes, though, have survived, for I still have a few in my possession, since he at one point delegated me to be his inquirer into the tangled web of English adverbs and so passed on to me his jottings thereanent. They make interesting reading. First, they contain brilliant *aperçus*—if not yet analyses as such—and secondly they consist in large part of stray scraps of paper saved from the Schuylkill. They comprise as follows: (1) a note on formal University of Pennsylvania letterhead addressed to "Dear Watt"; (2) six notes on 8 $^1/_2$ × 11 brown-flecked blue-lined notepaper; (3) two notes on 5 $^1/_2$ × 7 notepaper; (4) one note on a different 5 × 8 notepaper; (5) twenty-nine notes on 3 × 5 sheets torn from some tablet; (6) twenty-three 4 × 6 sheets excised from some other tablet; and lastly (7) two notes on the reverse (flap side) of two University of Pennsylvania envelopes, one small and one letter-size. They constitute a set of casual records of a superbly talented linguist's cogitations on language—probably, given the ordinary evanescence of such things in the destructive course of time, among the best we'll ever have.

As to their contents, they were, as just noted, mere notes, except for the letter addressed to me. "How many differences," he asks me, "can you get between *time-point* adverbs (*yesterday, at 10 a.m.)* and *time-aspect* (*recently, frequently, generally*)?" And then on one of the lesser sheets he queries the possible difference between "He only slept an hour" and "He slept only an hour." On another, he wonders about "She is often tired" in relation to both "However often she is tired" and "However she is often tired." On still a third, about the possible (to me, dubious) relation between "Their names were linked romantically" and "Their arms were linked in a romantic way." On a fourth, about the best comparative analysis of similar adverbial locutions such as those contained in "The deer was killed with a blowgun," ". . . near the brook," ". . . by moonlight," and ". . . at nine." And so on. He was, in other words, searching through his interior sense of the English language, unrelentingly and unflinchingly, for thorny problems demanding respectful and hopefully explanatory solutions. A model, surely, for any intending language analyst. (And, by no coincidence, the exploratory model to which all contemporary linguists adhere, be their purview widened to take in many other languages or even such dialects as that found on one wharf of some obscure Sardinian village where a distinctive version of Catalan is spoken.)

We come now to a still more revealing incident in Harris's life, and for that matter my own, that has not hitherto been disclosed to the public eye. In 1969, having become aware that on October 12 of that year (Julian calendar) Harris would celebrate his sixtieth birthday, I conceived the notion that the occasion mustn't pass unremarked, and gained the assurance of Mouton & Company, in the Netherlands, that that concern would publish a Festschrift should I be able to garner the requisite number and quality of participants. Accordingly, I wrote some of the prominent linguists of the day, therefore including a good few of Harris's onetime students, asking if they'd be interested in contributing. The response was overwhelming, and the Festschrift, to be entitled with maximum simplicity "To Honor Zellig Harris at 60," was thereby set in motion. The 31 who agreed to submit tributary articles ranged widely over the fields to which Harris had made major contributions, but they were naturally concentrated in linguistics and its computational applications. Listed alphabetically, they comprise: Yehoshua Bar-Hillel, Dwight Bolinger, William Bright, I. D. J. Bross, A. F. Brown, Paul G. Chapin, Noam Chomsky, Charles A. Ferguson, Bruce Fraser, Lila Gleitman, Henry Hiż, Carleton Hodge, Henry M. Hoenigswald, Fred W. Householder, Dell Hymes, Ray Jackendoff, Aravind K. Joshi, Sheldon Klein, Susumu Kuno, George Lakoff, Robin Lakoff, Leigh Lisker, Yakov Malkiel, Christine A. Montgomery, David Perlmutter, John Robert ("Haj") Ross, Naomi Sager, Arthur Schwartz, Carlota S. Smith, Zeno Vendler, and C. F. Voegelin, plus myself. (A few of these acceptances were tentative, and there were others whom I solicited but who pled a supervening and perhaps subsequent commitment.) The promised participants in the projected volume included, then, a representative selection of his onetime students (Noam Chomsky chief among them), plus a few others, among them the most respected names in the scholarly world of the study of language in its various aspects.

Readers need not cudgel their wits for memory of this volume, for it never appeared. Harris aborted it. He learned of the planned Festschrift, just in advance of his returning to the States and there receiving my letter apprising him of it (these things are supposed to be a surprise, after all), while passing through the Netherlands offices of Mouton & Company. His refusal of the intended honor was at first acerbic. "Dear Watt," he wrote me on October 20, 1969, in his tiny longhand,

> I am sorry to intervene in your actions, but I am writing in a matter in which I have human rights. It has come to my attention that you and

Mouton are planning a Festschrift for me. Such a publication would be a deep personal affront to me and to my sense of values. I have managed to live this long with the principle that scientists can be people who do the best work they can for the sake of knowledge and of its human value. Any special—and unavoidably invidious—recognition of their work, such as honors, prizes, and Festschriften, is abhorrent to me, and would violate what I feel is a human right and dignity.

Therefore, I ask you to withdraw this activity. . . . Many years ago, during Bloomfield's lifetime, I had to get a similar project stopped for Bloomfield's sake, and I am sorry that now I have to do it for myself. I am sure, however, that you will understand me, and will respect my principles even if they may seem excessive.

With best regards,
Zellig Harris

P.S. I have just seen your letter [a greeting to him announcing the occasion], after writing the above. Thanks for writing me, & I will answer your letter tonight, although the above (for which I apologize again) will indicate how I feel in the matter. Yours, Z. S. H.

Here, beyond cavil, was a response to a prospective honor—and one granted to few—from an honorable man. Moreover, one couched in such a way as to cause me, the offender, the least pain, partly by basing his declining the proposed honor on his having scuttled a similar tribute to Leonard Bloomfield, one of the earlier gods of linguistics. My reaction, besides of course immediately resolving to cancel the projected Festschrift and to write its promised participants to that effect, was also to arrive at a new respect for the opinions that Harris had just evinced and to conclude that, in his sense, Festschriften are indeed an abomination of a sort.

Having canceled the Festschrift, I so informed Harris. Before he could receive my notification of withdrawal he wrote me again, as promised, in a way still more indicative of what he held to be "human values" and also of his sensitivities to a very junior colleague (whom, after all, he might rightly have suspected of an activity not wholly divorced from self-aggrandizement). His second letter, also dated October 20: "Dear Watt," he wrote (I should explain that, just as in California nobody has a surname, in Philadelphia, except in the case of strangers or when extreme deference is due, a man is typically not addressed by any other), "Thank you for your kind letter, and I would never have been able to write as I did yesterday had I seen [it] first—though it may be just as well for my earlier letter represents my feelings. . . . Small as the whole issue is, I think you too see that there are values involved. As for me, anything that I could have gotten from the Festschrift, I think I have gotten from the tone of your letter, for which I thank you." And then after Harris had received my notice that the abominable Festschrift had indeed been aborted there followed still a third letter, which he concluded by saying, more broadly and more personally, that "anyway, it is good sometimes to air one's feelings about the culture we live in (I don't mean only ours, or only now—the others are even worse). . ."

A final note on the aborted Festschrift may serve to deflate a certain rumor. Some believe that Noam Chomsky, Harris's best student, had a violent falling-away from his mentor and that there was "bad blood" between them. There's good reason to doubt this. Chomsky's letter to me, accepting my invitation to contribute to the Festschrift, betrayed not the slightest hint of such a rift. He wrote that he'd be "very pleased" to contribute and offered moreover to go over my list of proposed contributors to "see if any other suggestions come to mind." This was after all in 1969, well after the publication of *Syntactic Structures* and after he had completed, under Harris's direction, his iconoclastic dissertation at Penn. I may mention that during my own tenure at Penn, Chomsky was a sometime visitor at Harris's seminars, and certainly no animus was evident, to me, on either side. In fact, I don't think either Harris or Chomsky (or for that matter anyone else who had to any degree absorbed Harris's gentle nature) would be capable of the kind of pettiness it would have taken to sour their relations to the degree some have postulated. I may also mention that in Harris's introduction to his monumental *Methods in Structural Linguistics*, dated 1947, he gave due credit for "much-needed assistance with the manuscript" to one "N. Chomsky." "N. Chomsky" was then 19.

There is after all a human quality fitly called "nobility of character." It isn't found only at the great universities—I came upon it one desert morning in a radiator-repair shop in Kingman, Arizona—but when encountered in a university setting, it's likely to affect a great many lives, and for the better. Zellig Harris, besides being a towering figure in linguistics—and one many of whose insights and discoveries will be of perennial relevance—had, to my eye, that quality and in spades. It was manifested in many ways, and not least in his modesty: "I'd give it all up if I could write one good sonata," he once confided to his onetime student A. F. Brown, of his scholarly achievements and their resulting renown, as "Pete" Brown told me one day. *"Leges sine moribus vanae"* has long been Penn's motto: roughly, "Laws without morality are useless." Though from Horace (xxiv in Book III of his *Odes*—there really *was* a time when a university's motto was likely to be drawn from Horace or Virgil) it well conveys Harris's own voice, in his time there, and as it continues in many of us who had the privilege of studying under him. In those, as in many others who knew him, surely, his spirit still lives. At Penn, as I've noted, he was a rather elusive figure, and once when he remarked in class that any of us with questions were welcome to seek him out of his office, a student (Tolly Holt) called out, "But you're always absent!" Without a moment's hesitation Harris replied, "That's false. I'm always present somewhere." And now we know where.

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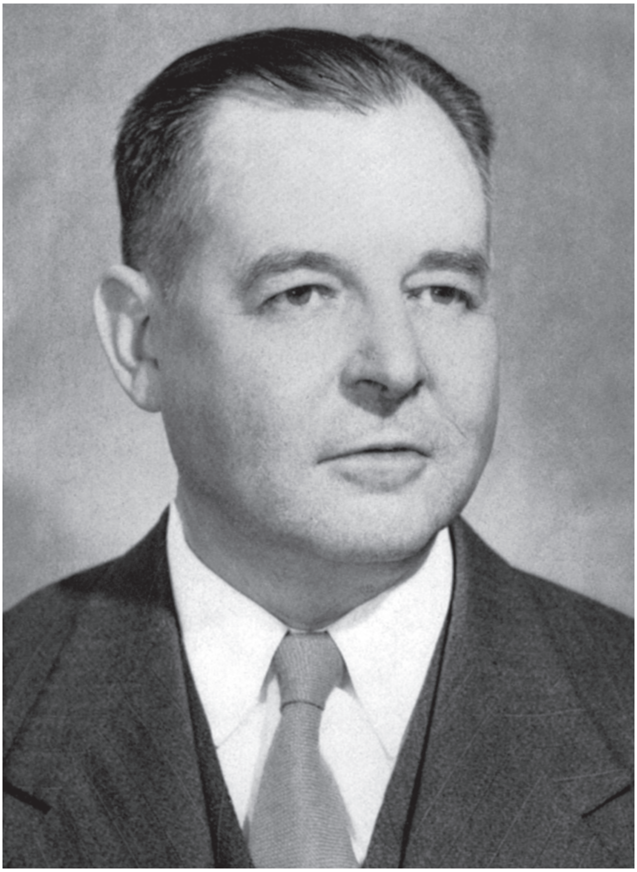

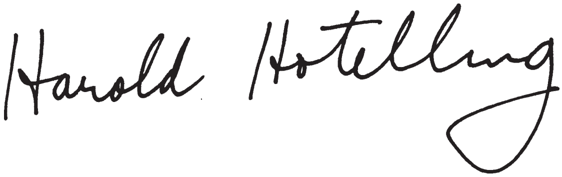

HAROLD HOTELLING

September 29, 1895–December 26, 1973

BY K. J. ARROW AND E. L. LEHMANN

HAROLD HOTELLING WAS A man of many interests and talents. After majoring in journalism at the University of Washington and obtaining his B.A in that field in 1919, he did his graduate work in mathematics at Princeton, where he received his Ph.D. in 1924 with a thesis on topology. Upon leaving Princeton, he took a position as research associate at the Food Research Institute of Stanford University, from where he moved to the Stanford Mathematics Department as an associate professor in 1927. It was during his Stanford period that he began to focus on the two fields—statistics and economics—in which he would do his life's work. He was one of the few Americans who in the 1920s realized the revolution that R. A. Fisher had brought about in statistics and he spent six months in 1929 at the Rothamstead (United Kingdom) agricultural research station to work with Fisher.

In 1931 Hotelling accepted a professorship in the Economics Department of Columbia University. He taught a course in mathematical economics, but most of his energy during his 15 years there was spent developing the first program in the modern (Fisherian) theory of statistics. With the aid of a grant from the Carnegie Corporation, he was able to appoint a research associate who would also teach courses; among those who held this position were Abraham Wald and Henry B. Mann. He had as graduate students Samuel S. Wilks, W. Allen Wallis, Jacob Wolfowitz, Albert Bowker, and Meyer A. Girshick, who in turn set up similar programs at Princeton, Stanford, Chicago, and Cornell.

With the aid of the research associates the statistics program was indeed first-rate; it had a separate listing in the catalogue but there was no department and no degree associated with it. His intellectual leadership, kindness, and generosity to his students were legendary among them, and his house in Mountain Lakes, New Jersey, witnessed his and his wife's monthly open houses for them and other statisticians. He had none of the prejudices then still common; refugees from Europe and students from India could count on his warm support and very practical help.

His human and liberty-loving sympathies made him a bitter opponent of Hitler and an early advocate of intervention in World War II. When we entered the war, he persuaded the U.S. military to create a statistical research group at Columbia, of which he was made director. The group worked on many problems of quality control for munitions and other statistical issues relevant to the war effort.

The Columbia administration resisted his efforts to create an independent department of statistics with permanent faculty. It persisted with its refusal even in 1946, when Hotelling received an exceptionally attractive offer from the University of North Carolina to start a statistics program with strong external financial backing. As a result, Hotelling left Columbia for Chapel Hill, where he quickly built up a strong Department of Mathematical Statistics.

Hotelling married Floy Tracy in 1920, and they had two children. After her early death in 1934, he married Susanna Edmundson, with whom he had five sons. He retired in

1966 but remained at Chapel Hill, where he died in 1973 after a long period of ill health.

CONTRIBUTIONS TO ECONOMICS

(For some of the information on Hotelling's career as an economist, we draw on the work of Adrian Darnell [1990].)

Harold Hotelling wrote six major papers on economics. Four of them have had a profound effect on the field, though frequently only after a long period of time. The other two are equally ingenious but deal with matters on which others were also working. Hotelling was interested in economics from his undergraduate days. Though his undergraduate degree was in journalism, he did take some courses in economics and in later years seemed to regard them as equally important to his journalism. His Ph.D. thesis in mathematics concerned topology, but his economic interests led to a paper, published almost contemporaneously with one based on his dissertation, on the true economic meaning of depreciation (1925). (Incidentally and possibly irrelevantly, the uncle of his thesis advisor, Oswald Veblen, was a very well-known though highly unorthodox economist, Thorstein Veblen.) The topic of depreciation is somewhat specialized, but Hotelling's treatment is at once highly original and fully in accord with standard economic reasoning. This paper became the standard for all subsequent work in the field. It did require a reasonable amount of mathematics, including calculus of variations and solving an integral equation in the most general case. Although there had already been a tradition of the use of mathematics in economics, indeed with predecessors in the late eighteenth century the use of mathematics had not spread far at this point. The continuous use of mathematics and economics really begins with Hotelling and some European contemporaries.

Another paper, whose inception appears to date from the same period but was published somewhat later (1929) in a major economic journal, dealt with a topic that he introduced, that of spatial competition. He recognized that when establishments are spatially separated, the cost to the consumer includes not only the price but also the transportation costs. As a result, he concluded that firms will tend to be concentrated in the middle. He noted that spatial differentiation was not only interesting in itself but could also be regarded as a metaphor for quality differentiation in products. His method of analysis was to find the Nash equilibrium point of a two-stage game; in the first state the players locate themselves; in the second they choose prices. (This was, of course, long before game theory was defined.) He noted that the analysis could also be applied to competition between political parties; they would both tend toward the center. Thirty years later and with additional contributions by Howard Bowen (Bowen, 1943), Duncan Black (Black, 1948), and Anthony Downs (Downs, 1957), the idea entered political science as the median voter theorem and has remained a staple of formal political theory to this day.

Unfortunately, Hotelling's work on the economics of spatial location was vitiated by an error (he used local conditions for a maximum, which were not sufficient in this case), as first pointed out 50 years later (d'Aspremont et al., 1979), an error that, however, did not affect the political implications.

The most influential of all his papers, at least in recent years, was that on the economics of exhaustible resources (e.g., minerals, such as oil) (1931). He gave a formal argument that the price of a commodity (more precisely, the excess of the price over the cost of extraction) would have to rise over time at the same rate as the rate of interest. The argument, like some parts of the paper on depreciation, used the calculus of variations, well beyond the capacity of the great bulk of economists then, and the paper was rejected by one journal before being published. With the post-World War II concern over resource exhaustion, the paper became acknowledged and forms to this day an important component of the analysis of the future of resource scarcity.

The two papers on the theories of the firm and of the consumer (1932, 1935) were the best expositions of their subjects. He connected them soundly to the general mathematical theory of unconstrained and constrained maxima and put especial emphasis on the symmetries in the cross elasticities of demand and supply implied by the first-order conditions and on the implications of the second-order conditions. The first paper was a systematic treatment for any number of commodities of standard results derived graphically for two commodities. The second broke ground new to the general economics profession, but it turned out that its basic ideas had been anticipated by Evgenii Slutsky, the Russian probability theorist, in an Italian actuarial journal 21 years earlier. These papers were the basis of his Columbia course in mathematical economics, at least in 1941-1942.

The paper that received the most immediate recognition was his presidential address to the Econometric Society on the economic criteria for judging whether a policy change represents an economic improvement (1938). Though the general idea had been adumbrated over the history of economics, the precise statement and interpretation of the welfare criteria had never been stated clearly. He not only clarified the interpretation and gave a precise and simple proof but raised new ideas for measurement of welfare improvements, especially in a world of many commodities.

This paper and some remarks in earlier papers also made clear Hotelling's inclinations to some kind of market socialism. Despite the fact that his work was based on standard economic principles, he pointed out that they did not necessarily lead to *laissez-faire* implications.

Finally, one of the most used of Hotelling's economic insights is contained not in a paper but in a letter to the director of the National Park Service (Hotelling, 1947). The director had asked him and others how to measure the economic value of national parks. Since the entrance free is relatively small, it is clear that people are probably getting satisfaction from the park well in excess of the fee charged. Hotelling noted that people usually have to travel considerable distances and thereby incur significant money costs. If we find the largest distance traveled, then those individuals can be thought of as getting zero net satisfaction (the value of the park less the transportation costs). Then all nearer individuals get a surplus that can be computed. By integrating over the population, we can get a total measure of satisfaction. This method, called the "transport cost method," has been used numerous times to evaluate the social gain to parks and similar geographically defined sites.

CONTRIBUTIONS TO STATISTICS

Hotelling's statistical work falls into two categories. On the one hand, he made deep and highly original research contributions; on the other, he was in the broadest sense the teacher of a generation of American statisticians.

In one of his earliest statistical papers, written with Holbrook Working (1929), Hotelling obtained confidence bands for regression curves several years before Neyman developed his theory of confidence sets. His best-known and most influential paper (1931) extended Student's t-test (in both the one- and the two-sample cases) from univariate to multivariate distributions. The resulting test is known as Hotelling's T^2-test. As in the earlier paper, he points out that the tests can be converted into confidence statements, this time for the unknown multivariate mean. He also introduces invariance considerations to simplify determining the null distribution of the test statistic. This is an idea that entered the mainstream only much later. Hotelling continued his contributions to multivariate analysis with, among others, two basic papers on principal components (1933) and canonical correlations (1936).

A paper pointing in quite a different direction was "Rank Correlation and Tests of Significance Involving No Assumption of Normality" (1936). Of it, Richard Savage, in his 1953 "Bibliography of Nonparametric Statistics," writes that "papers related to nonparametric problems were published in the nineteenth century, but the true beginning of the subject may be taken as 1936, the year in which Hotelling and Pabst published their paper on rank correlation."

Again, invariance considerations are central and are used to motivate the reduction to the ranks of the observations.

It may seem surprising that Hotelling, having conceived of such basic ideas as confidence sets and invariance and having used them more than once, did not take the additional step of formulating them abstractly and developing their properties. But that was not his style. Mathematicians distinguish between problem solvers and system builders. Hotelling (at least in his statistical work) was emphatically the former. He took on a difficult problem and after having solved it, moved on to tackle the next one. As a result, confidence sets are credited to Neyman, who discovered the idea independently a few years later and developed it into a general theory; a general concept of invariance was first formulated in the late 1940s by Hunt and Stein.

Hotelling's role as an educator may be said to have begun when—it seems on his own initiative—he reviewed the first edition of Fisher's *Statistical Methods for Research Workers*, published in 1925, for the *Journal of the American Statistical*

Association (JASA). His review ended by noting that "the author's work is revolutionary and should be far better known in this country." Hotelling considered the task of making American statisticians aware of Fisher's work so important that he went on to review the next six editions of the book as well as the first two editions of Fisher's *The Design of Experiments.*

In the same vein, after spending six months with Fisher in Rothamstead, Hotelling published two survey papers in *JASA*: "British Statistics and Statisticians Today" (1930) and "Recent Improvements in Statistical Inference" (1931). They summarized what he had learned in England.

During the 1930s, knowledge of the new methodology developed by Fisher and expanded by his successors spread in America into many different fields of application, and led to the setting up of statistics courses in the corresponding university departments. However, these courses were not taught by statisticians, since that profession did not yet exist. Instead, they were typically assigned to the youngest, most recently hired member of the department, who often knew nothing of the subject and had to learn it one lecture ahead of the students.

The situation dismayed Hotelling, and in 1940 he analyzed it in a paper (1940,2), "The Teaching of Statistics," that raised a question basic to the future of the discipline.

> The growing need, demand and opportunity have confronted the educational system of the country with a series of problems regarding the teaching of statistics. Should statistics be taught in the department of agriculture, anthropology, astronomy, biology, business, economics, education, engineering, medicine, physics, political science, psychology or sociology, or in all these departments? Should its teaching be entrusted to the department of mathematics, or a separate department of statistics, and in either of these cases should other departments be prohibited from offering duplicating courses in statistics, as they are often inclined to do?

Hotelling continued this discussion five years later with a talk on the slightly broader topic—"The Place of Statistics in the University" (1949). He concluded the paper by recommending that "organization of the teaching of statistical methods should be centralized and should provide also for the joint functions of research and advice and service needed by others in the institution and possibly outside it, regarding the statistical aspects of their problems of designing experiments and interpreting observations."

In the following decades, the issues raised by these two papers were hotly debated in universities across the country, but in most cases were gradually settled along the lines suggested by Hotelling, with independent departments of statistics offering not only centralized teaching of the subject but also consulting services to fill the needs of researchers in other departments.

The most central aspect of Hotelling's educational activities was, of course, his development of two outstanding statistics groups at Columbia and Chapel Hill and the training of statisticians he carried out at those institutions. It is no exaggeration to state that during the 1930s and early 1940s, Hotelling nearly single-handedly brought American statistics into the modern age and laid the foundation for the extraordinary development of the subject after the Second World War.

