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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 91

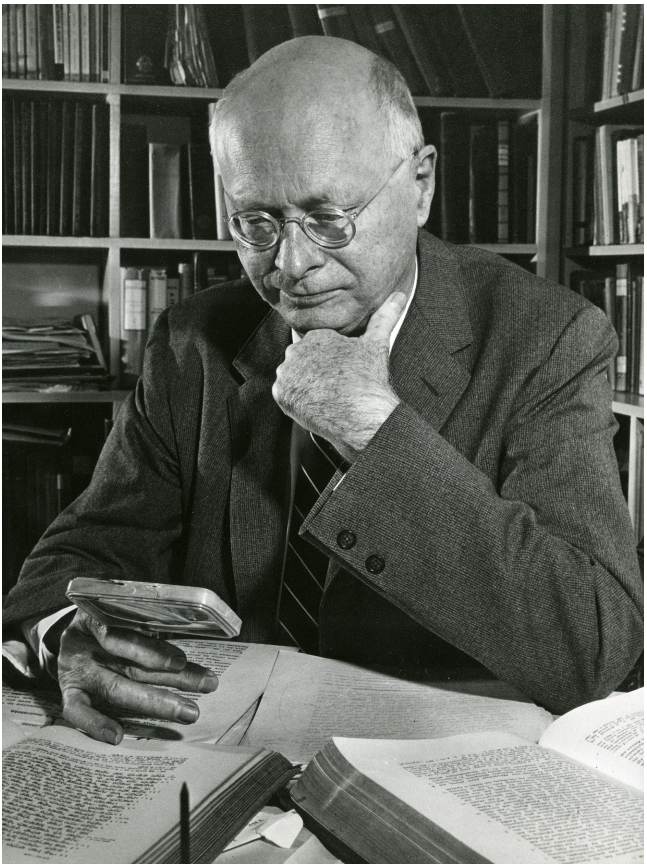

Photo Courtesy of Ferdinand Hamburger Jr. Archives, Johns Hopkins University.

W. F. Albright

WILLIAM FOXWELL ALBRIGHT

May 24, 1891–September 19, 1971

BY THOMAS E. LEVY AND DAVID NOEL FREEDMAN

ALTHOUGH THE GREAT AMERICAN SCHOLAR William Foxwell Albright passed away many years ago, he is still regarded by most Levantine archaeologists, biblical scholars, and other Near Eastern researchers of the world of the Bible as a genius. The word "genius" is not used lightly here. Albright was a master of so many disciplines linked to the study of the ancient Near East, in particular the world of the Old Testament (Hebrew Bible), that he is considered one of the last great orientalists. Having its origins as far back as the 12th century, orientalism was the study of the synthetic and simultaneous study of the history, languages, and culture of the peoples of Asia. By the 18th century, orientalists such as Sir William ("Oriental") Jones mastered 13 languages and "dabbled in 28." Unlike today's scholarly world of specialization the orientalist was a polymath able to work with multiple ancient and modern languages and in a wide range of scholarly fields. While the idea of the orientalist took on negative overtones through the work of postmodern researchers in the late 1970s and 1980s, more objective approaches by scholars such as the anthropologist Ernest Gellner and others have shown that orientalist scholars such as the German Gustav Dalman, Palestinian Toufic Canaan, Alois Musil from Moravia (now Czech Republic), and men like Albright carried out their works more from a sense of humanism and a profound interest in the history of the peoples of the Bible lands rather than as cynical tools of imperial powers. Even today ASOR (American Schools of Oriental Research), the flagship scholarly organization that Albright helped develop in the 20th century, retains the name that harkens back to the days when orientalism had a positive connotation.

Professor Albright's legacy today rests in his extraordinary record of scholarly publication. In 1941 biblical scholar Harry M. Orlinsky of the Hebrew Union College in Cincinnati assembled and published Albright's bibliography in honor of his 50th birthday (Orlinsky, 1941). At that time there were approximately 500 entries that spanned 30 years of scholarly work—an incredible amount of research that any scholar would be proud of. But this was only the midpoint in Albright's scholarly career that continued for another 30 years, with an additional 600 scholarly entries in the ledger. The grand total is just under 1,100 items, including books, peer-reviewed articles, notes, book reviews, and other items that must surely set a record for productivity in the field of ancient Near Eastern studies and related fields. A complete record of Albright's publications spanning 1916 to 1971 was prepared by one of us (D.N.F.) (Freedman, 1975) and was published as a book by the American Schools of Oriental Research.

Albright wrote with authority on the then developing field of ancient Near Eastern studies at a time when some of the most important discoveries were being made in the Holy Land (today's Israel, Palestine, Jordan, Lebanon, southern Syria, and the Sinai peninsula). The fields of scholarly research that Albright controlled were vast and included archaeology, Semitic linguistics (including all branches of the great family of languages, especially the numerous dialects of Northwest

Semitic, but not neglecting Akkadian and Arabic), epigraphy, orthography, ancient history, chronology, historical topography, mythology—in short, all facets of ancient Near Eastern civilization from the Chalcolithic period (ca. 4500 BCE) through the Greco-Roman period. Professor Albright was the recipient of an unparalleled number of honorary degrees and a multiplicity of honors and awards from distinguished universities, learned societies, and other institutions around the world. The honor he prized most was his 1955 election to the National Academy of Sciences. It is ironic that Albright chose to be a member of its Anthropology Section, since his quarrels with anthropologists of different schools were well known at the time.

Albright's scholarly authority in ancient Near Eastern studies was so profound that the intellectual paradigm that he helped create, biblical archaeology rooted in a fairly literal interpretation of the history embedded in the Old Testament, was unchallenged during his lifetime. While archaeology in the English-speaking world—primarily the United States and the United Kingdom—was undergoing a major paradigm shift in the 1960s with the birth of the new archaeology, archaeologists working in Syro-Palestine insulated themselves from this fundamental intellectual change. This was due in great part to Albright's authority based on the way he single-handedly shaped the archaeology of the Bible lands during his long career. It was only long after Albright's death that scholars began to challenge the paradigm he established, with forays into processual, and then post-processual archaeology.

In the remainder of this memoir we will try to understand the forces that shaped Albright's approach to ancient Near Eastern studies in the Holy Land and assess how he shaped so many disparate fields, such as biblical archaeology, Assyriology, Ugaritic studies, Dead Sea Scrolls research, and other areas. Finally a brief assessment of Albright's legacy will be made.

YOUTH AND EDUCATION

William Foxwell Albright was born in Coquimbo, Chile, on May 24, 1891. Albright's parents, Wilbur and Zephine, were earnest Christians and strict Methodists who before having children applied to the Methodist Episcopal Mission Board to work as missionaries on behalf of the church. The board appointed them to go to Chile, where the Rev. Albright served as the head of a boys' school in the copper port town of Coquimbo.

For the young Albright growing up in Chile in the late 19th century with a crippled left hand and severe nearsightedness was difficult. On a family trip to their home in Iowa, Albright tragically caught his left hand in a farm machine on his grandmother Foxwell's farm. The hand healed curled up with little movement and it wasn't until much later in life that he had an operation that slightly straightened it. Nevertheless, he felt himself crippled from the age of five. Faced with taunts from the poor children of the nearby *barrio* where Albright lived in Chile, the child faced a barrage of name calling such as *gringo* and *canuto* (protestant). By the time he was 10 years old his parents promised to buy him his most cherished wish, provided he would fetch bread from the local bakery to help save the family money. That wish was the two-volume *History of Babylonia and Assyria* recently published by Professor R. W. Rogers of Drew University. These volumes were the beginning of Albright's lifelong interest in the Near East.

By the time Albright was 22 he earned himself a much needed scholarship to Johns Hopkins University. Shortly before embarking on his studies, he confided to his mother and aunt his passion for the life of scholarship that awaited him in Baltimore by saying: “I am neither man nor woman. I am neither brute nor human—I’m a scholar!”

Albright’s road to becoming an orientalist was built on his voracious reading of ancient history and his self-taught Hebrew and Assyrian, in addition to the study of French, German, Latin, and Greek; these complemented his native languages, English and Spanish. For the first time, at Johns Hopkins University, Albright was exposed to Jews and was able to learn Modern Hebrew. Albright’s most important teacher was Paul Haupt, the distinguished professor of Semitic languages whose own polymath abilities in Assyriology, the Bible, Hebrew grammar, etymology, and lexicology (reflected in 522 scholarly works) no doubt had a lasting influence on Albright, the budding orientalist.

At the end of Albright’s first year of studies Professor Haupt offered him the generous Rayner Fellowship for the following year, which provided $400 plus $150 for tuition—$50 more than his first-year award. This welcome news came at a time when Albright wrote on several occasions that he felt he was on the edge of a nervous breakdown. He felt that if only his eyesight and his mental stability would continue strong until his written exams, he would prevail in his studies; and they did. By the end of his third year he spent 18 hours a day for three days at Prof. Haupt’s home in Baltimore writing terrifying exams for the Thayer Fellowship on Syriac, Arabic, Hebrew, Greek, Latin, French, and German; Hebrew Bible Literature and Criticism; geography; archaeology; history; and epigraphy. By the summer of 1916 Albright passed his oral exam and was awarded the Ph.D. for his dissertation on “The Assyrian Deluge Epic” (1916).

Not long after earning his doctoral degree Albright received his military draft questionnaire, filled it out, and dutifully returned it. With poor eyesight and a crippled hand, neither he nor his family expected he would be drafted or involved in the war in any way. However, by July 1918 while relaxing at home in Virginia and studying Ethiopic in a rocking chair, he received his call-up and was inducted into the U.S. Army for limited service. For just over six months Albright became a potato peeler and dishwasher for the war effort. By Christmas 1918 he was discharged and returned to Baltimore to resume his postdoctoral studies and teaching duties. It was then that he learned that he had been awarded the prestigious Thayer Fellowship with a stipend of $1,000—enough to barely pay for his first travels to Palestine. At this time funds were short and to save money Albright was still wearing his army uniform around town. Instead of rushing off to Palestine (ever since Albright was a boy he had worried that all the archaeological sites would be discovered before he arrived in the Holy Land), he held off for another half year and was made a Johnston scholar from Hopkins—an additional $1,200 to help the young scholar embark on what would become his life's intimate connection with Palestine.

ASSYRIOLOGY

From childhood Albright considered Assyrian to be the most challenging language and literature in the world. He also thought Benno Landsberger to be the best Assyriologist in the world, and the Assyrian dictionary then being produced by the Oriental Institute of the University of Chicago to be the greatest project ever. He envied Landsberger for being the guiding genius of the latter and would cheerfully have traded places either with Landsberger or with Albrecht Goetze (at Yale), because he considered them the leading figures in this field. Albright blamed his failure to achieve on the same scale on his poor eyesight. However, the archaeological world may be grateful that he devoted so much of his time to the Bible and its family of languages.

The scholarly mystique of Assyriology was cultivated in Albright by Professor James Alan Montgomery, who taught Hebrew and Assyrian at the University of Pennsylvania from 1909 to 1938. Montgomery was editor of the *Journal of Biblical Literature* and the *Bulletin of the American Schools of Oriental Research* (BASOR); he served as Albright's mentor when the latter became director of the American School of Oriental Research in Jerusalem from 1920 to 1929. Albright tried to model his behavior on aristocratic scholars such as Montgomery and his own teacher Haupt, the famous German orientalist from Johns Hopkins University and an expert in Assyriology. One should remember that Albright's background was primarily rural and he was quite conscious of this; born in faraway Chile, he grew up between farms in Iowa and South Dakota. This provincial background served as a catalyst for Albright in making Assyriology his first scholarly love. From 1912 to 1926 Albright published 35 original articles on Mesopotamian chronology, philology, history, literature, and religion. As pointed out by P. Beaulieu (Beaulieu, 2002), Albright left Assyriology after embarking on his landmark archaeological excavations at Tell Beit Mirsim, which directly influenced his choice to devote his career to the archaeology of Palestine.

BIBLICAL ARCHAEOLOGY

Albright's most enduring legacy is his contribution to the establishment of a new paradigm of ancient Near Eastern studies called biblical archaeology. More than any other scholar Albright's astounding corpus of books, articles, and public lectures defined a new relationship between archaeology and biblical studies. It was only after Albright's death that scholars had the gumption to seriously challenge the paradigm that Albright created. Albright defined the geographic and temporal focus of biblical archaeology as "all Biblical lands, from India to Spain, and from southern Russia to South Arabia, and to the whole history of those lands, from about 10,000 BC or even earlier, to the present time" (1966,1, p. 13).

Albright's expansive and deep-time perspective on biblical archaeology grew out of his generalist, or orientalist, approach to understanding the evolution of the biblical world in all its intricate facets. His 50-year odyssey of scholarly activity that resulted in a well-conceived and powerful scholarly paradigm began when Albright challenged the then dominant paradigm of Old Testament (Hebrew Bible) studies established by Julius Wellhausen (1844-1918), the German biblical scholar who developed the documentary hypothesis to understand the development of the written Bible and Herman Gunkel's form criticism that aimed at clarifying the oral traditions in the Hebrew Bible that preceded its codification.

The documentary hypothesis argues that the first five books of the Hebrew Bible (Genesis, Exodus, Leviticus, Numbers, and Deuteronomy), or the Torah/Pentateuch, are a collection of documents from four separate sources or editors and that all were combined by a single editor (called the redactor or R) sometime in the sixth century BCE. This was a radical departure from the earlier paradigm (still held by many devout Jews, Christians, and Moslems) that the Torah/Pentateuch was authored by Moses. Amongst the many assumptions in this hypothesis is the perceived very late writing of the Hebrew Bible, effectively casting doubt on the historicity of many events, peoples, and places who play critical roles in the text from the patriarchs and matriarchs (Abraham, Isaac, Jacob, Esau, Rebecca, Miriam, Moses, Joseph, et al.) to the Exodus from Egypt and the subsequent settlement in the land of Canaan, to the early Hebrew kings such as David and Solomon and so on.

In the early 20th century other biblical scholars such as Albrecht Alt and Martin Noth were especially interested in searching out the formative historical events that influenced the configuration of the Hebrew Bible accounts based on the internal analysis of the biblical text. Albright continued to engage in this type of internal analysis of the text using his arsenal of intellectual tools, but of more importance was his then new insistence on using external evidence from the archaeological record of the ancient Near East, in particular Palestine. Albright's definition of biblical archaeology reflects his intellectual approach to investigating the historical underpinnings of the Hebrew Bible: to situate ancient Israel in the broad traditions of the ancient Near East based on a comparative approach using both ancient texts and material culture.

Albright and his students, such as Nelson Glueck, had a significant influence during the inter-war years on American culture that included helping to shape the structure of curricula (theological, biblical, and ancient Near Eastern studies) at all the major universities—establishing what was perceived as the historical "truth" of the Old Testament, as well as the popular notion of archaeology in society as highlighted by Glueck's speech at the inauguration of President John F. Kennedy and his being featured on the cover of *Time* magazine. As the British archaeologist Roger Moorey (Moorey, 1991, p. 55) observes, "In retrospect the years between the World Wars have come to be seen as the time when biblical archaeology, particularly through men like Albright and Glueck, had an academic status and a self-confidence that it had not enjoyed before and was rarely to achieve again." How did Albright achieve this Olympian status in the scholarly world and public perception as evidenced by his remarkable number of publications, honors, awards, and accolades?

While Albright did not have a long career as a field archaeologist, the work he carried out had a profound influence on the archaeology of the southern Levant at the time and continues to this day. Albright carried out his first excavation in Palestine at Tell el-Ful, a site he identified as Gibeah of Saul. According to J. P. Dessel (Dessel, 2002, p.43) the selection of Tell el-Ful as a major excavation project in the early 1920s was unusual since it was the large tell sites with respected biblical pedigrees that were targeted at that time in the first wave of field archaeology in the Holy Land in the late nineteenth and early twentieth centuries. He chose Tell el-Ful in part because of its biblical reputation as an Israelite regional social and religious center and for logistical reasons; it would be cheaper to excavate a smaller mound than a major site. Following a similar pattern between 1926 and 1932, Albright directed four excavation seasons at the obscure site of Tell Beit Mirsim situated near the junction of the southern Judean hills and the Shephelah region some 20 km southwest of Hebron. These excavations, his innovative ceramic analysis, and their rapid publication are what established Albright's influence as a leading archaeologist.

There is no consensus on why Albright chose this site and not one of the more famous ancient mounds that retain clear links to the Hebrew Bible: places like Hazor, Gezer, and Dan. Early on, Albright proposed identifying Tell Beit Mirsim with biblical Debir (Kiriath-Sepher), which means "city of the book" or "scribe." According to Philip J. King (King, 1983, p. 80) in his authoritative *American Archaeology in the Mideast—A History of the American Schools of Oriental Research*, it was this tentative identification that interested Melvin G. Kyle, then president of the Xenia Seminary, to finance the excavation. The implication of the name Kiriath-Sepher may have led Kyle to believe that this ancient Canaanite site would yield a rich trove of cuneiform tablets and other inscriptions. While scholars continue to debate whether Tell Beit Mirsim is indeed biblical Debir, the importance of Albright's research at the site stems from his studies of the ceramic material from the Middle Bronze through Iron Ages that made it the type site for Palestinian archaeology for more than 60 years.

Albright built his ceramic analysis of the material from Tell Beit Mirsim on the 1890 work of the great British archaeologist Sir Flinders Petrie at Tell el-Hesi located on the edge of the Negev coastal plain. He relied especially on Petrie's development of the revolutionary principle of seriation, a relative dating tool still used by archaeologists around the world today. While Petrie had established his career as an Egyptologist, he was invited to excavate in the Holy Land by the Palestine Exploration Fund as part of their effort to gain a foothold in the country. Petrie's knowledge of Egyptian material culture from Predynastic to later periods enabled him to establish a fairly reliable dating for the Palestinian pottery found in association with Egyptian artifacts in the various strata at Tell el-Hesi. Thus, Albright used the stratigraphic record and the rich collection of pottery found at Tell Beit Mirsim to pioneer the establishment of the first rigorous ceramic chronology for the second and early first millennia BCE of Palestine. Following his first season of excavation, Albright (1926,1, p. 6) wrote: "As will be seen, we have an extraordinary opportunity here for highly interesting discoveries, and best of all to the archaeologist, excellent conditions for the study of pottery, since the strata are horizontal and exceptionally well defined."

Central to Albright's methodology was the typological method he developed for the subfield of ceramic analysis. As will be seen below, Albright's general concern and interest in methodology—whether it be in archaeology, ancient history, or epigraphy—helped him set the research agenda in Near Eastern studies during his lifetime. While some recent scholars have criticized Albright's reliance on loci with uniform ceramic deposits (i.e., not mixed with material from different periods) and the fact that he generally ignored material from debris layers, it remains that for the first time Albright brought systematic order to the key element of material culture associated with archaeological periods linked with the Old Testament in the southern Levant for all phases of the Bronze and Iron Ages. According to Larry Herr (Herr, 2002, p. 52), while earlier scholars such as Petrie ushered in typological and chronological analysis of artifacts and pottery, it was Albright who raised the standard of pottery publication and presentation in the early 1930s. Whereas earlier scholars published only complete vessels, Albright saw the utility of studying broken pottery sherds, carefully illustrating rim profiles, and using only the highest quality photographs. In addition, Albright's stratigraphy at the site was remarkably clear, making his work the most advanced of his day and unchallenged until relatively recently (Greenberg, 1987). Of key importance was Albright's timely reports on the excavations at Tell Beit Mirsim, published in the *Annual of the American School of Oriental Research* between 1926 and 1933, which present the first clear ceramic chronology for Palestine based on his careful stratigraphic analyses. These studies, presented in four volumes, became the foundation for the ceramic typology and chronology for the Holy Land still utilized today. Albright's typological study collection of Tel Beit Mirsim is still housed in the basement at the American Schools of Oriental Research facility in Jerusalem that now bears his name: the W. F. Albright Institute of

Archaeological Research (formerly the American School of Oriental Research).

Some scholars have tried to belittle Albright's ceramic edifice because they claim that his identification of Tell Beit Mirsim with biblical Debir (Joshua 15:15-17, Judges 1:11-15) was wrong and that the correct identification should be Khirbet Rabud. Without this "historical" link for Tell Beit Mirsim, it is claimed that his chronological framework is deeply flawed. However, as noted above, it was never Albright's intention to excavate a major biblical site but rather a locale where, amongst other issues, the ceramic typology of ancient Palestine during biblical times could be investigated.

Albright's methodological vision of using ceramic typology to help refine the dating of the archaeological record of the Holy Land was truly pioneering and helped lay the foundation for the first systematic archaeological field surveys by one of his most famous students, Nelson Glueck. During the 1930s, Glueck, using camels and donkeys, single-handedly surveyed most of western Transjordan (previously known as eastern Palestine) revealing for the first time an archaeological past that could be linked, with Albright's ceramic typological system, to western Palestine. As these two regions are part of the same geographic territory where so much of the Hebrew Bible narrative takes place, Albright's ceramic work at Tell Beit Mirsim was the single most important research tool enabling the historical archaeology of the Holy Land to take on even more importance than it previously held. During Albright's tenure as director of the American School of Oriental Studies in Jerusalem from 1921 to 1929 (and semiannually from 1932 to 1935), the American School in Jerusalem became the major foreign center of archaeology in the city and the hub for the analysis of pottery. Albright's style of publishing ceramics was enormous. The growing number of Jewish (later Israeli) archaeologists working in Palestine adopted his system, as did many researchers from European and British institutions. When the noted Israeli archaeologist Ruth Amiran published what is still the handbook of pottery analysis entitled *Ancient Pottery of the Holy Land*, shortly before Albright's death in 1970, he wrote her a thank-you letter for the copy of the book and for dedicating it to him. Thus, the foundations that Albright laid for ceramic analysis, the most important relative dating tool in archaeology in the Holy Land, are still very much used by scholars today.

EPIGRAPHY, UGARITIC STUDIES, AND ALBRIGHT

It is well known that the decipherment of Egyptian hieroglyphics and Assyrian cuneiform during the 19th century opened up the worlds of ancient Egypt and Assyria to scholarship and the public imagination. The stimulus for the birth of both Egyptology and Assyriology was the quest to expand the investigation of the biblical world to ancient Israel's most important neighbors. By 1926 Albright had mastered more than 26 ancient and modern languages but his love of the southern Levant drew him to focus on those ancient languages that led to formation of Hebrew. Specifically, he concentrated on Proto-Canaanite, the earliest Northwest Semitic alphabetic text and scripts that date before ca. 1400 BCE, which are primarily pictographic in character. Albright studied the Proto-Canaanite inscriptions found by Petrie in the southwestern Sinai Peninsula and first published by Alan Gardiner in 1916 as well as a small number of examples found in Palestine.

ALBRIGHT AND THE DEAD SEA SCROLLS

With the discovery of the Dead Sea Scrolls in 1947-1948, Albright was the first scholar to authoritatively assess them as "the most momentous discovery in modern times pertaining to the Bible." Like the discovery of the early East African hominid skeleton Lucy and Tutankhamen's tomb, the Dead Sea Scrolls are one of the few archaeological discoveries that have changed existing paradigms and caught the imagination of the world community. The Dead Sea Scrolls still attract hundreds of thousands of visitors each year to exhibitions held around the world because they include the earliest copies of the Old Testament (Hebrew Bible) dating from around 200 BCE to 100 CE. At the time of the scrolls' discovery the oldest copies of the Hebrew Bible dated to ca. 100 CE. It was his prowess as an epigrapher specialized in Northwest Semitic scripts, especially all the known variants of ancient Hebrew, that "pre-adapted" Albright to immediately understand the great significance of the scrolls shortly after their discovery by some Ta'mireh Bedouin shepherds in caves around the western shore of the Dead Sea. At the time of their discovery many scholars could not believe in the antiquity of the scrolls and some argued they were fakes or dated to the medieval period. How was Albright able to assess the antiquity of the scrolls so fast?

Some 12 years before the discovery of the Dead Sea Scrolls the *Baltimore Sun* published an article stating:

The world for three decades has possessed, without knowing it, a fragment of the Old Testament in Hebrew which was written before Christ, it has been determined by Dr. William F. Albright…This fragment, the Nash Papyrus, long has been recognized as the oldest Hebrew copy of the Aramaic script in which it is written; however, Dr. Albright has discovered it was from a much earlier period. It was written less than a century after the writing of the latest books of the Old Testament.

This fragment contains the Ten Commandments and the *Shema Israel*, used as a Jewish prayer and it was bought early in the 20th century by an Englishman, Walter L. Nash, from locals in Egypt and given to the Cambridge Museum.

In 1937 Albright published his study of the document ("A Biblical Fragment from the Maccabaean Age: the Nash Papyrus") in the *Journal of Biblical Literature.* Years later this study proved to be foundational for assessing the age of the Dead Sea Scrolls.

In 1948 the scrolls were brought to the American School of Oriental Research in Jerusalem for evaluation, where they were photographed by John Trever. Trever airmailed two small Leica photographs of a column or two of the scrolls that had been brought to the school on February 18, 1948, by Metropolitan Athanasius Yeshue Samuel and Father Butros Sowmy of St. Mark's Monastery for the evaluation. David Noel Freedman recalls Albright saying that within an hour of first looking at the photographs he knew it was a genuinely ancient discovery and that the scrolls dated from the last two centuries BCE and the first century CE. As Mrs. Albright related the story, it may have taken Albright 20 minutes to form a judgment, and 19 of those minutes were spent trying to find his 1937 article on the Nash Papyrus, with photograph, somewhere on his stacked desk. This was an example of Albright's remarkable memory for form and detail. He recognized in the tiny Leica photographs four letters with distinguishing characteristics that were definitely older than those he had written about and dated in the Nash Papyrus over 10 years earlier. As editor of the *Bulletin of the American School of Oriental Research,* Albright published an article in the April 1948 volume announcing the discovery of the Dead Sea Scrolls. In the October 1949 volume Albright himself published one of the first scholarly articles on the scrolls' discovery, entitled "On the Date of the Scrolls from Ain Feshkha and the Nash Papyrus," that included a good infrared photograph of the Nash Papyrus for comparison. When news of Willard Libby's new method of dating ancient remains using carbon 14 (^{14}C) reached Albright, he announced in an Associated Press article that he was eager to try out the new technique in Egypt and the Bible lands; Albright was always eager to apply new discoveries and new methods to archaeological and biblical research.