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MARTIN GLOVER LARRABEE

January 25, 1910–June 16, 2003

BY DAVID R. BURT

Born in Boston and educated at Harvard, Martin Larrabee showed signs of his New England background and upbringing throughout his long career. Mart was strong on character. He was hardworking, but not to excess, and persistent. Whatever he undertook was done thoroughly and properly. He did not believe in shortcuts; for instance, he disapproved of double publication of similar findings both in book chapters and in research papers, so that some of his students' thesis work appeared in print only as part of chapters in symposium volumes. He was generally serious, but also was kindly and knew how to have fun, with a dry, understated sense of humor. He enjoyed making and jury-rigging his own equipment, stayed in the lab as much as possible, and abhorred waste and sloppiness. He was a respected father figure and example to his graduate students and postdoctoral fellows. I was fortunate to be one of his students during and after his election to the National Academy of Sciences in 1969 and, after my graduation in 1972, to be one of his many friends.

His scientific contributions dealt mostly with miniscule bits of tissue, sympathetic ganglia from rats, chick embryos, and other organisms. (Rat ganglia weigh about a milligram.)

As a Ph.D. student with Detlev Bronk at the University of Pennsylvania, working with the cat stellate ganglion, he was the first to describe what was later called post-tetanic potentiation. In his later years, although nominally a biophysicist, he was mostly interested in metabolism and its relationship first to nerve activity and later to development. He thus helped to pioneer important areas of neurochemistry. He also took some side trips into other areas, including actions of general anesthetics, viruses and nerve growth factor, and pathways of signal transduction (not recognized as such at the time), making contributions in all of these areas as well.

FORMATIVE YEARS

Mart writes in his brief autobiography (1999), the source of most of this memoir, that he was already drawn to the concreteness of physics and mathematics in high school (Newton Country Day School), even though his father was a physician. Mart enjoyed building electric motors and radios in his spare time. He graduated as a physics major from Harvard College in 1932 after also considering engineering. During college he had spent a summer doing optics at Dartmouth and, more significantly, spent the summer of 1931 working with Keffer Hartline at the Marine Biological Laboratory in Woods Hole, Massachusetts. In that same summer Hartline first recorded from single fibers in the eye of the horseshoe crab. (Continuation of this work eventually earned him a Nobel Prize.) That summer and the next, Mart also worked with Baldwin Lucké (also of the University of Pennsylvania, but again working in Woods Hole) on the osmotic properties of Arbacia eggs. Mart's introduction of a diffraction method of measuring egg diameter helped gain him his first coauthorship (1935).

Initially attracted by the notice of a generous $300 scholarship in the fall of 1932, Mart entered graduate school at the University of Pennsylvania in the Johnson Foundation, which overlapped with the Biophysics Department. After several rotations, he was "adopted" by Detlev Bronk, the director, who introduced him to the study of sympathetic ganglia. (Their biophysical studies coincided with Feldberg's pharmacological studies of synaptic transmission in ganglia.) The advantages of this preparation include a relatively simple anatomy and physiology, easy accessibility, and a size, at least for rat and chick ganglia, small enough to be supplied adequately with oxygen and nutrients by simple diffusion. Mart was so convinced of these advantages that he stayed with ganglia for half a century when he resumed their study after the war.

His major finding during his thesis research with Bronk (1947) was that increasing numbers of ganglion cells were recruited to fire following a conditioning train of electrical stimuli (preganglionic nerve impulses). They termed this phenomenon "prolonged facilitation," but it was later renamed "post-tetanic potentiation." This early example of synaptic plasticity in the peripheral nervous system helped stimulate many studies of similar phenomena in the central nervous system. Some of these phenomena are now thought to underlie learning and memory.

Another piece of thesis research (1948), important at the time, helped to establish that conducted nerve action potentials and transmission across synapses are similarly sensitive to ischemia. This lack of oxygen and nutrients results from circulatory block, as in stroke. This demonstration was facilitated by the anatomy of cat stellate ganglia, which possessed both axons of passage and synapses. During this period he also contributed to some technical advances in electrophysiology.

Mart's doctorate was awarded in 1937, but he stayed on at the Johnson Foundation until Detlev Bronk was recruited to be the chair of physiology and biophysics at Cornell Medical School in New York City in 1940. At Cornell for one year he helped to teach physiology and helped to train a postdoctoral fellow, Clint Knowlton, who was working with pulmonary reflexes (1946). In 1941 Mart returned from Cornell to the Johnson Foundation when Detlev Bronk did.

The year 1941 also marked the country's entry into World War II, which helped to delay publication of much of Mart's earlier research and drastically shifted the focus of research at the Johnson Foundation and elsewhere. During the next four years Mart got to help with many projects connected to the war effort. One contribution in the area of respiratory physiology he relates (1999, p. 199) was to save the lives of three professors stuck in an improvised altitude chamber by taking over the controls from a high school student. His only publication from wartime research involved measurements of nerve regeneration (1948).

After the war Mart soon returned to the study of sympathetic ganglia; he worked with the first of his many international postdoctoral fellows, Jean Posternak, from Switzerland, whom he often visited in later years. Among other studies, they investigated the effects of general anesthetics on synaptic transmission through ganglia, contrasting the lower concentrations needed to block synapses with the higher ones needed to block axonal conduction (1952). This groundbreaking work still is cited in discussions of mechanisms of general anesthesia. Some of his other work at this time included the use of an oxygen electrode developed by Philip Davies. Inserted into cat stellate ganglia, it enabled estimation of their oxygen consumption and demonstration of an increase with electrical activity (1952). Thus began Mart's interest in the relationship between activity and metabolism, which he continued for many years.

Mart writes very fondly (1999, pp. 202-203) of his years in the Johnson Foundation with a paternalistic mentor. There he was privileged to work with a close-knit, friendly, and supportive group funded modestly but adequately by endowment and foundation money before the days of government largesse and competition for grants.

HOPKINS

Detlev Bronk became president of Johns Hopkins University on January 1, 1949. He brought with him Mart and most of the group who had earlier followed him to Cornell Medical School and back, founding the new Thomas C. Jenkins Department of Biophysics chaired by Keffer Hartline. Also included were some graduate students who later became Mart's fellow faculty members: Ted MacNichol and Frances Carlson. Mart found himself in temporary quarters in the medical school, in frequent contact with such luminaries as Vernon Mountcastle of physiology, Stephen Kuffler of ophthalmology, and David Bodian of anatomy, until he moved to the department's new building on the Homewood campus in 1953. Space there became available when Bronk moved again to become president of the Rockefeller Institute in New York City. Although he was invited to join Bronk, Hartline, Brink, and others who moved to Rockefeller, Mart elected to stay behind at Hopkins.

Meanwhile, his research had returned to general anesthesia, and in particular to the issue of whether anesthetics' actions on synaptic transmission were direct or indirect through block of metabolism. Working with Juan Garcia Ramos from Mexico and Edith Bulbring from England on rabbit ganglia, he showed that anesthetics blocked transmission at concentrations that did not affect oxygen consumption, whereas metabolic poisons blocked both in parallel (1952).

At about this time he started work with excised rat superior cervical sympathetic ganglia, enabling a variety of subsequent studies of the acute metabolic requirements of ganglionic transmission in terms of oxygen, glucose, and other nutrients. These studies helped form a foundation for understanding some of the early brain damage in strokes. Rat ganglia also supported a brief diversion into virology and observation of a puzzling pattern of ganglionic discharge brought on by infection with pseudorabies virus (1955). They also supported training of his first graduate student, Charles Edwards, who showed anesthetics to increase glucose uptake and lactate output (1955). Other students followed, including William Stekiel, Frank J. Brinley, and Paul Horowicz. Mart began to use radiotracer methods, and his lab was able to account completely for the metabolic products of labeled glucose (chiefly carbon dioxide and lactate) and to show that glucose metabolism did not account completely for resting oxygen uptake or for its increase with activity. Publications from this period include Horowicz and Larrabee (1958, 1962) and Larrabee et al. (1957). He became full professor of biophysics at Johns Hopkins in 1963.

A diversion into phospholipid metabolism was prompted by findings of Lowell and Mary Hokin. They reported that labeling of phosphatidyl inositol from inorganic phosphate could be increased by applying high concentrations of acetylcholine to sympathetic ganglia and other tissues. (Acetylcholine is the major neurotransmitter in ganglia.) Mart, working with Jack Klingman and William Leicht, was able to reproduce and extend these findings to natural activity under much more physiological conditions (1963, 1965). We in the lab in 1969, when Mart was elected to the National Academy of Sciences, speculated that these findings had helped to put him over the top for selection, even though nobody really knew then what the phenomenon meant.

At the time I was making my minor contribution by finding in my thesis research (1973) that ganglionic subcellular fractions with increased labeling of phosphatidyl inositol included synaptosomes, pinched off nerve terminals with adherent postsynaptic membrane fragments. Concurrently, a postdoctoral fellow from England, Godfrey L. White, was finding a lack of acute effect of activity on labeling of higher inositides. In hindsight, these and similar results most likely reflected the presence in ganglia of the phosphoinositide-phospholipase C signal transduction pathway.

Another area Mart explored was nerve growth factor (NGF), occasioned by the gift of some by Rita Levi-Montalcini, its discoverer, and brought into the lab by Giovanni Toschi from Rome in 1965. Graduate student Lester M. Partlow showed its dramatic effects on nerve outgrowth from chick sympathetic ganglia to be independent of synthesis of ribonucleic acid and protein (1971). Another student, David C. Halstead, studied effects (immunosympathectomy) of an antiserum to NGF. These and other early results with NGF foreshadowed discovery by others during the next 30 years of the key roles of neurotrophins (NGF and three others) on neuronal growth and maintenance in the brain and helped to stimulate Mart's later interest in development.

During this period Mart was treasurer of the new Society for Neuroscience (1970-1975). His other memberships included the American Physiological Society, the Biophysical Society, and the American Society for Neurochemistry. He was also awarded an honorary M.D. degree from the University of Lausanne in Switzerland in 1974.

Mart formally retired from Hopkins in 1975 upon reaching the age of 65 but was able to gain emeritus status, hold on to some research space, and maintain his research grant from the National Institutes of Health. This grant, titled "Metabolism and Function in Sympathetic Ganglia" and first awarded in 1954, was the only one Mart ever sought outside Hopkins. It was renewed over and over again to reach a total funding period of 43 years, one of the longest in NIH history. Working mostly on his own, Mart stayed in the lab for more than 20 years beyond his formal retirement and published 15 additional research papers. He maintained his interest in intermediary metabolism but turned from studying effects of activity in rat ganglia to studying development in chick ganglia, noting transitions in glucose utilization from anaerobic glycolysis (poor blood supply) to pentose cycle (supporting lipid synthesis) to conventional aerobic metabolism (1985, 1987). He started computer modeling in an effort to assess in detail the time course of partitioning of glucose carbons among the various pathways. This involved iteratively making assumptions about pools and fluxes, deriving corresponding equations, and embracing the new age of computers for the first time by trying to fit his data (1978, 1980, 1989). This was a lot to take on at an age when most have retired for real, but the challenge helped to keep him mentally young.

He also identified alanine as a major product of glucose metabolism besides lactate and that both were released to extracellular pools before being reabsorbed (1992). Finally, he established a preference of nervous tissue for lactate over glucose in fueling oxidative metabolism both in embryonic ganglia (1996) and, using calculations based on others' data, in brain (1996). Most surprisingly, his calculations suggested that blood lactate levels reached during intense exercise (20 mM) would supply about 60 percent of brain carbon dioxide output even in the presence of 5 mM glucose. (One is tempted to speculate about a relationship of these findings to recently-established beneficial effects of exercise on brain function.)

Before I consider briefly some other, more personal aspects of Mart's life, I would like to recall an episode that occurred during my time at Hopkins. Presumably because of Mart's care and thoroughness (nitpickiness?), he had been put in charge of distributing keys to departmental locks and of maintaining the lines feeding distilled water into departmental laboratories. On one April 1 (1966?) he entered the building to find a goldfish apparently swimming in one of the distilled water lines. He sputtered and fumed magnificently until it was pointed out to him that the glass tubing with the goldfish had been added in parallel to the actual distilled water line. He later accepted this student prank with typical good humor, although it did take him a while to cool down at the time.

FAMILY AND FUN

Mart was the son of Ralph Clinton Larrabee and Ada Perkins (Miller) Larrabee. He fathered one son, Benjamin Larrabee Scherer, in a first marriage to Sylvia Kimball and another, David Belcher Larrabee, in a second, much longer marriage to Barbara Belcher (more below). He wed his third wife, Sarah Galloway, two years after Barbara's death in 1996. Mart spent his final 10 years in a retirement community outside Baltimore, entered when Barbara became ill (and where he later met the widowed Sarah). He kept doing science for his first few years there, as noted above. After his marriage to Sarah, they got to spend several happy summers with family at her farm house in West Glover, Vermont.

Mart was a keen outdoorsman, maintaining an active interest in hiking, mountain climbing, and downhill skiing until he was well into his eighties. He inherited this from his father, who made time from his medical practice to pursue similar interests, in particular helping to construct a system of trails in the White Mountains of New Hampshire.

Mart writes (1999, p. 194) that he first met Barbara Belcher, later his wonderful wife for 53 years, at a mountain hut on one of these trails in 1936.

Beginning when I was a student, Mart undertook to emulate his father by building 30 miles of new trails in the nearby Gunpowder Falls State Park and Prettyboy Reservoir watershed, nominally under the auspices of the Sierra Club. (He also belonged to the Appalachian Mountain Club and Mountain Club of Maryland.) His volunteer laborers consisted mostly of departmental graduate students (and their spouses). He would go out the day before and lay string where we were to cut and mark the trails as we went along with painted baby food jar lids provided by his technician. We were motivated not only by our desire for a degree (the trail work actually was optional, even for students in his lab) but also by being outdoors in good company and most especially by delicious Sunday dinners provided afterward by Barbara. An ardent naturalist and birder, she sometimes accompanied us on the trail-cutting expeditions and provided us with nature lore. After I graduated and the trails were completed, Mart continued to organize occasional maintenance expeditions for many years, reuniting former students still in the area and recruiting new biophysics students. His endurance on these expeditions put some of us to shame, even though we were 30 to 40 years younger.

During their long and happy marriage, Mart and Barbara took many international trips, often doubled up with meetings, and especially favoring the mountains of Switzerland. They typically also spent several weeks in August on Monhegan Island in Maine. I twice got to house-sit at their lovely Baltimore County home while they were in Maine.

On these occasions, and at the dinners after trail cutting, I and my fellow students admired a piece of hydraulic engineering Mart had undertaken to feed water into a bird bath from the tank of his upstairs toilet. A long piece of thin plastic tubing dripped just fast enough to balance evaporation (and splashing by the birds).

Mart was survived by his wife, Sarah; his sons, Benjamin and David; and their families.

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MELVIN LAX

March 8, 1922–December 8, 2002

BY JOSEPH L. BIRMAN AND HERMAN Z. CUMMINS

MELVIN (MEL) LAX WAS A versatile and productive theoretical physicist who made major contributions in many areas of science, including acoustics, multiple scattering of waves, disordered media, coherence and fluctuations in classical and quantum systems, applications of group theory to solids, phonon production and optics, and high-power lasers. His classic 1951 *Reviews of Modern Physics* paper on multiple scattering theory has been used in many areas of physics. For example, it led to the coherent potential approximation for disordered systems. His 1958 analysis with J. C. Phillips of electron motion in disordered systems showed how impurities randomly placed in a semiconductor crystal, or with random variation of interaction strength, will affect the energy levels and hence the conduction properties of the medium. This analysis was crucial for the early understanding of impurity bands in semiconductors, a topic of central importance in semiconductor device applications.

Mel wrote another influential *Reviews of Modern Physics* paper on "Fluctuations from the Nonequilibrium Steady State" in 1960. The laser was first demonstrated that same year, leading Mel to study fluctuation phenomena in lasers. During the 1960s he wrote a series of papers on classical and quantum noise. Almost all of these pertain to fluctuation phenomena in lasers. The results obtained enabled the signal, which contains the relevant information, to be separated from the unwanted (uncontrollable) random noise and gave rise to the Lax-Onsager regression theorem. This work now underlies many aspects of the design of optical communications devices and was part of Mel's lifelong, deep interest in random processes. Most recently he worked in the area of inverse scattering techniques needed to extract information from noisy measurements, such as using light scattering to study clouds with lidar (light detection and ranging) techniques; searching for oil-bearing layers using acoustic backscattering; and detecting possible tumor nodules in the human breast using pulsed, noninvasive infrared light. These major directions of his work do not exhaust all of his significant contributions in physics, as we shall describe below. In his own resume Mel listed eight areas of physics where he had made significant scientific contributions: (1) multiple scattering of waves; (2) multiphonon processes in solids; (3) application of group theory to solids; (4) coherence and fluctuations in classical and quantum systems; (5) nonlinear interaction of light with sound and other excitations in solids; (6) high-power lasers; (7) phonon production and phonon optics; and (8) hot phonon interaction with electrons in semiconductor quantum wells and heterostructures. Mel also did early work on quantum transport theory.

Mel was an indefatigable worker who maintained and regularly used three offices: at City College, at Bell Labs, and at home, all crammed with piles of documents. Somehow he always seemed to know in which office he had placed any specific article. He had an inexhaustible curiosity and interest in nearly every branch of physics. More broadly he was deeply interested in any field of human endeavor that could be made quantitative. This included fields as diverse and as far from theoretical physics as finance (long before finance became a career objective of some of our brightest Ph.D. physicists), the law, traffic control, biology, and the philosophical underpinnings of quantum mechanics.

Computers were a particular fascination for him. When computers were first introduced, Mel quickly became expert in their use for specialized computations (splines and other esoterica) and ultimately contributed an article to a specialized journal on computer science, as well as a review article on "Wave Propagation and Conductivity in Random Media" in the 1973 proceedings of the Society for Industrial and Applied Mathematics and American Mathematical Society. He early realized the importance of computer typesetting of scientific manuscripts and served on a committee of the American Physical Society that explored the best way to have physics manuscripts prepared for electronic submission to the APS journals. As a result, APS initiated its Compuscript program, initially accepting manuscripts in TROFF (a document-processing system), opening the way for the now nearly universal practice of electronic journal submissions as TeX files.

Mel had a very broad but also very deep store of knowledge about the many areas of physics to which he contributed. Colleagues at all stages of his career who asked him a question were guaranteed to get a long and detailed answer and never a simple "yes" or "no." When Michael Lubell asked Mel why the UNIX system he had installed at City College was superior to the VMS Mike knew from Yale, Mel launched into a very long discussion of several hours duration, finally concluding by giving Michael some thick notebooks and manuals from his bookshelf and saying, "Look these over in the next couple of days and let me know if you have any questions." Lubell reported later to us: "Next couple of days? For Mel, perhaps. For me it was a year's work."

Mel was never satisfied with simple answers; he attacked any problem in physics with verve, enthusiasm, and confidence. He was sure he could solve it, because he was certain that he either had already mastered the needed mathematical skills or, if need be, could invent new ones. Eli Burstein reports an event from the 1950s. The work of T. S ("Ted") Berlin and J. S. Thomsen on the interaction of dipoles in simple lattices, using a sphericalization technique first introduced by Ellliot Montroll but assuming only nearest neighbor forces, was brought to Mel's attention by Montroll. Mel was confident that he could solve the spherical model, including long-range as well as short-range dipole-dipole interactions, and he ingeniously overcame the difficulties that are encountered in a straightforward generalization of the Berlin and Thomsen work. In his 1952 paper "Dipoles on a Lattice: The Spherical Model" he also proved that in the spherical model a permanent dipole lattice is equivalent to an induced dipole lattice with an effective polarizability, and he pointed out that the generality of his results is due to the fact that they have been expressed in terms of the eigenvalues rather than the interaction energies. Moreover, he noted that his treatment, including short- and long-range interactions, is also applicable to the spherical Ising problem. It was an impressive achievement. This interaction with Mel convinced Eli that optimism is always a valuable asset when tackling a challenging problem. Mel had an abundance of optimism and confidence about solving physics problems to which he directed his attention. He was never satisfied with writing a physics paper unless every single detail was under control and understood—at least by him!

This brings to mind Mel's generosity with his ideas. One striking example of this concerns the history of the coherent potential approximation, or CPA, which is now widely used for effective treatment of a system with random distribution of impurities. In his much earlier treatment of multiple-scattering theory, Mel realized that some effective or average potential could incorporate many-impurity effects in a self-consistent fashion. We will give more details below when discussing Mel's work at Bell Labs. Suffice it to say that the CPA is attributed to other physicists; but if one looks at the acknowledgements in the papers most cited as the original work, one sees (as P. W. Anderson put it) that "Mel was a prime mover behind this work. It is impressive to realize that no matter what you do in the study of disordered systems, you are following in the footsteps of Mel Lax, since he either invented or was crucial in the development of both of the major approaches to this problem." [1] Mel never complained about not getting proper credit for his pioneering steps here or in other cases like it.

Mel was not without idiosyncrasies. We both recall many years of City College colloquium dinners with Mel at Chinese restaurants and also one of us (J.L.B.) recalls a 1980 China trip with Mel that bring memories of one area where Mel had very profound and openly expressed strong opinions: He did not like spicy food! When our physics group would order some food of questionable pungency at a Chinese dinner, the server was always instructed to make the "Lax cut" (i.e., to separate all vestiges of the hot sauce for the rest of us) but give Mel the mildest version of the dish possible. Mel's family reports that whenever the family went out to dinner, be it Chinese, Indian, kosher, or other, the Lax cut was a necessity so that dinner could proceed. And, despite various family attempts to re-educate Mel to the pleasures of some spicy dishes, his preference was clear and unchangeable.

Mel was very deeply affected by three global events during his lifetime: the Great Depression of the 1930s, World War II, and the Shoa (Holocaust). We will comment later on the effect of the economic depression of the 1930s on his career choices. World War II occurred when Mel was in graduate school, and his work during that period for the Navy, concerning the loss of ships to submarine attacks, left in him a deep and largely unspoken "old-fashioned" patriotism, evident in his quick willingness to serve his country by giving his consulting services to scientific agencies of the United States. During his career he consulted for the Army Research Office in Durham, Aberdeen Proving Grounds, and various other Navy and Air Force offices of scientific research. As far as we know, all this work was classified, and we have no direct knowledge of specifics of the projects on which he worked. Yet there are evident echoes of this work in Mel's long interest in extracting signals from noisy data—for example, when laser radiation is scattered from turbid or partially opaque media, such as dust in the atmosphere. Interestingly, this work also later appeared when he began a long and fruitful collaboration with Robert R. Alfano at City College on extracting and interpreting signals from noise in the scattering of laser light from human tissue, for example, to distinguish healthy from morbid cancerous tissue.

Mel was in a very deep way an intellectual whose ethics were profoundly informed by his Judaism. He was a completely moral person in his dealings with other people. His word was his bond. He was a teacher par excellence in the sense of being an ethical role model in his dealings with students and colleagues. As a referee or editor for a scientific journal, he would often have to evaluate other scientists' work. He did this in a most scrupulous fashion. If he found some work incorrect or incomplete, he would write a report in a straightforward fashion, emphasizing how the author could improve the paper by making such and such changes. Often this meant totally redoing the analysis along the lines he indicated and of course rewriting the paper. He never asked credit for this, but he took a quiet satisfaction in knowing that, with his suggestions, at least that part of the scientific literature would be correct. He always thought it was his responsibility as a prominent physicist to be an educator-teacher in the broadest sense.

When Mel and the authors of this memoir were in Moscow after having organized a U.S.-U.S.S.R. symposium on "Laser Light Scattering," he joined American colleagues in visiting and supporting a group of *refusenik* physicists and mathematicians who were hoping to learn of the latest scientific developments in the West. Mel (and others) did not let them down. Later, when emigration from the former Soviet Union became possible, Mel sponsored new immigrants from the former U.S.S.R. (some of whom had been refuseniks) to work with him under the Program for Refugee Scientists. He later helped these scientists obtain permanent academic or other professional positions in the United States.

We will turn now to some chronological history of Mel's life and work.

THE EARLY YEARS

Mel Lax was born in New York City on March 8, 1922. His father, Morris Lax, owned a men's clothing store, Lax Haberdashers; his mother, Rose Hutterer, studied pharmacy. Morris and Rose placed tremendous emphasis on education and read the newspaper to their children every day. At an early age Mel was reportedly reading the *New York Times* by himself. In high school Mel excelled in math and science. He was president of the math club and editor of the student newspaper.

Growing up during the Great Depression left Mel deeply concerned with being financially secure. In high school, as he later recalled, he had some fine science and math teachers who were teaching there because they could not get jobs at universities at that time. He decided early on in high school that he would become a high school math teacher, a career that offered the security of permanent employment. His high school math teacher, however, urged him not to cap his ambitions at that level but to go on for advanced study, including graduate work. Fortunately, Mel took this advice, attended New York University as a full-scholarship Charles Hayden Scholar, and graduated with a B.A. degree (summa cum laude) in physics in 1942.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

After graduating from NYU, Mel was admitted to the graduate school of the Massachusetts Institute of Technology with a Fellowship in Applied Mathematics; later he was a teaching assistant and research associate in physics en route to his M.S. degree (1943) and Ph.D. (1947). In reflecting on this period of his studies he would often remark with pride that he was a teaching assistant to both P. M. Morse and H. Feshbach and that he had worked out solutions to every one of the problems in the well-known two-volume textbook of Morse and Feshbach, *Methods of Mathematical Physics*.

Mel's Ph.D. years at MIT during the Second World War included three years of research (1942-1945) at the MIT Underwater Sound Laboratory, under the direction of P. M. Morse, R. H. Bolt, and H. Feshbach. Mel worked with a group that developed devices to acoustically decoy torpedoes away from ships. These devices reduced the loss of ships transporting soldiers and military equipment to Europe.

When the war ended, he returned to finish his Ph.D. He proposed a thesis topic to Morse, the more senior professor. Morse told him the problem was too hard, so Mel switched to Feshbach as his advisor. In his Ph.D. thesis he calculated the cross-section for photoproduction and electron production of mesons. The technique he employed later came to be known as the impulse approximation. During this time Feshbach selected Mel with Arthur Wightman and Conrad Longmire to spend a summer at General Electric Research Laboratories in Schenectady, New York. At MIT Mel's roommate was Eli Burstein, who later joined the Naval Research Laboratory.

SYRACUSE

After completing his Ph.D. at MIT, Mel received several job offers, including a postdoctoral fellowship at the Institute for Advanced Study in Princeton. This was probably the best job in the country for a young physicist at that time, but it would last only two or three years and then would require looking for another job. Mel's concern with financial security led him to look for a job that would offer him tenure and permanent job security after two years. Syracuse University offered him a position with a guarantee of tenure in two years, which he accepted.

In his early years at Syracuse Mel worked in nuclear and acoustical physics. While he was at Syracuse, the field of solid-state physics (now condensed matter physics) was developing rapidly, stimulated by the invention of the transistor at Bell Labs in 1947. Mel became convinced that solid-state physics was an important emerging area and decided to move his research interests in that direction. As a first and quite decisive step, he announced a new physics lecture course on solid-state theory. Then and throughout his career Mel prepared and distributed extensive, detailed lecture notes for every new course that he taught, some of which later developed into books.

From his publications we can track Mel's shift of interest from scattering problems to meson physics, and then to topics in magnetism, such as his study of the spherical model for dipoles on a lattice (a problem that had engaged Elliott

Montroll and Mark Kac, among others), and then to his early studies on transport and capture of carriers by defects in crystals, then lattice dynamics (especially the density of phonon states and how to determine it from "inverse" methods). We will return to this topic later.

In 1951 Mel was invited to spend the summer as a consulting solid-state theorist at the Physics Section of the Crystal Branch, Naval Research Laboratory, which was headed by E. Burstein, his former roommate at the MIT Graduate House. The phenomena being investigated by Burstein and his group at that time involved radiative and nonradiative transitions of impurities in semiconductors. Mel extended the work of K. Huang and A. Rhys on the radiative and nonradiative transitions in F-centers that was restricted to the case of optical phonons. He included acoustic phonons and developed a new technique that, as he described it, "relied on delta-function tricks." Some years later he wrote, "If an optical experimenter does his work with lenses and mirrors, a theorist does it with delta functions and Green's functions." Aside from the new technical procedures that he introduced, the results that he obtained using a semiclassical approximation shed light on the Franck-Condon principle in solids. His 1952 paper, "The Franck-Condon Principle and Its Application to Crystals," became a citation classic (reported in "This Week's Citation Classic," *ISI Current Contents* No. 38, September 25, 1985). Mel found it amusing that his entry paper to solid-state theory became a citation classic and commented that perhaps this was because of its innovative approach to a long-standing problem.

Another topic that bridged his earliest to his most recent work concerned the simultaneous interplay between electronic and ion displacement or vibration in determining the absorption and emission of light and the electrical resistivity of materials. These subtle electron-phonon effects were meticulously analyzed by him in early papers on the Franck-Condon energy shift of emitted light compared with absorbed light in bulk crystals and then taken up again some 40 years later to explain how hot (out of equilibrium) electrons and hot phonons can interact to give negative carrier mobilities in modern quantum well or heterostructure systems. This explanation has important device applications and is a completely counter-intuitive result.

Based on work during this period he wrote a comprehensive paper with Eli Burstein in 1955 on the fundamentals of infrared radiation connected with the ionicity or homopolarity of crystals and how to understand and interpret the *reststrahlen* absorption of such crystals. This work carried further the pioneering studies of Max Born, T. von Karman, and the preceding generation into a fuller and more fundamental treatment.

In his early work on quantum transport theory Mel introduced a density matrix theory of linear response from equilibrium.[2] This method was also developed independently by Kubo[3] and others (e.g., Mori, Nakano, Feynman). This method is usually known as the Kubo approach and has also been referred to as the Kubo-Lax description of transport properties.[4]

Mel remained at Syracuse University from 1947 to 1955, advancing from assistant to full professor. His published work during that period on theories of magnetism, phonons, optical properties of solids, and multiple scattering brought him growing attention as a major young theoretical physicist in the rapidly developing solid-state physics community and led to his being recruited to join the new Theory Department at Bell Laboratories in Murray Hill, New Jersey.

BELL LABS

Before 1950 Bell Labs contained a group of experimental laboratories to which theorists were attached. Following the departure of John Bardeen, Bill Shockley, Charles Kittel, and Hal Lewis, Bell Labs created its first Theory Department (1111) around the remaining theorists. They recruited Mel as the first new full-time member. He served as a member of the technical staff from 1955 to 1972, as chairman of the Theoretical Physics Department from 1962 to 1964, and continued as a consultant to the Physics Research Laboratory until his death. By 1958, when Mel recruited John Hopfield, the department was fast becoming the preeminent condensed matter theory group in the world. The invention of the transistor was quickly recognized as a major breakthrough in the communication and telephone business of Bell Telephone that would lead to many other novel devices.

When Mel joined Bell Labs his first initiative was to suggest a problem to J. C. Phillips, who had been hired as a postdoc. The resultant paper on density of electron states in a disordered system was one of the first to explore the effect of disorder on the electronic spectrum of solids. Another important initiative of Mel's was to hire John Hopfield into the theory group. This was a magnificent beginning to what became a highly productive period in Mel's professional life. When Mel took over the reins as the chair of the theory group, further progress was assured.

Around 1960 the laser was invented, and this provided another huge stimulus to theoretical work. Mel rapidly grasped and investigated two main themes in laser physics: the effects of fluctuations, or noise, and the analogy of laser action to a phase transition in conventional crystalline solids. The output of Mel's work on noise was phenomenal: some six papers on classical noise and a baker's dozen on quantum noise. In short, Mel created this specialty in laser physics.

Much of Mel's work was stimulated by the steady outpouring of extraordinary experimental results on semiconductor physics at Bell Labs. One of the major directions this took was the study of optical properties of solids, for example, the investigation of the frequency dependence of the optical absorption coefficient in the infrared region of the spectrum, due to creation/emission of phonons. Another set of observations concerned the detailed mechanisms of optical absorption near the fundamental edge due to electronic transitions from valence (filled) to conduction (empty) states in the crystal. These studies, as well as more intricate ones involving the scattering of conduction electrons or holes by phonons that affected the electrical conduction of the semiconductor, and especially its temperature dependence, required a microscopic quantum mechanical picture and theory of the processes involved. One of the key ingredients needed here was the application of symmetry/group theory to develop selection rules for allowed/forbidden processes and for relating different processes to one another. Mel made important contributions here, using the subgroup methods that he developed. And at one stage three of the practitioners of this arcane topic (in alphabetical order: J. L Birman, M. Lax, and R. Loudon) came together to write a joint paper on the relevant electron-phonon intervalley scattering selection rules needed to interpret data on electrical conductivity in GaAs and related III-V semiconductors with cubic zincblende structure.

Additionally Mel made a significant contribution to the use of time reversal symmetry in further simplifying the calculations. Because of the importance of group theory in many branches of physics, including molecular and solid-state physics, Mel gave a series of tutorial lectures on the subject for a number of years at Bell Laboratories. He developed a subgroup technique to obtain symmetry-related selection rules governing the go and no-go alternatives for physical processes (for example, scattering or optical transitions) in crystalline solids. His lectures on this topic were published in his 1974 book *Symmetry Principles in Solid State and Molecular Physics*.

Another important direction of Mel's work in those years at Bell Laboratories was investigation of the crystal lattice normal modes or phonons for those crystals important in semiconductor studies, such as silicon, germanium, gallium arsenide, and related materials. Part of this work required doing the symmetry analysis, and the complementary part required detailed calculation of the dispersion of the phonon frequencies as a function of wavelength or wave vector of the phonon waves. Mel, with Joel Lebowitz, had initiated some work on phonon density of states when he was at Syracuse University studying the moment analysis of vibration spectra. His work at Bell Labs carried the analysis to a deeper level by studying several force-coefficient models, including shell and valence force models and applying the analysis to actual materials. Related to this were his work on microscopic and macroscopic theories of elasticity of crystals and ultimately his studies on damping and anharmonicity effects on phonons. This work was reported at the 1963 International Conference on Lattice Dynamics, where Mel's talks spanned the gamut from group theory methodology to concrete calculations of phonon frequencies using microscopic mechanisms and early many-body treatments of anharmonicity. This work on phonon physics led him to an investigation with Donald Nelson at Bell Labs of the photoelastic effect, to which we now turn.

The photoelastic properties of anisotropic media were investigated very thoroughly in the earliest days of the study of light passing through a crystal and being modified due to both natural birefringence and induced optical anisotropy caused by external fields like stress. A compendium of all possible optically anisotropic coefficients had been compiled in the late nineteenth century by F. Pockels, one of the pioneers of the subject, in his book *Lehrbuch der Kristalloptik*. But in some very careful experiments by Don Nelson at Bell Labs, discrepancies were discovered between Pockels's predictions and experiments.

Nelson's studies of the coupling of light waves and acoustic waves led him to initiate a collaboration with Mel in order to develop a fundamental formulation of optical phenomena, including optical harmonic generation, piezoelectric coupling, and the photoelastic effect. The papers they produced on this topic were exhaustive and covered the range from macroscopic measurable consequences of the new effects to microscopic, atomistic force models of the origin of these effects. Nelson realized that they had shown that the photoelastic effect was dependent on the displacement gradient (which is a sum of the long wavelength strain and long wavelength rotation), not on the strain alone as in the Pockels theory. This led him to measure the effect by Brillouin scattering experiments, first in rutile and then in calcite, resulting in the discovery of the discrepancies noted above, which were in dramatic agreement with the new predictions of their theory.

The rapidly growing interest in electrical response of semiconductors to high electric fields resulted in much new experimental information about nonlinear conductivity in such systems. This topic rapidly grew into a major subject of investigations: the effect of "hot electrons" on the nonlinear conductivity. With characteristic focus Mel carefully investigated analytical and numerical techniques for solving the coupled Boltzmann transport equations for the distribution functions of the hot electrons, holes, and acoustic and optic phonons.

Starting in the mid-1990s much work in semiconductors began on low dimensional systems. A typical example is the quantum well in which electronic carriers are confined to two-dimensional regions, such as thin layers or surface regions. Mel invented a way to deal with the interaction of three-dimensional phonons with the confined electrons. His key and novel idea was to introduce a wave packet of coherent phonons whose envelope function is determined by the confined electron wave function. This formalism enabled a realistic calculation showing that there is a reduction of one order of magnitude in the energy transfer from the electrons to the phonons when the phonon distribution has been "heated." A novel prediction of this theory, confirmed by time-dependent relaxation experiments when both electrons and holes are present, is that the system will exhibit negative mobility.

When Mel accepted a post at City College in 1971, he retained a position at Bell Labs as a consultant to the electronics division, where he collaborated with many experimental groups, particularly that of V. Narayanamurti on high field effects on semiconductor transport.

CCNY

We now turn to the period after Mel joined the City College (CCNY) of the City University of New York (CUNY) as Distinguished Professor of Physics. Since its founding in 1847, City College had been noted for excellence in teaching. Many outstanding physicists (as well as chemists, future physicians, and others) had passed through as undergraduates majoring in the sciences. Indeed, seven Nobel laureates are included in that group. However, by the 1960s it was clear that the Physics Department was in urgent need of moving forward to include Ph.D.-level work. And so in 1967 the Physics Department at the City College of New York, then chaired by Harry Lustig, won a competitive National Science Foundation departmental development grant to transform its undergraduate Physics Department into a research focused Ph.D.-level department, including the creation of three Distinguished Professorships. In 1970 CCNY President Robert Marshak recruited the first two City College Distinguished Professors (Mel Lax and Bunji Sakita), and Mel recruited the third (one of us: H.Z.C.). Mel served as Distinguished Professor of Physics at City College from 1971 until his death. Mel quickly set about recruiting other new faculty members in condensed matter theory and experiment, including the authors of this memoir, and Harry Swinney. This triple hire was code-named "the BCS package" at CCNY. Because of Mel's efforts and those of other faculty (some of whom, like Myriam Sarachik and Robert Alfanos, had joined CCNY earlier), the condensed matter physics activity at CCNY took a major step forward.

Mel quickly adapted to the academic life. He enthusiastically began teaching basic graduate physics courses, such as electrodynamics and quantum theory, and he also developed his own variety of specialized lectures while continuing his research programs, now with his students and postdoctoral research associates. Many of them have since gone on to major academic, industrial, and government laboratory careers in the United States and overseas.

At City College Mel taught and carried out theoretical research in condensed matter physics, laser physics, coherence and fluctuations in classical and quantum systems, and nonlinear interaction of light and sound in solids. And he developed a new direction for his investigations: foundations of quantum mechanics.

His City College colleagues considered Mel the complete and ideal colleague. He prepared his lectures with exceptional care and delivered them clearly to his students. In many of the advanced courses that he taught he prepared extensive and detailed notes, which he photocopied and distributed to the students. This was a considerable effort and was an enormous benefit to his students. He was always available to students, colleagues, and coworkers for lengthy discussions, including phone and e-mail exchanges, and he shared his ideas and insights freely and graciously. All this took a great amount of his time, which he cheerfully gave. He served on many departmental, college, and university committees, such as the University Committee on Research, where he chaired the All-University CUNY Physics Faculty Research Award Program for several years. He was a member and chair of many departmental promotion and tenure committees, including committees on promotion to distinguished professor of the university. He brought to this work the very highest standards, tempered with a deep understanding of the strengths and limitations of the candidates. When Mel supported appointing a new faculty member or awarding tenure or promotion, it was universally accepted that this action was well merited.

During his last years Mel worked in the area of inverse scattering techniques needed to extract information from noisy measurements. Applications included the use of light scattering to study clouds using lidar techniques, searching for oil-bearing layers using acoustic backscattering, and the detection of possible tumor nodules in the human breast using pulsed, noninvasive infrared light. In carrying out this research Mel and his colleagues reexamined the Boltzmann transport equation. Scientists had tried for decades to develop the analytical solution of the classic Boltzmann transport equation. Mel developed and extended the theory of light scattering and transmission through strongly turbid media.

He developed algorithms to extract meaningful signals that could enable differentiation of different constituents contributing to the scattered light. The objective was to determine different responses from malignant and healthy tissue. With R. R. Alfano, W. Cai, and Min Xu, Mel developed the analytical theory to the extent possible and then the algorithms and codes needed for detailed numerical analysis. Not surprisingly some of this work was an echo of his earlier work on light scattering in the dense atmosphere with inclusions of particulate matter. His new methods for the solution of Boltzman's equation by cumulant expansion was among the last projects he completed before his death.

At his death all his colleagues at City College shared the profound feeling of having lost a unique and irreplaceable colleague who played a key part in bringing the Physics Department at City College to national and international prominence during the years starting in the mid-1970s. Equally, his colleagues are deeply grateful to have had the opportunity to work with him during this exciting period in the life of the CCNY Physics Department.

PERSONALIA

Mel took deep pride in his family. Although he was relatively reticent about most domains of his life, he was quite outspoken with pride in the achievements of his wife, Judy, and their children and grandchildren. Mel was an enthusiastic tennis player and played regularly in the evenings. In December 2001, while driving home from a night tennis game, he suffered a massive stroke. By the following June he had recovered the ability to walk although he did not regain his full cognitive abilities. With his research associate, Wei Cai, and former students, Min Xu and Boris Yudanin, he was energetically trying to complete two monographs: *Random Processes in Physics and Finance,* and *Quantum*

Optics. At the time of writing this memoir (spring 2005) the first of these, *Random Processes in Physics and Finance* by Melvin Lax, Wei Cai, and Min Xu has been completed and will be published by Oxford University Press.

In the summer of 2002 Mel was found to have inoperable cancer, to which he succumbed peacefully on December 8, 2002.

Mel is survived by his wife, Judith; his daughters, Laurie and Naomi; his sons, David and Jonathan; and five grandchildren, Eric and Lena Lax, Hannah Kober, and Dahlia and Orli Katz.

SERVICE AND HONORS

Mel was always prepared to offer service to the physics community. His early work for the American Physical Society as chair of its Publications Committee was instrumental in creating the present system of electronic submission and processing of manuscripts, which is now practically universal. Mel served on the editorial boards of *Physical Review* and of *Quantum Optics*, as a member of the advisory board of World Scientific's *Modern Physics Letters B* and *International Journal of Modern Physics*, and as editor of *Advanced Series in Applied Physics*. He was a member of the Basic Research Advisory Committee of the National Academy of Sciences and provided scientific services to the Naval Research Laboratory, Los Alamos National Laboratory, the U.S. Army Research Office, and the U.S. Department of Energy.

Mel was elected to the National Academy of Sciences in 1983 in recognition of his many contributions to science. He served as secretary of Class III (Engineering and Applied Physical and Mathematical Sciences) 1989-1992 and 1995-1998. He was a Fellow of the American Academy of Arts and Sciences, the American Physical Society, the American Association for the Advancement of Science, and the Optical

Society of America. In 1999 he received the Willis Lamb Medal for Laser Science and Quantum Optics, together with Lorenzo Narducci and Herbert Walther. In addition to his long-term faculty appointments at Syracuse University and the City College of New York, Mel also taught at Princeton (spring 1961) and Oxford (1961-1962) and gave many lecture series around the world: in Vancouver, Tokyo, Trieste, Florida, Varenna, Israel, Kyoto, Beijing, Lausanne, and New Mexico.

IN WRITING THIS MEMOIR we drew extensively on autobiographical material in the National Academy of Sciences' files by Mel, as well as material by Mel and by P. W. Anderson collected in the volume *CCNY Physics Symposium: In celebration of Melvin Lax's Sixtieth Birthday*.[1] We also acknowledge contributions of material by Mel's family and colleagues, including Judy Lax, David Lax, Eric Lax, Eli Burstein, Wei Cai, Harry Frisch, Charles Henry, John Hopfield, Donald Nelson, Takashi Odagaki, and Michael Lubell.

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LEONARD MANDEL

May 9, 1927–February 9, 2001

BY ROBERT J. SCULLY, MARLAN O. SCULLY, AND
H. JEFF KIMBLE

LEONARD MANDEL WAS the Lee DuBridge Professor of Physics and Optics at the University of Rochester: masterful scientist, exemplary teacher, generous colleague, and beloved family man. He is widely credited with being one of the founding fathers of the field of quantum optics, which sprung from the marriage of quantum mechanics and optics in the 1960s into one of the most exciting areas in modern science. He made seminal contributions to experimental and theoretical problems of optical coherence, laser physics, and quantum optics, including laser phase transitions, locality violations in optics, tests of quantum mechanics, and non-classical states of light. A central theme of his research was a continuing quest to explore and elucidate the quantum character of light by way of insightful theoretical analyses and a set of pioneering experiments that have become landmarks in the field. He deepened our understanding of quantum mechanics in important and lasting ways through ingenious experiments that provided convincing demonstrations and precise tests of many of the most counterintuitive aspects of the quantum nature of light. Rarely has any one individual so intimately investigated and so dramatically advanced our understanding of the quantum mechanical nature of light.

Although Leonard began his research career in cosmic-ray physics in the late 1940s, he soon developed an interest in light and how it was measured. Using his knowledge of particle detectors to advantage and intrigued by the 1956 experiment of H. Hanbury Brown and R. Q. Twiss, he early on made important contributions to the field of photon-correlation spectroscopy, in which correlations between counts in independent photodetectors yield information about the source of light (e.g., about the diameters of stars). He then went on to make some of the earliest measurements of interference between independent lasers.

These experiments raised the hackles of some physicists at least in part because of Nobel Laureate Paul Dirac's often quoted statement that "photons only interfere with themselves," suggesting that photons from independent sources do not interfere. In fact these experiments demonstrate something deeper, namely the existence of higher order correlations of the radiation field that were not yet thought of when Dirac made his statement. Over the course of his career Leonard devised ingenious experiments to explore the quantum statistics of light and thereby to advance profoundly our understanding of its quantum character.

In 1977 Leonard's quest led to the pioneering demonstration of the manifestly quantum, or "nonclassical," nature of light by way of the first observation of photon antibunching, here for the fluorescent light from a single atom. These measurements are widely regarded as having ushered in a new era in quantum optics involving nonclassical effects. Moreover, the same techniques are now employed as a definite test to identify individual quantum emitters in diverse physical systems and to characterize nonclassical light for various applications, including for secure quantum communication schemes.

In the early 1980s Leonard initiated what would become a landmark set of experiments involving pairs of photons produced by the fission of a pump photon in the process of parametric down conversion, including the creation of a localized one-photon state. He developed new experimental tools based upon parametric down conversion to produce seminal and striking experimental proofs of some of the most unsettling aspects of basic quantum mechanics. Leonard showed that Dirac's well-known statement about single-photon interference must be modified to assert that "in fourth order interference, a pair of photons interferes only with the pair itself." In a classic experiment with photon pairs in 1995 he demonstrated with brilliant clarity a central tenet of quantum mechanics, namely, that there is no physical reality for elemental quantum processes in the absence of a measurement. Leonard's ability to identify critical issues and make their consequences manifest are evidenced in many other important contributions to both classical and quantum optics.

Mandel was honored by his peers, receiving awards that included the Frederic Ives and the Max Born medals of the Optical Society of America, the Thomas Young Medal of the British Institute of Physics, and the Marconi Medal of the Italian Research Council. He was elected to the New York Academy of Sciences and to the American Academy of Arts and Sciences. Shortly after his death he was posthumously elected to the National Academy of Sciences.