Albright's early assessment of the antiquity of the Dead Sea Scrolls played a critical role in determining the authenticity of this remarkable discovery, their importance for future scholarly research as well as their rapid purchase for museums in Israel and Jordan and their ultimate conservation for future generations.

IN CONCLUSION

While Albright made very few scholarly mistakes in his life, he himself described one or another admitted errors as beauties. He was sure that Ekron, the ancient mound site in southern Israel, was not where the more recent excavations have proved it to be. In addition, Albright was convinced that the patriarchs and matriarchs could be located in the Middle Bronze Age and his famous interpretation of Genesis 14 has proved to be unworkable.

Frank Moore Cross and David Noel Freedman mentioned in a footnote to their book on *Early Hebrew Orthography* relating to the famous ninth-century BCE Mesha or Moabite stone (found in Transjordan) that the chronology of both Mesha and the Bible works better if one doesn't emend the text and drastically reduce the number of years it represents, as Albright in fact did. It is interesting that when Albright reviewed the manuscript, he left the footnote as it was.

Albright was the leading figure in the fields of Levantine archaeology and biblical studies for most of the 20th century and produced more major scholars than anyone else. A total of 57 Ph.D. dissertations were produced under his guidance. Albright's students, such as G. Ernest Wright, trained the senior cadre of biblical or Levantine archaeologists leading the field today, including W. G. Dever, Lawrence Stager, and others in the United States. During Albright's lifetime, British and European researchers were also deeply influenced by his view of the relationship between the Old Testament and archaeology. Consequently, Albright has left a lasting imprint on the nature of historical archaeology in the southern Levant and biblical studies in general. In spite of attempts to deconstruct Albright by leaders in the field of archaeology today (Dever, 1993) as a positivist, heavily influenced by his own religious conservatism and methodological flaws, these charges fail to appreciate the revolutionary impact Albright's establishment of a new scholarly paradigm—biblical archaeology—continues to have on Levantine archaeology.

Albright deeply influenced the development of archaeology in the newly founded state of Israel, where his biblical archaeology paradigm continues to play a role in shaping research directions and the study of historical archaeology at the major institutions, such as the Hebrew University, Tel Aviv University, Bar Ilan University, Ben Gurion University, and other organizations where the aim is to situate biblical history in the greater context of the ancient Near East by taking an interdisciplinary approach as first advocated by Albright. Similarly, in the United States and a number of European countries (Germany, Switzerland, Norway, the United Kingdom, and others) wherever the archaeology of the southern Levant is taught, in spite of different approaches (traditional historical, biblical minimalist), the general questions regarding archaeology's relationship with ancient text as formulated by W. F Albright during the 20th century still lay at the core of the field.

CHRONOLOGY

1891	Birth, May 24, Coquimbo, Chile (U.S. citizen)
1912	B.A., Upper Iowa University
1916	Ph.D., Oriental Seminary, Johns Hopkins University
1918	Military service in the U.S. Army
1919	Thayer fellow, American School of Oriental Research
1920-1929	Director, American School of Oriental Research, Jerusalem
1921	Marries Ruth Norton, August 3
1922	Director, excavations at Tell el-Ful, Palestine Mandate
1926-1932	Director, excavations at Tell Beit Mirsim, four seasons, Palestine Mandate
1929-1958	W. W. Spence Professor of Oriental Languages, Johns Hopkins University
1930-1968	Editor, *Bulletin of the American School of Oriental Research*
1932-1935	Director, semiannual basis, American School of Oriental Research, Jerusalem
1937	Publishes study of Nash Papyrus establishing basis for authenticating Dead Sea Scrolls
1946	Visiting professor, University of Chicago
1947-1948	Authenticates the Dead Sea Scrolls
1947-1948	University of California African Expedition to Sinai Peninsula with Wendell Phillips
1949-1950	Expedition to Saudi Arabia with Wendell Phillips
1955	Elected to the National Academy of Sciences
1956	Establishes the *Anchor Bible* commentary series with D. N. Freedman
1958	Retires from Johns Hopkins University
1969	Declared *Ya'qir Yerushalyim* ("notable of Jerusalem") by the President of Israel
1971	Death, September 19, Baltimore, Maryland

SELECTED AWARDS AND HONORS

1936	Honorary Th.D., Utrecht University
1946	Honorary Th.D., University of Oslo
1949	Honorary L.H.D, St. Andrews University
1951	Honorary Litt.D., Yale University
1952	Honorary Litt.D., Harvard University
	Honorary L.H.D., Wayne State University
	Honorary L.H.D., Manhattan College
1956	Fellow, American Academy of Arts and Sciences
1957	Honorary Th.D., University of Uppsala
	Honorary D. Phil., Harvard University
1964	Honorary LL.D., Johns Hopkins University
1967	Gold Medal for Distinguished Archaeological Achievement, Archaeological Institute of America
	Award for Distinguished Scholarship in the Humanities, American Council of Learned Societies

SELECTED MEMBERSHIPS

American Oriental Society (president, 1935)
American Philosophical Society (vice president 1956-1959)
American School of Oriental Research
International Organization of Old Testament Scholars (president, 1956-1957)
Palestine Exploration Society (president, 1921-1922, 1934-1935)
Society of Biblical Literature (president, 1939)

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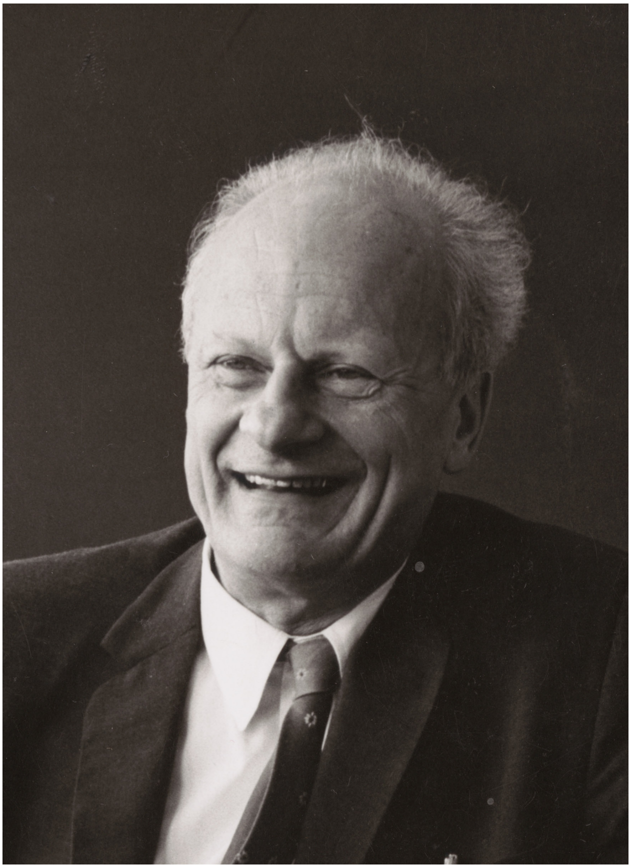

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H. A. Bethe

HANS ALBRECHT BETHE

July 2, 1906–March 6, 2005

BY GERALD E. BROWN AND SABINE LEE

HANS ALBRECHT BETHE, who died on March 6, 2005, at the age of 98, was one of the greatest physicists of the 20th century, a giant among giants whose legacy will remain with physics and the wider science community for years to come. He was universally admired for his scientific achievement, his integrity, fairness, and for his deeply felt concern for the progress of science and humanity that made him the "conscience of science." Bethe studied theoretical physics with many of the greatest minds within the physics community, including Sommerfeld, Ewald, and Bohr. His Jewish background made a career in Germany all but impossible, and after a brief spell in England between 1933 and 1935, he emigrated from Germany to the United States. He took up a post at Cornell University where he remained, with exceptions of his work at Los Alamos and several sabbaticals, until the end of his career. Hans Bethe was a universalist who contributed to scientific research for more than seven decades. He was awarded the Nobel Prize for his work on energy production in stars. Many other of his discoveries would have been worthy of a Nobel Prize, for instance, his

Adapted from the memoir published in *Biographical Memoirs of Fellows of the Royal Society*, vol. 53, pp. 1-20, 2007. Used with permission.

work on the Lamb Shift or the "Bethe Ansatz." Like many of his colleagues who had contributed to the development of nuclear weapons, Hans Bethe devoted much of his time and energy to the control of these weapons, to nuclear disarmament, and to the promotion of greater understanding between East and West, most notably through his activities within the framework of the Pugwash movement.

EARLY YEARS

Hans Bethe was born in Strasbourg, in Alsace-Lorraine (then part of Germany) on July 2, 1906. His parents were Albrecht Julius Bethe (1872-1954), at the time *Privatdozent* in physiology at Strasbourg University, and Anna (née Kuhn, 1876-1966), a talented musician and writer of children's stories. Both grandfathers were physicians: on his father's side a general practitioner in Stettin, northern Germany (now Szczecin, Poland), and on his mother's side a professor at the University of Strasbourg with specialization in ear, nose, and throat diseases.

Hans Bethe showed an early interest in numbers, discovering for himself the basic principles of arithmetic, including the decimal system. He was close to his father and they often talked about scientific matters. Albrecht knew some mathematics, mainly algebra, and he taught his son the use of the slide rule. Hans would use the slide rule for the rest of his life and when the most detailed calculations were made on supercomputers, he could be found behind a stack of computer output, analyzing it with the help of his slide rule. Father and son took long walks together talking about politics of the present and about German and ancient history. These early extensive conversations instilled in Hans an awareness of political developments and a sense of responsibility for shaping the world around him, be it as a scientist, a teacher, or a political advisor.

STUDENT YEARS

In 1924 Bethe enrolled at the University of Frankfurt in chemistry. He soon discovered that chemical experiments consumed too many lab coats and he switched to physics and within physics to theory. He was fascinated by the lectures of the ebullient Walter Gerlach and later of his successor, the spectroscopist Karl Meissner, who told Hans emphatically that he must not stay in Frankfurt but should go to a place with better theoretical physics. On his recommendation, Bethe applied to Arnold Sommerfeld in Munich for admission to his seminar. In 1926 Arnold Sommerfeld was the most influential physics teacher in the world, his recent prize pupils having been the future Nobel Prize winners Wolfgang Pauli and Werner Heisenberg. Sommerfeld worked in every area of theoretical physics and his lectures formed one of the best introductions to many branches of physics.

At Sommerfeld's institute, collaboration and exchange of scientific ideas were encouraged. German graduate students and foreign postgrads shared one big room that also served as a library and a place of scientific discussion. Here Bethe met Rudi Peierls, a year younger than himself, super quick of mind and congenial in his approach to life. Soon there were joint Sunday expeditions to the Alps for walking and skiing, and, most importantly, they enjoyed each other's humor which made possible political discussions, a very necessary relief in those years of the rise of Hitler. The friendship continued throughout their lives and included scientific collaboration, at first in England and later in Los Alamos where Peierls was part of the British Delegation.

Sommerfeld's students benefited from the respect in which the theoretical physics community held this grand-master of their discipline. New ideas and preprints of papers would land on Sommerfeld's desk for comment, and Sommerfeld passed them on to his students for discussion in his advanced seminar. Schrödinger's and de Broglie's work on wave mechanics was being developed in 1925-1926 and Bethe often argued that he entered the scene of serious theoretical physics research at an ideal time. Unencumbered by old concepts and theories, he was both keen to study and quick to appreciate the new ideas. His doctoral thesis, suggested by Sommerfeld, was a theoretical analysis of the Davisson and Germer paper on diffraction of electrons by crystals (Davisson and Germer, 1927). Since electrons have wave properties, as evidenced by their de Broglie wavelength, they diffract in crystals in a similar way to X-rays. The theory of x-ray diffraction had initially been formulated in 1912 by Max von Laue based on suggestions by Paul Ewald, and had been demonstrated in famous experiments by Walther Friedrich and Paul Knipping (Friedrich, Knipping and Laue, 1913). Sommerfeld suggested that Bethe look at a paper by Paul Ewald in which he had presented a dynamical theory of the diffraction of X-rays by crystals (Ewald, 1917). Bethe adapted Ewald's approach to the wave mechanical description of electron diffraction and found that it yielded very satisfactory results. (Bethe and Hildebrandt, 1988). Bethe started out by producing a more or less direct translation of Ewald's thesis from X-ray to electron diffraction and found out that it worked very well.

EARLY CAREER

After a short period in Frankfurt in 1928, Bethe became Paul Ewald's assistant at Stuttgart. The move to Stuttgart was a happy one, coming as it did at a time when Bethe's personal life was less than happy due to his parents' divorce in 1927. At Stuttgart Bethe was made welcome in the institute and in Ewald's family; many years later, in 1939, he married his mentor's daughter Rose.

Bethe thoroughly enjoyed his work at Stuttgart; Ewald was working on crystallography, the topic of Bethe's Ph.D. thesis. Bethe's knowledge was sought by colleagues and students alike. He was asked to lecture twice a week on the new quantum mechanics to Ewald and all of his assistants, and to the numerous visitors, who came from all over the world to study with Ewald. Werner Ehrenberg, assistant to the professor of experimental physics Erich Regener, once famously remarked: "If you want to see Hans, the line starts to form at 10 o'clock!"

In the midst of Bethe's happy situation in Stuttgart, Sommerfeld returned from a trip around the world. He wrote a postcard to Ewald saying, in effect, "Bethe is my student. Send him back to me immediately." Ewald could do little but obey his former teacher's request, and Sommerfeld created an attractive package for Bethe that allowed him to become a *Privatdozent* the following spring as well as provided a fellowship and a general travel allowance.

At Munich in the winter of 1929 Bethe wrote what he considered to be his best paper on the theory of the passage of fast corpuscular rays through matter (1930). The paper was submitted as his habilitation thesis, the research paper required to become a *Privatdozent.* It established the theory that has been of great importance for the interpretation of experiments using cosmic rays and particle accelerators. Thus, by the age of 24 Bethe had already left his mark in his chosen field of scientific research with thorough, insightful, and innovative contributions of long-lasting impact.

When Bethe was awarded a Rockefeller Fellowship for 1930-1931, he decided to visit Cambridge and Rome. In Cambridge he was welcomed particularly by Ralph Fowler and Patrick Blackett. In their company he discovered a new relaxed, yet respectful lifestyle in which in particular politics could be discussed without confrontation. In fact, it relaxed him so much that he forgot how SERIOUS science was and together with two other young visiting German scientists wrote a spoof: "On the Quantum Theory of the Temperature of Absolute Zero" in centigrade. It appeared in *Naturwissenschaften*, and he had to publish an apology as well as endure the anger of his beloved teacher, Geheimrat Sommerfeld. Later he would frequently recall how formative his work with Enrico Fermi in Rome had been for him. From him Bethe learned to look at a problem qualitatively first, and understand the problem physically before putting lots of formulas on paper. In contrast with Sommerfeld, whose method was to begin by inserting the data of a problem into an appropriate mathematical equation and solving the equation quantitatively according to the strictest mathematical formalism for those specific data, for Fermi the mathematical solution was more a confirmation of his understanding of a problem than the basis for its solution.

Although Fermi's main interest during the period of Bethe's visit was low-energy neutron scattering, he and Bethe coauthored a paper comparing three methods of treating relativistic electron-electron interactions (1932). In general, however, although Bethe took an interest in the experiments, he worked on his own. He worked out the solution of the linear chain during that period, introducing what C. N. Yang later named "The Bethe Ansatz" (1931). Of all his works, this result probably has had the most influence over a wider variety of fields.

Bethe's style of doing physics and of teaching became an amalgam of the influence of his two great teachers: Sommerfeld's mathematical rigor and Fermi's joy in the challenge of the problem at hand. Both encouraged free exchange of ideas and the close relation of theory and experiment. Bethe himself expressed his indebtedness to these two great teachers by saying, "If I am, in German parlance, my students' doctor father, they have two doctor grandfathers."

On his return to Munich, in collaboration with Sommerfeld, Bethe wrote one of his three great review articles "Elektronentheorie der Metalle" (1933). In fact, Sommerfeld wrote the first chapter and Bethe wrote the rest of the book.

In the summer of 1932 Bethe was offered an assistant professorship at Tübingen, but after Hitler's ascension to power in January 1933 and the enactment of the racial laws, Bethe was dismissed from his post in April 1933. His mother being of Jewish origin, by the new laws he was no longer regarded fit to serve the state. Bethe gladly accepted a temporary lectureship at Manchester. It allowed him to leave Germany and brought him together again with Rudolf Peierls, who also held a lectureship at Bragg's institute. Peierls was now married and living in a large house, which Hans happily shared.

Bethe often referred to the year 1933-1934 as his most productive. Working (and lodging) with Peierls was highly enjoyable for Bethe, and their collaboration produced several noteworthy papers. On the occasion of a visit to Rutherford's Cavendish Laboratory in Cambridge, James Chadwick acquainted them with an experiment carried out with a bright young graduate student Maurice Goldhaber on the photodisintegration of the deuteron (then called diplon). Chadwick challenged them to work out the theory of this reaction. Trains took a long time to go cross-country in England at that time, about four hours from Cambridge to Manchester. Bethe and Peierls had a solution to the problem by the time they reached Manchester (1934[1], 1935).

That year they also wrote two short papers on the neutrino (1934 [2]) and a paper on neutron-proton scattering (1935). In a different collaborative effort, Bethe and Walter Heitler wrote the paper "On the Stopping of Fast Particles and on the Creation of Positive Electrons" (1934[3]).

During an earlier visit to England, Bethe had made the acquaintance of Nevill Mott, who in 1933-1934 was a professor at Bristol. Bethe gave a talk there and intimated he would like to join the department. A few weeks later Mott offered him a fellowship at Bristol for a year. But in the summer of 1934 Bethe got a cable, seemingly out of the blue, from Cornell University offering him an acting assistant professorship, with the prospect that it might be made permanent. He accepted with some trepidation about going alone so far away from his family. But it was a good move: he remained a Cornell professor for the rest of his life, teaching for 40 years and officially retired for 30 more.

AMERICA, THE FIRST YEARS

Hans Bethe immediately felt welcome in America, and often reiterated that within a short period of time he came to feel that his growing up in Germany had been an accident and coming to America was like coming home. Starting in 1933, the physics department at Cornell had made plans to enlarge its activities, which until then had been focused on teaching, with research serving to provide thesis topics for M.A. and Ph.D. students. The new chairman of the department, R. C. Gibbs, along with one of Bethe's acquaintances from Munich days, Lloyd Smith, conceived a very different model for the department. Four new appointments were made in just two years: Lyman Parratt in X-ray spectroscopy, and three men in the very new field of nuclear physics: Stanley Livingston, who had just helped Ernest Lawrence build the world's first cyclotron, the young yet experienced experimentalist Robert F. Bacher, and to complete the team of builder, experimentalist, and theorist, Hans Bethe. Hans was welcomed warmly and very quickly felt very much at home. He was happy to continue shedding the stiff and formal life of Germany. He joined the luncheon table of physicists and chemists, where professors and students mingled freely, and he found colleagues who liked to hike the beautiful hills and gorges of the Ithaca area. He lived for a while in a fraternity house and then in Telluride House where politics was a lively topic of conversation, especially as the second Roosevelt election came up in 1936. After a summer spent in Germany, he was doubly happy to be living in America and began urging his mother to join him.

THE BETHE BIBLE

Bethe's unsurpassed ability to elucidate newly developed, complex physical knowledge had already been displayed in his *Handbuchartikel* in the early 1930s. When he joined the physics department at Cornell and found his colleagues to be more ambitious than knowledgeable in theory, he provided them with what later became known as the "Bethe Bible," three articles in the *Reviews of Modern Physics* (1936, 1937[1, 2]). Written in collaboration with his colleagues Bacher and Livingston, the articles presented a complete coverage of nuclear physics and were used like textbooks.

These review articles and a lecturing tour on which Bethe embarked soon after his arrival in the United States were evidence of his strong commitment to teaching at all levels. They also brought him a job offer from another American university and, when he chose to stay at Cornell, his promotion to full professor.

THE CARBON CYCLE

The fourth Washington Conference, jointly organized by Merle Tuve of the Carnegie Institution and John Fleming of George Washington University in March 1938 was devoted to problems of stellar energy, particularly as these related to nuclear physics. Several investigators whose work was especially concerned with the internal constitution of stars joined forces with physicists working on the problems of nuclear transformations.

Bethe and Charles Critchfield had investigated the "proton-proton chain" as a possible mode of energy production in the Sun (1938). Their work had used Eddington's value for the central temperature of the Sun, which was later found to be inaccurate, and therefore their calculations had led to an inaccurate value for the luminosity of the Sun. At Washington, Bethe heard of Strömgren's new estimates of the solar interior temperature (Strömgren, 1937), and these estimates brought his calculated predictions for solar luminosity much closer to the observed radiation, and his and Critchfield's theory of energy production in the Sun and less massive stars worked just fine.

The question of energy production in larger stars remained unsolved. The proton-proton reaction did not predict this accurately, since the rate of the reaction increases slowly as the temperatures rise, in contrast with the known phenomenon in larger stars where the core temperatures increase slowly with increasing mass, but the luminosity increases very rapidly. Bethe considered this particular problem when he left the conference. Contrary to legend he did not figure out the carbon cycle (and thereby understand the energy production in larger stars), the discovery that would earn him the Nobel Prize in 1967, on the train on the way back home from Washington. But he did start thinking about energy production in massive stars upon his return to Ithaca and he soon worked out that the reaction would have to involve heavier nuclei. Within two weeks, Bethe had worked out the six-step cycle in which carbon and nitrogen act as catalysts in producing a ^{4}He nucleus from hydrogen atoms. He did this simply by looking through the possible reactions that had been measured by Willy Fowler and his collaborators at Caltech.

$$
\begin{aligned}
&^{12}C + H \rightarrow {}^{13}N + \gamma \\
&^{13}N \rightarrow {}^{13}C + e^{+} + \gamma \\
&^{13}C + H \rightarrow {}^{14}N + \gamma \\
&^{14}N + H \rightarrow {}^{15}O + \gamma \\
&^{15}O \rightarrow {}^{15}N + e^{+} + \gamma \\
&^{15}N + H \rightarrow {}^{12}C + {}^{4}He
\end{aligned}
$$

The 1939 paper on "Energy Production in Stars" was a landmark paper that formed the basis of much work in astrophysics for decades. It also brought him the A. Cressy Morrisson Prize from the New York Academy of Sciences, and 29 years later the Nobel Prize.

The years immediately preceding the Second World War were an exciting time for Hans Bethe scientifically, and they were also a happy time personally. His mother arrived in the United States safely in 1939, and in September of that year, he married Rose Ewald, the daughter of his former mentor at Stuttgart. As Bethe would later recall, they were married by a judge who recited the marriage ceremony in its briefest possible form—everybody thought it was a rehearsal. But at some stage the judge said: "Now you are married." And so they remained for more than 66 years.

THE WAR EFFORT

Keen to contribute to the war effort after hostilities had commenced in Europe in September 1939, Bethe initially had to limit himself to work that did not require security clearance. Together with his fellow noncitizen, Austrian-born George Winter, a civil engineer who had specialized in elasticity of steel, he wrote a paper on armor plate deformation and shielding. The paper was never published and as a potentially significant contribution to the war effort it was soon classified and thereby put outside Bethe's reach, who—as enemy alien—was not allowed to access such classified material! Only after the war Nevill Mott told him that it had been very useful.

A second important paper was written, at the suggestion of Hungarian aeronautical engineer and physicist Theodore von Kármán, by Bethe and Edward Teller who produced a theory of how the equilibrium of a gas is reestablished behind a shock wave. This paper was later to become the basis of much work done by aerodynamicists, as it gave insights into the use of shock waves in the investigation of properties of gases (1941). Bethe considered this paper one of the most influential either he or Teller ever wrote.

After becoming an American citizen in February 1941, in December of the same year Bethe finally received his clearance to work on classified military projects. The first such project was linked to the radar that was being developed at the Radiation Laboratory at MIT; Bethe invented the so-called "Bethe coupler," a device used to measure the propagation of electromagnetic waves in waveguides. In 1942 while working on the radar project, Bethe participated in a summer study group at Berkeley, organized by Robert Oppenheimer, which led to the creation of the Los Alamos Laboratory in 1943. Like many others, Bethe joined the Manhattan Project out of fear that Nazi Germany might be developing a fission bomb. As head of the theoretical division in Los Alamos, Bethe led the effort to assess theoretically the performance of the evolving designs of what ultimately became the Hiroshima and Nagasaki bombs. Never before or after did he work so intensely. He was ideally suited for his role at Los Alamos. He coupled deep theoretical insight into nuclear physics with organizational talent and stamina.

The wartime experiences had changed Hans Bethe profoundly, not merely in his evaluation of the interrelationship of science and politics. As a division leader within the Manhattan Project he had learned to organize work for others, to keep people happy working together creatively amidst the tense wartime atmosphere, and he had had to deal with issues of "nonscientific" personnel management. As a result he became more confident in human relations. Likewise, his already considerable range of scientific knowledge expanded further into chemistry, metallurgy, explosives, electronics, and others. He also realized the role science would play in government policy and politics after the war and the urgency of increased understanding of science and numbers by the general public. In January 1946 he returned to Cornell.

QED AND THE LAMB SHIFT CALCULATION

After the end of the Second World War, the U.S. National Academy of Sciences sponsored a series of conferences in theoretical physics. The first of these meetings, the Shelter Island Conference of June 1947 on the Foundation of Quantum Mechanics revisited Quantum Electrodynamics (QED). Invited participants, including Oppenheimer, Bethe, Weisskopf, Feynman, Lamb, Teller, and Schwinger, discussed the experiments of Lamb and Retherford to measure the fine structure of the energy levels of the hydrogen atom (Lamb and Retherford, 1947). Lamb and Retherford had measured what later became known as the "Lamb Shift," the frequency of a microwave field that induced transition between the lowest two excited states of the hydrogen atom.

According to QED, the observed energy of an electron was the sum of two unobservable quantities, the bare energy as an energy of the electron if it were uncoupled from electromagnetic field and the self-energy, which results from electromagnetic coupling. At Shelter Island, Hendrik Kramers presented a model of the electron where all parameters in the theory were made to refer only to observable quantities, by incorporating the infinite contribution of the electromagnetic self energy into the parameter that corresponded to the observed mass. At the conference, it was suggested by Oppenheimer, by Weisskopf, and by Schwinger that Kramers' ideas could be applied to calculate the Lamb shift. Hans Bethe, on a relatively brief train journey from New York to Schenectady after the conference, made use of these ideas to calculate quantum mechanically the level shift for a nonrelativistic electron and found substantial agreement with the experimental value. The Lamb Shift calculations were a fine example of what I (G.E.B.) call the "H. A. Bethe way." When confronted by any problem Bethe would sit down with paper, pen, and his slide rule and calculate the problem in the most obvious way. Most importantly, he would not be deterred by anyone who would tell him—as most colleagues would—that the situation was much more complicated than you thought. Bethe could identify the essential physics and see the light at the end of the tunnel. Once he had focused on that light, he would move toward it, undeterred by temporary obstacles and helped by his formidable mathematical mind and his prodigious memory, which gave him a command and control over the entire discipline that was second to none.

While making seminal contributions to the development of QED, Hans Bethe was also heavily involved in particle physics in the early 1950s. Under his leadership Cornell became a world center for high-energy particle physics. During a sabbatical at Cambridge in the mid-1950s, he gave a substantial lecture course on fields and particles, closely related to the two-volume study of *Mesons and Fields* (1955), which he had written together with Sam Schweber and Frederic de Hoffmann.

Bethe's outstanding abilities and his significant contributions to the progress of physics were widely recognized by that time. This was evident in the numerous prizes and awards he received. He was elected to the National Academy of Sciences in 1944. The award of the 1947 Henry Draper Medal was followed by the Max Planck Medal in 1955, the Ben Franklin Medal in 1959, the Enrico Fermi Award and the Eddington Medal in 1961, the Rumford Prize in 1963, the Nobel Prize for Physics in 1967, the Order Pour le Merite for Arts and Sciences of the German Government in 1984, the Los Alamos National Laboratory Medal and the Bruce Medal in 2001, and—awarded posthumously—the Benjamin Franklin Medal, 2005, to name but the most important honors.

LOS ALAMOS REVISITED: THE H-BOMB AND THE SHAPING OF PUBLIC POLICY

In 1942 Edward Teller had suggested to focus nuclear weapons research on the development of a hydrogen bomb, and although not a priority, work on thermonuclear weapons was actively pursued during the war years. Bethe viewed the H-bomb an unnecessary and undesirable escalation of destructive capability and he opposed the development with all means at his disposal. He had hoped to be able to prove that such a bomb would not be technically feasible. When he realized, however, that this was not the case, and after a decision had been made to develop such a device, he returned to Los Alamos in an attempt to make his influence felt within the establishment (Edson, 1968).

Following his initial work on the atom and the hydrogen bombs and throughout the four decades of the Cold War, Bethe advocated and worked tirelessly to create effective tools for verifying and validating negotiated agreements to slow down the arms race between the two superpowers. (Drell, 2006) As one of the original members of the President's

Advisory Committee, Bethe proposed a study of the possibility and implications of a ban on nuclear weapons tests. His chairmanship of the interagency panel created to assess the American ability to detect Soviet nuclear tests allowed him to gain the expertise to actively participate in the 1958 Geneva talks on test bans, work that culminated, five years later, during J. F. Kennedy's presidency, in the conclusion of the Limited Test Ban Treaty between the United States, the Soviet Union, and Great Britain.

Bethe's guiding principle in his government service was that he did not have the right to withhold his help. This help included insisting on truth over wishful thinking, but leaving the final decision-making to the elected politicians. However, as a citizen he had the obligation publicly to speak his mind which led him to write and to follow numerous invitations to speak.

When ballistic missile defenses were proposed, first in the 1970s, then again with the "Star Wars" proposal in the 1980s, Bethe argued against them, both on policy grounds and as useless in practice because the antimissiles could be defeated by decoys. Together with Richard Garwin, he wrote a substantial article for the *Scientific American* (1968) showing the scientific problems of Star Wars. Together with Kurt Gottfried, he also wrote several op-eds published in *The New York Times* (1982 [1,2]) discussing the policy implications.

It is indicative of Bethe's continual grappling with moral issues that on the occasion of the 50th anniversary of Hiroshima he addressed the scientists assembled at Los Alamos to convince them that one should not work on the further improvement of nuclear weapons. Given the changed context brought about by the collapse of the Soviet Union he urged that fellow scientists collectively take a Hippocratic oath not to work on designing new nuclear weapons.

He was a man of reason with great faith in the power of reason to create a peaceful world. Among his friends it earned him a soubriquet "conscience of science."

THE NUCLEAR MANY-BODY PROBLEM

In 1955 Nevill Mott invited Bethe to spend his sabbatical leave at the Cavendish Laboratory in Cambridge, England, and be a visitor at Caius College. It turned out to be a year of reunion with old friends and family, of professional and pleasure travel in England and on the continent, and in his research the emphasis shifted to the nuclear many body problem in a more focused way. This work involved the use of the *G*-matrix theory developed by Keith Brueckner in order to "tame" the extremely strong short-range repulsions entering into the nucleon-nucleon interaction (Brueckner et al., 1954; Brueckner, 1954, 1955 a,b; Brueckner and Levinson, 1955). Brueckner's pioneering approach to solving the two-body scattering problem in the nuclear medium by rearranging perturbation theory in such a way that the contribution to the total energy at each order was proportional to the number of particles allowed a better understanding of nuclear properties. Energies per particle were manifestly finite. The challenge of calculating these nuclear properties was yet another one for which Bethe's abilities, experience, and knowledge were ideally suited. His approach, based on a diagrammatic expansion of perturbation theory in a series ordered by the number of interacting particles, had "Hans Bethe" written all over it. In one substantial paper (1956) Bethe gave a self-contained and largely new description of Brueckner's method for studying the nucleus as a system of strongly interacting particles with the aim of developing a method that was applicable to a nucleus of finite size while at the same time eliminating any ambiguities of interpretation and approximations required for computation. Thus Bethe—

using the work of Brueckner and collaborators—produced an orderly formalism in which the evaluation of the two-body operators *G* would form the basis for calculating the shell model potential *V (r)*. As in other scientific contexts, analytic solutions to specific problems were a source of additional insight for Hans Bethe. Therefore, with Jeffrey Goldstone he went on to investigate the evaluation of *G* for extreme infinite height hard core potential, and he encouraged his graduate student David Thouless to investigate the problem: what properties of *G* would produce it given the empirically known shell potential *VSM (r)*.

Then, progressing from Goldstone's work and the Goldstone diagrams (Goldstone, 1957), Bethe took the logical next step and investigated three-body correlations, in the course formulating what is now known as the Bethe-Faddeev equations. Using Ludwig Faddeev's work on scattering of systems of three particles (Faddeev, 1960) he generalized the approach and formulated the problem in terms of the three-body wave function. He developed the tools to evaluate the three-body contributions to the binding energy, and after combining it with Wong's idea of a "soft repulsive core" (Wong, 1964), he arrived at binding energies that were in much better agreement with observations than previous estimates (1965, 1967).

CONSULTING FOR INDUSTRY & THE GOVERNMENT

Following WWII, Bethe became active in industrial R&D. He consulted on the design of nuclear reactors, starting in the mid forties at General Electric, later at Detroit Edison, where a breeder reactor was attempted, and still later at General Atomics, where he worked on improving the efficiency and safety of reactors. In the 1950s he began a long association with AVCO Corporation, an aerospace company, working initially on nose cones for missiles and spacecraft reentering the atmosphere, later primarily on making magneto-hydrodynamics commercially feasible.

The twenty-five years after WWII were busy and productive years for Bethe. He was able to share his knowledge and ability with others who were intent on building a better world; he did good physics and had many good students who became his friends.

In the 1970s, when the oil embargo created the first energy crisis, Bethe became interested and active in proposing solutions. He remained convinced that nuclear power was the key to immediate solutions, and that all other sources not derived from fossil fuel or water were then too expensive or needed thirty to forty years of R&D to become economically useful.

ASTROPHYSICS AGAIN

I (G.E.B.) had got to know Hans Bethe in the 1950s, when I was a lodger—at Birmingham—at the home of his close friend from Munich days, Rudolf Peierls. Their comradeship affected many physicists who benefited from their numerative collaborative efforts and from the Birmingham-Cornell pipeline that ensured the cross-Atlantic transfer of knowledge and personnel. Not long after Bethe had begun working on the nuclear many-body problem during his 1955 Cambridge sabbatical, I began work on applying finite nuclei to effective range interactions he had obtained for infinite nuclear matter, and I often visited him at Cornell to discuss these topics. I soon learnt why Hans had had no long-term collaborators earlier in his life, aside from Peierls. Even after he retired, it was nearly impossible to keep up with him. He did not work rapidly, but he could always identify the essential physics immediately and saw the light at the end of the tunnel. Once identified, he would move toward that light like a bulldozer, undeterred by temporary obstacles.

At his retirement party from Cornell University in 1975 I approached Hans with the proposal of a collaboration. But it was not until three years later, when he visited Copenhagen where I was professor at NORDITA, that we got started. I proposed to work out a theory of supernovae and while at Copenhagen Bethe read through the existing literature on the core collapse of massive stars. He quickly realized that all supernovae calculations contained an error. The consensus was that the core collapse ended when the core density reached less than 10 percent of the nuclear density, whereas in fact it continued to densities well in excess of nuclear density. Together with Jim Applegate and Jim Lattimer, Bethe and I wrote the paper, nicknamed "babble" by William Fowler, and generally referred to as such in the astrophysics community. It derived the equation of state in stellar collapse from simple considerations, notably Bethe's early insight that the entropy per nucleon remains small in the order of 1 (in units of $k_{Boltzmann}$) during the entire collapse (1978).

Babble turned out to be the beginning of a long and fruitful astrophysical collaboration between us. For 19 years we spent the month of January together on the West Coast, at Santa Barbara, Santa Cruz, or Caltech, producing more than 20 joint papers in the process. At the outset I would bring up ideas and what he called the "don't know how," and Hans would provide his seemingly inexhaustible 'disc storage' of problems he had worked through or thought about. Hans started his research of the day, as he had done for decades, with a stack of white paper on the upper-left-hand corner of his desk, fountain pen and slide rule in hand. Then he would begin working out whatever problem he had planned to do, remembering all constants, and would fill the white sheets at a nearly constant rate. He headed straight toward the light at the end of the tunnel. If he ran into a barrier, he would go around, over, or under it, filling more of the paper. In short: the Bethe Way!

In the evening I would bring up the problems I wanted Hans to think about the next morning. The master bedroom always had a large bath, as Hans felt his mind to be clearest in the morning in the bath. He would come after his bath to the massive breakfast: sliced meat from the roast or joint or chicken supper the evening before, hot bread rolls, raspberry jam, and lots and lots of weak tea. Over breakfast he outlined the line of attack we should use on the problem at hand. He would estimate what we could get done by noon, and he was in less than good humor if he missed his goal, because he wanted to set out for lunch by noon. In the late afternoon after coming home from the Institute, over tea we would crosscheck our solutions for the problem we had worked during the day. Hans would have done his numerical work with his slide rule; I would have done mine with a $16.00 calculator. Usually we agreed on the results.

When asked once how he wanted to be remembered, after some puzzlement at the question, he said "as a scientist." It would therefore have pleased him that in 2008 a younger generation of physicists in Germany, where the immediate post-war generation repudiated him as one of the evil geniuses of the atomic bomb, wished to perpetuate his name through the Bethe Center for Theoretical Physics at Bonn University and the Hans Bethe-Strasse in the neighborhood of the natural science campus of the University of Frankfurt am Main.

Hans Bethe defied the notion that physics is a young person's pastime. He published significant papers in every decade from the 1920s into the 21st century. As John Bahcall remarked: "If you know his work, you are inclined to think that he is many different people, all of whom have gotten together and had formed a conspiracy to sign their papers under the same name"(Bahcall, 2005).

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Chandler McC. Brooks

CHANDLER McCUSKEY BROOKS

December 18, 1905–November 29, 1989

BY KIYOMI KOIZUMI AND MARIO VASSALLE

CHANDLER McC. BROOKS (elected to the National Academy of Sciences in 1975) was born in West Virginia on December 18, 1905, the son of a Presbyterian minister. His family was respected and loved by the people of the various communities in the area. This environment influenced Chandler very much in his early years, shaping the foundations of his philosophical approach to life, his sense of obligation, and his long-term aims.

When he was 12 years old, his family moved from West Virginia to Massachusetts. He was emotionally tested as an adolescent by the death of his mother, an event that drew him closer to his father. According to his own account, the schools he attended in the poor districts of Massachusetts gave him little education throughout his high school period. For his college education he turned to the Midwest, where he attended Oberlin College in Ohio.

The rough experience of precollege school was not entirely negative. Brooks wrote:

My experience with barbaric young people and an antagonistic community in my youth may have enabled me to be tough too and survive in Brooklyn for over a third of a century and administer my institution during the dark days of the late 60's when faculties and students were in destructive revolt against the system and destroyed many presidents and deans. My parents and their beliefs typify what is essential to our culture and the preserving of man's greatest attainments.

Although Brooks considered entering the ministry, he liked athletics and science at college, majoring in zoology and taking courses in the classics, English literature, and history. His performance earned him Phi Beta Kappa membership. He was also a marathon runner during his college years, winning prizes and varsity letters.

Upon his graduation, Brooks was helped by an Oberlin professor in obtaining a fellowship in the Biology Department at Princeton University. It is of interest that he not only wished to study science but also philosophy and theology at the seminary in Princeton. Brooks's reason for this was that he saw the limitations of a single approach. He felt that scientists were too narrowly focused and philosophically naïve, while the ministry was unaware of scientific foundations and of what should be its broader obligations. Not surprisingly, that plan did not materialize due to opposition by both schools. He wrote, "I was unhappy at Princeton and did poorly until I met Dr. Philip Bard and became a physiologist." In spite of all that, Brooks completed his Ph.D. in biology at Princeton in only three years, in 1931.

Brooks followed Bard from Princeton to Harvard University, where Brooks was a fellow in the laboratory of Walter B. Cannon for two years. There he began his experimental studies on the neural control of the endocrine system, a research field still unknown to most scientists in those days. He went to Johns Hopkins University in 1933 when Bard moved there to become the chairman of the Department of Physiology.

At Johns Hopkins he worked and taught as instructor first, then as associate professor in 1941. In those years

Brooks's research spanned from the central control of the motor and autonomic nervous systems to the neural control of endocrine glands through the hypothalamus. These were very productive years for him, for he made many important contributions in all of those fields.

In 1946, as a Guggenheim fellow, Brooks went to Dunedin, New Zealand, to work with Professor John Eccles, who later became Sir John and a 1963 Nobel Laureate. Brooks later told us that at this point in his research on the central nervous system control of the endocrine system he needed more knowledge and experimental skills either in endocrine physiology or in neurophysiology. He wished to work with either Professor Bernardo A. Houssay in Argentina, the famous endocrinologist and a 1947 Nobel Laureate, or with Professor John C. Eccles, who was a leading neurophysiologist on the central nervous system. Professor Houssay advised Brooks not to come to Argentina, as Houssay was in trouble with the political regime in his country.

Following a 40-day trip by ship from New York to New Zealand, Brooks and his wife, Nelle, settled in Dunedin for two years. In Professor Eccles's laboratory Brooks wished to learn how to record the electrical impulses from neuroendocrine cells in the hypothalamus in order to understand how the nervous system controls the endocrine function through the pituitary. Brooks later told us that Professor Eccles immediately rejected the idea, saying, "Brooks, one cannot record electrical impulses from such cells; they are gland cells and not neurons; you better work with me on the spinal cord." So he did, and their work resulted in many significant papers published in the *Journal of Neurophysiology*. (The actual recording of electrical impulses from those hypothalamic neuroendocrine cells was accomplished in Brooks's own laboratory in the early 1960s.)

In 1948 Brooks was invited to become professor and chairman of the Department of Physiology and Pharmacology at the Long Island College Hospital School of Medicine, in Brooklyn, New York. In 1950 the school of medicine became the State University of New York Downstate Medical Center. Getting an "almost non-existent" department, Brooks, with his unique energy, talent, discipline, and vision, began to create and organize the new unit, hiring young faculty members to carry out research and teach medical students. He had the foresight to establish a visiting professor system, so that he could bring outstanding senior scientists to help establish the research activity by younger faculty in the department. He decided that the department would concentrate in at least two fields of research: neurophysiology and cardiac physiology.

The first visiting professor was Oscar Orias, who came from Argentina in 1949. Brooks knew Professor Orias, an expert in cardiac physiology, from Cannon's laboratory at Harvard. Employing the open-heart preparation in dogs, Orias, Brooks, and associates began studies on cardiac excitability with help from an expert engineer recruited from Professor Eccles's laboratory in New Zealand. This very first accomplishment led to international recognition of the department and to the publication in 1955 of the now classic, widely cited book *The Excitability of the Heart*. Around the same period Brooks and his associates began their pioneer work on intracellular recordings from heart cells, a brand-new field in cardiac research at that time.

In the field of neurophysiology, with the help of visiting professor M. G. F. Fuortes from Italy in 1950, the Brooks laboratory began electrophysiological work on the spinal cord in cats. These early accomplishments led to an invitation to contribute a review article "Excitation, Conduction and Synaptic Transmission in the Nervous System" to the *Annual Review of Physiology* in 1952.

In 1956 Brooks made the decision to separate the Physiology and Pharmacology Departments. He continued as chairman of the Physiology Department and recruited Robert F. Furchgott to chair the new Department of Pharmacology. Professor Furchgott received the Nobel Prize in Physiology and Medicine in 1998.

Brooks's research activity later expanded (in addition to cardiac physiology) to the autonomic nervous system and the neuroendocrine system. In regard to "the most important discoveries" in his long career as a physiologist, Brooks wrote:

> It is somewhat difficult for me to identify my most important discoveries. I have always been a generalist and I have done work which I consider to have been significant in at least four fields as well as in history and philosophy of science. If I can lay claim to any uniqueness in accomplishment it is based on the number of important contributions to numerous fields rather than on one or two major discoveries.

His major accomplishments in four fields (autonomic nervous system, neurophysiology, neuroendocrinology, and cardiac physiology) are summarized below:

THE AUTONOMIC NERVOUS SYSTEM

His interest in the autonomic nervous system arose from his association first with Professor Walter B. Cannon at Harvard Medical School. (Brooks later held the symposium to commemorate Professor Cannon's accomplishments, with Cannon's former pupils and Cannon's son as speakers. The presentations were published in 1975 in the book *Life and Contributions of Walter Bradford Cannon*). His major discoveries are: (1) studies of the somato-autonomic reflex (i.e., how the afferent impulses from the skin and muscle evoke electrical discharges in pre- and postganglionic fibers (autonomic efferents), causing various organ responses; (2) involvement of the autonomic nervous system in certain endocrine functions, namely, the adrenals and the pineal glands controlling the circadian rhythms; (3) autonomic control of the heart, particularly by direct recordings from the cardiac vagal and sympathetic nerves in various conditions, and their reciprocal and nonreciprocal actions on the heart; (4) interactions between the heart and hypothalamic neuroendocrine system that control the water balance in the body: and (5) development of the idea that the autonomic nervous system is the great integrator of body functions in that it participates in all functional activities and affects all body tissues and organs. Brooks greatly influenced this field by employing electrophysiological techniques in studying the autonomic reactions. This had been somewhat neglected during the 1950s and 1960s, since most autonomic work then was done only through the recording of effector organ responses.

NEUROPHYSIOLOGY

(1) His discovery in the 1930s that destruction of the hypothalamic ventromedian nuclei produced obesity in rats and monkeys opened the field for later intensive research. (2) The discovery of cortical locus of control of hopping and placing reactions raised the question of cortical localization and plasticity. (3) Studies of central inhibition and the role of Golgi II cells in the spinal cord carried out with J. C. Eccles have become a classic in the understanding of inhibitory processes in the nervous system. (4) In his electrophysiological studies on the spinal cord and midbrain, Brooks and his associates made a number of discoveries early that were developed later by others (e.g. studies on origins of the dorsal root reflex, evidence of presynaptic inhibition, effects of cold on the central nervous system and their mechanism [namely, changes in the accommodation process in single

motor neurons], long-lasting facilitatory influences exerted by the reticular formation on spinal neurons). This last observation has developed into the popular "LTP (the long-lasting tetanic potentiation)," forming the basis of learning.

Neuroendocrinology

Brooks was among the first neuroendocrinologists, having been chosen as 1 of 21 in the world to be listed in the first volume of *Pioneers in Neuroendocrinology* in 1975. According to Brooks, he entered the field "because the hypothalamus was involved in control of the autonomic system and its functions." His accomplishments include (1) before the discovery of relationships between the hypothalamus and the anterior pituitary (hypothalamo-adenohypophysial system), he cut the pituitary stalk or made hypothalamic lesions to locate the pathways for various phenomena (e.g., ovulation, pseudopregnancy, and diabetes insipidus). He claimed that he may have been the first to produce diabetes insipidus in monkeys. His early work suggested the presence of "releasing factors" but their existence was not demonstrated by him. Later accomplishments are (2) in the early 1960s he and his associates recorded electrical activities from neurosecretory cells in the supraoptic and paraventricular nuclei in the hypothalamus. This pioneering technique spread widely in the succeeding years to England and elsewhere. With this technique they were able to show that "natural" stimuli, such as distention of uterus and vagina and gentle tactile stimuli to the mammary glands in pregnant animals, released oxytocin. It was also shown that hypertonic stimuli to these neurons produced and released vasopressin into the vascular system. (3) The first intracellular recordings from the hypothalamic neurosecretory cells in dogs, cats, and rats were made as well as studies of many factors influencing activities of these neurosecretory cells and the release of hormones leading to changes in various body functions. (4) Recordings from pineal gland cells showed that these cells respond to light through the sympathetic nerves innervating the pineal. As for many other studies that Brooks and his associates initiated, their early findings often stimulated many other investigators, resulting in greatly expanded work in the field of neuroendocrinology in later years.

Cardiac physiology

As mentioned before, Brooks began research in this field with Oscar Orias, the first visiting professor in Brooks's newly created department. The combined expertise of Orias in cardiac physiology and that of Brooks in neurophysiology led to the work on cardiac excitability. (1) The first discovery was the period of cardiac vulnerability and its relation to arrhythmias. The work led to publication of *The Excitability of the Heart* in 1955. (2) These studies led to the development of the principles for the use of acute and chronically implanted pacemakers. The first chronic pacemaker was implanted in a dog, showing the heart could be driven artificially. This feat was done in the early 1950s, long before the beginning of development of the artificial pacemaker. (3) Brooks's group was at the forefront of intracellular recordings in cardiac muscle cells and Purkinje and sinoatrial node pacemaker cells, with the first publication appearing in 1952. This developed into many discoveries, including the role of calcium ions in the action potentials of the sinoatrial node dominant pacemakers. (4) They also conducted some of the earliest studies of the intrinsic and extrinsic factors affecting the discharge of sinoatrial node pacemaker cells, including fast drive, and intracellular recordings of isolated pacemaker cells.

Brooks's publication list covers all those fields. He is the author of 4 books, editor of 7 books, contributor of 26 chapters, and author of over 200 reviews and original papers in scientific journals, spanning the period from 1929 to 1989, not including numerous abstracts. His contribution to physiology also includes the founding of the *Journal of the Autonomic Nervous System* (by Elsevier) in 1978, when no such journal existed in English (the only one then was in Japanese). He was editor in chief of that journal for seven years. The journal has been continued to the present as the *Journal of Autonomic Neuroscience*, resulting in the formation of the International Society of Autonomic Neuroscience.

In addition to being a very successful physiologist, Brooks was a creative and skillful organizer in promoting teaching and research in the medical school. As chairman of the Department of Physiology he encouraged and nurtured young faculty members in his own department, but he also invited numerous visiting fellows and scholars from all over the world. This was particularly significant in 1950s, when he could invite many Asian physiologists, giving them an opportunity to do research in the United States. On returning to their own countries they could make big contributions to the promotion of research there. Brooks's help to science and scientists in war-devastated Japan after the World War II was recognized when the Order of the Rising Sun (3rd Class) medal was given to him by the emperor of Japan in 1979. He also received many medals and honors from Korea and Taiwan.

Brooks's contributions to medical education are also noteworthy. He was a founder and the first dean of the graduate school at The State University of New York Downstate Medical Center, a founder of the State University of New York Press, and for one year the acting president and acting dean of the medical school. He also promoted basic research, organizing the Sigma Xi chapter at SUNY Downstate Medical Center, and numerous lectures and symposia for all faculty and student bodies. In addition, Brooks was sought after by the State University of New York to serve on numerous committees concerned with health education and university-wide affairs.

One of his notable contributions to the medical school was establishing the Visiting Scholar Program during the 1960s and 1970s to foster the cultural interests of the faculty and medical students. There were many distinguished speakers, such as Arnold Toynbee, Archibald MacLeish, Marianne Moore, W. H. Auden, Edwin Reischauer, Aaron Copland, Harold Clurman, William Stockhausen, a rabbinical scholar, and a Jesuit theologian, who not only gave lectures but also spent two full days on our campus meeting with students and faculty at breakfast, lunch, and dinner.

After his retirement as chairman of the Department of Physiology in 1972, he continued to be active in research at the school as a distinguished professor. He also turned his talents to philanthropy, as a trustee and chairman of the grants committee of the International Foundation. With his interest in helping worthy projects (matched by his meticulous attitude and conscientious thoroughness), he spent hours of his busy days scrutinizing health applications and meeting people in faraway places in order to find out how funded plans were doing. His idea was to help projects that needed start-up funds that would continue to develop into a successful operation. His work continued to his untimely death in 1989.