EARLY LIFE

Leonard Mandel was born in Berlin on May 9, 1927, the only child of Naftali and Rosa Mandel. Naftali had grown up in Poland, but his parents sent him to Berlin when he was 16 because of their concern that he would be killed if conscripted into the Russian army. From a penniless beginning and with the help of a stranger, Naftali was ultimately able to start a successful business of his own. Leonard's mother, Rosa, led a very different life. She was one of 12 siblings and grew up in a close-knit family. Of the 12 children in her family, only Rosa, three of her sisters, two brothers, and their families survived the Holocaust. Of his siblings, only Naftali survived.

In his early life in Berlin, Leonard was able to stay focused on his studies and excel in school, despite the violent anti-Semitism he was beginning to experience. He was deemed a gifted violinist but realized at a young age that his life's interests lay elsewhere, although he did continue to play for pleasure throughout his life. Leonard's family was pursued by the Gestapo; however, with the help of Rosa's sister, who was already in England, they escaped to London, leaving most of their belongings behind. While his parents struggled to rebuild their life, Leonard lived in a hostel with other Jewish refugee children who were evacuated to Bedford for safety when the war started. Some years later, Leonard and his parents moved to Llangollen in North Wales. It was here that he had his first physics lessons. He had never encountered a subject that excited him so much, and he knew right away that he wanted physics to be part of his life. He showed exceptional aptitude, winning the science prize and prompting his physics teacher to write on his report card, "Surely this boy is a genius." The family returned to London, only to be greeted by Flying Bombs and V-2s, and spent their nights sleeping on a subway platform. Back in London, Leonard attended the William Ellis School, a distinguished boy's grammar school, where he excelled academically and also became the most honored student in the school (as the head boy).

Because he had such an outstanding school record, his teachers strongly supported his decision to apply for a Cambridge scholarship and fully expected him to get one, but he was turned down. He was not eligible for several other scholarships because he was not British born. It was then that his physics teacher, Miss Shavelson, to whom he was forever grateful, suggested that he apply to Birkbeck College, University of London, where he could become financially independent by working during the day and taking classes during the evening. Leonard was accepted as a student and also employed as a lab assistant in the physics department. In two years he earned his B.Sc. in mathematics and physics, graduating with first-class honors in 1947. In only one year more (rather than the usual two), he obtained a B.Sc. special in physics, also with first-class honors. He then joined the Cosmic Ray Group of E. Paul George to work toward his Ph.D., having previously assisted in building equipment for this group.

Leonard's graduate studies were in the field of cosmic-ray physics, which required him to construct and transport delicate particle detectors to an observatory at the Jungfraujoch high in the Swiss Alps. In the Alps he not only enjoyed working but also relished the opportunity to ski and to climb mountain ridges alone. On one of these escapades across a glacier, he surely would have been lost for good sliding toward a precipice were it not for his trusty penknife, which is a story his own students would hear told matter-of-factly in years to come. Leonard completed his Ph.D. in only three years, graduating from Birkbeck in 1951 with a thesis titled "Interactions of Non-Ionizing Cosmic Ray Particles." He was always grateful to Paul George for giving him the freedom to work on his own ideas in his own way. Throughout his own career Leonard enabled his students by entrusting them with the same faith and confidence in their abilities that George had shown in him.

After obtaining his doctoral degree, Leonard joined the Research Laboratories of Imperial Chemical Industries Ltd. in Welwyn Garden City, where he was appointed technical officer and published his first paper.[1] Even in this earliest paper it is possible to glimpse the origins of what would become a lifelong interest in stochastic processes in the paper's discussion of limitations to measurement accuracy due to noise from Geiger tubes. After a few years at ICI, Leonard decided to return to academia but had a hard time convincing colleges that he was willing to take a large cut in salary to do so. He was very pleased when he was offered a lectureship in the Physics Department at Imperial College, where he moved in 1955, later becoming a senior lecturer.

While a graduate student at Birkbeck College, Leonard met his future wife, Jeanne. She was a physics undergraduate student who had heard that he was the best student advisor around, and who then learned first-hand that this was true. In her own words, "His answers to my questions were so clear and simply explained that I was left wondering why I had ever thought there was a problem." Jeanne later worked as lecturer demonstrator and printed all the photographs for Leonard's doctoral thesis, where she experienced first-hand his perfectionist nature.

Leonard and Jeanne were married in 1953. Their courtship was cemented through parallel academic interests and similar hobbies. They excelled at playing doubles in table tennis, enjoyed ice skating together, and did very well in ballroom dancing. Their daughter, Karen Rose, was born in 1956, followed by a son, Barry Paul, in 1959.

During the years at Imperial College, Leonard began what would become a lifelong association with Emil Wolf, who by then had moved from the University of Manchester in England to the University of Rochester in New York. Jeanne relates the story of Emil and Leonard working at the Mandel home on a joint paper late into the night to finish before Emil's departure back to Rochester. Emil explained how much easier the collaboration would be if only Leonard would move to the United States. He continued to press his case, and in 1960 and again in 1963 the Mandel family spent a few months in Rochester. During these visits, Leonard realized that here he would be much freer to pursue his own lines of research. In 1964 Leonard was offered a professorship in the Department of Physics at the University of Rochester, where it was hoped that his appointment would strengthen a new research group in the rapidly developing field of optical coherence. It was a difficult decision for the family to leave England for the United States, but in 1964 Leonard left Imperial College to become professor of physics at the University of Rochester, where he remained until his death in 2001.

PROFESSIONAL HISTORY

Although Leonard's initial research had been in nuclear physics, his interest in optical coherence was stimulated by the Brown-Twiss experiment in 1956. Leonard's entrance into the debate about the appropriate theoretical description of this experiment were classic papers in 1958-1959 in which the so-called "Mandel formula" for photoelectric counting made its first appearance.[2] This work first, and firmly, established the relationship between the classical statistical properties of incident radiation and the emitted photoelectrons. It has become a mainstay of photon-counting measurements.

Soon after his move to the University of Rochester, Leonard and his student R. Pfleegor reported in 1967 a now famous experiment involving interference at the single photon level.[3] More precisely, two beams derived from independent lasers were attenuated to a level such that the average interval between photon emissions was very long compared with the transit time through the apparatus. Even though each photon was absorbed at the detector long before the next one was emitted, interference fringes were nonetheless recorded. Leonard concluded that it is better "to associate the interference with the detection process itself, in the sense that the localization of a photon at the detector makes it intrinsically uncertain from which of two sources it came." In an ingenious extension of the Brown-Twiss experiment, Leonard employed a detection scheme exploiting interference of fourth-order in the amplitudes of the relevant fields, as opposed to the more familiar second-order interference (e.g., as in a Michelson interferometer) that would have been absent here. Over the next three decades, Leonard continued to refine fourth-order interferometry into an extremely powerful tool for investigations of the quantum character of light, so much so that it is now a standard technique employed by groups worldwide.

The growth of quantum optics was triggered and sustained by the invention of the laser. With the hindsight of history, it may be surprising that there was considerable controversy about the quantum statistical properties of the laser and other light sources in which Leonard and his colleagues George Sudarshan and Emil Wolf were actively involved.[4,5] Perhaps spurred by this debate, Leonard and his students F. Davidson, D. Meltzer, and others were among the first to carry out basic experiments to elucidate the coherence properties of the laser in the transition from incoherent to coherent emission around threshold.[6,7,8] Carried forward by several successive generations of graduate students, these early studies of the light from a single-mode laser grew over the ensuing decades into diverse investigations of more complex laser behavior, including mode correlation and competition in ring lasers, and optical bistability and first-order phase transitions in dye lasers.[7,8]

Although quantum theories are sufficient to understand the laser and general optical interactions, it is nonetheless the case that the statistical properties of optical fields can be described almost without exception in terms of so-called semiclassical theories in which the material system is quantized but the field is treated classically. Since the earliest days of quantum optics, much attention has thus been directed to finding physical processes that generate nonclassical fields and to identifying unique experimental signatures. Through diverse theoretical analyses, Leonard played a vital role in this effort, including the prediction that the fluorescent light from a single atom would exhibit "photon antibunching,"[9] which is a manifestly quantum effect derived independently by H. J. Carmichael and D. F. Walls. Leonard and his students H. J. Kimble and M. Dagenais carried out an experiment to investigate this effect, and in 1977 succeeded in making the first observation of photon antibunching.[10] Subsequently, R. Short and he made the first measurements of another nonclassical effect, namely sub-Poissonian photon statistics, again in the setting of single-atom resonance fluorescence.[11]

The observation of photon antibunching by Leonard and his students is widely regarded as having initiated a new era in quantum optics. Beyond being simply a matter of semantics, such nonclassical fields are required to achieve measurement precision beyond the standard quantum limits, and more generally form the basis for advances in quantum information science related to quantum computation and communication.

In further investigation of nonclassical light, Leonard recognized in the early 1980s the tremendous potential associated with the process of parametric down conversion, in which a single pump photon is split into a pair of "daughter" photons. His insight came in large measure from his own theoretical investigations, including work on configuration-space wave functions for photons[12] and on two-photon interference.[13] Over the next two decades, Leonard and his students would achieve a dazzling set of experimental advances related to nonclassical states of light, to two-photon interference, and to quantum entanglement, which he has reviewed.[14] This work continued to advance and refine the theme of fourth-order interference that Leonard had previously employed but now with pairs of photons.

In his early work on parametric down conversion, Leonard and his students S. R. Friberg and C. K. Hong first demonstrated the nonclassical character of the down conversion process[15] and were thereby able to achieve a localized one-photon state.[16] They further made the first demonstration of quantum cryptography with correlated pairs of photons.[17] With Hong and Z. Y. Ou, he introduced what is known today as the Hong-Ou-Mandel interferometer.[18] In an ingenious experiment with Ou, Leonard utilized polarization entanglement for photon pairs produced in down conversion to achieve a violation of Bell's inequality (i.e., local realism).[19] In work with Ou, L. J. Wang, and X. Y. Zou, he systematically explored other aspects of quantum entanglement, including phase memory due to entanglement with the vacuum,[20] frequency entanglement to produce spatial quantum beating,[21] and phase entanglement to achieve nonlocal interference in separated photon channels.[22]

With these advances, Leonard played a pioneering role in laying the foundations for modern research in quantum optics and quantum information science, including for Bell-state detection to enable diverse tasks, such as quantum teleportation of the polarization state of single photons. The realization of quantum computation by way of linear optics and photon detection incorporates the Hong-Ou-Mandel interferometer as a ubiquitous element.

Throughout the 1990s Leonard continued a prodigious output of remarkable experiments to advance our understanding of quantum measurement, of nonclassical field states, and of nonlocal effects. In 1991 Leonard and his students L. J. Wang and X. Y. Zou reported what has since been referred to as a "mind-boggling" experiment involving interference from the superposition of fields from two different but coherent parametric processes.[23] In this experiment the mere *possibility* of making a measurement was shown to destroy an interference pattern, even if this possibility went unrealized and absent any external "disturbance." Following the theory of M. O. Scully in 1982, Leonard realized that this experiment could also be interpreted as a realization of a "quantum eraser," in which information contained in one photon, or the erasure of this information, could change the way we viewed another photon.[24] In another experiment Leonard and his students D. Branning and J. Torgerson and research associate C. Monken made a striking demonstration of one of the most unsettling aspects of the quantum realm, namely, that there is no physical reality in the absence of a measurement.[25]

Regrettably, it has not been possible in this biography to offer more than these few glimpses into the set of topics encompassed by Leonard's remarkable career.[7,8] We might just mention that in addition to the study of nonclassical fields, he was also deeply interested in the question of quantum phase for the electromagnetic field and, together with his students A. Fougeres and J. W. Noh, developed an operationally based theory and carried out extensive experiments to test this and other competing theories.[26] Leonard's ability to identify critical issues and make their consequences manifest are evidenced in many important contributions, including his fundamental analysis of photon cloning[27] and his work on cross-spectral purity, concerning a criterion ensuring that the spectrum of light remains unchanged on superposition in an interference experiment.[7] With his last student, A. Kuzmich, and his colleague N. Bigelow he initiated a new line of research related to nonclassical states for a collection of atomic spins, which led to one of the first demonstrations of "spin squeezing,"[28] thereby helping to initiate what is by now a very active area of research worldwide.

Leonard supervised the thesis research of 39 students, many of whom subsequently became leading figures in diverse areas of science and technology. He earned the lifelong respect of his students for his detailed and conscientious stewardship of their scientific development. Research problems were chosen to fit a particular student's capabilities with an eye toward improvement with each experience. Most often twice per day he made the rounds of his laboratories to interact with each of his students, with help ranging from optical alignment to electronic design to theoretical calculations. Not wanting to interrupt a student at work, he would sometimes quietly approach in his soft-soled shoes to view the activity at hand, leading to more than a few startled heart skips. In response to a student's query ("Can you see a signal yet?") as some component of an apparatus was being aligned, would come his distinctively British reply, "Not a sausage!" Once when the need arose to regulate the water cooling for an oil diffusion pump, he arrived the next morning with a flow switch commandeered from a washing machine at home, which did indeed solve the problem.

Notes with calculations or designs given to him were promptly returned with extensive annotations (corrections of grammar as well as physics), most often the following morning. At least one student carried these exchanges over the Queen's English forward into later life by sending Leonard a letter with a dangling participle hidden in the text only to receive a prompt reply with the postscript, "I found it!"

In addition to his work at the frontiers of physics, Leonard established a reputation as an excellent teacher at all levels of the university curriculum. He had a special interest in neophyte science students and developed the first course at the University of Rochester for nonscience majors, which he taught regularly for 20 years. In 1992 he received the university's Faculty Award for Graduate Teaching.

Leonard published about 300 papers, was the coauthor with Emil Wolf of a comprehensive book of more than 1,100 pages entitled *Optical Coherence and Quantum Optics,*[7] and was the coeditor of several conference proceedings. In 1980 his paper on coherence properties of optical fields coauthored with E. Wolf in 1965 was designated a Citation Classic by the Institute of Scientific Publications.[5] In 1988 the paper was listed as one of the 100 most cited articles published in the *Review of Modern Physics* since 1955. Beyond the many top awards that he received, during the period 1966-1995 he was one of the main organizers of the Rochester Conferences on Coherence and Quantum Optics.

An account of Leonard's career and of his scientific contributions and the enumeration of the many honors he received is only a part of the story. What it does not bring out is that Leonard was a scientist with a warm personality, humility, and compassion and with a delightful sense of humor. He enjoyed a happy family life with Jeanne, a ballet teacher and with whom he shared the love for ballet, and with their two children, Karen and Barry. He had a warm and close relationship with his children and four grandchildren, always finding time to become actively involved in their lives. He was never too busy to help with homework, teach table tennis, go to concerts and sports events, or simply to sit down and discuss anything that interested them. He only gave advice when asked. They all remember the many family dinners when he would think of a problem and, with a twinkle in his eyes, give it to them and then listen as they came up with some interesting but sometimes unlikely solutions.

All who were fortunate to have been associated with Leonard Mandel will always remember him with deep affection and utmost respect. His example as mentor, colleague, and friend will vividly continue to shape our own lives. His career provides a standard for accomplishment and integrity that exemplifies the best of a noble profession. He is greatly missed.

THE AUTHORS ARE grateful to the Mandel family for sharing many insights into Leonard's life. H.J.K. acknowledges the contributions of Emil Wolf, who coauthored an obituary, which appeared in the August 2001 issue of *Physics Today,* as well as the contributors to the symposium honoring Mandel,[8] upon which some parts of this article are based.

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RICHARD DRUMMOND MCKELVEY

April 27, 1944–April 22, 2002

BY THOMAS R. PALFREY

RICHARD MCKELVEY DIED TOO young, on April 22, 2002, at the age of 57, and the social sciences lost a great scholar. He was a deep, creative thinker who set the standard for mathematical rigor in political science and also contributed in major ways to other fields. He was a central intellectual figure in the first generation of formal political theorists and was on board at the embarkation of the exciting new field of positive political theory. Positive political theory applied and further developed the sophisticated tools of game theory and social choice theory to the analysis of politics and political phenomena. Like several others, that generation was largely a product of the Rochester school, the great legacy of Bill Riker. McKelvey helped spread that legacy.

He was best known for a series of pathbreaking papers on the mathematical theory of voting in the 1970s, but he also made significant contributions to the application of statistical techniques to the analysis of political science data, social choice theory, computational techniques in economics, experimental economics and political science, and game theory. He was a scientist in the true sense of the word, developing intricate theoretical models of politics, testing his and others' theoretical models in the laboratory, learning from his experimental findings, and building new theories and models based on these findings. While his early theoretical papers on voting are what most political scientists associate with McKelvey, the rigorous interchange of theory and data was the true hallmark of McKelvey's career.

FAMILY BACKGROUND AND FORMATIVE YEARS

Richard McKelvey, or Dick, as some of his friends called him, was born in Geneva, New York, on April 27, 1944. The second of four sons of John McKelvey Jr. and Josephine ("Jo") McKelvey, he was raised in a family that valued and nurtured intellectual and scientific pursuits. Whether a result of genetic structure or simply from growing up in a close family environment where knowledge and understanding were cherished, it is not surprising that Richard developed into the great scientist he was.

John was an agronomist specializing in plant pathology. He and Jo attended Oberlin (class of 1939). John went straight to graduate school, writing a master's thesis at the Virginia Polytechnic Institute under the soon-to-become-distinguished agronomist J. George Harrar, with whom he later worked as an assistant. He obtained his Ph.D. in economic entomology from Cornell in 1945, shortly after Richard was born—perhaps a sign of things to come from the next generation. Jo received her degree in Classics and later earned a master's degree in library science (Columbia, 1971). She enjoyed a second career for two decades at the Chappaqua public library, following her successful first career raising the four boys.

The intellectual heritage goes back at least a generation further, to his paternal grandfather, John Sr., who graduated from Oberlin in 1884 and was cofounder of the *Harvard Law Review* in 1886, becoming its first editor in chief. He authored *McKelvey on Evidence,* among other influential law texts. How apropos, given his grandson's later passion for confronting theories with data. Richard's maternal grandfather, Edward Faulkner, wrote extensively on the economics of soil management and was best known for the classic *Ploughman's Folly*.

The family moved to Mexico City in 1945 when John took a position with the Rockefeller Foundation to study how to control pests that damaged such grain crops as corn and wheat. These were very exciting times in agronomy, and the Rockefeller Foundation project in Mexico produced pathbreaking developments in hybrid wheat and corn, leading to Norman Borlaug's 1970 Nobel Peace Prize. Richard spent the first seven years of his life there, before the family moved back to New York, this time in Chappaqua, where he lived until college.

Richard achieved success despite difficulties early in his school life. His academic performance in Mexico was less than stellar. This continued throughout grade school, where he also suffered socially as the smallest kid in the class.

In junior high school Richard's scholastic interests drifted toward mathematics, and in high school he found a mentor. He wanted to take an advanced high school math class taught by an eccentric but brilliant and dedicated math teacher, Edwin Barlow, but his grades didn't qualify him. So he spent the summer boning up on math and was accepted in the fall of his junior year. Barlow was so impressed with Richard's math skills that several years later he urged him to return to Chappaqua and teach math.

Richard was involved in an array of extracurricular activities. He rose to the rank of life scout, played trombone in the band, and even pole-vaulted for the track and field team. The latter activity didn't earn any medals but did produce a broken arm in a crash to the ground, possibly the cause of a subsequent collapsed lung.

But what Richard liked to do most in his spare time growing up was to solve puzzles and games, sometimes inventing them. He also engaged in pranks. In one that became a family legend, he swiped Girl Scout cookies from a box being sent to his older brother in college, substituting macaroni, a substance carefully selected for weight and the noise it made when shaken.

HIGHER EDUCATION

Following in the footsteps of his parents, grandparents, and several aunts and uncles, Richard enrolled at Oberlin College in 1961. His interest in mathematics continued to focus his studies. His inventiveness apparently did not let up either. He either invented or heard about an exciting gaming adventure in the form of a human random walk (literally), which he called a "penny hike." At every intersection, he or his friend flipped a coin, turning left on heads and right on tails. This was probably his first experiment in decision making under uncertainty, and clearly an early indicator of his budding interest in stochastic processes (and mixed strategies). Eventually the experiment had to be abandoned when the coin appeared to be sending them to Alaska. It is perhaps worth noting that in his laboratory experiments Richard frequently preferred to randomize using dice, coins, and bingo cages, even after the advent of computers.

A second creation was a light-switch device designed to ensure that the lights in a room were always on when there was at least one person in the room and always off when the room was empty. This invention, which used electric eyes connected to counters, went through several phases with borderline success.

Following his graduation from Oberlin (in 1966), and a year obtaining a master's degree in mathematics at Washington University (in 1967), Richard enrolled in the Ph.D. program in political science at the University of Rochester. Great things were happening under the direction of Bill Riker, and it was at Rochester where Richard found his calling in science. Kenneth Shepsle (one year ahead of Richard in the Ph.D. program) recalled how Riker had expressed great enthusiasm about McKelvey's decision to enter the program, and the graduate students were all very excited about meeting this new hotshot. When Richard arrived in the fall and they finally met him, they were surprised at his modest, unassuming demeanor and wondered whether he could really be that good. Indeed he was, as they quickly discovered.

A revolution was under way, and some of the best minds in political science were perpetrating it. In fact, the list of students and faculty during Richard's seven years there is a virtual who's who in positive political theory and the positivist approach to substantive subfields of political science: Peter Ordeshook, McKelvey, Bill Riker, Ken Shepsle, Mo Fiorina, Dick Fenno, Bruce Bueno de Mesquita, John Aldrich, David Rhode, Bing Powell, Jerry Kramer, and others.

CARRYING THE TORCH FROM ROCHESTER TO CARNEGIE TO CALTECH

Before receiving his Ph.D. in 1973, Richard took a position at Rochester first as an instructor and later as an assistant professor. In his brief tenure there, Richard mentored several students who went on to distinguished careers, including John Aldrich and Keith Poole.

McKelvey moved to Carnegie Mellon in 1974, which at the time was second only to Rochester as a hotbed of positive political theory. Ordeshook, Howard Rosenthal, Toby Davis, and Melvin Hinich were the principal actors, and it was a different sort of place than Rochester. For one thing, a number of economists at Carnegie's Graduate School of

Industrial Administration and School for Urban and Public Affairs were excited about positive political theory and joined in the fun. Modern political economy was more or less created at Carnegie during the 1960s and 1970s. It was during this period that McKelvey and Ordeshook began their pioneering collaboration in laboratory experimentation, a collaboration that lasted for more than a decade. At the same time, McKelvey was busy proving and publishing his fundamental theorems about the instability of majority rule.

Richard arrived at Caltech in 1978 and spent his first year there as a Fairchild distinguished scholar. Positive political theory now had another outpost in sunny Southern California. I know from my own experience there as a graduate student how exciting those times were. Charlie Plott, John Ferejohn, Mo Fiorina, Roger Noll, Gary Miller, Bob Forsythe, and McKelvey were doing positive political theory and laboratory experiments. As students, several of us had the good fortune to collaborate with them and learn this new methodology alongside them.

Except for that year as a Fairchild scholar, Richard never took a sabbatical from teaching during his entire 31-year career. He was a completely dedicated teacher and scholar. He taught classes until one week before he died in spite of a long and serious illness, which had become debilitating in the last two months. One year at Caltech, he gave up a month of summer salary in order to have money in his grant to pay for a graduate student. At Caltech, Richard produced some of the most talented graduate students in political science of the next generation, including Jeff Banks and Gary Cox.

From Oberlin to Washington University, to Rochester, to Carnegie, and finally to Caltech, Richard had finally found the place that was ideal for him. He could work without much to sidetrack him administratively (except for a one-year stint as executive officer, which he really disliked), and he was surrounded by colleagues who spent their days the same way he did, having tremendous fun coming up with brilliant ideas.