In 1986 Brooks added a new field of interest to his long career as a scientist, educator, and philanthropist: He became a fellow of the Center for Theological Inquiry in Princeton. At this late stage of his life and four years after the death of Nelle, his wife of 50 years, he turned his inquiry and learning toward more fundamental questions of life and of man.

We wish now to cite some words of his colleagues expressed in the book *Chandler McCuskey Brooks, The Scientist and the Man* published in 1990 on the occasion of a memorial ceremony for Brooks.

[Brooks made] many contributions to this institution, to the science of physiology, and in a real sense to the world at large…over a period spanning more than 40 years at this institution he profoundly affected the intellectual spirit at this institution through his many interactions with students, faculty and visiting scientists.

If I must identify one thing as most important, I select Dr. Brooks' dedication to the instruction of students … It was his firm conviction that if something is worth teaching, you'd better learn how to teach it well and do just that.

In his youth, Chandler was a long distance runner and so he was throughout his life. Of the long distance runner, he had the physical endurance, the discipline, the strength of character and a determination to win … The level at which he operated was set by his determination to pursue excellence and he worked at that with great tenacity throughout his life…[He] considered the Department of Physiology as a larger family and, by treating people accordingly, fostered respect, loyalty, and devotion. He knew the value of moderation and thoughtfulness in smoothing the inevitable contrasts that must arise in any community of individuals…He saw to it that no one should fail and he accomplished that not by lowering standards but by providing help or adjusting the goals to individual capacities. He felt that as scientists we have an obligation to train and educate young people who come to us from near and afar and to help them in their careers when they return home….In all his endeavors, he worked very hard because he realized that the attainment of quality performance requires not only worthwhile aims but also a careful attention of details. And this takes time, effort and organization….In spite of his many duties, he always sought any initiative that would constitute development and growth, from the Visiting Scholar to the Visiting Professor Programs. He left an imprint in those who worked with him by setting an example of industry, tenacity, perceptive intelligence, and fairness in human relationships, integrity of character and trust….Busy as he was, he showed his inner gentleness on innumerable occasions as when he wrote stories and drew cartoons for the little son of one of his associates.... As a result of his life-long labors, he established himself as a most prominent scientist, widely known internationally as testified by the numerous honors that he received during his long and productive career.

[Brooks] treated each of us in the Department with respect and trust. He was fair to everyone, and we in turn had tremendous respect and deep affection for him. Though definitely a "minority" member in the school, from the beginning I never felt discriminated against as a woman or Asian in his Department. This was unusual if one remembers that in the early fifties it was rather difficult for Asians to find a decent apartment even in Brooklyn...Chandler's kindness and willingness to help others often gave him an extra burden in his very busy life, but he was always willing and never complained. Not only did he take time to go to the airport to meet foreign visitors in person, but he and his wife took them to their home, found an apartment and helped them to settle. The basement of his house in Brooklyn was always full of baby cribs, folding beds and other pieces of furniture. When one family came, out went the furniture; when they left the USA the furniture went back to the basement...Chandler had a remarkable ability to create and organize many important projects; he not only had a very clear idea about them from the beginning, but also planned them in great detail. Many of his accomplishments, such as the initiation of the Graduate Education Program and the famous Visiting Scholar Program at Downstate, his establishment of the State University Press, his help in publishing *Japanese Physiology, Past and Present,* which was distributed to all International Union of Physiological Sciences participants at the Tokyo Congress in 1965 in order to introduce Japanese physiology to the world, his founding of the *Journal of the Autonomic Nervous System,* his active role in organizing several very significant symposia and many more activities, reflected his dreams, his intellectual perception and his determination to accomplish something he felt worthwhile and important.

In all the scientific work in Brooklyn, Chandler had the collaboration of younger scientists who had come from various parts of the world to work and study in Brooklyn. At one time, there were 15 Department chairmen in Japan who had studied in Brooklyn; in South Korea there were 11 Downstate people. There are people everywhere who have been touched by Chandler Brooks: in Scotland, England, New Zealand, Australia, Taiwan, India, Japan, Korea, Hungary, Italy, France, Switzerland, Finland, the Soviet Union, Bulgaria, Argentina, Chile, Mexico. With many of these visiting scholars, especially those who did not write English well, Chandler spent hours and hours helping them to improve their drafts and seeing the manuscripts through to publication.

In spite of all his own accomplishments, Chandler Brooks was a simple man. He did not put on airs, or strut about over his achievements. He recognized talent in others, and was generous in praise and support.…What made Chandler Brooks the way he was? The answer is not that difficult. His spiritual upbringing and his faith in God helped him hold a steadfast course in his life. The whole purpose of his life was service: service to science, service to society and service to mankind.

When asked why he brought non-doctors to a medical school [under the visiting scholar program], Dr. Brooks declared: "The physician must eventually confront and communicate with people as they are. The more he knows of man and his social state, his cultures and the basis of his mores, the better." I might add that he belonged to the Medieval Club of New York for some time.…Chandler's concept of medicine and medical education was unusually broad and deep and based on his desire to seek and understand the nature and predicament of man and ultimately the nature of God. It is for these reasons, I believe, that he spent countless hours in his later years as chairman of the Grants Committee of the International Foundation and decided in 1986 to become a fellow of the Center of Theological Inquiry in Princeton.…As a fellow Dr. Brooks' main concern was to learn more about the nature of the soul and immortality. …He was keenly aware that having spent the major part of his active life in medical research, he had no time left to read and digest the subtle and complex teachings of past theological and philosophical masters on basic issues of life. It seemed to him, and he was saddened by the fact, that not many contemporary students of theology were interested in the topics which occupied his mind. And yet, he questioned, sought answers and continued to learn.

As mentioned above, in the later years of his life Brooks became much interested in pursuing the fundamental questions in one's life, such as: what is man, what is man's soul. He wrote to his own minister: "Yes, faith, hope and love abide. But an even greater word is courage. Courage is what counts—the courage to move out into the wilderness of inquiry, the courage to move through the darkness to the light."

IN CONCLUSION

The above presentation of the life and work of Chandler McCuskey Brooks makes it clear that he was an exceptional man. What made him so exceptional? The underlying foundations of character can perhaps be traced to his family background. His father was a minister and when Chandler was young he wanted to become a minister himself. How important and long-lasting this interest was is shown by the fact that when he retired from Physiology he went to the Center for Theological Inquiry to pursue inquiries about spiritual matters. To the embarrassment of some of the theologians, he asked pointed questions about the nature of the soul. The inquiry in general reveals the scientist and the specific inquiry reveals his desire to understand matters that rise above the human clay. Of course, he did not get a definite answer, but pursuing those questions already gives a measure of the broad interests of his mind. In this connection it is of interest that there was a period of time in his life when he wrote to his father about different matters (including botany) *every day*.

This background, in which religious aspirations are imbedded in one's soul and not merely within the precinct of a church, may account for one of his major characteristics: integrity. That is to say: honesty based on principles, and therefore not easily transgressed. He was a man you could entirely trust.

Another of his outstanding characteristics (perhaps linked to the previous one) was self-discipline. His discipline allowed him to overcome many difficulties and obstacles, for it is at the school of discipline that he formed his character. Add to this a keen intellect, a sense of duty, foresight, perceptivity, and hard work and there is the substratum on which the rest of his qualities prospered.

The rest included a healthy ambition that was never pursued in itself, but it was almost viewed as a duty toward the development of his capabilities. And there was the genuine interest of a true scientist in the discovery of the marvels of nature—a passion and a wonder for understanding the works of the Creator and a deep appreciation of the exhilaration of discovery.

That was certainly enough to fill the life and aspirations of anyone, but not of Chandler. Even in science he refused the limitations of a too narrow approach. He was a physiologist of the body, not of a cell, an enzyme, or a molecule. He was a true physiologist who wanted to understand the secret mechanisms of the extraordinary human machine. At a meeting, speaking on the manipulations of certain experimental approaches, he remarked, "They are studying what the cell can do, not what the cell does." And that perhaps is the very source of many disagreements. And, if one does not watch out, sometimes one risks wanting to teach nature.

As one would expect, he took his administrative duties with a deep sense of responsibility and worked hard at them. He did not hesitate to serve for one year at the same time as chairman of the department, dean of the graduate school, acting president of the center, and acting dean of the medical school. And in the evening he had a timer that would switch off the lights at midnight so that he and one of us (K.K.) had to stop the writing of scientific papers. But his versatility was shown also by his understanding of human nature, the psychology of human relationships—a necessary requirement for anyone dealing with and leading a community of people. He was hard working and disciplined, but certainly neither rigid nor unperceptive.

Was he then without faults and limitations? Certainly not. But what is extraordinary is that he could accomplish so much by mastering the qualities of his merit. While he shared with everybody else the limitations of our human nature, not many others shared the qualities that he cultivated and practiced. But what really distinguished him was that in everything he did, he sought excellence. This was his secret, which he pursued no matter what it cost.

One might conclude with the following quote from *Chandler McCuskey Brooks, The Scientist and the Man*:

The force of example resides only in the receptivity of a willing and eager mind. However, the example itself must be made available not only to those who knew Chandler but to all who are interested in better understanding themselves through the mirror provided by the life and humanity of others.

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Herbert C. Brown

HERBERT CHARLES BROWN

May 22, 1912–December 19, 2004

BY EI-ICHI NEGISHI

HERBERT CHARLES BROWN, R. B. Wetherill Research Professor Emeritus of Purdue University and one of the truly pioneering giants in the field of organic-organometallic chemistry, died of a heart attack on December 19, 2004, at age 92. As it so happened, this author visited him at his home to discuss with him an urgent chemistry-related matter only about 10 hours before his death. For his age he appeared well, showing no sign of his sudden death the next morning. His wife, Sarah Baylen Brown, 89, followed him on May 29, 2005. They were survived by their only child, Charles A. Brown of Hitachi Ltd. and his family.

H. C. Brown shared the Nobel Prize in Chemistry in 1979 with G. Wittig of Heidelberg, Germany. Their pioneering explorations of boron chemistry and phosphorus chemistry, respectively, were recognized. Aside from several biochemists, including V. du Vigneaud in 1955, H. C. Brown was only the second American organic chemist to win a Nobel Prize behind R. B. Woodward, in 1965. His several most significant contributions in the area of boron chemistry include (1) codiscovery of sodium borohyride (1972[1], pp. 39-49) with his Ph.D. and postdoctoral mentor, H. I. Schlesinger, which helped modernize organoboron chemistry; (2) systematic exploration and methodological development of the reduction of a wide variety of organic compounds with sodium borohydride as well as other related borohydrides and aluminohydrides (1972[1], pp. 209-251); (3) discovery of hydroboration and subsequent developments of hydroboration-based organic synthetic methods (1972[1], pp. 255-446); and (4) development of asymmetric allylboration, crotylboration, and related reactions as a group of widely used methods for asymmetric carbon-carbon bond formation. Although Brown was not directly involved, his work on organoboron chemistry was instrumental in the discovery in 1978[1] by this author and the subsequent extensive development since 1979 by A. Suzuki, another former postdoctoral associate of H. C. Brown, of the Suzuki coupling (2003) as one of the most widely used methods for carbon-carbon bond formation. Notably, Brown was also actively involved in the industrial production of a wide range of organoboron reagents used in this reaction. Brown published nearly 1300 scientific publications, several books, and several dozen patents, averaging about 20 publications a year over nearly seven decades.

In addition to the Nobel Prize, H. C. Brown also received numerous other awards and recognitions. He was elected to the National Academy of Sciences in 1957 and the American Academy of Arts and Sciences in 1966. He received the American Chemical Society (ACS) Award for Creative Research in Synthetic Organic Chemistry in 1960, the National Medal of Science in 1969, the ACS Roger Adams Award in 1971, the ACS Priestley Medal in 1981, the Perkin Medal in 1982, the American Institute of Chemists Gold Medal in 1985, the National Academy of Sciences Award in Chemical Sciences in 1987, the Emperor's Decoration: Order of the Rising Sun, Gold and Silver Star (Japan) in 1989, and the inaugural ACS H. C. Brown Award for Creative Work in Synthetic Methodology in 1998. In 1998 *Chemical and Engineering News* named him one of the top 75 contributors to the chemical enterprise over the preceding 75 years. Brown also received 14 honorary doctorates, including one from the University of Chicago in 1968.

H. C. Brown was born in London, England, in 1912 to Jewish parents from Ukraine. Two years later his family moved to Chicago where he lived until he moved to Detroit in 1943. His childhood and early adulthood swung between school activities and hardship in the economic turmoil of the 1920s and 1930s. His father died when he was a teenager, which forced him to leave school to tend the hardware store his father started. He later returned to school and graduated in 1930. After a series of unsuccessful attempts to obtain a job, he entered Crane Junior College operated by the City of Chicago. It was at this college that he met Sarah Baylen, a 16-year-old chemical engineering major and his future wife. When Crane Junior College was closed shortly thereafter because of lack of funding, about 10 students, including Herb and Sarah, were invited by one of the professors, N. D. Cheronis, to conduct chemical experiments in a small laboratory at his home. When Wright Junior College was opened a year or so later, Herb and Sarah as well as Cheronis went there. On graduation day in 1935 Sarah prophetically inscribed in Herb's yearbook: "To a future Nobel Laureate." Herb won a scholarship to attend the University of Chicago in the same year. He took 10 courses per quarter and in 1936 obtained his B.S. degree in just three quarters. Sarah bought *Hydrides of Boron and Silicon,* a book by Alfred Stock, as a graduation gift for Herb. He learned from the book that hydrides of boron were prepared in only two laboratories in the world. One was Stock's laboratory in Karlsruhe, Germany, and the other was H. I. Schlesinger's laboratory at the University of Chicago. This was how Brown became associated with H. I. Schlesinger for his graduate research; he obtained his Ph.D. degree in 1939. In the meantime, Herb and Sarah got married "secretly," as stated by them, in 1937. Their enviable marriage lasted more than 67 years. As a married man with a Ph.D. degree, Herb tried to secure an industrial job but once again failed. He was then offered a postdoctoral position in the group of M. S. Kharasch, where he investigated chlorination, chloroformylation, and other free-radical reactions (1972[1], pp. 24-39). A year later he rejoined H. I. Schlesinger's group as a research assistant carrying the rank of instructor. There he discovered or developed methods of preparation and reactions of lithium and sodium borohydrides and other related borohydrides (1972[1], pp. 39-49).

Brown finally obtained his first independent academic position as an assistant professor at Wayne University in Detroit in 1943. He was 32. The new head of the Chemistry Department, Neil Gordon, who started the Gordon Research Conferences, hoped to develop a Ph.D. program at Wayne, but the department was still ill equipped. In search of research projects not requiring elaborate and expensive laboratory equipment, Brown decided to develop empirical theories to explain poorly understood steric effects. His studies, first at Wayne and later at Purdue, led to a convenient and useful set of qualitative theories or explanations based on his notion about F-, B-, and I-strains, where F, B, and I stand for front, back, and internal, respectively (1972[1], pp. 53-128). Brown's research at Wayne University quickly established him as an active, prolific, and pioneering researcher, and he was promoted to associate professor in 1946. In 1947 the head of the Chemistry Department at Purdue University, F. Hass, hired him as a full professor. At Purdue he continued his work on steric effects and electrophilic aromatic substitution and he resumed his research on the chemistry of borohydrides and aluminohydrides, which led to his discoveries of hydroboration with B. C. Subba Rao (1956) and its asymmetric version with N. R. Ayyangar and G. Zweifel (1964). Many consider the former to be the single most important discovery made by H. C. Brown. He often jokingly mentioned his parents' foresight in giving him the initials H, C, and B, which correspond to the three elements involved in hydroboration (Eq. 1).

$$\mathrm{H{-}B\langle} + \mathrm{\rangle C{=}C\langle} \longrightarrow \mathrm{H{-}\overset{|}{\underset{|}{C}}{-}\overset{|}{\underset{|}{C}}{-}B\langle} \qquad \text{(Eq. 1)}$$

The asymmetric version of hydroboration, exemplified by the facile synthesis of diisopinocamphenylborane, abbreviated as $HBIpc_2$, from inexpensive (+)- or (-)-pinene, is no less significant. As such, this represents one of the earliest examples of a highly enantioselective reaction under nonenzymatic conditions. Furthermore, the product $HBIpc_2$ and its derivatives are relatively inexpensive and widely useful reagents not only for asymmetric hydroboration but also for asymmetric reduction[2] as well as for allyl- and crotylboration.[3]

According to H. C. Brown, his investigation of steric effects led to his explanation of the surprisingly high *exo/endo* solvolysis rate ratios of norbornyl derivatives in terms of steric hindrance to ionization (1972[1], pp. 181-205). This, in turn, led him to doubt the validity of the growing tendency to represent a wide range of carbocationic species with "nonclassical" structures (1972[1], pp. 131-180). At the time this author joined his group in 1966, roughly half of his group members were engaged in the project on the classical versus nonclassical structures of carbocations. However, this author avoided the project despite his intense curiosity. Now that the dust appears to have settled, only the following brief personal view is presented here.

Fundamentally, any electron-deficient species that can participate in three or higher multicentered bonding may be considered to be nonclassical to varying degrees. From this viewpoint there seems no room for doubting the widely accepted conclusion that diborane and the parent norbornyl cation may be best represented by symmetrically electron-distributed nonclassical structures. There are however at least two factors to be carefully dealt with. One is structural perturbation, the other is the extent of ionization or electron deficiency. In most of Brown's solvolytic studies, structurally perturbed tertiary carbocationic species were investigated. Many of them were even benzylic or homobenzylic derivatives. In such cases more conventional electron delocalization effects, such as resonance and hyperconjugation effects, may become dominant, and the nonclassical framework, which is appropriate for the parent norbornyl cation, may not be the best representation. It is this author's view that Brown was primarily cautioning the overuse of nonclassical structure involving sigma carbon-carbon bond participation in cases where such structures may not be the most representative or significant. This issue is further complicated by the fact that unlike neutral and electron-deficient metal compounds, carbocations are associated with counteranions. This must always be taken into consideration in discussing carbocation structures.

Brown embarked on a major new area of the development of organoborane-based carbon-carbon bond formation in the mid-1960s. Ironically, some of his most creative and original investigations, such as those of carbonylation reactions (1972[1], pp. 255-446), have not yet been widely used by the synthetic community, perhaps in part because of their radically novel patterns of reactions that cannot be readily assimilated into the conventional organometallic carbon-

carbon bond-formation methodology. In the meantime, the Pd-catalyzed cross-coupling of organometals containing boron, zinc, aluminum, and so on that this author[1] and A. Suzuki (2003) have discovered and developed as well as the asymmetric allyl- and crotylboration most extensively developed by Brown's group,[3] all of which can readily fit into the patterns of conventional organometallic methods for carbon-carbon bond formation, have become widely accepted and used.

At Purdue, H. C. Brown was promoted to R. B. Wetherill Professor in 1959 and R. B, Wetherill Research Professor in 1960. After his formal retirement in 1978, he was R. B. Wetherill Professor Emeritus. All of the awards and recognitions mentioned earlier were received by him while he was a Purdue faculty member. In recognition of his great accomplishments and contributions to Purdue University, one of the two main chemistry buildings was named the Brown Building, and the entire chemistry department has been renamed the Herbert C. Brown Laboratories of Chemistry.

Besides being a premier scientist, H. C. Brown was a superb mentor to those who were sufficiently self-motivated and fundamentally well equipped to pursue research in a logical and rational manner. He himself possessed the kind of logical and rational mind reserved only for the very best scientists. One of the early projects suggested by Brown to this author dealt with the cyclic hydroboration of dienes and trienes (1972 [2]), which had been previously investigated by other well-known workers in the field. Primarily on rational grounds, Brown questioned several previous structural assignments. Detailed investigations eventually led us to correct them all (1972 [2]). Another lesson he instilled was to not only do research but also to live with eternal optimism, of course not in a quixotic manner but with well-calculated rationalism. This has easily been the single most important lesson this author has learned from him.

In his research group Brown expected us to actively and vigorously pursue our research projects and be successful and productive. However, he was far from being a "slave driver." One of his unmistakably clear policies was "time is your own." He would openly and repeatedly tell his group members that he did not believe in the value of spending much more than eight hours a day in the laboratories. Instead, he emphasized the significance of good thinking, planning, preparation, execution of laboratory experiments in a highly efficient manner, and timely interpretation of the results. In short, he expected each of us to be a successful entrepreneur, working jointly with him. He would even tell us before his departure on frequent trips, "Do not work too hard." And yet, many of the group members in the mid- to late-1960s were somehow motivated to work harder during his absences, perhaps to show what they were capable of doing in his long absence.

Outside the research arena H. C. Brown was a truly caring person. As one of the frequent beneficiaries of his kind and continual mentoring, it is very easy for the author to say what is stated above. This author still truly believes that many, if not all, of his former associates will echo the above statement. Herb and Sarah also were very generous donors. In addition to the ACS Herbert C. Brown Award for Creative Work in Synthetic Methodology, which was predominantly endowed by the Browns, they provided a series of endowments for funding the Herbert C. Brown Lectures in 1983, the Herbert C. Brown Distinguished Professorship in 1998, and the Herbert C. Brown Center for Borane Research in 1998.

Herb Brown, together with Sarah, indeed lived a very enviable life. They vividly demonstrated that with raw capability, dedication, and eternal optimism, a man and a woman together can make some remarkable achievements, become happier with time well into their 80s and beyond, and become rich enough to return part of their wealth to the community.

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GLENN WILLARD BURTON

May 5, 1910–November 22, 2005

BY ARNEL R. HALLAUER

GLENN W. BURTON IS A PROMINENT name in the recorded history of forage and turfgrass breeding during the 20th century. His formal research career of 61 years started with the application of the principles of Mendelian genetics for improvement of grasses to the application of molecular genetics for grass improvement during the latter years of his career. Burton made significant improvements in plant breeding and genetics of forage and turfgrasses, which had economic and social impacts on the forage-based cattle industry, the turfgrass industry, and agriculture of the southern areas of the United States and worldwide. He developed coastal bermudagrass and solved problems associated with its establishment and management. Coastal bermudagrass was officially released in 1943. Burton continued to make improvements in bermudagrass, with seven improved cultivars released during his career, including Tifton 85 (released in 1992).

The history of Burton's research on bermudagrass is a consistent theme throughout his 70 years in grass research. He integrated the different aspects of basic research with applied research to ensure that a product was delivered that was of benefit to the public. He had broad research interests, and he was convinced that both basic and applied research were necessary to develop cultivars that would be of benefit to the livestock and turfgrass industries.

Burton began his research on bermudagrass for forage in 1936. At that time cotton was the principal crop in the southern United States, and bermudagrass was considered the worst weed that plagued cotton growers. Burton's first hybrid bermudagrass was produced between a local bermudagrass cultivar and a cultivar from South Africa and was officially released in 1943 as Coastal bermudagrass. Coastal was a very poor producer of seed and had to be propagated by sprigs, not seed. Propagation by sprigs was met with resistance, but eventually this limitation became an important trait. Because Coastal bermudagrass produced few, if any, viable seeds, its potential as a weed in cotton fields was reduced significantly. Transforming one of the South's worst weeds into the best forage grass became one of Burton's greatest legacies. Burton continued working with bermudagrass. The second hybrid released was designated Coastcross, which was more easily digested by cattle. Further research resulted in the subsequent release of six additional cultivars that are grown on more than 10 million acres (nearly 4 million hectares) across the southern United States as pasture and hay for beef cattle and other livestock. More than 60 years after the release of Coastal bermudagrass, it remains one of the preferred cultivars for top hay and forage farmers. Burton developed and released Tifton 85 bermudagrass cultivar in 1992. Today there is probably more total pasture acreage of Coastal, but Tifton 85 is more productive and digestible. As new pastures are planted Tifton 85 has become the recommended cultivar to replace Coastal.

Burton's bermudagrass hybrids increased U.S. liveweight beef production by at least 1 billion pounds. Coastcross permits farmers to produce 30 to 40 more pounds of beef per acre. Tifton 44 produced 50 more pounds of beef per acre per year. By 1982 Coastcross was projected to be planted on 1 million acres and add 50 million pounds of beef at no extra cost. Tifton 44, released in 1978, has greater cold tolerance than previous cultivars, and permitted the growing of bermudagrass 50 to 100 miles farther north than previous cultivars. The source of greater cold tolerance in Tifton 44 was probably because of the source of germplasm used in the cross. Burton found one of the parents of Tifton 44 growing in a small patch along a railroad siding in Berlin, Germany. Other Burton bermudagrass hybrids are grown on golf courses, athletic fields, and lawns throughout the southern United States, the Caribbean, and Hawaii.

Burton and his fellow scientists conducted a broad research program in the improvement of grasses. Their research ranged from field selection to basic genetic studies of the fundamental nature of inheritance for different grass species. In addition to the highly successful acceptance of the bermudagrass hybrids, Burton's research program included sudangrass for forage, napiergrass for forage, bahiagrass, pearl millets for forage, pearl millets for forage and grain hybrids, and bermudagrass for turfs. A total of 44 cultivars were officially released for the different grass species. The development of better cultivars for each of the grass species was based on fundamental selection and genetic studies to determine the more efficient and effective improvement methods and agronomic field research to determine the husbandry practices that would maximize production and sustainability. These studies included methods to produce hybrids; estimation of the heritability of different traits; breeding and selection methods; recurrent selection methods for improvement of germplasm for breeding purposes; collection, evaluation, and maintenance of germplasm resources from all areas of world; cytological studies to determine the inheritance of traits; identifying sources of male-sterile germplasm for use in hybrid production; identifying sources of pest resistance to use in developing cultivars with greater tolerance to common pests and improved consistency of performance over locations and years; and identifying husbandry practices for stand establishment, fertility requirements, grazing and traffic tolerance, and carrying capacities for livestock. Results from his research program were reported in 777 publications from 1936 to 2003.

PROFESSIONAL HISTORY

Few agricultural scientists have accomplished as much as Glenn Burton did during the nearly seven decades of his research career. His entire professional career was as a research geneticist with the Agricultural Research Service (ARS), U. S. Department of Agriculture, at the Coastal Plain Research Station, Tifton, Georgia. After the completion of his graduate studies at Rutgers University, Burton went directly to Tifton to begin his career in grass research, studying the cytogenetics, breeding, and evaluation of problems related to hay, pasture, and turfgrasses adapted to the southern United States. Burton became one of the most highly recognized plant scientists in the United States and the world. He became the leading authority in grass breeding and genetics and served as a consultant and adviser for other scientists in grass breeding and related fields, for officials of state and federal organizations, for scientists and officials of foreign countries, and for representatives of regional agricultural groups and associations. He was a trusted source of information for industry, farmers, and the general public, and his research was accepted without reservation. The ARS accepted his research findings and when data were lacking, management helped to provide funds to conduct research projects under his direction.

Burton's research program included 12 grass species, of which five were studied intensively. Data collected and analyzed were published in a timely manner and is described in 777 publications from 1936 to 2003. In addition to the basic breeding and genetic studies, 15 improved cultivars or hybrids and 15 inbred lines for either forage or millet grain production were released for public use. Most releases had wide acceptance and use. He was very generous in sharing germplasm, released materials, and information with anyone interested in grass improvement. Burton's six turfgrass hybrids are planted on lawns, athletic fields, and most golf courses throughout the southern United States, as well as foreign countries. The released cultivars managed in accordance with his research findings and information distributed to the public have significantly changed the South from a row-crop cotton culture with its eroded fields and bare lawns to a profitable diversified agriculture with grasses adapted for reducing soil erosion, for greater economic benefits of the livestock industry, and for beautifying the environment.

Burton's comprehensive research program led to a better understanding of cultivar improvement in different grass species. Some examples of the extent of his research efforts were his discovery of the mode of reproduction, development of breeding methods, and solution to problems related to stand establishment and management of released cultivars of bermudagrass. Burton also discovered obligate apomixis in common bahiagrass and, with cytological assistance, determined the genetics of apomixis, and developed methods for its use in breeding. The methodology developed moved the germplasm to a sexual state for hybridization and recombination and back to the apomictic state to fix heterosis in hybrids, such as Tifton 54 bahiagrass. He developed techniques for the commercial production of hybrid seed in pearl millet. He developed four inbred lines that were crossed to give

Gahi 1 pearl millet. Gahi 1 yielded 50 percent more than common pearl millet and was the most widely grown summer grazing annual in the southern United States for many years, increasing yields 50 percent with a small extra cost of seed being the only additional input cost. Burton also found that mixtures of 75 percent hybrid and 25 percent parental pearl millet inbreds had yields similar to 100 percent hybrids. Pearl millet is grown on more than 40 million acres worldwide, with 27 million acres in India alone. Self-incompatibility was discovered by Burton in Pensacola bahiagrass and bermudagrass and proved very useful in producing crosses and polycrosses as well as hybrid seed on a commercial basis. Tifhi No. 1 was the first hybrid Pensacola bahiagrass cultivar produced by this technique. Burton also discovered cytoplasmic male sterility in pearl millet in 1956 and showed how reciprocal maintainer restorer relations between A_1 and A_2 sterile cytoplasms could be used in millet breeding and the production of hybrids for both forage and grain millets. Ancillary research also was conducted to enhance the basic discoveries for the different grass species, such as effects of radiation treatments to generate genetic variation; how to break dormancy in pearl millet seeds to permit greater flexibility in planting dates; used quantitative genetic models to determine the inheritance of different traits in different grass species; determined the effects of a single recessive pear millet gene (*tr*) on pleiotropy, including reduction of transpiration rates to increase drought tolerance; and developed the recurrent restricted phenotypic selection methods to reduce environmental effects in long-term selection for genetic improvement of germplasm.

Burton had other opportunities to change positions during his career, but he elected to spend his entire professional career as a plant geneticist with the U.S. Department of Agriculture at Tifton, Georgia. No single individual has had greater impact on forage and turfgrass development, production, and utilization. His contributions to the advancement of forage and turfgrasses have been recognized by his colleagues and peers. Burton received 80 awards related to his contributions in grass breeding, starting with the American Society of Agronomy Stevenson Award in 1949 and the Crop Science Society of America Presidential Award in 1997. Burton was elected to the National Academy of Sciences in 1975, and he was a regular attendee and contributor to Section 62 (Plant, Soil, and Microbial Sciences), Class VI (Applied Biological, Agricultural, and Environmental Sciences). In 1983 Burton was one of eleven scientists and engineers (including Edward Teller, father of the hydrogen bomb) who received the National Medal of Science presented by President Reagan. He was inducted into the Agricultural Research Service Science Hall of Fame in 1987. Burton also was active in his professional societies, serving on a number of committees and as chair of the Crops Division (1952), and as vice-president (1961) and president (1962) of the American Society of Agronomy. He held memberships in the American Society of Agronomy, Crop Science Society of America, American Genetic Association, Society for Range Management, and American Forage and Grassland Council.

PERSONAL HISTORY

Glenn W. Burton was born May 5, 1910, in Clatonia, Nebraska. He was the only child of Joseph Fearn and Nellie Rittenburg Burton and was kept busy helping his parents with farm work when not in school. In 1915 they moved to a farm near Bartley, Nebraska, where he grew up helping his father farm with horse-drawn equipment. He attended a one-room country school for grades one through eight and graduated from the four-teacher Bartley High School in June 1927. Burton enjoyed school, had an insatiable curiosity, loved to solve problems, and graduated number two in his class. He participated in all high school sports, music, and drama programs.

He farmed with his father in 1927 and 1928, when he enrolled at the University of Nebraska. Ward Shores, his high school superintendent, convinced Burton that he should go to college to study to become a vocational agriculture teacher. He pursued this course of study while working part-time in the Department of Agronomy until the beginning of his junior year. At that time F. D. Keim, head of the Department of Agronomy, convinced Burton that agronomy and plant breeding should be his major fields of study. Keim directed the course of his life through graduate school. Burton graduated from the University of Nebraska in January 1932 in the upper 3 percent of his class. Upon graduation he accepted a half-time position at Rutgers University, where he earned his master of science (1933) and Ph.D. degrees (1936). Howard Sprague was his major professor and provided excellent training, experience, and inspiration for Burton to be a successful plant breeder.

Glenn Willard Burton and Helen Maureen Jeffryes were married in 1934. Helen had majored in home economics and became a dietician. They were the parents of five children. They celebrated their 60th wedding anniversary before her death in 1995. In April of 1936 they moved to Tifton, Georgia, where he took a position as principal geneticist with the Division of Forage Crops and Diseases of the Agricultural Research Service of the U.S. Department of Agriculture at the Georgia Coastal Plain Experiment Station. For more than 61 years Burton continued to lead the Grass Breeding Department at Tifton. After his formal retirement in 1997, he continued his research program with funding from his salary savings. In 1950 when the Coastal Plain Experiment Station became a part of the University of Georgia, Burton became a member of the faculty of the College of Agriculture, serving as chairman of the Agronomy Division until 1964 when he was named Distinguished Alumni Foundation Professor. As a member of the university graduate faculty, he directed the research of 16 graduate students.

Glenn W. Burton died November 22, 2005, of natural causes in Tifton, Georgia, at the age of 95. He is survived by five children, eight grandchildren, and 12 great-grandchildren. He maintained an active research program to the time of his death, publishing more than 40 publications after his formal retirement in 1997. The Burton family was active in the First Methodist Church and was named Methodist Family of the Year in the United States in 1951. He enjoyed singing in the church choir and was a member of a quartet in his earlier years. He served as Sunday school teacher, was district lay leader, and held other leadership positions within the church. He also served on the Board of Stewards and as a lay speaker.

PUBLIC SERVICE

Plant breeding and the Methodist church received most of Burton's attention during his professional career. He was very active in the Methodist church, singing in the choir and serving on numerous committees. He was elected to the Tifton County School Board in 1952 and served on the board for four years. Burton was a member of five professional societies related to the breeding and genetics of plants. He was a regular attendee at the annual meetings and usually presented oral papers related to his research on forage and turfgrasses. Because of his preparation and speaking abilities, he played an active leadership role in the American Society of Agronomy (ASA), the largest professional society for all aspects of plant improvement and soils, serving as chair of the Crops Division (1952) and the Fellows Committee (1963) as well as vice-president (1961) and president (1962). He was chair of the Agronomic Science Foundation from 1972 to 1982. He frequently served on special awards and other committees of the Crop Science Society of America and the ASA during most of his career. He was a member of the American Genetic Association Council from 1973 to 1975. Within the National Academy of Sciences, Burton was chair of Section 62, Class VI from 1978 to 1980 and also served on ad hoc committees. Perhaps, Burton's greatest public service was the attention and time given to fellow researchers within the United States and foreign countries. He was always open-minded and generous in discussing his research, and freely sharing his plant germplasm with other researchers. He was always interested in people and trying to make a better world to live in. Work was his hobby, his joy, and the focal point of his life.

FOREIGN SERVICE

Burton had frequent requests to travel, lecture, and consult internationally. He traveled in 55 foreign countries, including the former Soviet Union (now the Russian Federation) and the Peoples Republic of China. In addition to lecturing and consulting with staff and students, he was an active collector of germplasm that could have value in crosses with domestic cultivars; collections were gathered in Nigeria, South Africa, Uganda, Kenya, Senegal, Brazil, Uruguay, Argentina, Germany, Switzerland, Italy, France, Great Britain, and India. During September and October of 1979, he chaired the meeting in India of the Advisory Committee on Sorghum and Millet Germplasm of the International Board of Plant Genetic Resources; discussed forage research at the Pasture Section of the Ministry of Agriculture in Greece; and consulted with forage research workers on a proposal for cooperative research in Israel. In 1981 he led a committee to review the U.N. Development Programme on sorghum and millets at the International Crops Research Institute for the Semi-Arid Tropics in Hyderabad, India. The same year, Burton chaired the meeting of the Advisory Committee on Sorghum and Millet Germplasm of the International Board of Plant Genetic Resources in Senegal.

In 1961 Burton initiated cooperative research with scientists working for the Rockefeller Foundation to help increase India's millet production on 27 million acres of land too dry to grow other grain crops. Burton's suggestion was to make new hybrids by crossing his own Tifton 23A pearl millet hybrid and the best Indian cultivars. The suggestion was successful, and Indian pearl millet production increased from 3.5 million metric tons in 1965 to 8 million metric tons by 1970. From Burton's seeds the Indian scientists produced new hybrids that yielded 88 percent more grain than the native landrace millets. Although the original hybrids later were susceptible to disease, Burton said that the biggest thrill of his life was that millet production more than doubled from 1965 to 1970. Burton's work on pearl millet and Nobelist Norman Borlaug's work on wheat are credited with helping to prevent famine in India during the 1970s.

HONORS AND AWARDS

1949	American Society of Agronomy Stevenson Award
	Fellow, American Society of Agronomy
1955	Honorary D.Sc. degree from Rutgers University
1962	Honorary D.Sc. degree from University of Nebraska
1968	Agricultural Institute of Canada Recognition Award
1973	DuPont Foundation Medal for Distinguished Service to Man
1975	Elected to the National Academy of Sciences
1979	DeKalb Crop Science Distinguished Career Award
1980	USDA Distinguished Service Award
	Southern Turfgrass Association Honorary Member Award
1981	President's Award for Distinguished Federal Civilian Service

1982 University of Nebraska Alumni Achievement Award
University of Nebraska Master Alumni Award
1984 Elected into University of Georgia Agricultural Alumni Hall of Fame
1985 Fellow, Crop Science Society of America
1988 The Alexander von Humboldt Foundation Award
Honorary membership in the Grassland Society of Southern Africa
1994 Inducted into Georgia Turfgrass Hall of Fame
1995 Inducted into Georgia Golf Hall of Fame
1997 Inducted into Georgia Cattlemen's Hall of Fame
Crop Science Society of America Presidential Award

Sources of information used to summarize the long career of Glenn W. Burton were:

(1) Joe Burton, Glenn W. Burton's son, a soybean research geneticist at the Agricultural Research Service, U.S. Department of Agriculture, stationed at Raleigh, N.C., provided information from Glenn W. Burton's career records;
(2) article by D. Wayne Hanna in TPI Turf News (March/April 2000, pp. 18-19); and
(3) an article in the Agricultural Research Newsletter (2[1981]:15-16).

Each of these sources provided information on his professional career, his personal interests, and his impact on the agriculture and turfgrass industries in the southern United States. My first contact with Glenn W. Burton was in 1962, when he was president of the American Society of Agronomy. I was very impressed with his organization, enthusiasm, and speaking abilities, qualities that had a lasting impact on me and others in the plant sciences. Although I did not have a close professional and personal relation with him, his dedication and interests in field plant research were those that I and others attempted to emulate.

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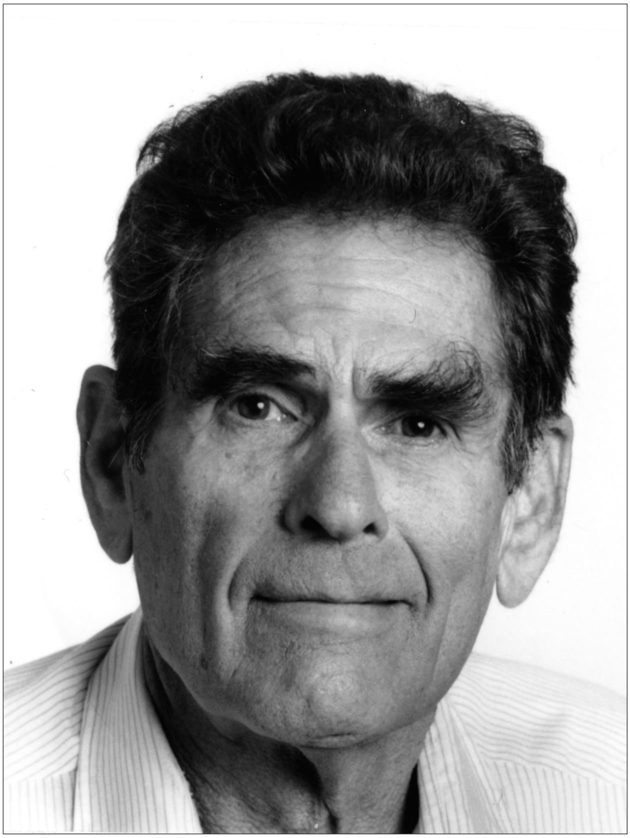

Photograph courtesy MIT Museum.

Peter Elias

PETER ELIAS

November 26, 1923–December 7, 2001

BY ROBERT G. GALLAGER

PROFESSOR PETER ELIAS, PROBABLY THE most important early researcher in information theory after Claude Shannon, died from Creutzfeld-Jacob disease at his Cambridge, Massachusetts, home on December 7, 2001. His three children—Daniel Elias, Paul Elias, and Ellen Elias-Bursac—were with him. His wife, Marjorie (Forbes), predeceased him in 1993 after 43 years of marriage. Pete was distinguished not only for his research but also as the head of the Massachusetts Institute of Technology's Electrical Engineering Department during the crucial period from 1960 to 1966 when the department was changing its education from engineering practice to engineering science and when computer science was starting to be recognized as a major part of electrical engineering.

Among other honors, Pete was a fellow of the IEEE, a charter fellow of the Association for Computing Machinery, and a fellow of the American Academy of Arts and Sciences. He was elected to the National Academy of Sciences in 1975 and the National Academy of Engineering in 1979. He received the Claude E. Shannon Award, the highest honor of the IEEE Information Theory Society, in 1977. Somewhat belatedly he was the recipient of the Hamming Award, a major medal of the IEEE, immediately before his death.

EDUCATION (1923-1953)

Pete was born on November 23, 1923, in New Brunswick, New Jersey, where his father was an engineer at the Thomas Edison Laboratory. He completed his secondary education at the Walden School in New York City in 1940 and then enrolled in Swarthmore College. He transferred to MIT for his final two years and received an S.B. in management in 1944. After serving as an instructor for radio technicians in the U.S. Navy for the remainder of World War II, Pete entered Harvard University, where he received a master's degree in computation.

In 1948, immediately after the field of information theory was created by Claude Shannon's masterpiece, *A Mathematical Theory of Communication*, Pete started to search for a Ph.D. topic at Harvard. He soon came upon Shannon's work and was hooked for life. He was fascinated by the intellectual beauty of Shannon's theory, and immediately realized, first, that it provided the right conceptual basis for communication engineering and, second, that a great deal of work remained to be done before the practical benefits of the theory could be realized.

Norbert Wiener's contemporary work on cybernetics provided a complementary set of theories about communication, computation, and control. Pete's Ph.D. thesis, "Predictive Coding," used some of Wiener's results on prediction to attack the information-theoretic problem of determining the number of bits required to represent an analog data source. Pete's underlying idea here was simplicity itself; the predicted value of a symbol, based on previous symbols, is determined by those previous symbols, and thus carries no new information. This means that only the error in prediction (which is known at the encoder via the feedback) needs to be encoded. The full development of this idea, carried out in the thesis, led to a number of insights about data compression and also to a better appreciation of the relationship between information theory and cybernetics.

After completing his Ph.D. thesis, Pete was appointed as a junior fellow in the Harvard Society of Fellows and spent the next three years doing research on a variety of subjects, including writing several pioneering papers on optical communication, continuing his core research on information theory, and working with Noam Chomsky on syntactic characterization in linguistic theory.

EARLY RESEARCH (1953-1960)

Information theory was a very popular topic among mathematicians, physicists, biologists, and social scientists during these years but most were looking in from the fringes. There was a relatively small community of researchers, concentrated at the Bell Telephone Laboratories and at MIT, who were focused on finding how the new theory could affect telecommunication systems. Robert Fano, the leader of the information theory group at MIT, persuaded Pete to accept an appointment at MIT in 1953 as an assistant professor in the Electrical Engineering Department.

The next seven years were extremely productive for Pete Elias, MIT, and information theory. The elegance and apparent importance of the theory attracted the very best graduate students, and the large number of accessible research problems created a heady and active research atmosphere. As will be seen, Pete's papers in this period opened up a large number of new approaches, forming the starting point for many Ph.D. theses then and later.

The cornerstone of information theory was Shannon's noisy-channel coding theorem, which showed that it is possible to send data over an essentially arbitrary noisy channel at any rate up to the capacity of that channel, and to do so with an arbitrarily small probability of error. Shannon showed how to calculate that capacity, but his proof about small probability of error was an ingenious existence proof with almost no clues about implementation other than the need for considerable delay and computational complexity.

Although it would take another 40 years to learn how to reach capacity in practice, Pete's 1954 paper, "Error-Free Coding," developed the first algorithm for achieving zero error probability at a strictly positive rate. The algorithm was simple and elegant, but more important it introduced product codes (as they were called later) and iterative decoding to the field. Both of these ideas were sharp departures from the approaches being used in the coding research of that era. Generalizations of product codes and iterative decoding appear in most practical communication systems of today, including turbo codes and low-density parity-check codes.

Pete's next major paper, "Coding for Noisy Channels," in 1955, is perhaps the most influential early paper in information theory after Shannon's original papers. This was another attack on the central problem of finding coding and decoding techniques for reliable data transmission on noisy channels at rates close to channel capacity. It was a more fundamental approach than his "Error-Free Coding," and it provided three giant steps toward the search for effective coding strategies.

"Coding for Noisy Channels" is restricted to a particularly simple model of a noisy communication channel known as a binary symmetric channel, but this model contains all the significant conceptual problems of coding and decoding. Many mathematicians in that era were trying to generalize Shannon's theorems to the most general channels, but Elias saw that the more critical problem was to understand the simplest nontrivial cases first.

The usual approach to error-correcting codes (including those by Shannon and Hamming) was block coding (i.e., transmitting binary data in blocks of some given length n). Only some smaller number k of those bits would be independent data bits and the rest would check on those data bits. As a trivial example with $n = 3$ and $k = 1$, a 0 would be converted to 000 and a 1 to 111. If any one of these 3 bits were corrupted by noise, the other two would still permit correct decoding by majority rule. Shannon showed that by making n very large and keeping the rate $R = k/n$ in data bits per transmitted bit constant but less than capacity, the probability of correct decoding could be made to approach 0 with appropriately chosen coding and decoding. More surprisingly, Shannon proved his result by random choice of codewords, thus showing that the choice of code was not critical if the block length could be made arbitrarily large.

Elias realized that the potential for practical error-correcting codes would depend on the complexity of coding and decoding, which increase rapidly with n. Thus it was important to find how quickly error probability could be made to decrease with increasing n and also to discover whether good codes with special structure could simplify the implementation.

To answer the first question, Pete showed that the error probability (using optimal decoding), averaged over all codes of given n and $R < C$, decreases exponentially with n. He also found a lower bound on error probability for the best possible code of given n, R. By comparing the average result with the lower bound, he showed that the best code of given n, R is not substantially better than the average code. Thus error probability decreases rapidly with block length, and the choice of code is not critical. This showed that the essential problem of coding is simple implementation rather than optimum performance. Later researchers used techniques generalized from those of Elias to establish the same result for arbitrary noisy channels.

Now that Pete had established that the essential problem was complexity of implementation, he went on to show that parity-check codes, which had dominated the search for good codes of short block length, were also, on average, as good as the best codes for long block lengths. This meant that researchers could restrict their search to parity-check codes (which had enormous advantages in implementation) and simply avoid those few parity-check codes with very poor performance. This was not as simple as it appeared, since arbitrary parity-check codes were still quite difficult to decode, and the desire for simple implementation typically led to codes that were very much worse than average. This led to the popular saying of the time that "all codes are good except those we can find."

The third major result of this paper was the invention of convolutional codes. These were not block codes but instead resembled the digital equivalent of the linear filters that were so popular in circuit theory. Pete showed that these codes, on average, performed just as well as block codes, but were simpler to work with in a number of ways. The majority of practical coding systems used in practice ever since have used convolutional rather than block codes.

In summary, "Coding for Noisy Channels" provided the trade-off between block length, rate, and error probability on the binary symmetric channel. It showed that the best code is substantially no better than average, and that the average parity-check code and the average convolutional code are equally good. The result was to change the focus in error-correction theory from finding the optimum block codes of given length to finding classes of codes with simple decoding algorithms.

It is somewhat characteristic of Pete that this blockbuster paper appeared only in the convention record of a conference. Pete was never one to expand his publication record by multiple versions of the same paper. The field was small enough at that time, however, that all the serious researchers were familiar with his work. This paper, as well as a number of his other papers, appeared later in the major anthologies of the most important papers in the field.

"Channel Capacity without Coding," in 1956, was another important paper. Shannon had just proven that feedback does not increase the capacity of a noisy channel, but it was clear that feedback could simplify coding and decoding. For analog sources, information theory showed that there is a minimum possible distortion when the source is encoded and sent over a given channel but that the necessary encoding could be very complex. For the special case of a band-limited Gaussian source sent over a band-limited Gaussian channel of the same bandwidth, however, no encoding at all was necessary to achieve the minimum mean-square distortion. If the bandwidths were different, however, all the complexity of the general case reappeared.

Pete's contribution here was to show that if the channel bandwidth were some integer multiple of the source bandwidth and if feedback were available, then, using the appropriate simple strategy, the need for complex coding disappears. As with a number of Pete's results, it was a very special case that was analyzed, but the idea was applicable in much greater generality. Elias's approach greatly simplifies many later results in the literature about coding with feedback.

"List Decoding for Noisy Channels," in 1957, was another paper that seemed quite specialized and impractical at first but which has had many later consequences. The idea here is that the decoder for a noisy channel, rather than choosing the most likely single transmitted codeword, might choose a small list of likely codewords. This could be envisioned as useful in multistage coding where the list of likely words at one stage is reduced to a single choice at a later stage. Actually, however, the analysis was primarily intended to provide a better understanding of why randomly chosen codes are so good, and more generally to provide a better understanding of the geometry of binary *n*-space. The results in this paper provide an important link in finding the best-known bounds on error probability for arbitrary noisy channels, and even in algorithms for formal mathematical proof checking.

Another influential paper of a very different kind was an editorial entitled "Two Famous Papers" in the *IRE Transactions on Information Theory*, which was the premier journal for information theory. In an effort to improve the quality of papers in the journal, Pete described two imaginary extremes of bad technical papers. The first is facetiously entitled "Information Theory, Photosynthesis, and Religion" and the imaginary paper uses each term in the title to further obfuscate the others. The other paper, with a title three lines long, adds incremental detail to a problem previously worked to death. This editorial became well known for its amusement value but also helped give reviewers the needed backbone to reject papers of no merit.

This editorial was viewed by some as an attempt to emphasize practical engineering over more speculative efforts to explore new areas, but this was incorrect. Pete always had very broad interests, ranging across telecommunication, mathematics, the sciences, and liberal arts. He had also become a founding editor somewhat earlier of the journal *Information and Control*, which was intended to publish papers of somewhat broader scope than the *Transactions on Information Theory*.

Pete also always found the time to listen to and help undergraduates, graduates, other faculty members, and researchers in all sorts of areas from speculative to very detailed, and he often encouraged people to look at problems from new angles. What his editorial was objecting to was not the honest effort of researchers to communicate but rather papers with no content or thought written solely to enhance a publication record.

DEPARTMENT HEAD (1960-1966)

In 1960 Pete was promoted to full professor and at the same time was appointed head of the Department of Electrical Engineering (later to become the Department of Electrical Engineering and Computer Science). He was 37 at the time, a remarkably tender age to be appointed department head of the largest department at MIT and the top-ranked electrical engineering department in the country. Two immediate questions come to mind: why was he appointed and why did he accept?

To understand why he was appointed, recall that 1960 was at the leading edge of the information age. It was clear at that time that communication, computation, and control were going to change the way we live, and the only uncertainty was the rapidity of the change and the exact nature of the change. Pete was a leader in one of these fields and highly knowledgeable about the others. He was also very knowledgeable about the physics-oriented part of electrical engineering, and recognized its necessity for developing the digital devices required for information technology. Perhaps most important, his integrity and goodwill were widely recognized. He could be counted on to recognize the many strengths in the department and to encourage each person to develop those strengths for the good of both the department and the individual.

It was a testament to the department and institute leaders at the time that the coming revolution was well anticipated, and that Pete's wisdom and capabilities to meet the challenge were recognized.