DISTINCTIONS AND RECOGNITION

It seems odd to talk about the honors and distinctions that were bestowed on Richard for his many scholarly accomplishments. When he received such honors, the initial reaction was typically a combination of surprise and embarrassment. Of course he was very happy to be elected to the National Academy of Sciences in 1993, but I'm sure he never spent a minute of his time beforehand wondering whether it ever would be. The same is undoubtedly true for his election to the American Academy of Arts and Sciences in 1992 and fellowship in the Econometric Society in 1994. He was honored as a Rochester distinguished scholar at the Rochester commencement ceremonies in 1999.

He was surely delighted to be awarded a Fairchild Fellowship at Caltech in 1978, not for its prestige but because it meant he could spend the whole year doing research in a fertile intellectual environment. Caltech later awarded him the Edie and Lew Wasserman chair. He was also invited as a fellow at the Center for Advanced Study in the Behavioral Sciences, and surely would have accepted the invitation in due time, had health problems not intervened. Sadly, there are no awards for simply being a humble, unselfish, unassuming, and absolutely sincere and honest human being.

RICHARD'S CHILDREN

His children were a great joy to him, partly I guess because kids enjoy playing games so much more than most adults. The early years at Caltech were a difficult time personally, because of a divorce. His two young sons, Christopher and

Kirk, lived apart from him. Religious differences between him and his wife, and the pursuant implications over the next 10 years about how the boys were to be raised and schooled, were a source of conflict and family tension. Fortunately, this did not derail Richard either professionally or personally. He soon met and married his second wife, Stephenie Frederick, and they raised a daughter, Holly, as he continued to develop his relationship with his two sons. Holly is an outstanding student in high school. Both sons carried on the family tradition of higher education and professional careers. Kirk graduated from Oberlin College and became a computer scientist. Christopher obtained a Ph.D. in economics at the University of California, Los Angeles, and recently accepted an assistant professor position at University of Maryland. Richard was justifiably proud of all three children.

Christopher recounted to me many pleasant childhood memories, including games that Richard would invent for them to play. Of course, Richard believed in game theory and created incentives. For example the winner of the game would earn the right to order "anything" at a local ice cream shop, provided he could eat it on the spot. Christopher recalled some outrageously huge sundaes that he and his brother indulged in. He also recalled longer-term projects, such as building a glider, which went through cycles of crashing and patching.

Richard used a scheme on family vacations to decide where to eat each night. Each person was given a fixed number of otherwise worthless tokens at the beginning of the vacation. Every time a decision had to be made, each family member could use the tokens to bid for the right to be dictator on that decision.

He also proposed a fundraising scheme for his daughter's school based on incentives. Under his scheme, the school would ask all the families to state how much money they were willing to donate that year to the school. Then the school would announce the minimum stated donation, and each family would donate that amount. The idea behind Richard's "public good mechanism" was that if a family's donation was the minimum one, and there were 1,000 parents, then that family could raise an extra $1,000 simply by increasing its donation by $1. With this huge multiplier effect, one would expect a very high minimum donation. And of course, if a family's stated donation was not the minimum, then raising it wouldn't cost a cent. The school apparently rejected the scheme because it seemed too complicated and risky.

McKelvey also had a longstanding academic interest in the design of mechanisms for efficient, fair, and stable committee decision making. To Richard a mechanism is a useful gadget, and gadgets are fun to design (especially when they work). His academic research on jury voting mechanisms (2000), bargaining mechanisms (2002), implementation theory, and convergence of beliefs to common knowledge (1986) reflect several different dimensions of this interest.

As a final personal note, Richard's favorite holiday of the year was April Fool's Day, a day for which he would plan and orchestrate elaborate stunts and pranks on his friends and family. In fact, to celebrate his pranksterism, there were a number of pranks set up at his house after his memorial service: an upside-down jar filled with hundreds of marbles as well as examples of his esoteric projects. This included a mock up of the light-switch counter and his huge credit card collection: When he traveled around the world to conferences, he would seek out stores that offered free credit cards. He managed to amass thousands of them.

MCKELVEY'S SCIENTIFIC CONTRIBUTIONS

McKelvey's influential series of papers in the 1970s revealed in stark mathematical terms the instability of majority rule. The McKelvey chaos theorem shows that under very general conditions, majority rule exhibits global cycling. Condorcet had noticed almost 200 years ago that there exist majority rule cycles of three alternatives. That is, in some committees it may be possible that alternative A beats alternative B by majority rule, B beats C, and C beats A. This apparent voting paradox hardly seems likely to happen on the face of it, so for years it remained more of a curiosity than a fundamental result. What McKelvey (1976, 1979) showed is that it is not just an example but a pervasive phenomenon that almost always happens. For almost all committees, if the set of possible alternatives is rich enough, there exists an agenda (i.e., a sequence of majority votes between pairs of alternatives) that winds its way through the entire set of alternatives and ends up at the initial proposal.[1]

He proved this result constructively, so his results also had implications for agenda manipulation. His proof contained a recipe according to which a clever agenda setter could manipulate any committee in any desired way (1983). The proof itself sheds light on how McKelvey approached theoretical problems. He thought in a very detailed, algorithmic way and sought a physical or mechanical understanding of the model.[2] This may seem odd to some who saw Richard as esoteric and theoretical, a guy who wrote papers mired in notation, in complex argument, and who sometimes lectured to the board as he wrote down this entire notation.

At this same time in his career, he began his long collaboration with Peter Ordeshook, a collaboration best remembered for its pioneering work in laboratory experimentation, including some of the first tests of game theoretic models of voting in committees and game theoretic models of candidate competition. The most influential of the experimental projects with Ordeshook investigated the extent to which voter ignorance and informational barriers impede competitive (median voter) political outcomes. Through an ingenious series of experiments on spatial competition, McKelvey and Ordeshook (1984, 1985) pursued this issue from different angles. How much information is required of voters and candidates in order for median voter outcomes to arise? Not much. In a striking experiment, candidates knew nothing about voter preferences, and only a handful of voters in the experiment knew where the candidates had located in a single left-right policy dimension. Still, with polls and interest group endorsements voters were able to vote rationally. In a theoretical model of information aggregation adapted from the rational expectations theory of markets, they proved that this information alone is sufficient to reveal enough to voters that even uninformed voters behave as if they were fully informed. This is exactly what they observed in the experiment.

A later experiment (1987) explored whether median outcomes can arise purely from retrospective voting. Voters observe only the payoff they receive from the winning candidate after the fact—not even the platform adopted by the winning candidate or the platform of the losing candidate. There are no campaigns or polls. Voters either reelect the incumbent or elect an unknown challenger. Candidates are better informed: They observe all the platforms that their opponent has adopted in the past, as well as the past election results. But candidates are given no information about the distribution of voter ideal points. Even with such limited information, median voter outcomes tend to occur.

Laboratory experimentation became a huge part of McKelvey's career, and he helped found and later became director of the Hacker Social Science Experimental Laboratory at Caltech. (Richard was delighted to be associated with a computer lab named "Hacker.") He branched out to study a wide variety of political phenomena in the laboratory, including bargaining and negotiation, different voting rules for juries, information aggregation, political models of economic growth, and many abstract games—some invented by him.

Among his many significant contributions to game theory, two stand out. One is the computational project called "Gambit" (http://econweb.tamu.edu/gambit/), a computer program to compute Nash equilibria in games, later extended to compute sequential equilibrium in games. That project began in the early 1990s in collaboration with Andrew McLennan and Ted Turocy, and the program is widely used today. It represents the state of the art in computer programs to solve for equilibrium points games.

In keeping with Richard's inventive spirit, he believed that game theory really needed a tool to find all the equilibria to any game. In the late 1980s Richard contemplated a mid-career switch to computer science. He loved programming (just as he loved anything algorithmic) and actually spent a lot of time writing various programs to explore examples that he could not solve. Basic research in computer science had a certain appeal to him, and I have no doubt he could have succeeded in that field, too. Instead, he undertook a gigantic programming project. As the examples and applications (and programs) for computing equilibria grew more and more elaborate, such as the numerical solution to perturbed equilibria in repeated centipede games (1992, 1993), it was clear to him that a general game-solving program would be a valuable addition to the game theorist's toolkit.

Ted Turocy was a Caltech undergraduate student at the time and took a year off before graduate school (Northwestern) to work full time on the project, together with Eugene Grayver, a Caltech undergraduate.[3] Andy McLennan shared Richard's interest in the mathematical properties of the set of Nash equilibria as well as his interest in designing efficient algorithms to compute that set. They also wrote some theoretical papers on the number of mixed Nash equilibria, but the most important product of their collaboration was Gambit. Once the first phase of the project was done, which is a graphical interface for entering and solving specific games, the team developed a programming language (Gambit Command Language) so that one could solve for equilibrium correspondences in families of games.

The second fundamental contribution was the development of Quantal Response Equilibrium (QRE), a statistical model of equilibrium in games that significantly generalizes Nash equilibrium, and a tool for the statistical analysis of game theoretic data. Many of Richard's later experimental papers explored the theoretical properties of quantal response equilibrium and the testing of that theory in experimental games where the Nash equilibrium made starkly different comparative static predictions compared with QRE. In most of these games it was clear that the quantal response approach was describing the qualitative features (and obviously fitting better, too) better than the Nash equilibrium. The approach is now used widely in the analysis of experimental data and to analyze game theoretic data from the field in both political science (crisis bargaining) and economics (auctions).

(The next 10 paragraphs are adapted from a chapter I wrote for a book tentatively titled *Positive Changes in Political Science: The Legacy of Richard D. McKelvey's Most Influential Writings*, eds. J. Aldrich, J.

Alt, and A. Lupia. Used with permission from the University of Michigan Press, Ann Arbor.)

Giving a statistical facelift to traditional noncooperative game theory was a very McKelveyish idea. Specific ideas about how to formalize it emerged about 1990 and evolved over the span of a few years into QRE. This approach became a central node in McKelvey's complex network of interrelated research topics. It lies at the junction of econometrics, game theory, laboratory experiments, and numerical computation—four of McKelvey's greatest interests.

One interpretation of QRE also places the concept in the category of behavioral economics, as it is often referred to as a boundedly rational version of Nash equilibrium. In spite of Richard's strong, almost visceral negative reaction to any use of the term "bounded rationality," he also clearly saw it as a rigorous way to try to bring in behavioral factors to the language and equations of game theory.

It started with an experimental study of the centipede game (1992). Two players move alternately, each with the opportunity of terminating the game by grabbing the much larger of two piles of money. Both piles of money double after each passing move, and there are a known finite number of possible moves. Intuitively, if the number of possible moves is large, then players will pass at first in order to let the pile grow, and both players will do quite well no matter which player grabs it later. But in any Nash equilibrium the first mover should immediately stop the game by taking the larger pile.

In order to really understand how people might play this game and why, we would have to see some data. Thought experiments and introspection could only take us so far. So we designed and conducted a laboratory experiment, not to test any particular theory (his usual *modus operandi*), but simply to find out what would happen.

However, after looking at the data, there was a big problem. Everything happened. Some players passed all the time, some grabbed the big pile at their first opportunity, and others seemed to be unpredictable, almost random. The only clear pattern was that the take probabilities increased as the piles grew.

This presented two challenges for analyzing the data, if it was to be done "right" (always a requirement for McKelvey). In this case "right" meant three things. First, it had to fit the aggregate pattern of take probabilities. Second, it had to account for the variation in behavior across subjects (i.e., the fact that we saw every kind of behavior at least once). Third, the theoretical model had to be internally consistent. To McKelvey this means it had to be publication-proof.[4]

To apply standard statistical techniques to the data, the standard model was embellished to include behavioral types (altruists, who were predisposed to pass) and errors in action (trembles). All players were assumed to be aware that all players (including themselves) trembled and might have unusual behavioral types. Hence, the assumption that rationality is common knowledge was simply replaced by the assumption that a specific form of bounded rationality is common knowledge.

This enriches the model sufficiently to obtain a good fit of the data. When the paper was nearly finished, it became clear that a more reasonable model of trembles would be one where the tremble probabilities depended on the relative costs of the errors, measured in expected payoffs (1992, p. 827). Quantal Response Equilibrium (QRE) was the next step.

The early versions of the first QRE paper (1995) defined quantal response functions as a general class of stochastic choice functions that possessed some desirable properties of stochastic choice. Choice probabilities of an action were assumed to be continuously increasing in the expected payoffs of the action; and actions with higher expected payoffs were chosen with higher probabilities than actions with lower payoffs.

The published version of the paper defined a more general version of QRE based on the connection between QRE and Harsanyi's (Harsanyi, 1973) idea of randomly disturbed games. One could rationalize the "errors" in QRE by assuming that players had privately observed payoff disturbances, producing a game of incomplete information. The term "quantal response" was adopted from the statistical literature, which had used similar terminology for stochastic models of discrete choice.

Besides QRE, Richard made at least two other important methodological contributions. The first, ordered probit, developed in collaboration with Bill Zavonia (1975), is now a widely used statistical technique in economics, political science, and several other fields. The second is the scaling method he developed in collaboration with his student John Aldrich (1977) to apply the spatial model to real-world data.

Richard's research continues to play out posthumously. The last project he embarked on, at a time when he knew death was imminent, combined nearly all his intellectual interests: methodology, theory, statistics, computer algorithms, laboratory experiments, game theory, and a contest. He became enamored with a variation on the Turing Test: to develop a completely new and general methodology for evaluating models of human behavior. In doing so he was able to turn even dry statistical testing into a game.

The goal was to compare models in the form of computer algorithms, called emulators, which simulate human behavior in some specific context (repeated games was his initial application). The question, of course, is how to evaluate the performance of such models. He proposed doing so by creating two linked contests between computer programs, one contest between emulators, and the second contest between detectors, which were programs designed to measure how good the emulators were. The way it worked is that each emulator produced a batch of simulated data about play in repeated games. He conducted identical repeated games in the laboratory with human subjects, which produced a parallel batch of "real" data of equal size. The detectors then looked at all the data and had to identify which batches of data were generated by humans and which were generated by emulators. The winning detector was the one with the most accurate classification of human and computer generated data—and was awarded a large prize (thousands of dollars). The winning emulator is the one that was most successful at fooling the winning detector. This linked contest was called the Turing Tournament (http://turing.ssel.caltech.edu/), and a paper (2005) describing the results is in press.

The Turing Tournament is obviously not the only way that McKelvey's impact will continue to play out in the future. His many fundamental contributions to political science, game theory, and laboratory experiments have had an enormous and continuing impact in the social sciences, and his students, many now professors at the most prestigious universities, are eagerly passing on his approach to social-scientific inquiry to the next generation. But he is already missed, both as a scholar and a person. It is unlikely that you or I will see in our lifetimes a scholar more humble, unselfish, sincere, and at the same time brilliant, as Richard D. McKelvey. They only come around once a generation.

EDWARD MCKELVEY AND JOHN MCKELVEY JR. provided much of the background information and insights about Richard's formative years. I benefited as well from conversations with Chris McKelvey and Stephenie Frederick. Edward McKelvey and Kenneth Shepsle provided helpful comments and corrections on a draft. I am responsible for any remaining shortcomings.

NOTES

1. Several other scholars were working on the same problem about the same time, notably Norman Schofield (Schofield, 1983), with whom Richard later collaborated (1987).

2. An illustration: McKelvey built a contraption out of string and weights that automatically computes the competitive solution in spatial voting games.

3. Turocy continues to develop Gambit.

4. A theoretical model of behavior is publication-proof if it will still accurately describe behavior after the model becomes public information. See McKelvey and Riddihough (1999) for elaboration.

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Photo by Gittings

EUGENE PLEASANTS ODUM

September 17, 1913–August 10, 2002

BY GARY W. BARRETT

EUGENE P. ODUM WAS recognized nationally and internationally as a pioneer of ecosystem ecology. It is rare that an individual makes major contributions in each essential component of academic life: education, research, and program development. A brief summary of his accomplishments in these areas is outlined below.

CAREER AS AN EDUCATOR

Odum considered one of his most important contributions, perhaps the one for which he is best known, the book entitled *Fundamentals of Ecology*. Although Sir Arthur C. Tansley first proposed the term "ecosystem" in 1935, and Raymond L. Lindeman called attention to the trophic-dynamic relationships of ecosystem function in 1942, it was Eugene P. Odum who began the education of ecologists when in 1953 he published the first edition of *Fundamentals of Ecology*. The clarity of and enthusiasm for his holistic and ecosystem approach to both terrestrial and aquatic ecosystems in the second edition, published in 1959 in collaboration with his brother Howard T. Odum, helped to educate generations of ecologists throughout the world (Barrett and Likens, 2002). The fifth edition of this book, authored with Gary W. Barrett, Odum Professor of Ecology at the University of Georgia, was published after Odum's death (at the age of 88). *Fundamentals of Ecology* was ranked first in a survey of the membership of the American Institute of Biological Sciences as the book that had the greatest impact on career training in the biological sciences (Barrett and Mabry, 2002).

In an award-winning video *Eugene Odum: An Ecologist's Life,* Odum is depicted as providing a commensurate education, whether through invited speaking engagements with citizens, discussions with community organizations, or dialogue with individual students walking across a university campus. In later years of his life Odum authored several books and publications that focused attention on Earth as a life-support system. For example, in 1989 he published *Ecology and Our Endangered Life-Support Systems* (second and third editions were published, respectively, in 1993 and 1997) and in 1998 a book entitled *Ecological Vignettes: Ecological Approaches to Dealing with Human Predicaments.* These books were intended to provide a clear understanding of current and future challenges for public consideration in order to move toward sustainable societies. "Great Ideas in Ecology for the 1990s," published in *BioScience* (1992), placed his understanding of and goals for ecology during the last decade of the twentieth century in a public forum.

Odum was the recipient of numerous awards in ecological education, including the Educator-of-the-Year in 1983 awarded by the National Wildlife Federation, the Environmental Educator Award in 1992 from the Society of Environmental Toxicology and Chemistry, and the Distinguished Service Award in 1998 from the United States International Association of Landscape Ecology.

CAREER AS A RESEARCHER

Odum received his Ph.D. from the University of Illinois in 1939 under the mentorship of S. Charles Kendeigh. Odum focused on the heart rate of birds in his Ph.D. dissertation. This investigation, published in *Ecological Monographs* (1941), attests his early interest in physiological ecology. This curiosity in physiology challenged him not only to study functions within an organism but also to study how organisms function in their environment as a whole. Odum's involvement with holistic science continued to develop under the influence of Victor Shelford, who viewed ecology as the study of biotic communities. Shelford instilled in Odum such concepts as the whole is greater than the sum of its individual parts; that nature tends toward stability in its mature stages; and that ecology is the study of large-scale systems and the interrelationships therein. Odum built his study of ecosystem ecology on this holistic perspective of nature.

Odum's research on structure and function of ecosystems, trophic-level dynamics, and ecosystem development is recognized internationally. For example, H. T. Odum and he were the recipients of the Mercer Award, awarded in 1956 by the Ecological Society of America, for their coral reef paper, "Trophic Structure and Productivity of a Windward Coral Reef Community on Eniwetok Atoll," published in *Ecological Monographs* (1955). He was the founder of the Savannah River Ecology Laboratory in 1951. Personnel at the laboratory focused their early research on secondary succession as described in Odum's classic paper "The Strategy of Ecosystem Development" (1969). In yet another classic paper (1977) he described how ecosystem development served as the central or unifying theme for early research at the Savannah River Ecology Laboratory.

Eugene conducted early groundbreaking research not only on coral reef energetics and old-field community development but also on bird fat metabolism, radiation ecology, and salt marsh dynamics. For example, in addition to the Savannah River Ecology Laboratory, the Marine Biology

Laboratory of the University of Georgia was established on Sapelo Island in 1953 as a result of interactions between Eugene and Richard J. Reynolds Jr., who maintained a plantation on the island. Odum, his students, and a small academic resident staff, including Lawrence R. Pomeroy, Robert A. Ragotzkie, and Theodore J. Starr, initiated an ecosystem-level investigation of the estuaries and extensive salt marshes, probably the first of its kind in that environment. The early work at the Sapelo Island laboratory was influential in raising awareness of ecological interactions between rivers, estuaries, and salt marshes, especially the interactions of the physical, chemical, and biological components.

Odum was a pioneer in the interaction termed "mutualism." He frequently quipped, "When the going gets tough, it pays to cooperate"—indicative of a philosophy that he applied across levels of organization or when describing attributes of ecosystems during mature seral stages of development. Odum is also recognized for seeing the "big picture" or what later became known as holistic science. Instead of starting research dealing with components of a system, Odum started with the ecosystem as a whole, investigating how interacting parts function to produce unique features of the whole.

Under the bronze bust of Eugene sculpted by William J. Thompson, which was presented on September 17, 1984, on the occasion of his retirement from the University of Georgia, is engraved the statement, "An ecosystem is greater than the sum of its parts." For his research accomplishments he was elected to membership in the National Academy of Sciences in 1970, the first member of the University of Georgia faculty to be elected to the academy. In 1974 he received the Eminent Ecologist Award from the Ecological Society of America and that same year became an elected honorary member of the British Ecological Society.

SERVICE CAREER

Perhaps Odum's greatest achievement was the establishment of the Institute of Ecology, which today is recognized as one of the leading institutions in the world for training ecologists (Barrett and Barrett, 2001). The institute offers both undergraduate and graduate degree programs in ecology. Graduates of these programs now command positions of leadership throughout the world. Odum was appointed director of the Institute of Ecology in 1961 and served in this capacity until he retired in 1984. He also held appointments as Alumni Foundation Distinguished Professor (in 1957), Callaway Professor of Ecology (in 1973), and director emeritus (in 1985).

His professional service and national and international awards and honors are extensive. He served as president of the Ecological Society of America (1964-1965). In 1975 he and H. T. Odum jointly received the $80,000 international Prix de l'Institut de la Vie awarded by the French government. In 1975 Odum was the recipient of the prestigious Tyler Prize for Environmental Achievement, accompanied by a $150,000 cash award, which he contributed to the University of Georgia Foundation as an endowment for the Institute of Ecology. Other endowments in his name support numerous functions (e.g., Odum Lecture Series) in the Institute of Ecology at the University of Georgia. In his will he established an Odum chair in ecology at the University of Georgia; the author of this memoir holds the first Odum Professor of Ecology.

He also established endowments for the University of North Carolina, University of Virginia, University of Illinois, and the E. P. Odum Award for Excellence in Ecology Education for the Ecological Society of America. In 1978 Odum received the Distinguished Service Award from the American

Institute of Biological Sciences. Likely his most prestigious award occurred in 1987 when, with his brother Howard, he received the Royal Swedish Academy's Crafoord Prize, which often is considered to be equivalent to a Nobel Prize, which is not given in the field of ecology.

PERSONAL HISTORY

Eugene Pleasants Odum was born to Anna Louise and Howard Washington Odum on September 17, 1913, while Anna Louise was vacationing on Lake Sunapee in New Hampshire to escape the summer heat in Athens, Georgia. The brother of Eugene, Howard Thomas Odum, was born on September 1, 1924. Composing a tribute to Eugene that Howard Thomas was to make at the University of Georgia during the memorial entitled "A Celebration of the Life of Eugene P. Odum" on October 16, 2002, would be one of the latter acts of his life. Howard Thomas died on September 11, 2002, in Gainesville, Florida. Elizabeth C. Odum delivered the intended words of her husband, H. T., at the celebration. A sister, Mary Frances Schinhan, born on September 17, 1919, presently resides in Chapel Hill, North Carolina, near the gracious Odum family home, which now serves as an affiliated center for the community.

Eugene's father, Howard Washington Odum, was a distinguished scholar and published numerous books on social justice, southern regionalism, and racial equality (see Craige, 2001 for details). In 1920 Howard W. Odum, along with his family, moved to Chapel Hill, North Carolina, after receiving a professorship in sociology at the University of North Carolina. Howard developed the concept of regionalism in his widely read book *Southern Regions of the United States* (Odum, 1936). Likely his holistic approach to problems confronting the southeastern United States at that time was reflected in the later integrative concepts and research approaches developed by his son Eugene (referred to by his friends and colleagues as Gene).