From Pete's standpoint his research career was in high gear, and he was in the right place with the right capabilities to solve even more fundamental problems in information theory, digital communication, and several new areas of computer science. He was being asked to put this research on hold and to lead a department of 72 faculty members, many older and more experienced than he, and to do this with almost no infrastructure of separate areas within the department. The departmental politics, given the size of the department, were relatively benign, but the rapid changes on the horizon could have led to ugly repercussions without remarkable tact and understanding from the department head.

Added to this, Pete was clearly an academic and an intellectual who fully enjoyed the pursuit of research problems. At the same time he had very general interests and thoroughly enjoyed interacting with the rest of the department to better understand what they were doing. He was also a humanist and enjoyed helping others in both technical and nontechnical ways. As department head, he would be in a position to interact with a large and outstanding group of people.

Pete's style of leadership was to listen to everyone carefully and help them develop ways of contributing, given the constraints on the department. He was not a person who enjoyed controlling others, but he did enjoy understanding and helping. I was one of Pete's Ph.D. students, and he was enormously helpful in discussing many issues with me, but he was never directive. I didn't realize until later what a great gift it was to be able to develop my own skills for formulating and doing research.

In the end Pete accepted the appointment, with some qualms but with an eagerness to meet the challenge. His style of leadership turned out to be just right, and the department changed and prospered enormously over the next six years.

During this period, the department grew by more than 50 percent, and research topics changed even more. Many of the new hires were in the computer area, and many were in the communication network area. Many more were in system and control areas, which were in a state of rapid change and growth in the 1960s. Before 1960 the department viewed its research mission as being divided between processing and transmitting information on one side and processing and transmitting energy on the other. By 1966 the information side had dwarfed the energy side, and the information side had split into a very large number of new areas, with computer science being the most rapidly growing.

The department's mission while Pete was department head was not only education and research but also educating young faculty members in these new fields for careers in other universities and laboratories. The Ford Foundation was financing this effort, and it was highly successful as a means for rapid transfer of new research fields.

LATER CAREER (1966-2001)

When Pete completed his term as department head, he returned to a more academic life of research and teaching. He was by now viewed as a senior statesman and was in considerable demand for government, MIT, and professional committees requiring people of wisdom and tact. One particularly important MIT committee that he chaired was the Ad Hoc Committee on Family and Work. This committee was a response to the intense work pressure generally experienced at MIT, and explored how the institution could help people find a healthy balance between work and family. The report was issued in 1990 and is generally credited with a major increase in administrative sensibilities about these issues.

Pete's research after 1966 shifted somewhat toward the computer field, and he became affiliated with the Laboratory for Computer Science (now called the Computer Science and Artificial Intelligence Laboratory, or CSAIL). Much of his work in this area used information-theoretic arguments to approach questions about storage, organization, and retrieval for large files.

One of the interesting questions about file storage is how to achieve efficient storage in a universal manner. Information theory had solved the problem of efficient storage (encoding) for data with known probabilistic structure, but in practice the probabilistic structure is usually unknown. One would like to achieve the same efficiency without that added knowledge. This was a problem that had been attacked several times earlier, but Pete developed a number of simple theoretical and practical approaches that have found their way into many later universal coding schemes.

Pete became an Emeritus Professor in 1991. It is inappropriate to say he retired, since he still enjoyed coming to his office most days. He was still active advising students, organizing department colloquia, or simply playing an active role in the intellectual life of the community. He was a wonderful conversationalist, so well informed and well balanced that everyone just enjoyed being around him. The many colleagues who knew him miss him greatly.

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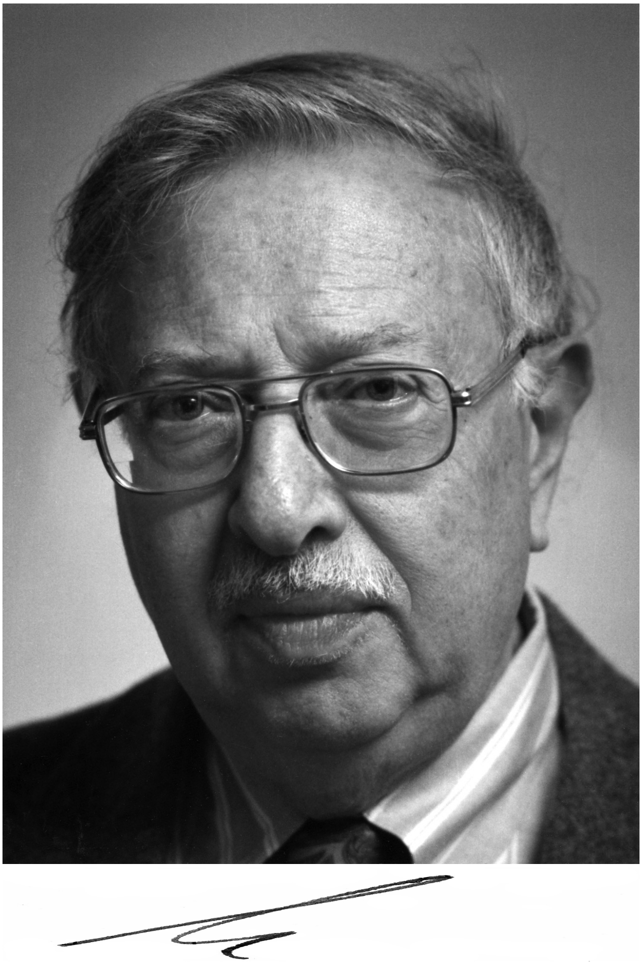

STANLEY MARION GARN

October 27, 1922–August 31, 2007

BY C. LORING BRACE

STANLEY MARION GARN WAS ONE of the most important figures in the field of biological anthropology in the second half of the 20th century. He is well known for his longitudinal studies of biomedical problems, as well as the relationship between nutrition and osteoporosis. He also completed key studies on family histories, hair, nutrition, odontogenesis, and obesity.

Born on October 27, 1922, in New London, Connecticut, Garn was the grandson of a rabbi. Garn grew up in Providence, Rhode Island, where his father was involved in house construction. Garn retained an interest in construction for the rest of his life and although he went on to become a university professor, he kept a tool shop in his basement until the time came for him to go into assisted living. He also retained an interest in landscape and gardening for the same span of time.

Garn became interested in science early in life by reading books from the Rochambeau branch of the Providence Public Library, only three blocks from his house. He received books on home science and even some chemistry sets from his cousins. Collecting forays involved a trip with his parents to Diamond Hill, Rhode Island, so that he could gather minerals;

to the Great Swamp to hunt for pitcher plants; and to the Eastern Scientific Company, where he bargained for leftover reagents, scratched test tubes, and chipped beakers.

At Providence Classical High School he had excellent science teachers who gave him problems to solve and questions to answer that went beyond the class requirements. Two of his violin teachers even contributed to his scientific education; one introduced him to carboniferous fossils and another to the rudiments of photography, which was to play a role in his career with newspapers and the Polaroid Corporation.

Garn acquired a broad but eclectic knowledge of the history of science in high school, saying, "I was very weak in theoretical and quantitative aspects, but strong in taxonomy and *materia medica.*"

Garn entered Harvard in 1939 and found the wealth of opportunities positively overwhelming, noting that "it was Anthropology and specifically Physical Anthropology that captured my attention for it dealt with people and human biological variability and evolutionary practice and primates. Moreover, Anthropology introduced me to exotic places, a most tantalizing introduction for a lad from Providence, Rhode Island who had only once gone as far as New York City."

From his freshman year on, Garn was active as a part-time college reporter for various Boston newspapers. The pay was 25 cents per typeset inch, but on *The Daily Record* the headlines were counted, too. Being a correspondent with regular deadlines, he could not afford writer's block, and Garn was lucky, because he claimed he never suffered from that affliction. After three years at Harvard, Garn earned his degree, writing a thesis cum laude on dental variability.

In the fall of 1942 he entered Harvard Law School because he thought anthropology was unlikely to let him earn a living, but three months later he was a research associate in chemical engineering at the Massachusetts Institute of Technology, working as a photographer and photomicrographer. He took delight taking photos of asbestos fibers between crossed Nichol prisms.

Fortuitously, however, physical anthropology reentered the picture because the Chemical Warfare-MIT lab became heavily involved in gas-mask design and there were problems with fitting them. Therefore, Garn enrolled in the graduate school at Harvard, finally convinced that physical anthropology was practical and that one could even make a living at it. Taking one reading course with Earnest Albert Hooton each semester, he continued to live in Divinity Hall right near the Peabody Museum and its library. In 1944 he went to Polaroid as a technical editor, and his day-to-day work brought him close to Edwin H. Land.

As World War II ended and the guided-missile project at Polaroid came to an end, Garn was faced with three choices. The first choice was to stay with Polaroid, transferring to the new camera division; the second choice was to become a scientific writer elsewhere; and the third was to return to graduate work full-time with the goal of getting a Ph.D. He was even described as a “famous anthropologist” at that time in some of the Polaroid advertising copy; because he borrowed part of the Polaroid 77 design from Eskimo “sunglasses” in Harvard’s Peabody Museum.

Upon returning to graduate school, Garn was given the opportunity to participate in the first Summer Seminar in Physical Anthropology at the Wenner-Gren Foundation in New York. This was exciting to Garn because all the big names were there, and human genetics and population biology were strongly represented. By the time of the 1950 Cold Spring Harbor Symposium, he and others had a better idea of what population genetics was all about.

In the summer of 1946 in New York Garn had a chance to test successive modifications of the new nosepiece he was designing for the Polaroid 77 sunglass because New York was full of diverse people with noses of different shapes. In the fall of 1946 Garn began a part-time appointment as a research fellow in cardiology at Massachusetts General Hospital, working with Paul Dudley White. To aid in White's study of coronary heart disease, Garn worked on anthropometric measurements and somatotyping, using Sheldon's method. Out of this work came dozens of papers and an awareness that cholesterol/phospholipid ratio was more telling than cholesterol alone, anticipating current attention to the high-density lipoproteins (HDL). For this work Garn was paid $1,000 a year; it was half-time work and low pay but residents and fellows at Massachusetts General Hospital told Garn how lucky he was to be working with Paul White. Through this work Garn also met Fuller Albright, Nathan Talbot, and Joe Aub, all three of whom helped him on later hair research.

In 1947 Garn began his second part-time job at the Forsyth Dental Infirmary in Boston. This was in line with his undergrad thesis, *Anomalies and Variability of the Dentition*, and he was expected to teach dental interns and study the growth of orthodontic patients; in fact, Garn says that what he learned at Forsyth led later to more than 100 publications. His first clinical paper (published in the *British Dental Journal*) was on the dentition in Morquio's syndrome. Garn also got to know M. M. Cohen, who became a collaborator and coauthor on many publications.

In the spring of 1948 Garn finished his Ph.D. on human hair and quickly prepared to join the Harvard-Peabody Aleutian Expedition organized by William S. Laughlin. Among Garn's discoveries was that all the hypercholesterolemics in Umnak, Alaska, were members of one extended lineage.

In 1950 Garn married Priscilla Crozier, who survives him. In 1952 Garn was invited to the Fels Research Institute in Yellow Springs, Ohio, to interview for a position. He discovered that it came complete with a 23-year longitudinal database on various dimensions of human health, a solid departmental budget, and the opportunity to teach at Antioch College. At the end of two days in Yellow Springs, after the salary and perks had been described, Garn was asked for an immediate reply. He said yes and took the train back to Boston to tell the news to his wife, who merely asked, "When do we go?"

Before arriving in Yellow Springs, Garn was promoted to the rank of associate professor. One of his projects there involved the study of adult bone loss and the study of Xenia, Ohio, senior citizens. One of Garn's approaches to determining bone quality (percent cortical area or the amount of cortical bone in the cross-section) has since been named the Garn Index.

At Yellow Springs, Ohio, Garn supervised the Department of Growth and Genetics at the Fels Research Institute, but he also taught anthropology at Antioch College there. One of Garn's students was the late Stephen Jay Gould (1941-2002), long-time professor of paleontology at Harvard University and a monthly columnist for *Natural History* magazine for over a quarter of a century. Some of the anti-evolutionary ethos that Gould represented in dealing with the human fossil record may have been derived from Hooton's outlook at Harvard as transmitted in the Antioch classroom by Stanley Garn. The other thing that survived for a long time after his indoctrination into it at Harvard was his commitment to the biological reality of human "races." The Coon, Garn, and

Birdsell book (1950) is an early manifestation of this outlook and it lasted for a good two decades, continuing even after Garn had moved to Michigan, where biological anthropologists in the Department of Anthropology had pioneered in the documentation of the nonexistence of human races.

In 1968 Garn was offered a newly created position at the University of Michigan, as a fellow of the Center for Human Growth and Development. Not long before coming to the University of Michigan he was elected president of the American Association of Physical Anthropologists. And eight years after he moved to Michigan, he was elected to the National Academy of Sciences—in 1976.

One of the things that the profession at large will remember about Stanley Garn is his legendary productivity. With nearly 850 publications to his name, he overshadowed just about everyone else in biological anthropology. He did not type those contributions himself. At his retirement dinner on the University of Michigan campus in November of 1992, the long-time director of the Center for Human Growth and Development, Robert E. Moyers (1919-1996), recalled Garn's technique of getting a publishable manuscript. Calling to his secretary, he would say, "Shirley, take a paper," after which he would dictate what was to be sent to the appropriate journal. Fortunately he had good laboratory and secretarial help wherever he was located. "Greetings!" he would say to start the workday, and with his perpetually cheerful and upbeat manner, his crew always got a great deal of positive work done. During his career at Michigan, he was a regular member of doctoral thesis committees. He also coordinated the background that generated the data on human nutrition and growth that a stream of graduate students used for doctoral dissertation projects.

For most of his career Garn's focus was on the nature of human biological variation. His doctoral dissertation had been on human hair, its cross-section, texture, and variation, and for much of the rest of his life he dealt with many aspects of human growth and development, both normal and abnormal. He is well known for his Garn Index, the loss of bone density during the aging process, and numerous longitudinal studies of biomedical problems.

SOME OF THE INFORMATION in this memoir was provided by Barbara Garn, daughter of Stanley and Priscilla Garn. Barbara and her brother William David Garn live in San Luis Obispo, California, and I thank them both for the help and information they have given me.

CHRONOLOGY

1922	Born on October 27 in New London, Connecticut
1942	B.A. degree in anthropology, Harvard University
1942-1949	Research Associate in Chemical Engineering, Chemical Warfare Service Development Laboratory, Massachusetts Institute of Technology
1944-1946	Technical Editor, Polaroid Corporation
1946-1947	Consultant in Applied Anthropology, Polaroid Corporation
1946-1950	Research Fellow in Cardiology, Massachusetts General Hospital
1947	M.A. in anthropology from Harvard University
1947-1952	Anthropologist, Forsyth Dental Infirmary, Boston
1948	Aleutian Islands field research
1948	Ph.D. in anthropology, Harvard University
1948-1952	Instructor in anthropology, Harvard University
1950-1952	Director, Forsyth Face Size Project, Army Chemical Corps
1952-1968	Associate Professor and Professor of Anthropology, Antioch College
1952-1968	Chairman of the Department of Growth and Genetics, Fels Research Institute
1968-1993	Fellow of the Center for Human Growth and Development, University of Michigan;
1968-1993	Professor of Nutrition, School of Public Health, University of Michigan, Ann Arbor

1972-1993 Professor of Anthropology, University of Michigan
1993 Emeritus
2007 Died August 31 in Ann Arbor, Michigan

AWARDS AND HONORS

1976 Elected to the National Academy of Sciences
1981 Neuhauser Lecturer, Society for Pediatric Radiology
1988 Raymond Pearl Lecturer, Human Biology Council
1987 Harvey White Lecturer, Children's Memorial Hospital
1994 Charles R. Darwin Lifetime Achievement Award, American Association of Physical Anthropologists
2002 Franz Boas Award, Human Biology Council

OTHER POSITIONS

1958 Visiting Professor, University of Chicago
1962 Visiting staff member, Institute of Nutrition for Central America and Panama
1976 Visiting Professor, Southern Methodist University
1986 Walker-Anes Visiting Professor, University of Washington

MEMBERSHIPS

American Society of Naturalists
American Anthropological Association (Fellow)
American Association of Physical Anthropologists, President, 1966-1967
American Academy of Arts and Sciences (Fellow)
American Academy of Pediatrics (Fellow)
American Society of Clinical Nutritionists (Fellow)
International Association for Dental Research
American Institute of Nutrition (Fellow)
International Organization for the Study of Human Development
Human Biology Association
International Association of Human Biologists
American Society for Nutritional Science (Fellow)
National Academy of Sciences

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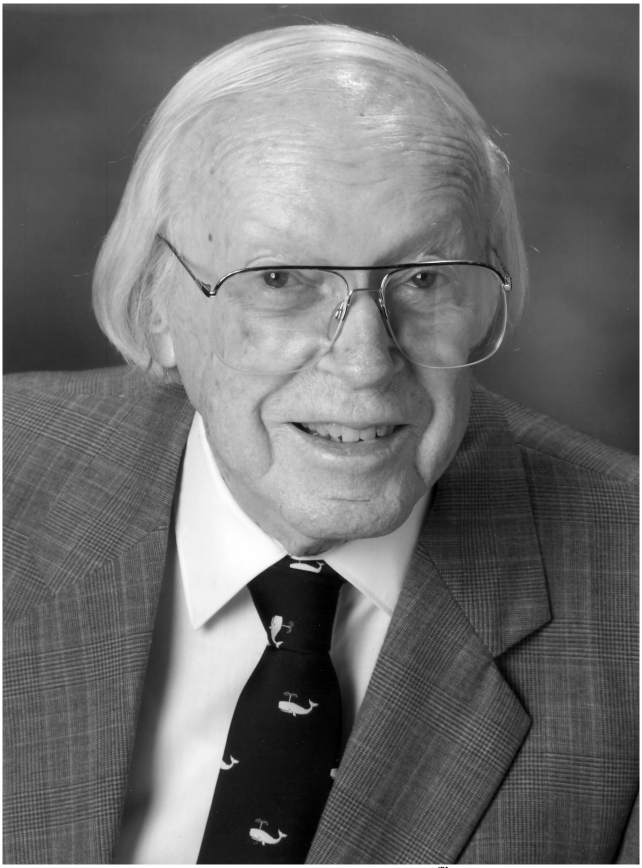

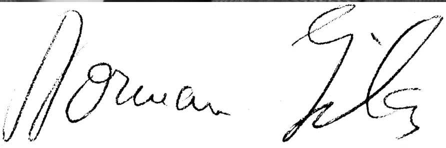

NORMAN HENRY GILES

August 6, 1915–October 16, 2006

BY MARY E. CASE AND FREDERICK J. DE SERRES

Norman Giles was recognized as a pioneer in the fields of radiation cytology and fungal genetics. His early studies (1939-1955) were on microsporogenesis and chromosome aberrations in *Tradescantia*. His first use of *Neurospora crassa* as an experimental organism was in reversion analyses of inositol mutants. This followed the work of Beadle and Tatum, who dealt with reversion of nutritional mutants. Subsequently, a number of important papers by Giles followed, including contributions on intragenic complementation, gene conversion, and analysis of gene clusters. He made particularly significant contributions to our molecular understanding of regulation of the genes of biochemical pathways in microorganisms, especially *Neurospora crassa*.

Norman was born in Atlanta, Georgia, on August 6, 1915, to Norman H. Giles Sr., a realtor, and Alice Guerard Giles, a registered nurse. Early on, he exhibited an interest in natural history, and at the age of 11, in 1926, he became a charter member of the Georgia Botanical Society. However, his major interest then was in birds, an interest fostered by his mother, who maintained a bird feeder shelf. In 1936 Norman and his birding companion Don Eyles initiated the publication of a quarterly journal devoted to ornithology in Georgia, *The Oriole*. He was a charter member of the

Georgia Ornithological Society, founded in December 1936. At the first biennial meeting of the society, in 1937, *The Oriole* became the official organ of the society. These bird watching and natural history activities would play a predominant role in his life for many years, even after his retirement.

Norman attended public schools along with his younger brother Cuthbert ("Bert"). He then attended Boys' High School in Atlanta, and upon graduation in 1934 he received a first-year scholarship to Emory University, to which he commuted by streetcar. At Emory he majored in biology, with an emphasis on genetics. It was at Emory that he met his first wife, Dorothy Evelyn Lunsford, who was also a biology major. In later years Dorothy served as one of his research assistants at Oak Ridge and then at Yale, especially in experiments with *Neurospora*.

Upon graduation from Emory with an A.B. degree in biology in 1937, Norman was awarded a Beck Foundation Fellowship. He decided to attend Harvard, entering in the fall of 1937. He married Dorothy Lunsford on August 26, 1939. Later, in 1951 and 1953, they adopted two children: Annette Guerard Giles (later Annette Brown) and David Lunsford Giles.

At Harvard Norman soon came under the influence of the world-renowned cytogeneticist, Karl Sax, the "father" of radiation cytology, and decided to complete his Ph.D. with Sax. At that time Norman was associated with an exceptional group of graduate students in the Biology Department, many of whom were later elected to membership in the National Academy of Sciences: Charles Rick, Carl Swanson, Reed Rollins, Bob Galambos, Donald Griffin, Carol Williams, and Vincent Dethier.

Following graduation Norman remained at Harvard on a Parker Fellowship for a postdoctoral year and performed, at the suggestion of Sax, the classical experiments on the induction of chromosomal rearrangements in Tradescantia microspores by fast neutrons. Norman demonstrated that chromosomal rearrangements, translocations induced in microspores of *Tradescantia paludosa* by fast neutrons, exhibited a linear relationship to dosage (one-hit aberrations). This was in marked contrast to the same rearrangements induced by X rays, which had been shown by Sax to increase as the square of the dose (two-hit aberrations). His later studies with X-ray-induced rearrangements involved analyzing the effects of oxygen in increasing aberration frequencies. One of the most striking results was his demonstration that the effect of oxygen is immediate.

In 1941 Norman accepted his first professional appointment as instructor in botany at Yale, where he remained for the next 30 years. This included a three-year leave of absence from 1947 to 1950, during which he carried out research at the Biology Division of Oak Ridge National Laboratory in Tennessee. At Yale he served as instructor in botany (1941-1945), assistant professor (1945-1946), associate professor (1946-1951), and professor of botany and then professor of biology (1951-1961). His final appointment at Yale was as the Eugene Higgins Professor of Genetics (1961-1972).

Norman spent the summer of 1941 at the Cold Spring Harbor Laboratory on Long Island. There he attended the summer symposium on "Genes and Chromosomes—Structure and Organization," which lasted for several days and provided an opportunity for him to meet a number of seminal figures in genetics, including Demerec, Rhoades, McClintock, Delbruck, Luria, Muller, Wright, and Stadler.

During the fall of his first year at Yale, Norman was very excited by the publication in the *Proceedings of the National Academy of Sciences* of a paper by Beadle and Tatum on biochemical mutants in *Neurospora crassa.* He decided to use *Neurospora* as an experimental organism in his studies of radiation-induced mutations, especially back mutations. His first paper in this field was coauthored with Ester Lederberg and appeared in 1948.

With respect to research in genetics, the 1940s were an exciting time to be in the Botany Department at Yale. Edward Tatum had just moved to Yale from Stanford and was soon joined by an exceptional graduate student, Joshua Lederberg. Tatum's continuing research on *Neurospora*, especially Lederberg's exciting experiments on mating in bacteria, eventually resulted in the award of Nobel Prizes to Beadle, Tatum, and Lederberg. In addition, there was a second active research group at Yale working with *Neurospora* led by David Bonner, which included Patricia St. Lawrence and Charles Yanofsky.

While on leave from Yale from 1947 to 1950, Norman served as principal biologist in the Biology Division at Oak Ridge National Laboratory. Under the inspiring leadership of Alexander Hollander, a great deal of innovative research was being performed in the division. Norman continued his studies on chromosome rearrangements in *Tradescantia* induced by fast neutrons and X rays and began his research with back mutations (reversions) of inositol mutants in *Neurospora*.

Norman's associates in research at Oak Ridge included Fred J. de Serres, Herbert Parkes Riley, Alvin Beatty, and Alan Conger. During this period, Norman employed one of us (M.E.C.) as a research assistant in experiments performed with *Neurospora*. When he returned to Yale in the fall of 1950, M.E.C. decided to move to New Haven and later enrolled as a graduate student working with Norman. She received her Ph.D. degree in 1957 and became a research associate with Norman, the beginning of a long and mutually fruitful collaboration.

Norman's next area of research involved studies of ultraviolet- and X-ray-induced reversions of inositol mutants of *Neurospora crassa*. He demonstrated that most reversions were the result of back mutations at the same locus, although some resulted from suppressor mutations at other loci. Different mutants altered at the inositol locus exhibited markedly different rates of reverse mutation, and these differences were inherited in a Mendelian fashion. Studies with M.E.C. on allelic recombination at the pan-2 locus demonstrated for the first time that copy-choice mechanisms could involve several different mutational sites at one locus.

In the early 1950s Norman became interested in utilizing mutations induced by UV and X rays to determine the nature of the mutations blocking various biochemical pathways in *Neurospora*. Complementation studies were used to separate the mutants into those altered in different loci.

Complementation analysis of purple adenine mutants by one of us (F.J.D.) indicated that these mutations could be separated into two very closely linked loci, *ad-3A* and *ad-3B*. These two loci were later found by others to be responsible for two sequential enzymatic steps in adenine biosynthesis. F.J.D. was Norman's first graduate student.

Studies by his graduate students Norma Nelson and Dow Woodward and his research associate C. W. H. Partridge conducted with mutants altered at the *ad-4* locus, which lack adenylosuccinase, provided the first examples of allelic complementation and the first allelic complementation maps. The mechanism of allelic (or interallelic) complementation was revealed much later by others; certain alterations in different regions or domains within the same polypeptide chain can complement each other in multimeric proteins.