Gene's studies in zoology began at age 15 at the University of North Carolina (A.B. 1934 and A.M. 1936). Gene developed and maintained throughout his life a keen interest in ornithology and avian ecology (Meyers and Johnston, 2003). Odum's early research centered on avian research, especially the role of fat deposition for protracted migratory flights, and on avian natural history subjects. Gene and Martha Ann Huff Odum, whom he had met as a student at the University of Illinois, moved to the University of Georgia in 1940, where he served his entire career.

In 1946, spurred by colleagues who supported the concept that ecology was not a basic discipline of biology, Gene began writing the first edition of *Fundamentals of Ecology* (1953). Later he acknowledged that one of his most pleasing accomplishments was his contribution to the evolution of ecology from a subdiscipline of biology to a stand-alone discipline (1977).

Gene Odum collaborated with his younger brother, Howard Thomas Odum, on several major projects. For example, H. T. collaborated with Gene on the second edition of *Fundamentals of Ecology,* published in 1959. Their final paper together, entitled "The Energetic Basis for Valuation of Ecosystem Services," published in *Ecosystems* (2000), builds on their earlier classic works on the structure and function of ecological systems to the benefits supplied to human societies by these same ecosystems and landscapes.

Gene and Martha's son, William Eugene Odum, a professor of environmental science at the University of Virginia, died suddenly in 1991 of cancer at the age of 48. This loss took an emotional toll on Martha and Gene and left a mark on them, the depth of which was only revealed to a few. Martha died on June 29, 1995, after a courageous struggle with cancer, ending 56 years of marriage with Gene. Using her artistic talents, influences afforded to her in her early life, and extensive travel during her life with Gene, she continually crafted their lifetime primary dwelling at Beech Creek in Athens, Georgia. An early study in design and a developed interest in architecture led Martha to the loving restoration of a cabin (reconstructed with one numbered piece at a time). This cabin, along with the watershed property located in Madison County, Georgia, was named Spring Hollow. Gene and she willed Spring Hollow to the University of Georgia Foundation for use by the Institute of Ecology for research and education. The Eugene and Martha Odum Gallery for the decorative arts, situated in the Georgia Museum of Art, stands as a testament to their dedication to preservation and encouragement of regional craft. Through her paintings Martha enriched Gene's perspective of nature (Odum and Odum, 2000). And the influence of her beautifully appointed parties, appreciated by generations of friends, colleagues, and students, led to the creation of several gifts by Gene that emphasized the importance of and support for social activities and gatherings within academic units, among them a special fund at the University of Virginia.

Gene Odum portrayed intellectual and personal growth throughout his life, evolving from an avian ecologist in the 1930s and 1940s to an ecosystem ecologist and a holistic thinker later in his life. From the 1950s through the 1980s he helped establish the Savannah River Ecology Laboratory, the Sapelo Island Marine Institute, and the world-famous Institute of Ecology. It was also during this time that his writing began to emphasize the role of mutualism, mechanisms of ecosystem development, and energetics as a common denominator across levels of organization. In reflection he then became a philanthropist providing monies for his programs, an environmentalist recognizing the need to protect ecological systems, and a teacher promoting integrative science.

Gene was a vibrant individual until his death, continuing to negotiate the limitations of aging and sharing the wisdom and gifts accrued in his long life. He shifted from an avid tennis player in his youth and middle years, to a worthy opponent on the croquet court in his later years. He was a relentless birdwatcher, a tireless traveler, and an enthusiastic organic gardener until his death. Although encouraged to officially retire in 1984, he continued to devote his time to writing, contributing to research publications and the fifth edition of *Fundamentals of Ecology* (2005). He was devoted to public communications that promoted ecological awareness. In addition to his reputation as a research ecologist, Eugene also became a respected environmentalist during the decade of the environment and was quoted frequently in *Time*, *Newsweek*, and *Life* magazines.

He conducted his life as both an ecologist and an environmentalist. He played a major role in the passing of the Coastal Marshlands Protection Act of 1990 in the State of Georgia. He frequently noted that politicians act when citizens speak in a unified voice. Eugene designated in his will that more than half his 26-acre estate at Beech Creek be placed in permanent conservation protection, thus providing habitat for the wildlife he loved. His legacy of generosity will benefit generations in pursuit of education, research, and service.

HONORS AND AWARDS

1945 Fellow, American Ornithologists Union

1950 Fellow, American Association for the Advancement of Science

Delegate, first Atoms-for-Peace Conference, Geneva, Switzerland

1956 Mercer Award (shared with Howard T. Odum), Ecological Society of America

1957 Foundation Distinguished Professor, University of Georgia
National Science Foundation Senior Fellowship

1964 President, Ecological Society of America

1967 Georgia Scientist of the Year, Georgia Science and Technology Commission

1970 Member, National Academy of Sciences

1973 President, Ecology Section, American Society of Zoologists

1974 Eminent Ecologist Award, Ecological Society of America
Honorary member, British Ecological Society

1975 Prix de l'Institut de la Vie, French government (shared with Howard T. Odum)
Member, American Academy of Arts and Sciences

1976 Conservationist-of-the-Year Award, Georgia Wildlife Foundation

1977 Tyler Prize for Environmental Achievement

1978 Distinguished Service Award, American Institute of Biological Sciences
Honorary member, Southeastern Estuarine Research Society

1979 Member, Environmental Advisory Committee, U.S. Department of Energy

1981 Cynthia Pratt Laughlin Medal, Garden Club of America

1983 Educator-of-the-Year, National Wildlife Federation

1985 Odum Lecture Series, University of Georgia

1987 Crafoord Prize in Ecology, Royal Swedish Academy of Science, Stockholm (shared with Howard T. Odum)

1989 Chevron Conservation Award, Washington, D.C.

1990 Honored at the seventy-fifth annual meeting of the Ecological Society of America

1991 Theodore Roosevelt Distinguished Service Award

1992 Environmental Educator Award, Society of Environmental Toxicology and Chemistry

1997 Lifetime Achievement Award, Georgia Environmental Council
Estuarine Federation Lifetime Achievement Award, established to honor Eugene P. Odum, Howard T. Odum, and William E. Odum

1998 Distinguished Service Award, United States Section, International Association of Landscape Ecology

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Harrison Shull

HARRISON SHULL

August 17, 1923–July 28, 2003

BY DONALD S. MCCLURE AND MICHAEL KASHA

HARRISON SHULL BUILT AN influential scientific career in research, specializing in the quantum mechanics of small-molecule electronic spectra. He later showed a gift for administration as a chief academic officer of several major educational institutions. He was born into a family of highly achieving scholars and scientists; a hefty book titled *Shull Genealogy* was a proud part of his early personal library. It was clear that this background contributed strongly to his self-confidence and growth, and it can also be stated that he added much luster to the already illustrious Shull name.

Harrison Shull made early, seminal contributions to the theory of molecular energy levels, taking advantage of the growing capabilities of large-scale computers in the decades of the 1950s and 1960s. As his career developed, he found ways to promote the use of computers in chemistry, increasingly using his administrative talents to the great benefit of his colleagues in acquiring access to these facilities

Harrison Shull was born in Princeton, New Jersey, where his father, George Harrison Shull, was a professor of botany at Princeton University. George Shull had become famous for his part in the development of hybrid corn, which had an enormous impact on the practice of agriculture worldwide. The four brothers and two sisters in the Shull family grew up in a house on Jefferson Street with a special tree their father designated for each one to look after. As a child, Harrison was often hampered by allergies and illness and, being confined to the house, he did a lot of reading. In the summers, however, young Harrison aided his father's genetic research by picking up stones and removing insects for penny wages in the primrose research fields. He worked through grade school and Princeton High School at the top of every class. His high school principal wrote a recommendation for him to go to Princeton University, saying in part, "Harrison Shull is in my opinion the strongest student scholastically that has ever enrolled in Princeton High School." He ranked number one in his high school class and was not yet 17. He entered Princeton University in September 1940 and, repeating his high school performance, graduated number one (in a tie) within three years in the accelerated wartime program. He presented the traditional salutatorian address in Latin at the graduation ceremonies.

Upon entering Princeton he had no firm idea of a major subject, but seeing his three older brothers enroll in Princeton as biology majors (taking their father's course), he decided to do something different. He thought he was better at chemistry than physics, so chemistry was his choice.

His career began as a civilian in the chemistry division of the Naval Research Laboratory in Washington, D.C., during World War II. It wasn't long before every coworker under the age of 26 was drafted. They added basic training to their work schedule, and emerged with the rank of ensign. His association with the Navy continued in various ways throughout his lifetime, and finally, after a long career, ended as the chief administrative officer (provost) of the Naval Postgraduate School in Monterey, California.

Harrison was always certain that he would go to graduate school. He decided that at this time in his life he should see what California was like and so applied to Caltech, Stanford, UCLA, and UC Berkeley. Berkeley won because they sent him a telegram of his acceptance while the others relied on air mail. He also had advice from Henry Eyring, who often walked down Jefferson Street (in Princeton) on his way to the university. He said, "Only Caltech and Berkeley are worth consideration, but Linus Pauling is ill so don't go to Caltech, go to Berkeley and work with Gilbert Lewis, he'll last longer." So Harry packed up his car and drove to Berkeley and within days, in early November 1945, began to work with G. N. Lewis.

Berkeley was an exciting place then as always: Seaborg and others had been discovering the transuranium elements, and a large nuclear research laboratory was growing on the hills above the campus. William Giauque had developed a world-renowned cryogenic research program; Gilbert Lewis had turned to molecular excited-state researches; and Melvin Calvin was using isotopes to trace biochemical processes in living organisms. Several Nobel prizes would be awarded to UC chemists and physicists in years soon to come. Harrison's work was to follow the discoveries of G. N. Lewis and Michael Kasha on the phosphorescence of organic molecules and the electronic triplet state hypothesis of its origin.

Working in the Lewis lab then were Michael Kasha, who had already completed his Ph.D. (in 1945), and Robert Nauman, who was in his last year of research with Lewis. Donald McClure arrived in February 1946. Then Eyring's predictions went badly wrong: Lewis died in March 1946, while Pauling lived another 48 years! Wendell Latimer, then dean of the College of Chemistry, made the decision to keep the group together and persuaded George Gibson to become its faculty supervisor. Latimer also persuaded Michael

Kasha to remain working as the laboratory spectroscopist until Nauman, Shull, and McClure completed their research and embarked on their own (and as it proved, highly successful) research careers. Later Shull, Kasha, and McClure were each elected to membership in the National Academy of Sciences.

Gibson's way of running a research project could not have been more different from Lewis's. Whereas Lewis would have twice-daily meetings with each student, giving him ideas to follow up, Gibson just let things happen. But he did have regular meetings to learn group theory and some necessary parts of quantum mechanical theory. He promoted discussion and had interesting ideas. He was a wonderful person and the group flourished under his leadership.

Harrison Shull emulated Gibson's research style more than Lewis's when he later had his own research groups. Harrison enjoyed the considerable independence that the Gibson group offered. So later in his own work in Iowa and Indiana, he particularly arranged for his graduate research group to be somewhat remote from his own office, confiding to some of us that the separation cultivated more independent thinking instead of letting the professor solve each puzzle.

Among the important questions of those days (1945 to the 1950s) was the characterization of excited states of molecules. The first property to learn was the group representation of the state. Harrison Shull's Ph.D. thesis attempted to do this for the triplet electronic states of benzene, and for the lowest triplet state this could be done by an analysis of the vibrational spectrum of the phosphorescence (which Lewis and Kasha had characterized as a triplet-singlet transition). Shull's first paper, published in 1949, was on this analysis. Later, John van der Waals in Holland extended this work, using Harrison Shull's observations.

During his stay in Berkeley, Harrison lived at the Alpha Chi Sigma house, the chemistry fraternity. His leadership qualities, intelligence, and good humor made him a natural choice for president of this local chapter. This was just another example of how he came to the top in the many institutions where he worked.

The urge to see what California was like was expanded into a meandering 20-day trip from Berkeley to Princeton in August 1947. Accompanied by Robert Nauman (who kept a detailed log) and Alan Smith, he traveled 8,000 miles through 19 states and 4 Canadian provinces.

Living in Berkeley as single men with responsibility only for academic work allowed unrestricted concentration on research. Attracting a suitable potential wife was another goal of the young group (all working on triplet states of molecules). As their Ph.D. work came to a conclusion, almost simultaneously each of the three remaining members of the group (Shull, McClure, and Kasha) had succeeded, the laboratory humor resonating with "the triplet state has worked again."

In considering his next move Harrison thought that the postwar climate for financing scientific research might be unfavorable for an experimentalist, who would require expensive equipment. A theoretical capability would be more portable and less expensive. Latimer urged Shull to apply for a National Research Council Fellowship, which he won and which could be used anywhere. He chose to go to the University of Chicago to study molecular orbital electronic theory with the physicist Robert Mulliken. Shull's second paper was on the calculation of transition probabilities in diatomic molecules (1950), the first in a series of his theoretical papers on this subject. It was thought that the theoretical f-numbers (oscillator strengths for band intensity) might be more accurate than measured ones and that the results would be useful to astrophysicists trying to determine the numbers of molecular species in interstellar gas clouds.

After a year in Chicago, Harrison Shull moved to Iowa State College (later University) at Ames, Iowa, at the urging of Fred Duke. The latter was formerly a professor of analytical chemistry at Princeton and then at Iowa State who had been following Shull's progress for the past six years. There was another connection between Princeton and Iowa State: Harrison's father, George H. Shull, was awarded an honorary degree by the college in 1942 for his part in the development of hybrid corn, which had an enormous effect on Iowa and many other parts of the world.

One advantage of Iowa State was that the government-sponsored Ames Laboratory was located there, and Shull was given a joint appointment in that laboratory and in chemistry as an assistant professor. The Ames Laboratory under Frank Spedding (a G. N. Lewis disciple) was a source for spectroscopic equipment and money for summer salaries. The Ames Laboratory specialized in rare earth (4f-element) chemistry, which was a fundamental basis for the chemistry of uranium-plutonium (5f-elements) in the World War II Manhattan District projects. Spectroscopic research in this field had been nurtured in G. N. Lewis's laboratory, with collaborative work by Frank Spedding, Simon Freed, and Noel S. Bayliss (the last was later prominent in Australian spectroscopy). Harrison Shull's students there included several who later had outstanding careers, especially Lionel Goodman, Stanley Hagstrom, and Frank Ellison. His research topics at Iowa included papers that he wrote on the comparison of transition probability calculations using dipole-length versus dipole-velocity operators, which give different results due to the use of approximate wave functions. An important calculation for that time was the all-electron self-consistent field treatment of H_2O with Frank Ellison. There were several papers on π-electron systems in organic molecules with Lionel Goodman.

The first international conference on quantum chemistry was held on Shelter Island, New York in September 1951 with Robert Mulliken as one of the organizers. Harrison Shull was invited. At the Shelter Island Conference he roomed with Per-Olov Löwdin, who had just completed his Ph.D. in Sweden. Harrison and Löwdin began an inspired friendship and a lifelong collaboration. Early in 1954 at the urging of Löwdin and Harrison's wife, Jean, Harrison applied for and won a Guggenheim Fellowship, which would support the parents and their four children for a year in Sweden. But the department chairman refused to give Harrison a leave of absence; Harry said he would quit, and did so even when the leave was later granted. The self confidence illustrated by this act was entirely justified by later events.

A year earlier, Iowa had tried to hire Walter Moore (author of a landmark text in physical chemistry and later of a definitive biography of Erwin Schrödinger), but Walter elected to accept an offer from Indiana University instead. Mutual admiration led Harrison and Walter to stay in touch and this led to an invitation for Harrison to give a talk at Indiana before going to Sweden. The Shull family departed for Sweden in September 1954, and for a Christmas present that year Shull got an offer from Indiana, which was quickly accepted.

Shull never returned to Iowa, but having shown the faculty there the value of a chemical theoretician, he made it easier for them to hire Klaus Ruedenberg, saying in a letter to Klaus that "everybody would be gaining by the two moves, each of us two by getting a better job, Indiana University by getting a good theoretical chemist, and Iowa State University by getting a better theoretical chemist."

The collaboration in Sweden with Löwdin in the academic year 1954-1955 resulted in the development of a significant new idea, the concept of natural orbitals. Their papers on this and several other aspects of electronic structure calculations were published in 1955. Taking leave from Indiana in 1958-1959, Shull again spent a year with Löwdin in Uppsala, this time as assistant director of the quantum chemistry group. These visits convinced him to concentrate on *ab initio* quantum theory and to use large-scale computers to solve the equations.

Shull's association with Indiana lasted 24 years, longer than with any other institution. He arrived as associate professor in 1955 and within three years was promoted to the position of full professor; in 1961 he was named research professor. He enthusiastically promoted the use of large electronic computers, and later as director of the Computing Center he established the principle of free use to researchers, paying the operating expenses by separate grants. He founded the Quantum Chemistry Program Exchange (software), which put Indiana on the map for everyone doing quantum chemistry. Ultimately he became dean of the graduate school (1966-1972) and then vice-chancellor for research and development (1972-1976). He was recognized as a brilliant administrator who brought in fresh ideas for handling the tough problems of appointments, tenure, promotion, and salary increases.

The Chemistry Department at Indiana was a vibrant place with Harrison Shull, Walter Moore, Vernon ("Jack") Shiner Jr., Henry Mahler, Frank Gard, Felix Haurowitz, and others there during the period of their tenure. The graduate students in Shull's group were having fun both scientifically and socially. They were often invited to the Shull home for parties with the family or included in faculty gatherings. He was a stimulating person and would bring visitors to

Bloomington with their new ideas. Harrison was very good at debating, and the graduate students jokingly said that the criterion for deciding when it was time to face the world and leave the university was when you could win a scientific argument with Harrison. After they left, he would follow their careers with interest and pride. One, Ralph Christoffersen, shared Harrison's bent for administration and went on to become vice-president of the University of Kansas and then chancellor of Colorado State University at the very time Harrison was chancellor of the University of Colorado.

Indiana University is famous for its Music Department. Harrison's interest in music was evident during his Berkeley days, when he and McClure would go to hear the Thursday night performance of the San Francisco Symphony orchestra. In all his later university positions he encouraged musical performances and attended many.

During his tenure at Indiana and especially after his election to the National Academy of Sciences in 1969, Harry accepted many invitations to join committees and boards. He was very effective, and in many cases he became the chairman after several meetings. It seemed especially appropriate that he joined the Advisory Council to the Chemistry Department at Princeton University and eventually became its chairman. He became well known nationally and met many people who would become important in his life. One of the scientists important in Harrison's life was George Low, a brilliant engineer and manager who among many other things had been chairman of the Manned Lunar Program Planning Group, which provided the technical background for President Kennedy's decision in May 1961 to commit the U.S. government to landing a man on the Moon before the end of the decade. Subsequently, Low held major positions in NASA. In 1976 George Low accepted the offer of the presidency of Rensselaer Polytechnic Institute, his alma mater. Low soon found that he needed a provost and made the offer to Shull. Low's idea was to make Rensselaer into a major research university, and the new position seemed to be an attractive challenge, which Harry quickly accepted in the fall of 1979.

Harrison spent only three years at Rennselaer but was influential in wisely and gently nudging its faculty members toward a higher level of academic achievement and in rewarding them with a better scheme of salary increases. His colleagues in administration and on the faculty recognized him as a warm and supportive person who always acted with high and strongly held academic principles. While at Rennselaer, Shull was aggressively pursued by the University of Colorado, which was seeking a chancellor for its large Boulder campus. Harrison and his second wife, Wil, had spent a summer there while he lectured on quantum chemistry in 1963, and they loved it there. Shull refused the offer at first, persuaded to remain at Rensselaer by George Low, but he was finally persuaded to make the move.

In the Colorado University system there was a president over all branches and a chancellor for each campus. Harrison's term as chancellor of the Boulder campus was from the spring of 1982 to the summer of 1986. Harry got along well with the president, Arnold Weber, and began some bold initiatives. He invested about $1 million in a chancellor's computer fund to enable the faculty to buy personal computers at about a third of the market price. This had followed some intense negotiations with computer manufacturers in which he was able to persuade the companies that it was good business even if they didn't make money on the deal in the first year. The scientists, of course, loved it, and the rest of the institution rapidly became computer literate. Arnold Weber left to become president of

Northwestern University in 1985, and under the new administration it was difficult for Shull to operate, so he considered moving again. Harrison and Wil had discussed retirement times and places so that these factors entered into the choice for the next move. Providence descended again when Wil noticed an opening for provost at the Naval Postgraduate School in Monterey, California. Harrison Shull was well known in the Navy and once his name came up for this position, the school was ready to do anything to get him.

During the period from 1988 to 1995 he held the positions of provost and vice-president for academic affairs at the Naval Postgraduate School. There he was able to increase significantly the number of faculty members and improve the quality of graduate education. Harrison was very impressed by the education plan and its effect at the Monterey school. The students were Navy personnel of considerable practical experience. As students, they were older than normal college students, often were already head of a family, and dedicated to advanced technical education. Their focus, dedication, and desire to succeed were thrilling to Harrison to see in action. He was inspired by the effectiveness of the students' achievements. No low grades, low aspirations, or failures there. Admiral Ralph West, superintendent of the school, wrote a penetrating evaluation of Shull's performance in these years, and it seems to apply to all of Shull's academic administrative positions, so we quote from it here.

> Harrison was an experienced academic administrator who was very effective in generating change to strengthen the school's educational capabilities. He always approached issues with a long-term view and the patience and ability to achieve his goals. He put in place tenure guidelines that resulted in continuous improvement in the faculty. He ensured departments conducted rigorous recruitment campaigns to provide the selectivity needed to be able to hire only quality faculty. All of these occurred during

a period in which the Navy's size and therefore the school's input was declining. Through Harrison's efforts the school was able to retain top faculty personnel, ensure sufficient openings for hiring strong new talent, and meet the strictures of reduced funding.

While Harrison outwardly appeared to be a kindly grandfather figure, his incisive method of leadership ensured a detailed knowledge of and impact on all issues associated with instruction, faculty and educational support. He had unusual strengths in the area of informational technology and was able to ensure smart decisions were made concerning computer services and equipment as well as library support. At the personal computer level, Harrison showed an uncommon ability to use various software for combining multiple data sources to compare performance, reveal problems, and evaluate solutions. Harrison was the perfect person to have in the academic leadership position at the school during a time of change and the need to strengthen the school's capabilities. The results of his efforts played a major role in justifying the retention of the school during the reviews of the early 1990s to close military bases.

Harrison and Wil built their dream house on a hill overlooking Monterey Bay and lived there after retirement in 1995. There was enough room in it for the many visiting children and grandchildren and for the many friends made in different parts of the world during a long and highly visible career.

CONTRIBUTIONS TO SCIENCE

The following four paragraphs were contributed by Professor Ernest R. Davidson, formerly a Shull student at Indiana.

During the 1950s Shull produced his most lasting contributions to the theory of electronic structure of atoms and molecules. Several of these papers are still frequently cited 50 years after they were written. Before this time, mixing of "configurations" was generally limited to configurations that had a clear physical significance as nearly-degenerate zero'th order approximations to electronic states of the system. Also, perturbation theory used a formal expansion in eigen-functions of operators with both a discrete and continuous spectrum. With the access to digital computers in the 1950s it quickly became clear that the Hartree-Fock method would not produce results of sufficient accuracy. Description of the correlation energy (defined as the error in the Hartree-Fock energy) using digital computers required a new approach.

Working with Löwdin, Shull approached this as a problem in numerical analysis rather than a problem in chemistry. They showed that the continuum problem could be avoided by simply expanding in a complete discrete basis set with no physical significance [1955]. They then used this approach to generate an accurate wave function for the helium atom. Factorization of this function into its expansion into Natural Orbitals led to a very compact and easily interpreted result even though the individual terms had no relation to any zero'th order model problem [1955, 1959]. Shull extended this approach to the hydrogen molecule [1959] and then showed how the results could be simply interpreted [1960].

Shull and Löwdin extended the helium atom results to the ground and first excited triplet state of the iso-electronic two-electron atomic ion series, H^-, He, Li^+, etc. [1956]. They were the first to note that the correlation energy of the ground state of these ions is nearly independent of the atomic number, Z. This led Shull to two additional papers. In one, he examined other iso-electronic atomic ion sequences and showed that those which became degenerate in the high-Z limit had correlation energy which was linear in Z, while those that remained non-degenerate had a correlation energy that was nearly independent of Z [1960]. This has been a key paper in constructing accurate tables of atomic energies. In the second, he showed that direct variational calculation of the excited state energy as a higher root of a secular equation could lead to the exact wave function without convergence of the lower states [1958]. Before this it had been assumed that the lower roots would have to be converged in order to get an accurate wave function for the higher state.

This flurry of key papers profoundly changed the way people approached the electronic structure problem. Before these papers, the problem had been approached using qualitative reasoning or semi-empirical explanations for spectra and bonding. After these papers, the problem was viewed more as a problem in numerical analysis and construction of rapidly converging sequences of approximations. Other research groups around the world with access to digital computers soon adopted this viewpoint.

Shull's publication list of about 80 papers contains work on several other subjects, some of them already mentioned earlier in this account. In his later years he published articles and editorials on the social aspects of the educational process.

PUBLIC SERVICE

Shull's ability to work with people was expressed both in his university administrative positions and in being a member or chair of committees. Especially during the Indiana years he may have had as many as 10 such obligations at once.

The Naval Studies Board was created by the National Academy of Sciences at the request of the Navy Department in 1974 to be a source of independent, long-range scientific and technical planning advice for the Navy and Marine Corps. Shull was one of several people who worked with Admiral Zumwalt, chief of Naval Operations, and Philip Handler, president of the National Academy of Sciences to organize the Naval Studies Board and then served on it for six years, and again for six years after his retirement in 1995. While still at Colorado, he was a member of the Advisory Committee on Chemistry for the Office of Naval Research and during his tenure at the Naval Postgraduate School was a member of the workshop on Navy intelligence technology.

Harrison Shull made notable contributions to the field of human resources in science and engineering, mainly through the Office of Scientific Personnel of the National Academy of Sciences, which was reorganized in 1974 as the Commission on Human Resources. He was its chairman from 1977 to 1981, but had been a consultant before then on the use of computers to organize large data files on Ph.D.s in science and engineering. The committees meeting under the aegis of the commission had to be chosen, their agendas defined, and their reports monitored. The commission conducted postdoctoral associate programs for several dozen of the leading scientific organizations of the federal government, such as the National Institute of Standards and Technology, the Naval Research Laboratory, NASA, National Oceanic and Atmospheric Administration, the Air Force, and the Army. Both the quality and the numbers of Ph.D. graduates had to be considered, and this led to studies of the fluctuations in supply and demand for these valuable people. Shull noted that, since it took four to five years to get an advanced degree, what looked like a good employment prospect at the start might not be so good at the end, leading to fluctuations that government programs might be able to damp by granting or withholding grants at the right times. Shull wrote an article on this plan for the 1978 National Research Council annual report.

Some appointments to other National Academy of Sciences endeavors were the Committee on Science and Public Policy (1957-1974), the Stockpile Assessment Panel of the Committee on Demilitarizing Chemical Munitions and Agents (chairman, 1983-1984), the Committee for Joint U.S.-U.S.S.R. Program on Fundamental Science Policy (chairman, 1983-1991), and many others.

FAMILY LIFE

Harrison was the youngest in a family of four boys and two girls and one father and one mother. His own family came to eight children with two wives. Jean, whom he married in 1948, had a son, James, from a previous marriage, and they had a son, George, and two daughters, Kathy and Holly. He and Jean, who did not return from Sweden when Harrison and the boys returned to Indiana, were divorced in 1962. Harrison met and married Willa Bentley in 1962. She had two boys, Jeffrey and Warren, from her previous marriage, and they had a son, Stanley, and a daughter, Sarah, together. Harrison and Wil made an excellent academic team with Willa cheerfully taking on the role of university first lady as Shull moved up the academic ladder. A Shull reunion was a big, happy gathering with siblings, nephews and nieces, children, and grandchildren. (One special reunion was a surprise seventieth birthday party in Monterey organized by Wil.) The children of these unions got along well with one another. Most obtained university degrees, and there are some with professional degrees and Ph.D.s. Harrison was a central and beloved figure in this expanding group.

Harrison had a great sense of humor. His children and grandchildren remember his twinkling eyes, followed by a joke. We remember locating him in a noisy crowd by hearing his uninhibited laugh. Still he could be disappointingly critical when he thought you could do better.

EPILOGUE

Harrison Shull, Harry to all of his friends and associates, will always be remembered for his great skills, both scientific and administrative. But most of all, we share the warm recollection of his good-humored nature, his wise advice on many occasions, and especially for his fair-mindedness and elegance as a great human being.

Members of the Shull family were very helpful to the writers of this memoir. Harrison's wife Willa Shull was central in providing details of his life and making contacts for us. His son Stanley gave details of family life and a recording of his conversation with Harrison about his career. Harrison's older sister, Georgia Vandersloot, who helped bring him up, wrote about his early life, and her daughter, Karen Richards, wrote about family relationships.

We thank his former students and university colleagues: Robert Nauman (Berkeley); Lionel Goodman, Klaus Ruedenberg (Iowa); Stanley Hagstrom (Iowa and Indiana); Vernon ("Jack") Shiner, Ernest

Davidson (Indiana); Ralph Christoffersen (Indiana and Colorado); Gary Judd, Van C. Mow, and Don Miller (Rennselaer); Charles Depuy (Colorado); Ron Taylor (Naval Studies Board); William Kelly (Commission on Human Resources); Ralph West (Naval Postgraduate School).

Details of Harrison's father's part in the development of hybrid corn appear in the recently published book *Mendel in the Kitchen* by Nina Fedoroff and Nancy Marie Brown (Joseph Henry Press, Washington, D.C., 2004, pp. 57-62).

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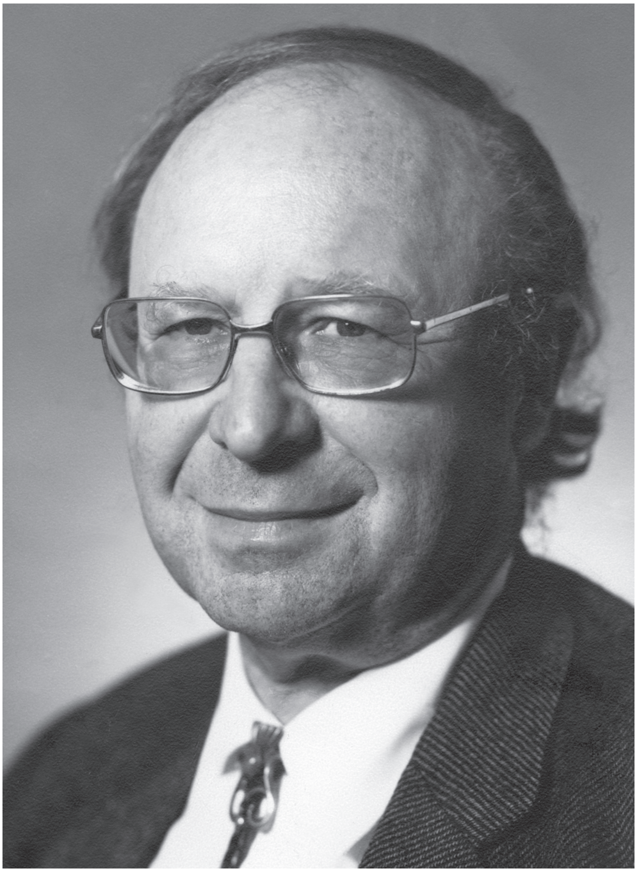

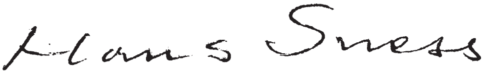

HANS E. SUESS

December 16, 1909–September 20, 1993

BY HEINRICH WAENKE AND JAMES R. ARNOLD

CONTRIBUTION BY HEINRICH WAENKE

HANS E. SUESS WAS A member of a dynasty of famous Austrian scientists. The founder of this dynasty was his grandfather Eduard Suess (1838-1914). He was a professor at the University of Vienna (1857-1908) and president of the Austrian Academy of Sciences (1898-1911). He became well known in geology and paleontology, especially by his book *The Face of the Earth* (in German: *Das Anlitz der Erde*), which became fundamental in geology and geotectonics. For the first time he not only described geological phenomena but also tried to find physical and geological reasons for them. The book has been translated from German into all the major languages. Aside from his work as a scientist, Eduard Suess was engaged in politics and became the first member of the City Council of Vienna and later on a member of the Austrian Parliament. His name is also connected with a new system for the supply of water brought from the Alps to Vienna over about 200 km, which he designed and fought for its realization politically.

The father of Hans E. Suess was Franz Eduard Suess (1867-1941). His field was geology and petrography, and he became a professor at the University of Vienna in 1908.

Hans E. Suess, born in 1909 in Vienna, grew up in an environment of scientific excellence. He received in his young years a good intuition as to what could be right and what probably was wrong. In his acceptance speech for the Leonard Medal of the Meteoritical Society, he said, “When I was a little boy, I was told all about continental drift and plate tectonics, and how mountains were folded asymmetrically. Later, however, I was told by others that this was all fantasy.”

Hans studied physical chemistry at the University of Vienna. He received his Ph.D. in 1936. Two years earlier appeared his first publication on experimental studies with heavy water (only discovered two years earlier), which dealt on the inversion of cane sugar in mixtures of light and heavy water (1954,1). He became especially interested in the reaction rates and equilibria in solutions of heavy water (1956; Goldschmidt, 1954), but Hans also worked on topics like the kinetic of thermal polymerization of dissolved styrene (Burbidge et al., 1957; Revelle and Suess, 1957) and other problems in physical chemistry like the thermal disintegration of dioxane (Suess, 1954b).

The first 10 papers Hans published in the first five years of his career show the wide spectrum of his interests. Aside from the papers just mentioned, there is one dealing with photochemistry of the Earth’s atmosphere (1959), two on the radioactivity of potassium and its use for the determination of the age of elements in meteorites (1960,1,2), and three on capture reactions of thermal neutrons. He had irradiated gaseous ethylbromide with thermal neutrons and found that in the gas phase all activated bromine atoms were set free and could be separated to 100 percent. The later investigations were carried out in Hamburg at the Institute for Physical Chemistry, to which Hans had moved in 1938. Earlier he had visited Zurich to do research at the ETH (Swiss Technical University).

During World War II, Hans Suess worked especially on exchange equilibria of $H_2 + HDO \leftrightarrow HD + H_2O$. He became an expert on heavy water and a scientific advisor to Norsk Hydro, the Norwegian plant in Vemork, producing hydrogen by electrolyzing water and as a by-product of heavy water. In this capacity Suess was sent several times to the heavy-water plant in Norway. For these trips he was allowed to travel through Sweden. That is why he liked these trips very much: He was able to buy goods in Sweden that had long since disappeared from German shops.

Aside from his work with heavy water he started working on the cosmic abundance of the elements, a topic that he continued to work on for decades. Following the pioneering work of V. M. Goldschmidt by plotting mass numbers versus cosmic abundances, Suess found smooth curves for both nuclei with even mass numbers as well as for odd mass numbers. In this way he was able to correct the abundances of elements for which the experimental data were uncertain. In the graphs that were made with the corrected abundance values, the nuclei with certain proton and neutron numbers (magic numbers) were easy to recognize. The two most distinct numbers N and Z = 50 and 82 had been pointed out previously by Elsasser in 1933 and 1934.

The papers of Hans Suess dealing with the cosmic abundances of elements played a fundamental role in the physical explanation of "magic numbers." These numbers not only showed up in the cosmic abundances but also in various properties of the nuclei like the binding energy or the neutron absorption cross-sections. At that point the topic became more a problem of nuclear physics. For that reason Hans joined forces with two physicists, first with Hans Jensen (Heidelberg) and later also with Otto Haxel (Göttingen).

The breakthrough came with the assumption of a strong spin-orbit coupling in the nuclei. The energy levels of a single nucleon in the potential of the rest-nuclei split due to the strong spin-orbit coupling in such with parallel and antiparallel position of spin and orbit. Filling the energy levels with protons or neutrons Haxel, Jensen, and Suess (1949, 1950) could with this model show that binding energy for nuclei with a magic number becomes larger compared with neighboring nuclei just as observed. They could also show that the magic numbers (2, 8, 20, and 28, 50, 82, 126) in fact belonged to two different series (the smaller three and the larger four). This break in the series of magic numbers was explained in the following way. Nuclei with smaller mass numbers have a weak spin-orbit coupling, and the corresponding shells follow the energy levels given by the orbital momenta. For mass numbers higher than 20 the spin-orbit coupling becomes dominant, and the energy levels are governed by the total angular momenta.

In his book *Chemistry of the Solar System* (1987) Hans Suess downplayed his own contributions in respect to the magic numbers and the related breakthrough of the shell model for atomic nuclei by writing in his book: "In 1948, Maria G. Mayer published convincing evidence for the significance of the magic numbers in nuclear structure, and two years later, she succeeded in postulating a theory to explain them. The same explanation was proposed at exactly the same time completely independently by Haxel, Jensen, and Suess" (1949, 1950).

Aside from detailed papers on the shell model of atomic nuclei by Haxel, Jensen, and Suess (authors in varying sequences), there are among the papers Hans Suess published after the war (before he left Hamburg to move to the United States) two on the radioactivity of potassium-40. He questioned the then-new measurements on the branching ratio of the decay of potassium-40, in which for the ratio of K capture to total decay rate values of up to 0.78

had been obtained. Using geochemical evidence, Suess concluded that a value of 5 ± 2 percent for the K capture fraction to be more likely, or definitely less than 10 percent. In this respect F. G. Houtermans once joked, "Suess is noted for the fact that he comes to the right conclusions on the basis of very scanty evidence or no evidence at all."

Another important paper of this period was the one on the abundance of rare gases in the Earth's atmosphere. Suess stated that the ratio of the number of xenon atoms in the atmosphere to the number of silicon atoms in the bulk earth is about 10^7 times smaller on the Earth than in the universe, whereas for neon this figure exceeds 10^{11}. He further found that abundances of Ne, Ar-36 and Ar-38, Kr and Xe follow an exponential function suggesting loss to space by selective diffusion and stated that a proof could be obtained by studying the isotopic ratios of neon and argon, as the lighter isotopes should be preferentially lost leading to a decrease in the neon-20/neon-22 and argon-36/argon-38 ratios. The proof came from the discovery of the solar wind implanted rare gases, which clearly showed that in the terrestrial atmosphere the light isotopes of neon and argon are indeed depleted.

In 1949 Harteck and Suess published a short note on deuterium content of the hydrogen of the Earth's atmosphere. The Linde Company producing rare gases by separation from air provided a fraction of what they called "raw" neon, which contained besides helium and neon also hydrogen from the atmosphere. From a total of several $10^5 m^3$ air each; two samples of 30 cm^3 water were obtained. From the density of the water a 25 ± 7 percent higher deuterium content was deduced and explained as preferential removal of the lighter isotope in the continuous loss of hydrogen from the atmosphere to space.

CONTRIBUTION BY JAMES R. ARNOLD

Scholarly dynasties are much commoner in Europe than in this country. Hans Suess was fortunate as a member of such a dynasty. Both his grandfather Eduard Suess and his father Franz Eduard Suess were distinguished earth scientists in their day. Hans Suess was born on December 16, 1909, and was brought up in the center of Vienna. His vacations were spent frequently in Maerz, a small town not far from the Hungarian border, where his grandfather had built a representative summer villa within a large park.

On one occasion he showed us two of his grandfather's nineteenth-century field notebooks. They contained not only clear, legible notes but also skillful pen and ink drawings of geological features, much clearer it seemed than any photograph could be. Something gained, something lost. Hans Suess carried with him visible traces of this old world culture.

His first trip to the United States in the aftermath of World War II resulted directly from the coincidence of the two papers announcing the shell theory of nuclear structure, which were submitted on both sides of the Atlantic Ocean on the same day. It was the German discovery paper that led after some time to his transfer from West Germany to this country. He was soon invited to visit the University of Chicago's Institute for Nuclear Studies (now for many years the Fermi Institute) and to meet his friendly competitors there. He met with Maria Mayer at that time and gave a seminar that drew a large audience.

During that first extended visit he became acquainted with the work of Prof. Willard Libby on the development of C-14 dating, which was being carried on (in part) in the next-door laboratory to his own. Libby's work was just at the point of producing the first paper, which showed success in matching C-14 dates with ancient samples of known age.

The institute was one of the most exciting places on Earth for a physical scientist at that time, and among the many interesting projects under way there this one attracted his particular interest.

Suess saw the possibilities of the technique at once, especially for its application to the climatic history of the Earth, most of all that portion of the record that can be reached by the radioactive isotope C-14, that is to say, the period from the later portion of the most recent ice age to the modern era. This subject of climatic history was familiar to him from the researches of his father and grandfather.

By the time Libby and coworkers had established the basic validity of the method in the early 1950s, Suess had begun to publish papers in English and to establish himself in the United States. Libby was beginning to offer instruction in the technique of the method to a few interested persons, and Suess was among them. In Libby's laboratory the samples for C-14 dating were prepared for counting by converting their carbon content to solid carbon, which was inserted in counters designed for their measurement. All but one of the early users of the method followed his procedures in detail. The exception was Hans Suess, who saw a virtue in counting the samples in the gas phase. The gas he chose was acetylene, C_2H_2, because of its high content of carbon (1954,1). Soon many others chose to use gases, but they all avoided acetylene because it is known to explode under some conditions. Suess quietly used it without problems throughout his career.

Although his research work from then on was carried out mainly in the United States, he continued to visit Germany and the German laboratories in his field, particularly the Max-Planck-Institut for Chemistry at Mainz, for the rest of his active life.

His own group's first successful C-14 laboratory was created at the U.S. Geological Survey in Washington, D.C. His first "date list" (1954,2) was published in 1954; three years after Libby's first list had appeared. His early results came mainly from samples of wood collected from locations in the northern U.S. states, usually found as stumps or logs knocked over by the advancing ice sheet. The dates clarified and extended the few measurements earlier reported by Libby's group. This subject continued to be central to his studies in the few years he remained in Washington.

Libby was particularly pleased by this first paper, as it provided proof that important C-14 results could be obtained by other workers than him. His remark to Suess and others was, "I never wanted to be the pope of C-14 dates." The number of productive C-14 laboratories increased rather rapidly thereafter in countries around the world.

Suess's work in this period was by no means confined to C-14 measurements. Another strong interest was the abundance in the sun and in meteorites of the chemical elements and their isotopes. This subject had made important progress through the work of V. M. Goldschmidt, which was summarized in a book (Goldschmidt, 1954) providing him with a starting point. However, the literature still contained many erroneous values, most of them too high, as was becoming apparent.

Suess's approach to laboratory work was to think calmly and thoroughly about the plan, and then do it right the first time. He might modify the system in small ways and then turn the day-to-day lab work over to a technician, or later sometimes to a student. His role was to calculate the results and then to write the paper.

Some details of his laboratory technique made good stories. I'll give two examples. His first successful C-14 laboratory was established at the U.S. Geological Survey in Washington, D.C. Meyer Rubin, his first assistant, became his successor there when Hans moved to California. The counting system worked well for about a year, after which it was necessary to call in an expert to diagnose and cure a malfunction. As they were taking it apart, Rubin remarked, "Probably the C-14 counter's high voltage center wire will be attached to the electronics by a paper clip." It was.

With the improvements in analytical techniques and progress in understanding the structure of the sun and other stars, improved chemical abundance data were becoming available. A paper by Suess and Harold Urey (1956) produced a further large step forward. Here one of Suess's most striking qualities was demonstrated, namely his remarkable ability to pick out correct values from a mass of unreliable numbers. His intuition in such matters was proverbial, and seldom failed. It was helped by his earlier work on the "magic numbers," which had led to the shell theory of nuclear structure and which also pointed to elements with especially high abundance. The graphs in that paper set the style for further improvements as data and theories became more reliable. Especially notable was the paper universally referenced since as B^2FH (Burbidge et al., 1957), which created the modern theory of the origin of the elements in stars, making much use of the Suess-Urey data.

It was soon after the appearance of these papers that Suess was recruited by Roger Revelle to join the small group of geochemists and geophysicists at what was soon to be known as the University of California, San Diego. Still housed in the Scripps Institution of Oceanography, this small group was the nucleus of an idea that emerged in response to the shock the United States had experienced when the Soviet Union was the first nation to launch a satellite into orbit around the Earth. It took some years for Suess's new C-14 laboratory to come into full existence, but in the meantime he could do some interesting theoretical work.

He joined Scripps Institution of Oceanography in 1955. After his arrival, it required years before his complete C-14 system was ready for use. I arrived in 1958 and was present one day when workers who were soldering his iron counter shield together using an acetylene torch had gone off for lunch. Hans came over to our lunch table on the grass looking very pleased. He had just disconnected the workers' acetylene tank from their solder gun and directed the acetylene gas flow through the counting system inside. Then he had turned on the counter and it worked. By the time the workmen returned from lunch he had concealed the evidence.

The first paper in a series, written by Roger Revelle and Suess (Revelle and Suess, 1957), was one of three that appeared in a single issue of the European journal *Tellus.* The others, covering much the same ground, were by Ernest Anderson and myself, and by Harmon Craig. These papers signaled the creation of a field with a new name, C-14 geochemistry, by analyzing the distribution of CO_2 among the major terrestrial reservoirs of carbon dioxide, the atmosphere, land vegetation, and the ocean. All the papers calculated in various ways the time rate of exchange of carbon dioxide among the reservoirs.

The Suess-Revelle paper was, however, the only one of the three to stress the growing quantity of CO_2 contributed by our burning of fossil fuel, and to call attention to the fact that it might cause global warming over time. This was later mentioned in the prize awarded to Revelle as the effect became visible.

Another paper by Suess on C-14 measurements was published in 1959. It reported the record of increase (briefly a doubling) in worldwide atmospheric C-14 as the result of the hydrogen bomb testing by the United States and the Soviet Union. This was also the first paper on which the name of George Bien appeared as a coauthor. The close collaboration between Suess and Bien continued for many years thereafter. Generally, Bien did the measurements and Suess the calculations and interpretation.

This paper was followed by a first Scripps Institution of Oceanography date list (1960,1). The next C-14 paper (1960,2) was another landmark. It reported the first extensive series of measurements of C-14 activity in carbonate from seawater, specifically in the Pacific Ocean, at various locations and depths accessible during cruises of the institution's research fleet. The purpose was to gather data that could be used to shed light on the movement and mixing of ocean water on a wide scale. The techniques for gathering seawater samples, and extracting the CO_2 from them onboard ship, while avoiding contamination or loss, had to be developed first. In modeling the data the effect of bomb-produced C-14 had to be taken into account, especially in the surface layers of the ocean.

The exchange of CO_2 across the air-sea boundary was shown in this paper to require a significant time. The transport of carbonate ions to deep Pacific water, and horizontally as well, required a much longer period, more than a thousand years in some cases. Suess's pioneering measurements provided a framework for the much more extensive surveys that followed.