Norman's next studies involved extensive genetic and biochemical analyses of what was first interpreted as a complex of five enzymes in the polyaromatic biosynthetic pathway involving the conversion of DAHP (3-deoxy-D-arabino-heptulosonate-7-phosphate) to ESSP (3-enolpyruvylshikimic acid-5-phosphate). These studies involved characterizing scores of arom mutants, including ones shown to be the result of nonsense mutations in the first gene in the cluster, resulting in the loss of the five sequential enzymatic activities. Others later showed that the arom region encodes a dimer composed of two pentafunctional polypeptide chains. In the studies of arom mutants the absence of one class of mutants in the gene-encoding biosynthetic dehydroquinase was eventually explained by the discovery of two dehydroquinases in *N. crassa*, one constitutive and one inducible . The discovery of the inducible dehydroquinase led to the detection of another complex region in the genome of *N. crassa*, the *qa* gene cluster encoding five structural and two regulatory genes involved in the utilization of quinic acid as a carbon source.

Norman's first wife, Dorothy, died in January 1967, and he married Doris Vos Weaver on August 1, 1969, in the process acquiring two stepdaughters: Gayle Weaver (who died in 1970) and Alix Weaver.

In 1972 Norman accepted an appointment as the Fuller E. Callaway Professor of Genetics at the University of Georgia. Five of his Yale colleagues accompanied him to Georgia: Wyatt Anderson, Bruce Carlton, Mary Case, Ronald Cole, Howard Rines, and Dan Vapnek. At Georgia he established a program in genetics, which in 1980 became the Department of Genetics with Wyatt Anderson as its head. Norman often told his associates at Georgia and elsewhere that Thomas Wolfe was wrong. You can go home again.

Probably the most significant contribution to genetics made by Norman and his collaborators, including his fellow faculty members Sidney Kushner and Daniel Vapnek, was the detailed analysis of the *qa* gene cluster.

These studies eventually led to the cloning and sequencing of the entire cluster of seven genes. They provided early definitive evidence for the expression in *E. coli* of eukaryotic genes. They also provided the first evidence for transformation in *N. crassa* by the expression of *Neurospora* genes carried by hybrid *E. coli* plasmids.

The *qa* cluster was shown to consist of five structural and two regulatory genes located on an 18×10^3 base pair region in a continuous array. The *qa* genes are induced by quinic acid and are coordinately controlled at the transcriptional level by the products of the positive and negative regulatory genes, *qa-1F* and *qa-1S*, respectively, which are transcribed in opposite directions. The $qa\text{-}1F^{+}$ allele encodes an activator protein required for its own transcription (autoregulation) and for synthesis of the other *qa* mRNAs, including *qa-1F* mRNA. On the other hand, $qa\text{-}1S^{+}$ encodes a repressor that blocks the activity of the activator and indirectly controls its own expression. The activity of the repressor is inhibited by the inducer quinic acid. The $qa\text{-}S^{-}$ mutants encode super-repressors insensitive to inducer inhibition, whereas $qa\text{-}1S^{C}$ mutants encode inactive repressors. The positive role of *qa-1F* in transcriptional activation is supported by the *in vitro* demonstration of activator binding to 16 base pair target sequences in front of each *qa* gene.

In additional studies the relevant DNA sequences of 23 regulatory mutants were determined from three classes of pleiotropic mutants and were interpreted in terms of functional domains within each regulator. The results are consistent with the roles ascribed to the two regulatory genes in mediating expression of the *qa* cluster. These overall results were summarized in a paper published in the *Journal of Molecular Biology* (1989) with seven of his collaborators: Robert F. Geever, Layne Huiet, James A. Baum, Brett M. Tyler, Virginia B. Patel, Barbara J. Rutledge, and Mary Case.

Norman had an unusual ability to understand the potential of many diverse organisms for genetic research. While he was at Yale, he mentored students who worked not only on *Neurospora* but also on other fungi, such as yeast and *Aspergillus* as well as an alga (*Chlamydamonas*) and moss (*Physcomitrella patens*). This diversity of systems provided a rich educational environment for his students. Many continued these studies successfully in their future careers. For example, Gerald Fink studied histidine biosynthesis in *Saccharomyces cerevisiae* and demonstrated that three enzymatic activities for that pathway are contained on a trifunctional polypeptide chain. The complex contained two identical chains. (Fink was later elected to the National Academy of Sciences).

In addition to these genetic studies, Norman, with Ernest Chu, carried out a series of cytological experiments at Yale involving primate chromosomes. They determined the chromosome number and morphology of several different primate species. In addition, they carried out experiments on the induction of chromosomal aberrations in human cells in tissue cultures.

One of Giles's graduate students at Yale was Karim Abdul El-Eryani from North Yemen. Karim had received his bachelor's and master's degrees from the University of Georgia before deciding to attend Yale for continuing graduate work in genetics. (His stay at the University of Georgia occurred long before Norman decided to move there.) Karim's research at Yale dealt with the genetic control of the biosynthesis of phenylalanine and tyrosine. Karim later became the prime minister of North Yemen.

When Norman and Doris moved to Georgia in 1972, their relocation provided an opportunity to construct a wonderful new home. They had an architect design a very modern house situated on 10 acres of deciduous woodland in a new subdivision named Hanover. Their house was placed above a rushing stream with a natural waterfall. This site enabled them to continue their interest in gardening in an untouched setting.

Norman's return to Georgia resulted in a renewal of his interest in botany. He reinitiated his work on polyploidy and geographic distribution of species in the genus *Cuthbertia,* a botanically close relative of *Tradescantia* in the southeast. His additional studies established the existence of a new hexaploid complex in *Cuthbertia.* This population occurred in central Florida and resulted from a cross of diploid *C. ornata* with tetraploid *C.* graminea followed by chromosome doubling.

While living in New England, Norman continued his membership in the Georgia Ornithological Society; his return to Georgia permitted an active renewal of bird watching in the southeast. Norman was an inveterate world traveler, having been around the world five times. He visited over 60 countries and all seven continents, including three trips to Antarctica. Many of Norman's trips involved attendance at international genetics meetings or were simply for the joy of traveling.

During Norman's stay at Yale, he took two sabbatical leaves, both supported by Guggenheim fellowships. On his first sabbatical, in 1959, he and his family spent a year in Copenhagen, Denmark, where he carried out research at the Institute of Genetics headed by Mogens Westergaard at the University of Copenhagen. His stay in Copenhagen was also supported by a Fulbright Fellowship. On his second sabbatical in 1965 Norman and his family spent eight months in Canberra, Australia, at the Australian National University in the Laboratory of Genetics headed by David Catcheside.

The following excerpt concerning Norman's travels is quoted from a letter from one of his genetics colleagues, David Perkins.

Most appraisals of Norm's impressive career concern laboratory research and academia. There is another facet that is known only to a few of us. His world travels have not been devoted entirely to bird-watching. For two decades, beginning with a visit to China, he was responsible for enriching the collection of wild *Neurospora* strains from exotic places. T. C. Sheng at Fudan University was aware, in 1979, of Norman's interest in sampling wild Neurospora populations. Before the revolution, Sheng had used *Neurospora* for his Ph.D. work at Columbia University and as a postdoc at Cal Tech. When he returned to China, first Lysenko genetics and then the cultural revolution made it impossible for him to continue. He retained his interest, however. Following the downfall of the Gang of Four, Sheng made an announcement at the first meeting of the Genetics Society of China, asking members to send him *Neurospora* from their home areas. The responses were generous, ranging from Tibet to Yarbin to Yunnan. How to get the strains out of China was problematic because of official policy about export of natural resources. It was Norm Giles who solved the problem by carrying the cultures with him when he left.

On later trips, Norm was on the lookout for *Neurospora* and was prepared to take his own samples. As a result, the Fungal Genetics Stock Center catalog now lists Neurospora strains that he picked up in 1992 from the Caribbean area and from South America, and in 1997 from Pacific islands that lie between Hawaii and Tahiti. A number of the strains are the only samples present from the geographical areas where they were found.

Norman served in many capacities and received numerous honors during his carrier. He was elected to Phi Beta Kappa and Sigma Xi in his junior year at Emory. Upon graduation he received a Beck Scholarship for further graduate study. Norman was awarded an honorary Sc.D. from Emory in 1980 and an honorary M.A. degree from Yale in 1951.

From 1954 to 1964 Norman served as a consultant to the Atomic Energy Commission. He was a member of the Genetics Study Section at the National Institutes of Health from 1960 to 1964 and a member of the Genetics Training Committee of NIH from 1966 to1970. He served on the education advisory board of the John Simon Guggenheim Memorial Foundation from 1977 to 1986. He was a member of the editorial boards of *Radiation Research* from 1953 to 1958, the *American Naturalist* from 1961 to 1964, and *Developmental Genetics* from 1979 to 1986. He served on the Board of Directors of the University of Georgia Research Foundation from 1979 to 1985. He held Fulbright and Guggenheim Fellowships from 1959 to 1960 (while on sabbatical leave from Yale). He received a second Guggenheim fellowship in 1966.

Norman received two awards from the University of Georgia: the Lamar Dodd Award in 1985 for research, and the bicentennial Silver Medallion in 1984. He received the Thomas Hunt Morgan Medal of the Genetics Society of America in 1988. He was elected a fellow of the American Academy of Arts and Sciences and a member of the National Academy of Sciences in 1966. He was chairman of the Genetics Section of the National Academy of Sciences from 1976 to 1979. His membership in the Genetics Society of America included a term as treasurer form 1954 to 1956 and as president in 1970. He also served as president of the American Society of Naturalists in 1977. He was an honorary member of the Genetics Society of Japan and a foreign member of the Royal Danish Academy of Sciences and Letters. He was also an associate of the American Ornithologists' Union for over 70 years.

When Norman retired in 1986, the Department of Genetics established an annual lectureship in his name. The department also conducted a successful fundraising campaign to endow a professorship in his name. The first recipient of the Norman and Doris Giles Professorship in Genetics was Jeffery Bennetzen, who was appointed in 2003. Bennetzen was elected to membership in the National Academy of Sciences in 2004.

Norman Giles died on October 16, 2006, in Manchester, New Hampshire. His wife, Doris, had died in August 2004. He is survived by his daughter, Annette Giles Brown of Norwich, Vermont; his son, David Giles of Jacksonville, Florida; his stepdaughter, Alix Weaver of Northridge, Massachusetts; and his grandson, Dylan Giles Brown of Philadelphia, Pennsylvania.

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Photograph courtesy University of Chicago.

Clyde A. Hutchison Jr.

CLYDE ALLEN HUTCHISON JR.

May 5, 1913–August 29, 2005

BY DONALD S. McCLURE AND JACOB BIGELEISEN

BEGINNING IN THE EARLY 1950s, Clyde Hutchison Jr. explored the chemical applications of electron spin resonance (EPR), a type of spectroscopy that had just been invented and was undergoing explosive development. He was the first person to measure the electron spin resonance in the triplet states of organic molecules, thus finally confirming the triplet nature of the phosphorescent state of these molecules. He made wise choices of problems for his laboratory to work on, resulting in many firsts and a stream of well-trained and enthusiastic students. He did this with a combination of intense dedication and delightful wit that gave him a unique role in the world of physical chemistry.

Clyde was born into the family of a Methodist minister living in Alliance, Ohio, in the northeast of the state. He was followed by three siblings: sister Frances, brother Dwight, and sister Betty Lu. Their father expected each child to play at least two musical instruments and to practice at least an hour a day. Thus Clyde became an excellent pianist and organist and could sight-read any music placed before him. He played the organ in his father's church, and in later years he played the piano for the annual chemistry department Christmas party and much later for his friends in the retirement home where he lived.

Methodist ministers were subject to the church doctrine of itinerancy, and by the time Clyde was ready for high school the family was living in Plain City, Ohio, after having moved six times. This must have been a disadvantage in school and when combined with his brilliance and small stature it brought on scrapes with the local bullies. His sister recalls that he kept his dignity when these incidents happened. When he was ready for college, the family was living in the small rural town of Cedarville, Ohio, about 20 miles east of Dayton. Amazingly there were five colleges within 10 miles of each other, including Antioch, the most famous, and Cedarville college, the nearest and cheapest. Clyde took courses in Latin, religion, music, education, philosophy, and a little math and science. He graduated from Cedarville College in 1933 in the depth of the Great Depression, having taken summer courses so as to finish in three years. He had hoped to teach science courses in a high school but the only such jobs available also required him to handle the football team. This was outside of his area of expertise: He claimed to be an athletic moron; and he was smaller than any high school football player. All his college grades were "A's" except for physical education. Failing in that career choice, he decided to apply to graduate school.

GRADUATE STUDIES

Clyde's course of studies at Cedarville College did not prepare him for graduate work in chemistry. He had had a year of general chemistry and a year of analytical chemistry and no other courses in science by the end of his third year at Cedarville. He supplemented his education through a course in organic chemistry at Cedarville at the end of his third year and by lecture and laboratory courses in physical chemistry at Ohio State University during the 1933-1934 academic year. He began the required graduate core curriculum in chemistry at Ohio State in 1934, when he was also appointed a graduate assistant—a position he held for two years. In his last year, 1936-1937, he advanced to the position of assistant in general chemistry. During his graduate days, Clyde also took advanced courses in mathematics; there is no record of his having taken formal courses in physics or in the life sciences beyond high school.

Clyde elected to do his thesis research under the supervision of Professor Herrick L. Johnston. Johnston, as a graduate student with W. F. Giauque some five years earlier, had discovered the isotopes of oxygen, ^{17}O and ^{18}O. Following the discovery by Washburn and Urey of the electrolytic separation of deuterium from protium, Johnston measured the electrolytic separation of oxygen isotopes. Johnston was a difficult mentor. Most of his students had difficulty accommodating his volatile temperament. He was very demanding of his students and expected them to spend day and night, including weekends, in the laboratory. Clyde had but one difficulty in his relations with Johnston. In the spring of 1937, his last year of graduate work, Clyde announced to Johnston that he would not be spending the forthcoming Sunday in the laboratory. His parents along with his fiancé's parents were coming to Columbus to celebrate Clyde and Jane's engagement. Johnston warned Clyde that such action would jeopardize his professional career, but Johnston eventually adjusted to the reality of the situation.

The study Johnston had assigned to Clyde was the electrolytic separation of lithium isotopes at a mercury cathode. Since there were no mass spectrometers available to Johnston and Hutchison, they determined the isotopic ratios by gravimetric methods of a quality practiced for the accurate chemical determination of atomic weights. Aliquots of the aqueous and mercury phases were quantitatively converted to lithium fluoride (LiF). The crystals were purified and then melted and slowly recrystallized. The densities of the crystals were measured by their flotation temperatures in liquid bromoform to which small amounts of *n*-pentanol and *n*-hexanol had been added. The mixture was calibrated against water as a standard. The separation factor for lithium chloride solutions in water determined by Clyde was $\alpha = 1.055 \pm 0.005$ at room temperature. No change in the separation factor was found as a function of the amount of reaction or temperature. An identical separation factor was found for ethanol solutions. At least four other values of α had been published by the time Hutchison and Johnston's value had been published (1940). Fifteen years later workers at Oak Ridge National Laboratory published definitive equilibrium separation factors in agreement with the Johnston-Hutchison value.

Johnston and Hutchison's determination of the absolute density of LiF crystals was an order of magnitude better than any reported previously. From their density determination of LiF, the density of calcite and the lattice spacings of these two crystals, Clyde calculated the atomic weights of fluorine, and calcium from the known atomic weights of carbon and oxygen. Their value of the atomic weight of fluorine, 18.994, is to be compared with values determined from gas densities of 18.995 and 18.997. Their value for the atomic weight of calcium equal to 40.0842 ± 0.0049 is to be compared with Hoenigschmidt and Kempter's value of 40.085. We cite all of these to show the care with which Clyde did all his experimental work and the thorough analysis he made of all of his data. This is a trait we will find in all of Clyde's later work.

His personal life had advanced considerably in this period. He married Sarah Jane West of Cedarville in 1937 and their first child, Clyde III, was born the next year.

POSTDOCTORAL RESEARCH WITH HAROLD C. UREY

When Clyde completed his work for the doctorate he was awarded one of the two prestigious National Research Council Fellowships in chemistry for the year. Clyde was identified as one of the most outstanding doctoral recipients in chemistry of the year and a person who showed great promise. Clyde lived up to that expectation. He chose to utilize his fellowship to work with Harold C. Urey, the world's foremost isotope chemist of the time, at Columbia University. He, along with David W. Stewart, worked on the separation of the carbon isotopes, ^{13}C from ^{12}C, by counter current exchange between cyanide ion in aqueous solution and HCN gas. Inasmuch as HCN is extremely toxic, they kept a canary in the laboratory. If the canary was singing when they looked through the window in the laboratory door when they arrived in the morning, they knew it was safe to enter. It took two years to complete this study. During Clyde's second year he was supported by funds available to Urey from private foundations. To supply the demand for samples enriched in ^{13}C the Columbia plant was sold to the Eastman Kodak Co. in 1939.

Clyde accepted a faculty appointment at the University of Buffalo for the start of the 1939-1940 academic year. Their second child, Sarah Jane, was born in Buffalo in 1941.

SAM LABORATORY

It was not long before Clyde took a leave of absence from the University of Buffalo to respond to a call from Urey to work on the Manhattan Project at Columbia University in its SAM Laboratory. SAM was the code word for the Manhattan Project at Columbia University. The principal mission of that laboratory was to carry out the research and development of the gaseous diffusion method for the separation of the fissionable isotope, ^{235}U, from the abundant isotope, ^{238}U.

The laboratory also carried out a number of researches for the development of other isotope separation processes. Clyde never worked on the gaseous diffusion process. Urey felt a person with Clyde's abilities could be best utilized on these other programs, which included the gas centrifuge, separation of the boron isotopes, and chemical separation of the uranium isotopes. Clyde spent a year at the University of Virginia working with J. W. Beams on the separation of uranium isotopes by the countercurrent centrifuge. When primary responsibility for the development of the centrifuge was assigned to Westinghouse, Clyde returned to Columbia in 1943 and worked for a year on the boron isotope separation program. Clyde headed up the program to develop methods for the preparation of ton lots of high-purity crystalline boron of natural abundance and 50 kg lots of boron enriched in ^{10}B. After he carried out thermodynamic calculations on the feasibility of reducing boron halides with various reducing agents, Clyde chose the reduction of BCl_3 with H_2.

Early in 1944 Urey decided to revisit the possibility of chemical separation of the uranium isotopes. The success of the gaseous diffusion process was not certain. The other main enrichment process, electromagnetic separation, was experiencing serious problems. An impetus for reconsidering chemical exchange came from the theoretical predictions by Jacob Bigeleisen and Maria Goeppert-Mayer that chemical separation factors of the order of 1.001 could be realized in systems that involved changes in the bonding of the uranium atom. Clyde headed up a small group to explore such possibilities. One of the contributors to this memoir (J.B.) had the pleasure of working with Clyde during this period. Here he developed an appreciation of Clyde both as a person and as a scientist. This was a friendship that lasted for the rest of Clyde's life. The other one of us (D.S.M.)

remembers the after-hours study group established by Clyde, where we studied group theory and mathematical analysis. Clyde continued these meetings for many decades, and the attendees were not only from Clyde's research group but also from various others.

THE UNIVERSITY OF CHICAGO

In the summer of 1945 Clyde accepted a faculty appointment at a new institute at the University of Chicago, the Institute for Nuclear Studies (now the Enrico Fermi Institute). He was to join with Harold Urey and Joseph Mayer from the Columbia University Chemistry Department and Maria Goeppert-Mayer and Willard F. Libby from the SAM Laboratory. Because of space shortage at the university itself, Clyde spent his first year at Chicago working at the Metallurgical Laboratory. There he worked on the Daniels high-temperature pile, which had beryllium oxide as a moderator. Clyde studied many of the properties of the moderator. In addition, he edited two volumes of the National Nuclear Energy Series, NNES, which covered part of the research done at SAM. One volume covered *Chemical Separation of the Uranium Isotopes.* The second volume coauthored with Martin Kilpatrick, Ellison H. Taylor, and Charles Judson covered *The Separation of the Boron Isotopes.* This was mopping-up work for the war project.

In the summer of 1946 Clyde moved to Jones Laboratory in the Department of Chemistry. Along with Norman Elliott he set up equipment to measure the magnetic susceptibilities of uranium compounds using the Gouy method. The goal was to decide where a new series of rare-earth-like elements began in the seventh row of the periodic table. Up to this time magnetic susceptibility measurements on uranium compounds had not given a clear verdict. The care with which they did this work, and especially the extended temperature range used, showed that the magnetic measurements were only consistent with a 5f series. This conclusion was confirmed a few years later in Clyde's laboratory by Gruen's measurements of the magnetic susceptibility of neptunium compounds, this time using the more sensitive Faraday balance.

Starting in the fall of 1946, Clyde taught the course in physical chemistry. His lectures were a model of clarity. Undergraduates who took the course and then went on to graduate studies in chemistry remarked that they learned more physical chemistry in Clyde's course than in their first year graduate programs.

Clyde and Jane's third child, Robert, was born in 1948 in Chicago.

Advances in electronics made during the war period were used by others to develop the new spectroscopies of electron and nuclear magnetic resonance. Clyde set up the equipment (with Clarence Arnow, who was in charge of construction and maintenance of the spectrometers for many decades), and began publishing in this field in 1949. EPR spectroscopy was then an exciting area of physical chemistry, and Clyde soon became one of its leaders. In fact, he was the first person to observe, in 1949, the electron resonance of an F-center, a favorite object of study by solid-state physicists. He and Ricardo Pastor made the first observations of electron resonance of alkali metals dissolved in liquid ammonia (1951). Then with Pastor, Kowalsky, and Wheland he made the first observation of hyperfine structure in the ESR spectrum of an organic free radical in solution, diphenyl picryl hydrazyl. Another first was the observation of the electron resonance of organic molecules in their triplet states. To sum up, this was a period in which he successfully explored the possible uses of electron spin resonance in chemistry.

Clyde presented the opening paper at a symposium on liquid ammonia in June 1953 on the work he and Ricardo Pastor were doing on alkali metals dissolved in liquid ammonia. The resonance line width was extremely narrow compared with the line widths observed in solids. This remarkable discovery was the subject of a paper by C. Kittel and J. Kaplan, who worked out a theory of the structure of liquid ammonia solutions based on Clyde's data. The four papers on this subject from Clyde's laboratory had an immediate and profound effect on the field.

Armed with this new dimension of spectroscopy, Clyde returned to the lanthanide and actinide elements. This was aided by a Guggenheim Fellowship, which supported a visit to Brebis Bleaney's laboratory at Oxford University in the 1955-1956 academic year. By measuring the hyperfine structure he was able to determine the nuclear spin and magnetic moment of ^{235}U, and in another publication the nuclear spin, the nuclear magnetic moment, and quadrupole moment of ^{233}U. Clyde wrote the review of progress in magnetic resonance for the 1956 *Annual Review of Physical Chemistry.*

The 1944 paper by Lewis and Kasha, which explained the phosphorescent states of organic molecules as the triplet states, had great impact on the community of physical chemists. But direct proof of the triplet nature of the emitting state was needed. The enhancement of the transition rate out of the metastable phosphorescent level by substituting heavy atoms showed that spin-orbit coupling was involved, a strong hint that a triplet to singlet transition was taking place. In a few cases the paramagnetism of the excited state could be measured. But Clyde wished to measure the electron spin resonance of a molecule in its phosphorescent state to see what the properties of this state would be. The solid glassy solutions of naphthalene at 77K, which phosphoresced strongly with a 2.6 sec lifetime, did not show a resonance signal. Previous optical work on oriented crystalline durene with a small percentage of naphthalene by one of us (D.S.M.) showed that the dissolved molecules were all oriented in the only two ways permitted by the durene structure, whereas in the glassy solid solutions the orientation was random. This system showed the first magnetic resonance of a phosphorescent molecule, and finally gave unambiguous proof of the triplet state hypothesis. The crystalline solid solution method was applied many times in later work in the Hutchison laboratory.

The experimental results on naphthalene were expressed in terms of a spin Hamiltonian, two parameters of which described an anisotropic zero-field splitting. This anisotropy explained why the experiments with random solid solutions failed: It simply led to a very broad line. The values of these parameters were calculated by others, assuming that the two electrons of the triplet state were coupled by a magnetic dipole-dipole interaction. The experimental results supported the calculated values. More detailed information about the triplet state came from an analysis of the proton-π-electron hyperfine coupling. All of these results neatly supported the recently developed theories of π-electron systems of Pariser, Parr, and Pople. (The papers on naphthalene were the Citation Classics of *Current Contents* for June 29, 1992).

There followed a series of papers on triplet ground state molecules oriented in single crystals. For example, a host crystal of diphenyl-diazo-methane when irradiated produces some diphenyl methylene (whose ground state was shown to be a triplet) oriented in the same way as the host molecules. Here Clyde introduced the new technique of electron-nuclear double resonance (ENDOR spectroscopy) in order to find the electron spin density distribution in the aromatic system. In a tour de force he used this and related compounds to follow the kinetics of chemical reactions in the crystalline state. In another experiment he used a biphenyl host crystal with very low concentrations of naphthalene and phenanthrene to study the mechanism of the transfer of phenanthrene triplet excitation to the naphthalene triplet level. These experiments were not easy to carry out, requiring several types of spectrometers, but were done meticulously and thoroughly. Super technician Clark Davoust made important contributions to the development of these instruments. In all, there were about 37 papers on organic systems in triplet states published between 1952 and 1981.

At the same time, his interests in the lanthanide and actinide compounds continued. Much work had already been done on the optical spectra by the 1970s and the theories of the energy levels of rare-earth ions in crystals were well developed. Clyde's 1971 paper on the spin resonance of Nd^{+3} in host crystals of $LaCl_3$ was the first to explore the application of this spectroscopy to excited states. The data from this work led to several useful comparisons of theory and experiment. There were a total of 25 papers in this general area between 1952 and 1989.