By now he had published in a number of important fields, and he continued to widen his interests. A paper with Heinrich Waenke on C-14 in meteorites (1962,2) was the first of a long and valuable series of papers on meteorites and the light they shed on various processes in the solar system. It was soon followed by another (Suess, 1962) proposing a mechanism for the synthesis and accumulation of organic compounds in planetary atmospheres, with implications for the origin of life.

A field that preoccupied him until the end of his scientific career first surfaced in the mid-1960s (Suess, 1965; Stuiver and Suess, 1966). Its practical side was to produce precise corrections of C-14 dates for variations in the production of C-14 in the atmosphere. This could be determined by counting rings in suitable tree ring sections, an idea first developed at the University of Arizona into a reliable dating tool. De Vries in the Netherlands had been the first to show that in the seventeenth century (more precisely, during the reign of King Louis XIV), there was a measurable increase in the flux of cosmic rays entering the Earth's atmosphere, making C-14 ages in this period significantly too young. Suess undertook to extend this work backward in time, using overlapping tree samples made available by experts in Arizona and in Europe. Over many years he was to produce a widely accepted calibration curve going back eventually over 8,000 years. He demonstrated the existence of several other disturbances resembling that of this period. The shape of the calibration curve in such times has one unexpectedly ugly feature: For some decades during each such event the same activity can result from three separate dates, which may be spaced more than a hundred years apart.

In addition to their practical utility, these data suggested that a number of interesting processes might be responsible for the departures seen. Variations in the strength of the Earth's magnetic field are known to occur, especially in periods when the polarity of the field is reversing. A decrease in field strength lets more cosmic rays reach the Earth (and conversely). Another is sunspot intensity variations, since it is recorded that in the period reported by de Vries, the "Maunder Minimum," sunspots became very rare, again changing the flux of particles entering our atmosphere. This was a valuable contribution to the subject now called C-14 geophysics.

A theme that occurs often in the course of Suess's scientific work was "too soon." He often saw the deeper implications of his own work and that of others before they did. He often found frustration in their inability to grasp the clues that led him forward. The writers of this memoir more than once lacked vision in this situation. A related quality that was more widely appreciated was his ability to estimate quantities not yet determined by reliable experiments, for example, the abundance of some important elements in our sun (Revelle and Suess, 1957). The reason, perhaps, might have been the shorter time between his estimates and their experimental confirmation, but also that it is easier to grasp a successful estimate of a numerical quantity than that of a broadly applicable concept.

He was very much interested in the terms that others used to characterize particular phenomena. For example, at one period the leading students of rare gases embedded in stone or iron meteorites called one component "primordial rare gases." He saw that the word "primordial," meaning "present from the beginning," implied a model for which the evidence was weak, or even nonexistent. His response was to identify a colleague highly regarded in that field, in this case Peter Signer, and write a joint paper (1963,2) that introduced the term "trapped rare gases" instead. This paper changed the culture in the field quickly, since it eliminated what could now be seen to have been a false assumption.

Another example is closer to home. For a few years after a core group of chemistry professors (including Suess) formed the Chemistry Department of the University of California, San Diego, I taught the graduate course in quantum mechanics, an esoteric subject underlying all of chemistry. I taught from a textbook I'd learned it from in college, which in some ways seemed to me more like a cookbook than an insightful memoir. Then someone showed me a wonderful book by two Soviet theorists, Landau and Lifschitz, which introduced the subject from very clear first principles. So I used their text the next year. The students' eyes glazed over. They had no interest whatever. I told Hans my sad story, and he explained the effect for me. "People often confuse two unrelated ideas," he said. "One is 'simple' and the other is 'elementary.'" The new book was simple—and so was his explanation.

Those meeting him for the first time often came to the conclusion that he didn't work very hard. This resulted rather from the fact that he operated by picking important research problems, and spent a great deal of time thinking about them, with experimental work only used to confirm insights already arrived at, and to set the stage for the next steps. Not rarely he would ask his American friends, busy with proposals, organizing conferences, department meetings, and so on, "When do you have time to think?" There was no good answer.

Though his later scientific work was carried out almost entirely in the United States, he maintained close ties with friends in Europe. He traveled there often, and when that was not possible the transatlantic telephone was pressed into service.

Hans Suess was a professor at SIO and in the Department of Chemistry at UCSD during most of his scientific career. He was elected to the National Academy of Sciences in 1966 and received other honors as well. However, he always felt he was under-appreciated, and as a friend, I shared that opinion. I believe that the main root of this problem was that he had some of his best ideas "too soon," that is, before the rest of us had seen the steps between what was familiar and his new perception. He was going too fast for us. Still he enriched the lives of those of us who had the good sense to admire him, and to listen to him.

He lived in quiet retirement in his last few years and died of complex causes on September 20, 1990, at the age of 83.

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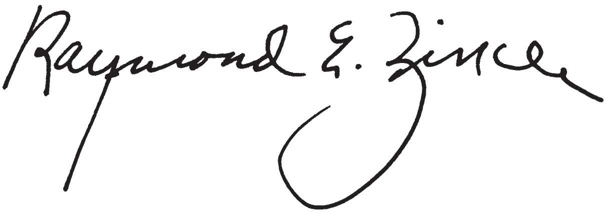

RAYMOND ELLIOT ZIRKLE

January 9, 1902–March 4, 1988

BY ROBERT P. PERRY

RAYMOND ZIRKLE WAS A pioneer in the field of radiation biology. He made seminal contributions to our knowledge of the effects of high-energy radiations on cells and devised ingenious means for using radiation to ablate small regions and individual structures of cells. His early studies of alpha-particle irradiation of fern spores—which identified the cell nucleus as the major target for radiation lethality and demonstrated the importance of linear energy transfer—were classical investigations of this type. In later studies he and his coworkers developed microbeam technology, which they used to dissect the functions of particular cell structures for chromosome movements during mitosis.

Ray Zirkle was born in Springfield, Illinois, and spent his early years on a farm in northern Oklahoma. His primary education was gained in one-room country schoolhouses in Oklahoma and later in southern Missouri. Both parents taught in country schools. In these rural settings Ray's main link to the outside world was through books, of which he was an avid reader. The first stimulus toward a scientific career might have been provided by his reading of *The Lost World* by Sir Arthur Conan Doyle. This adventure novel, an early example of science fiction, was serialized in a small weekly newspaper in the Missouri Ozarks. Nine-year-old Ray waited eagerly for each issue and was enthralled by the fantastic creatures and their exotic habitat.

After several years the Zirkles decided that farming in the Ozarks was not profitable, so they moved into the town of West Plains and bought a grocery store. From 1915 to 1919 Ray attended West Plains High School, where he showed an aptitude for mathematics and took several engineering courses. After graduation he joined the Missouri National Guard and served for several years. In 1924 he married Mary Evelyn Ramsey, who spent her early years in a rural area of western Kansas and, similarly to Ray, received her primary education in a one-room country schoolhouse. They had two children, Raymond Jr. in 1927 and Thomas in 1929.

In 1928 Zirkle received an A.B. from the University of Missouri, where he was elected to Phi Beta Kappa and to the honorary scientific society Sigma Xi. From 1928 to 1932 he carried out graduate studies in botany at the University of Missouri, earning his Ph.D. in 1932. During this time he served as an assistant (1928 to 1930) and later as an instructor (1930 to 1932) in the Botany Department.

Zirkle's thesis research was the forerunner of experiments and ideas that occupied him for the rest of his scientific career. In this research he irradiated spores of the fern *Pteris longiflora* with alpha particles emitted from a polonium source and studied the effects on their subsequent germination. The choice of alpha radiation with its relatively simple dosimetry and low penetrating power and the fern spores, which can be irradiated in a dry or slightly moistened state and have a size commensurate with the alpha-particle range, was well considered. In these premiere studies he observed that the alpha rays inhibited three distinct processes of germination: cracking of the spore wall, development of chlorophyll, and cell division. The effect on cell division was achieved at a substantially lower dose than those needed to inhibit the other processes. Most importantly, he noted that when the spores were oriented so that their nuclei were included in the radiation field, the effects were substantially greater than when the nuclei escaped irradiation. These results were developed and extended in his later work.

After receiving his doctorate Zirkle joined the Johnson Foundation for Medical Physics at the University of Pennsylvania in Philadelphia. Initially he had a fellowship from the National Research Council, which had also supported his graduate studies. He remained at Penn as a Johnson Foundation fellow and a lecturer in biophysics until 1938. During this time he investigated the quantitative relationship between ionization per unit path of alpha particles and their biological effectiveness, which was not clear-cut from the data obtained up to that time. By placing fern spore nuclei either near the beginning of the path (where the ionization density was low) or near the end of the path (where it was high) or in intermediate positions, he was able to calculate the number of alpha particles per nucleus that was necessary to produce a given effect, such as the inhibition of cell division. He found that the biological effectiveness is not only a function of the total number of ions formed in the nucleus but is also dependent on the variable concentration of ions formed in different portions of the path of the alpha particle. His data suggested the relationship $B = kI^{2.5}$, where B is the biological effectiveness per alpha particle, k a proportionality constant, and I the ionization per unit path. The quantitative aspect of this work was unusual for such studies at that time and established Ray Zirkle as a leader in the field of radiation biology. A generalization of these results to other types of radiation by Zirkle and others led to his later formulation of the concept of linear energy transfer.

Additional studies carried out at the Johnson Foundation, which enjoyed the interest and encouragement of its director, Detlev W. Bronk, dealt with the relative effectiveness of alpha particles, X rays, and fast neutrons on various biological materials and also explored environmental modifiers of radiosensitivity. In an incisive theoretical analysis he used existing knowledge of the specific ionization properties of different types of radiation to interpret experimental results with alpha particles, neutrons, and X rays. At one end of the spectrum are X or gamma rays, which set electrons in motion with the lowest specific ionization (ions per unit path traversed), and at the other end are neutrons, which accelerate mainly carbon, oxygen, and nitrogen nuclei with the highest specific ionization. Intermediate are protons and alpha particles. From this analysis he concluded that "the greater the specific ionization of a radiation, the greater the ionic effectiveness, that is, the less absorbed energy needed to produce a given degree of injury."

Zirkle developed a strong friendship with another Johnson Foundation fellow, the crystallographer A. Lindo Patterson, who became an assistant professor of physics at Bryn Mawr College in suburban Philadelphia when he completed his fellowship. A little later, in 1938, Zirkle was recruited to Bryn Mawr as an assistant professor of biology. Patterson and his wife, Betty, became lifelong friends of Ray and Mary despite both couples' moving to other academic locations.

In 1940 Zirkle was appointed professor of biology at the University of Indiana; however his academic career was interrupted during World War II when he became one of the principal investigators in the biological program of the Manhattan District. His research in this project was chiefly concerned with the comparative effects on living systems of fast and slow neutrons, beta rays, and gamma rays. A substantial part of the wartime research carried out under his direction was reported in several volumes of the National Nuclear Energy Series, of which he was the health editor.

Much of the biological research in the Manhattan Project was carried out at sites where particular radiation sources were located, such as the Clinton Laboratories near Oak Ridge, Tennessee; the Radiation Laboratory at the University of California; the National Cancer Institute in Bethesda, Maryland; and the Metallurgical Laboratory at the University of Chicago, which later became the Argonne National Laboratory. Research at the latter site brought Zirkle into contact with many faculty members from the University of Chicago who shared common interests with him. Thus, it is perhaps not surprising that in 1944 he was offered and accepted a professorship there and that in 1945 he became director of the newly founded Institute of Radiobiology and Biophysics. This institute, like the Johnson Foundation, became a focal point for scientists and students with a penchant for physics and an interest in biological problems. The Zirkles purchased a home in Olympia Fields, south of Chicago, which had space for a large flower garden. William Doyle, a faculty colleague of Ray's, recalled to me the many pleasant Saturday afternoons when he and his wife visited the Zirkles and enjoyed games of bridge with them.

The era of nuclear energy was spawned by the first chain reaction, produced by Enrico Fermi and colleagues under the West Stands of the athletic stadium at the University of Chicago. With this era came the Plutonium Project, which was assigned the task of purifying the artificial element plutonium for use in atomic bombs. The chain reaction used for making plutonium emitted huge amounts of gamma rays and neutrons. Moreover, diverse forms of radiation were present in the fission products produced in the purification process. Since these radiations posed serious hazards to personnel who would be involved in this project, it was important to set up stringent control measures for exposure and to carry out intensive radiobiological research in order to evaluate the nature and severity of the hazards. Zirkle was a major participant in the radiobiological studies, particularly in defining the acute lethal action of slow neutrons produced in the atomic piles. At the annual meeting of the Radiological Society of North America, held in Chicago in 1946, he hosted a symposium that brought together many of the other participants and summarized their major findings.

In the late 1940s and early 1950s Zirkle continued his theoretical and experimental studies of the effects of radiation on living cells. As knowledge of the chemical composition of biological material began to accumulate, he attempted to relate the chemical effects caused by the absorption of radiant energy to the ultimate biological effects. He fully appreciated that an understanding of the multitude of diverse radiobiological effects—such as gene mutations, chromosome breaks, increased membrane permeability, inhibition of cell division, induction of neoplasms, and lethality of cells and organisms—would require a detailed knowledge of the intervening chemical modifications. Yet, the level of knowledge of the molecular composition and dynamics of cellular constituents was still very primitive. The relationship of DNA and proteins to genes was still uncertain. Nothing was known about the existence of DNA repair mechanisms or the molecular basis of mitosis or the mechanisms responsible for cell proliferation and cell death. At this time one had to be content with discriminating direct from indirect effects of the radiation and for establishing criteria that could sort out the relevant chemical consequences of the ionization or excitation of molecules. Zirkle's analyses provided a rational conceptual framework for dealing with this complex problem.

Zirkle had a special interest in the quantitative aspects of dose-effect curves. Using methods based on the target theory of Timoféeff-Ressovsky and Zimmer, he developed mathematical formalisms for interpreting the experimental survival curves obtained when simple organisms were exposed to ionizing radiation. In a study with Cornelius Tobias the radiobiological influence of linear energy transfer was investigated with respect to the survival of haploid and diploid strains of budding yeast. At all values of linear energy transfer, the survival curves of the haploid strain were exponential, while those of the diploid strain were strongly sigmoid and of a shape essentially independent of linear energy transfer. They interpreted these results as being consistent with a theory in which cell division in haploid cells can be inhibited by inactivating any one of multiple chromosomal sites with a single ionizing particle, whereas in diploids it is necessary to inactivate both members of an allelic pair of corresponding sites. Although the observed variations of relative biological effectiveness with linear energy transfer were not consistent with simple target theory, they could be explained in terms of chemical intermediates that diffuse from their places of origin in the ionization tracks to the sensitive chromosomal sites. Further elaborations of yeast survival curve analysis were carried out by two of Zirkle's biophysics graduate students, Thomas H. Wood and Robert B. Uretz, as the subjects of their dissertation research.

During 1951-1952 Zirkle devised a new experimental approach to his long-standing interest in partial-cell irradiation. Together with William Bloom, a professor of anatomy at the University of Chicago, he developed the methodology for irradiating living cells with a microbeam of ionizing radiation and observing the consequences with time-lapse photography. Bloom, a coauthor with Alexander Maximov of a classical textbook of histology, brought to this project his knowledge of cell morphology, microscopy, and microphotography, as well as his expertise with the newly emerging technique of cell culture. Zirkle contributed his keen knowledge of the physics of radiation and of the instrumentation necessary to generate and control it. Their initial account of this project, published in a 1953 *Science* paper, was considered a tour de force.

The microbeam project necessitated careful consideration of a myriad of details. In order to produce a sufficiently intense collimated microbeam with minimal scattering, it was decided to use 2-Mev protons produced by a Van de Graaff electrostatic generator. An emergent macroscopic beam was allowed to impinge on a metallic shield pierced by a microaperture that produced microscopic beams as small as 2.5 microns in diameter. For biological material Zirkle and Bloom concentrated on actively dividing mitotic cells in cultures derived from newt heart. These large, relatively flat cells were excellent specimens for this type of experiment because of their favorable dimensions and their ability to proliferate at ambient temperature. The cells were grown on mica coverslips, 5 microns or less in thickness, in order to avoid serious energy attenuation of the protons. An effective locator system was devised for directing the microbeam to selected regions of the dividing cells and to ensure the observation of the same cell before and after irradiation. The cells were observed with customized phase-contrast microscopes and photographed with rigidly mounted 16-mm Bolex movie cameras. The highly skilled instrument makers and the superb machine shop facility at the institute were critical components of this endeavor.

The initial results of these experiments indicated the potential power of this approach for understanding the basic principles of chromosome movement and cell division. Irradiation of chromosomes in prophase or metaphase with a few dozen protons regularly produced fused chromosome bridges that interfered with the subsequent anaphase movement, or chromosome fragments that became detached from the spindle and formed small micronuclei after the completion of anaphase.

I vividly remember these experiments because I actually participated in them. In 1951 I entered the graduate program in biophysics, which was administered by a committee rather than a department. The committee was composed of a prestigious group of University of Chicago faculty members from various disciplines, many of whom had research laboratories in the building at 57th Street and Ellis Avenue, where the Zirkle and Bloom laboratory was located. On the first floor there was the laboratory of the physicist James Franck, who with Hans Gaffron was studying electronic transitions in the photosynthetic pigment chlorophyll. On the second floor, the physicist Leo Szilard and his colleague, Aaron Novick, were studying enzyme induction and feedback inhibition in the bacterium *E. coli*. The laboratory of Zirkle and Bloom was on the third floor. On the fourth floor, directly above the microbeam apparatus, was the temperamental Van de Graaff generator, its collimated proton beam protruding through a hole in the floor.

After I passed the requisite qualifying examinations in physics and physical chemistry, I joined the Zirkle-Bloom laboratory as a neophytic graduate student of Ray Zirkle. Before beginning my own thesis research I was engaged in the proton microbeam experiments in a variety of ways, the most interesting of which was keeping the mitotic cells in sharp focus during the pre- and post-irradiation filming. I soon learned that experiments of this complexity often do not go as smoothly as anticipated. Most trying on Zirkle's patience were problems with the generator's vacuum system or the operation of its belt, which seemed to happen at inopportune times when carefully staged mitotic cells were awaiting irradiation. He sometimes suffered from intense migraine headaches that seemed to be exacerbated by these annoying situations.

The technical difficulties with the microbeam experiments were eliminated when the radiation source was switched from protons to ultraviolet light. Zirkle's student, Robert Uretz, whose thesis research dealt with the additive effects of X rays and ultraviolet light on yeast cell survival, designed and built a relatively simple optical instrument that could focus an intense microspot of ultraviolet light on the mitotic cells. The ease with which this instrument could be used for perturbing the mitotic process by chromosome ablation or spindle destruction led to a large number of experiments, many with interesting, informative outcomes. Localization of the irradiated regions of chromosomes could readily be verified because the intense ultraviolet energy absorbed by the chromosomes caused a dramatic decrease in refractive index at the irradiated site and a concomitant loss of DNA from the site, a phenomenon that was termed "paling."

Zirkle and his coworkers selectively irradiated isolated centrophilic chromosomes that had not yet become aligned on the metaphase plate either in the kinetochore region or in a distal part of the chromosome. They observed that the normal movement of the chromosomes to the metaphase plate was inhibited when the kinetochores were irradiated, but not when the distal regions were irradiated. The chromosomes with ablated kinetochores drifted around until anaphase occurred and were squeezed into one of the two daughter cells after cytokinesis. This was the earliest observation of the importance of the kinetochore for what is now known as mitotic checkpoint control.

An especially striking effect termed "false anaphase" was observed when the mitotic spindle was destroyed by microbeam irradiation. In cells with irradiated spindles the orderly metaphase configuration of chromosomes became transiently deranged and then formed a quasirosette arrangement in which the kinetochores of whole chromosomes rather than sister chromatids were attracted to the centrosomes. Later the quasirosettes dissociated into two rosettes, followed by cytokinesis and nuclear reconstitution. In this case the chromosome complement was randomly distributed to the two daughter nuclei. These experiments clearly demonstrated a distinction between the molecular mechanisms responsible for moving chromatids to the spindle poles during normal anaphase and those responsible for moving the poles apart prior to cytokinesis.

Zirkle and Bloom amassed a huge collection of 16-mm movie films that documented the orderly progression through mitosis of normal cells and the abnormalities that occur in microbeam-irradiated cells. Zirkle continued to analyze this vast repository of data for more than a decade after the actual microbeam experiments were completed. In 1970 he published an extremely detailed account of his observations, some of which continue to be quoted until the present day. In the mid-1970s he retired from the university. The Zirkles moved to a home next door to their son Tom in the foothills of the Rocky Mountains, west of Castle Rock, Colorado. Zirkle died in a nursing home in Castle Rock in 1988.

Zirkle was a member of several scientific societies. He was president of the Radiation Research Society in 1952-1953 and a councilor from 1954 to 1956. He was a founding member of the Biophysical Society, in which he served as a councilor from 1957 to 1961 and again from 1964 to 1966. He served on the editorial boards of seven journals and on committees and study sections concerned with research in radiobiology and training in biophysics and medical science. He was honored by election to the National

Academy of Sciences in 1959 and to the American Philosophical Society in 1960.

Zirkle's research was carefully carried out, meticulously described, and cautiously interpreted. His experiments with alpha-particle-irradiated fern spores—which showed especially high sensitivity of nuclei compared with cytoplasm and demonstrated the importance of ionization density—were seminal discoveries, made prior to an understanding of the molecular basis of gene expression. Similarly his microbeam experiments provided the first evidence for the importance of kinetochores in mitotic checkpoint control. A colleague of mine who is presently studying the regulation of the mitotic process said, "Zirkle was ahead of his time. He doesn't get referenced as much as he should in the current literature." Nevertheless it is gratifying to know that his contributions are still appreciated. It is also noteworthy that these achievements were made by someone whose education began in a one-room country schoolhouse.

I AM VERY GRATEFUL for the personal information about Raymond Zirkle that was communicated to me several years ago by Mary E. Zirkle, Elizabeth Patterson, and William L. Doyle. I am also deeply indebted to Robert B. Uretz for critically reviewing this memoir and for helping me obtain reprints of Zirkle's publications, which were collected by the late Robert H. Haynes. A review of this memoir by Thomas H. Wood is also greatly appreciated.

HONORS AND DISTINCTIONS

Hitchcock Professor, University of California, Berkeley (1951).

Member of the editorial boards of the following journals: *Progress in Biophysics and Biophysical Chemistry* (1958-1962); *Biophysical Journal* (1960-1965); *Journal of Photochemistry and Photobiology* (1961-1964); *Radiation Research* (1953-1956); *Review of Scientific Instruments* (1948-1951); *Annual Review of Nuclear Science* (1953-1959); and the *Journal of Cellular and Comparative Physiology* (1951-1954).

Member of the following organizations: American Association for the Advancement of Science; Biophysical Society (councilor 1957-1961 and 1964-1966); American Physiological Society; American Society of Zoologists; Botanical Society of America; Radiation Research Society (president 1952-1953; councilor 1954-1956); American Society of Naturalists; American Society of Plant Physiologists; American Roentgen Ray Society; National Academy of Sciences; and the American Philosophical Society.

Member of the following committees: Biophysical Sciences Training Committee, National Institutes of Health (1958-1962); Radiobiology Study Section, NIH (1947-1949); Radiation Study Section, NIH (1955-1957); Subcommittee on Radiobiology, National Research Council (1947-1960; chair, 1953-1956); Committee on Nuclear Science, NRC (1953-1956); Medical Scientist Training Committee, NIH (1963-1967); U.S. National Committee for Pure and Applied Biophysics (1964-1968).

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