Between 1977 and 1996 there were 21 papers on determining the location of protons in molecules of biological interest. X-ray crystallography could hardly do this because of the relatively low density of the electrons in the vicinity of a hydrogen atom. ENDOR methods on the other hand could easily detect the nuclear hyperfine structure caused by H atoms. If a reference atom having a net electron spin were present in the molecule it could serve as the basis for ENDOR spectroscopy of the hydrogens. The test molecule chosen for many of these studies was lanthanum nicotinate dihydrate with a small proportion of the La atoms replaced by a rare-earth atom having an electron spin, such as neodymium. The hydrogens occurred in the waters of hydration and were involved in hydrogen bonds to nitrogens while the water oxygens were coordinated to the neodymium ion. The experiments were complex and the data analysis difficult, but hydrogen positions were found. Much of this work was done with Bleaney and his group at Oxford, where on his third visit, 1981-1982, he was appointed to the George Eastman Professorship. Clyde wrote a comprehensive article on the determination of the structure of molecules by way of resonance methods in 1992.

After reading a large number of Clyde's publications, one is impressed by the care taken with each one and how thoroughly he covered each field of investigation undertaken. There are no wasted moves in this body of work.

A UNIQUE PERSONALITY

Clyde Hutchison Jr. was a modest but self-confident individual. He was polite in all his interpersonal relationships. He never had a bad word to say about anyone. When he disagreed with you, he did so in a nonconfrontational manner. He respected confidences. Thus he never informed his wife, Jane, that their son, Clyde III, was a candidate for election to the National Academy of Sciences.

Clyde delighted in using precision to tease and tantalize. If someone, suggesting an appointment, said, "Could we meet at 5 o'clock tomorrow?" Clyde would answer, "Well, I'm usually asleep at that hour, but if that's what you would like, all right…(long pause)…Ooooh, perhaps you mean 1700 hours! Would that be right?" He insisted that the logical way to keep time was with a 24-hour day, so the clocks in his laboratory all read from 1 to 24, rather than from 1 to 12. His enemy was ambiguity.

He had lunch weekly with his graduate students, postdoctoral fellows, and a few colleagues. When he got the check at the end of the meal, he would calculate each person's pro rata share to within one cent. Clyde did not believe in dividing the bill equally. Each person took care of his own tip. When Clyde ate lunch at the Quadrangle Club with faculty members from the university, he regularly had a plain hamburger on a bun. There were no side accompaniments. This became known as a Hutchison. The reader of this memoir will note that there is no comma in Clyde's name. This is neither an error nor an oversight. This is the way Clyde wanted it. When someone, particularly an editor, wrote his name "Clyde A. Hutchison, Jr." Clyde would inform the individual that there was no comma and that he had the privilege of spelling his name. There was a lot of whimsy in Clyde. From time to time Clyde and Jane opened their home for a social evening, with good food, music, games, sparkling conversation, and much good humor. The Hutchison house on Harper Avenue was roomy and well appointed, with an eye for interesting art, and it featured Clyde's thriving collection of cacti. It was a special point that there was no TV anywhere. Clyde was much stimulated by his Chicago surroundings, and thrived in the company of the many other outstanding members of the faculty. He religiously attended seminars in many physical science disciplines.

Although Clyde was an outstanding lecturer in formal courses, in keeping with his own way of learning science, he considered formal courses at best a necessary evil. For many years he ran informal weekly study seminars that attracted students and faculty. He would choose a topic that he felt was important, usually new, something that physical chemists should know, and would then set about creating a series of reading and speaking assignments that made all the participants well educated in the new field.

Clyde was an excellent speaker and had more invitations—both foreign and domestic—than he could accept. Some of these lectureships were:

1959	Riley Lecturer, Notre Dame University
1963	Visiting professor, Department of Chemistry, Stanford University
1964	Venable Lecturer, University of North Carolina
1967	Physics lecturer and Royal Society Lecturer, University of Canterbury, Christchurch, New Zealand
1968	Lecturer, Royal Institute of Chemistry, London
1969	Visiting professor, State University of New York at Stony Brook
1970	Visiting lecturer, Weizmann Institute of Science, Rehovot, Israel
1971	Visiting professor, Pennsylvania State University
1973, 1982	Ehrenfest Lecturer, University of Leiden
1975	Visiting professor, Japan Society for the Promotion of Science
1978	Kistiakowsky Lecturer, Harvard University
1981-1982	George Eastman Professor, University of Oxford
1986	Visiting lecturer, Chinese Academy of Sciences

PROFESSIONAL ACTIVITIES

Clyde followed the family tradition of a dedication to public service. Both professional organizations and academic institutions called on him because of his leadership qualities, his good judgment, and the seriousness with which he took his commitments. At the University of Chicago he served as chairman of the Chemistry Department; he declined an invitation to be appointed dean of physical sciences because that position would take too much time from his research and teaching.

Clyde served a stint as editor of the *Journal of Chemical Physics*, following which he served on the Editorial Board of *Annual Reviews of Physical Chemistry*. He served on the visiting committees for chemistry at Brookhaven National Laboratory, Oak Ridge National Laboratory, Massachusetts Institute of Technology, National Bureau of Standards, and the National Science Foundation. He was a long-term consultant to both the Argonne National Laboratory and Los Alamos Laboratory. Additionally, he was a member of the Board of Directors of the Ohio State University Research Foundation and a member of the evaluation panel for the Chemistry Branch of the U.S. Air Force Office of Scientific Research. He served as chairman of both the Division of Physical Chemistry of the American Chemical Society and the Division of Chemical Physics of the American Physical Society. He also served on the Council of the latter organization.

Clyde was a member of the American Chemical Society, the American Association for the Advancement of Science, a fellow of the American Physical Society, and a member of Sigma Xi. He was elected to membership in the National Academy of Sciences in 1963 and to fellowship in the American Academy of Arts and Sciences in 1968.

ACKNOWLEDGMENTS

Clyde's three children, Clyde Hutchison III, Sarah Dunn, and Robert Hutchison, and his sister, Frances Bray, each contributed importantly to this memoir. His former students John Weil, Arthur Heiss, and Ralph Weber provided technical information and anecdotes about life in the Hutchison lab.

We are also grateful to Cedarville University (formerly Cedarville College) and the Ohio State University for information about Clyde in their records. The bulk of Clyde's papers and records are held in the archives of the Regenstein Library of the University of Chicago. We also thank Professor R. Stephen Berry for contributions about Clyde as a colleague at the University of Chicago. We also thank John Weil for his careful reading of the manuscript and many suggestions for its improvement.

HONORS

1937-1938 Postdoctoral fellow, National Research Council, Columbia University
1955-1956 J. S. Guggenheim Memorial Foundation Fellowship, Oxford University
1963 Member, National Academy of Sciences
1968 Fellow, American Academy of Arts and Sciences
1970 The Ohio State University Centennial Achievement Award
1972 Peter Debye Award in Physical Chemistry of the American Chemical Society
1972-1973 J. S. Guggenheim Memorial Foundation Fellowship, Oxford University
1981-1982 George Eastman Professor, University of Oxford

PROFESSIONAL APPOINTMENTS

1938-1939 Research associate, Columbia University
1942-1943 Manhattan District Project, University of Virginia
1943-1945 SAM Laboratories, Columbia University
1945-1946 Metallurgical Laboratory, University of Chicago

ACADEMIC APPOINTMENTS

1939-1945 Assistant professor, University of Buffalo
1945-2005 University of Chicago
1945-1948 Assistant professor, Enrico Fermi Institute
1948-1950 Assistant professor, Enrico Fermi Institute and Department of Chemistry
1950-1954 Associate professor, Enrico Fermi Institute and Department of Chemistry
1954-1963 Professor, Enrico Fermi Institute and Department of Chemistry
1963-1969 Carl William Eisendrath Professor, Enrico Fermi Institute and Department of Chemistry

1969-1983 Carl William Eisendrath Distinguished Service Professor, Enrico Fermi Institute and Department of Chemistry
1983-2005 Carl William Eisendrath Distinguished Service Professor Emeritus, Enrico Fermi Institute and Department of Chemistry

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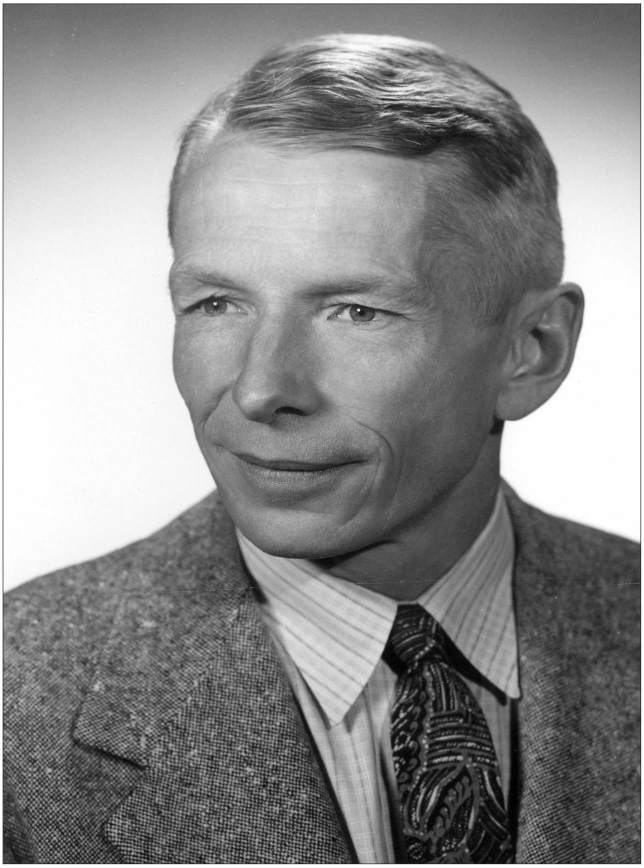

Konrad B. Krauskopf

KONRAD BATES KRAUSKOPF

November 30, 1910–May 4, 2003

BY W. G. ERNST

On May 4, 2003, Konrad B. Krauskopf died peacefully in his Stanford campus home. He was 92 years old. Konnie, as he was known to everyone, had been a member of the Stanford faculty since 1939, first as professor, then after 1976 as professor emeritus. The son of a chemistry professor at the University of Wisconsin, Konnie was born in Madison on November 30, 1910. He grew up there, and received his A.B. degree in chemistry at the University of Wisconsin in 1931. While a Wisconsin undergraduate, Konnie took a geology course from Professor William Twenhofel, that sparked his interest in the subject. However, the spark wasn't quite strong enough to cause Konnie to deviate from his path in chemistry—at least not at that time. He attended the University of California, Berkeley, for graduate study in chemistry and received his Ph.D. degree in 1934. His doctoral dissertation was entitled "Photochemical Studies: I. The Role of Oxygen as an Inhibitor for the Photosynthesis of Hydrogen Chloride. II. A Method for Deriving Reaction Mechanisms from Empirical Rate Laws for Chain Reactions. III. The Photochemical Reaction between Chlorine and Formaldehyde."

The Great Depression was still affecting the job market, and his Berkeley professors recommended him for a one-year

Modified from W. G. Ernst. *Proc. Am. Philos. Soc.* 149(2005):421-425

instructorship there, which he gladly accepted. Still in need of a permanent job in 1935, Konnie traveled down the San Francisco Peninsula to Palo Alto to discuss the possibility of an instructorship in either the Department of Chemistry or the Department of Geology at Stanford University. His meeting with the geology faculty there apparently was far more exciting than that with the chemistry faculty, so he decided to matriculate into the Ph.D. program in geology.

For his doctoral research Konnie worked with Professor Aaron C. Waters on the geology of the Okanagan Valley in northeastern Washington state. Konnie's thesis, completed in 1938, was entitled "Geology of the Northwest Quarter of the Osoyoos Quadrangle, Washington." He admitted that continuing to do bench chemistry was far less interesting to him than the fieldwork that geological studies permitted. At the same time, Konnie convinced Professor Robert E. Swain, a well-known chemist who was head of the physical sciences program at Stanford, that he would make a competent instructor of an undergraduate physical science course that combined his expertise in chemistry with his newfound, intense interest in geology. So Konnie served in this capacity while also working toward his doctorate in geology.

During this period, Konnie married Kathryn McCune, better known as Kay. His lifelong companion over 64 years, she spent many summers in the field with Konnie during his early mapping forays, as did their four children. Kay passed away in 2001 after a brief hospitalization. Their children, Karen Hyde, Frances Conley, Karl Krauskopf, and Marion Foerster vividly and fondly remember their school vacations spent in geologically interesting but remote mountainous areas. Not surprisingly—for above all, Konnie was a truly modest man—the children were almost completely unaware of their father's many intellectual accomplishments until the memorial service at Stanford on June 3, 2003.

His life was one of extraordinary achievement in both geology and geochemistry. Konnie's remarkably broad, doubly deep educational background equipped him for a scholarly career characterized by pioneering interdisciplinary scientific advances. He made numerous original contributions in all aspects of academic performance that we deem critically important—research, instruction, and public as well as professional service.

Conciseness and simplicity of expression characterized Konnie's teaching style. This resulted in crystal clarity and intense illumination of subjects generally considered especially challenging. Lectures were methodically and unhurriedly delivered; they were polished in terms of exactitude and precision—he chose his words very carefully. Konnie did not use lecture notes. In guiding the research of advanced students he allowed them considerable latitude to explore their projects but was ready to provide incisive guidance when and as appropriate. He was a teacher's teacher.

Konnie published a remarkably diverse spectrum of internationally recognized research investigations, broadly arching across the fields of hard-rock geology, petrology, aqueous geochemistry, engineering geology, and mineral deposits—bringing a new degree of quantification to all of the topics he studied. He provided seminal investigations regarding the trace-element constitution of seawater and the solubilities of silica and the manganese + iron oxides. Marine waters are strongly undersaturated in the 13 minor elements Zn, Cu, Pb, Bi, Cd, Ni, Co, Hg, Ag, Cr, Mo, W, and V, the behavior of which he analyzed and elucidated: adsorption and to a lesser extent, precipitation of sulfides were shown to control concentrations except for those of V, W, Ni, Co, and Cr, for which reduction, hydration, and organic reactions were noted in this early work. Extremely low SiO_2 dissolution kinetics were shown to account for the lack of attainment of silica equilibrium concentrations in seawater.

Employing first principles, Konnie also showed how contrasting pH-Eh-governed solubilities allow the stratigraphic separation of Mn and Fe during sedimentation. Krauskopf was one of the first to study quantitatively the geochemistry of ore-forming supercritical aqueous fluids. Economic mineral deposits are of diverse origins, but all are characterized by the marked concentration of scarce elements in the earth's crust. Many are the products of a multistage evolution, and the dissolution + aqueous transport as metal complexes followed by precipitation are governed by the laws of physical chemistry.

Detailed petrologic studies by Konnie included illuminating the origin and petrogenetic evolution of a number of Sierra Nevada and White-Inyo Range granitic plutons in eastern California (a lifelong interest), gneissic basement terranes in the Pacific Northwest, and the regional petrologic development of coastal Norway. His seamless integration combining meticulous geologic mapping, geochemical bulk-rock analyses, and radiometric dating studies of the full range of investigated rock types showed how gradational many deep-seated granitic batholiths are at midcrustal levels, as well as the difficulty (even inappropriateness) of distinguishing genetically separate plutonic bodies. The final upper crustal emplacement of granitic magma exhibits contrasting origins as well, including piecemeal stoping, shouldering aside of preexisting wall rocks, and subsolidus metasomatism.

In a very different sort of study Krauskopf illuminated the process of eruption of the brand-new volcano, Parícutin, in the Transmexican volcanic belt by demonstrating that high-standing lava in the volcano's conduit continuously spilled over into a network of fissures while large quantities of SO_2-rich gas vented directly from the crater pit.

Concurrent with these more theoretical and analytical works he generated both mineral deposit maps and general geologic maps for the California Division of Mines and for the U.S. Geological Survey (USGS), chiefly in the Sierra Nevada Range and the White-Inyo Mountains of eastern California.

Krauskopf wrote many seminal and—through their wide usage in education—highly influential books on application of the principles of physics and chemistry to the earth, having provided geoscientists with discipline-defining texts in geochemistry and physical geology over a span of five decades. Acclaimed books include *The Third Planet, Introduction to Geochemistry, Fundamentals of Physical Science, The Physical Universe, Radioactive Waste Disposal and Geology,* and *Introduction to Earth Science*. Most of these texts have run through several (up to 10) editions. These seminal works focused on and illuminated the fundamental chemical and physical foundations of the geological sciences. Special emphases have included elucidation of aqueous solution-metal complex equilibria as well as thermodynamic applications to solid-melt-fluid partitioning These pathfinding texts were published at a time when most earth scientists were mapping quadrangles. Konnie did that too, having published four USGS quadrangle maps with a combined area of approximately 975 square miles. Visitors in the field were greatly impressed with his wide-ranging geologic abilities as well as an encyclopedic knowledge of limericks—the latter enlivening campfire discussions over many years.

He was a civilian member of the military geology division of the U.S. Army during World War II. In 1947 he was appointed chief of the G-2 geographic section in Tokyo, and received a citation for meritorious civilian service. Krauskopf served for more than a decade as a member, then chair, of the National Research Council Board on Radioactive Waste

Management. His work on that board was in large part responsible for a series of outstanding, problem defining and quantifying National Academy reports on the subject.

Konnie received many honors during his long career. He was a recipient of Fulbright, Guggenheim, and National Science Foundation fellowships for research study abroad. Krauskopf was elected to membership in the National Academy of Sciences in 1959, and in the American Philosophical Society in 1967. He served as president of the American Geological Institute in 1964, received its Ian Campbell Medal in 1984, and its Legendary Geoscientist Award in 2000. In 1961 he was awarded the Arthur L. Day Medal of the Geological Society of America (GSA), and was elected GSA president in 1967. Konnie served as president of the Geochemical Society in 1970, and received its V. M. Goldschmidt Medal in 1982. These medals and awards are among the highest honors given by these professional societies. In addition, he was honored with the Mineralogical Society of America Distinguished Public Service Award in 1994.

With such an internationally acclaimed set of scholarly contributions, one might imagine that Konnie was only comfortable in the towers of academia, but he was also an accomplished field geologist. Starting in 1978 I began fieldwork in a part of the White-Inyo Range previously mapped by Konnie. In 1971 he had published a single-authored USGS quadrangle map of the Mount Barcroft area, the result of just three summers of work. Although impressed by its detailed accuracy, I fully expected to be able to improve upon it but couldn't during the course of more than 20 field seasons. Moreover, I mapped only a quarter of the complex geology covered earlier by Konnie. Few theoretical geochemists or textbook writers are that kind of field geologist.

Konnie was more than active—he was almost hyperactive—bounding up the stairs two at a time. You couldn't catch him as he briskly walked through the Stanford campus, or cut across the golf course. On occasion he and Kay would invite colleagues and spouses for a memorable Sunday picnic at their rustic cabin set in the redwoods of the midpeninsula. Somehow the suggested stroll after lunch generally involved several hours devoted to clearing brush from nearby mountain trails. One evening he and Kay arrived late to a small banquet we were attending at a local Chinese restaurant. Driving in, they had had a flat tire a few blocks away, and slowly crept along to the parking lot running on the rim. Afterward in the pitch dark Konnie doggedly refused help as he changed the tire and mounted the spare, gripping the jack handle menacingly when we offered to take over, or at least to assist him. He was determinedly independent.

In December 1999 a symposium was held at Stanford in honor of Konnie. It was attended by many of his students and professional associates. The meeting resulted in a two-part Krauskopf Volume, vol. 1, *Frontiers in Geochemistry: Global Inorganic Geochemistry*, and vol. 2, *Organic, Solution, and Ore Deposit Geochemistry*, published jointly by Bellwether Publishing and the Geological Society of America. When published in 2002, several of us were able to present him with his copy. I know that he really appreciated this symposium and his volume, but it was a small tribute for such an intellectual giant. Geochemistry has come a very long way, thanks to scientific leaders such as Konnie. I can think of no other geochemist who so conscientiously, selflessly, and effectively served the earth and environmental science profession in such far-ranging ways, being enormously impactful in all of them, as geologist, geochemist, and science-technology adviser to the nation. Konrad B. Krauskopf was a scientific icon. His purposeful stride, keen wit, sage advice, and numerous insightful scientific contributions are greatly missed.

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HERMANN JOSEPH MULLER

December 20, 1890–April 7, 1967

BY ELOF AXEL CARLSON

HERMANN JOSEPH MULLER is best known as the founder of the field of radiation genetics, for which he received the Nobel Prize in Physiology or Medicine in 1946. He was also a cofounder—with Thomas Hunt Morgan, Calvin Blackman Bridges, and Alfred Henry Sturtevant—of the American school of classical genetics, whose use of the fruit fly, *Drosophila melanogaster*, provided a remarkable series of discoveries leading to an American domination of the new field of genetics first named in 1906 by William Bateson.[1] Muller's career as a geneticist was productive and included 370 publications and participation in active laboratories in Texas (the Rice Institute and later the University of Texas at Austin), the Soviet Union (at their National Academy of Sciences in Moscow and Leningrad, Muller being a corresponding member), Edinburgh (the Institute for Animal Genetics at the University of Edinburgh), and Bloomington, Indiana (in the Zoology Department at Indiana University).[2]

Muller was a controversial critic of society who made an effort to decry abuses of genetics and who served on many national and international committees as an advocate for radiation safety. He was both a critic and advocate of eugenics, denouncing the American eugenics movement for its racism, spurious elitism, sexism, and mistaken assumptions on both the transmission of behavioral traits and the belief that many social traits were primarily innate. He promoted an idealistic eugenics throughout his life, believing that those with beneficial genes should have opportunities to transmit them. His early enthusiasm for socialism and communism cost him dearly later in life, despite his role as the leading world critic of the Lamarckian movement initiated in the Soviet Union by Trofim D. Lysenko.

FAMILY AND EDUCATION

Muller was born in New York City on December 20, 1890. His friends knew him as Herman (the last *n* being dropped) until the 1940s, when he shifted to Joe on the recommendation of his second wife, Dorothea (née Kantorowicz) Muller. Professionally, he used his initials and his articles appear as H. J. Muller. Muller was a third-generation American. His father's ancestors came to the United States from Coblenz (the Rhine Valley) in Germany after the unsuccessful revolution of 1848, which they supported. Three Muller brothers came to the United States that year. One died the following year when he tried to make his fortune in the California gold rush. The other two brothers established themselves in an art metal business in New York City on Canal Street, preparing bas reliefs, frames, and other objects for the middle class homes of that era.

The Mullers were originally Catholic, but on H. J. Muller's side they became Unitarians, the religion of upbringing of H. J. Muller. They were also sympathetic to the emerging labor and socialist movements, an influence that carried over to the young Muller. On his mother's side (the Lyons) the family was originally from England of mixed Jewish and Anglican background. This less-than-half "Jewishness" Muller used on occasion to offer his solidarity with Jewish colleagues who were victims of anti-Semitism in the United

States or Germany. Muller himself was an atheist and later in life chose the American Humanist Association as an outlet for his religious feelings, serving as president of that organization in 1957.

On his uncle's side Muller's penchant for academic life was shared by his first cousin Herbert J. Muller (English critic and author of *Uses of the Past*) and first cousin Alfred Kroeber (anthropologist known for his studies of American Indian cultures). Alfred Kroeber's daughter became the well-known science fiction writer, Ursula LeGuin. Muller had two children. With his first wife, Jessie (née Jacobs) Muller, he had a son, David Muller, who became a professor of mathematics. With his second wife (she called herself Thea) he had a daughter, Helen Muller, who became a professor of marketing and public health. David Muller has continued the academic tradition with a son, Kenneth Muller, a professor of neurobiology. Helen has also continued the tradition with a daughter, Mala Htun, who is a professor of anthropology.[3]

The young Muller attended Morris High School in the Bronx while commuting from upper Yorkville in Manhattan. He excelled in school and received a Cooper-Hewitt scholarship to attend Columbia University. His father died when young Muller was 10 years old, and the family lived on a modest income from the partnership with his father's brother. During his college years, young Muller had to work odd jobs part-time to help support his mother and sister. At Columbia, Muller received his B.A. in 1911 and Ph.D. in 1916.

He knew he wanted to be a scientist and had considered engineering and basic science as possibilities when he entered Columbia. He quickly chose the life sciences after taking courses with Edmund Beecher Wilson. Morgan at the time had not yet established himself as a geneticist and Muller's only course with Morgan did not mention his work on fruit flies.[4] Muller felt intellectually excited by Wilson's ideas on the cell and the chromosomes. He felt Morgan was muddled in his thinking on evolution and genetics. This surprising evaluation makes more sense to those who know that at the time when Muller was an undergraduate, Morgan was strongly influenced by the ideas and work of Hugo DeVries, one of Mendel's rediscoverers and the proponent of the mutation theory.[5] This theory believed new species arose *de novo* rather than by a gradual Darwinian change over hundreds or thousands of generations. Muller was a committed Darwinian and strongly supported natural selection as the basic mechanism of evolution, a view under academic attack in the early years of the 20th century.

While Muller was completing his bachelor's degree, he met Bridges and Sturtevant, who had a very different experience with Morgan. Bridges, an orphan, was benefited by a part-time job as a bottle washer and food preparer for an organism Morgan had been studying for two or three years on recommendation of William Ernest Castle at Harvard. Morgan was hoping to find new species of fruit flies just as DeVries found new species in the evening primrose, *Oenothera lamarckiana.* Sturtevant, who lived with his brother's family (he was a professor of linguistics at Columbia), impressed Morgan with his brilliance in class and with his initiative to write a paper on coat color inheritance in horses. Both Bridges and Sturtevant discussed Morgan's recent finding of a mutation (white eyes) and its unusual mode of inheritance. Muller, who shared an enthusiasm for biology through the university's biology club, was eager to join Morgan's laboratory after his graduation (he was working on a master's degree in nerve physiology at Cornell). Despite Muller's personality, Morgan took him on. Muller had a reputation for his own brilliance, especially in coming up with powerful interpretations of the new findings from Morgan's laboratory.

While a sibling rivalry simmered among the three graduate students, and Muller often was shunted to another room (to work with his lifelong friend and fellow high school alumnus, Edgar Altenburg, who was not accepted into Morgan's laboratory), these budding geneticists engaged in numerous debates and discussions of all their experimental work.[6] This makes the source of the ideas for their experiments and interpretations of the experiments difficult, if not impossible, to separate. It led to many disputes over priorities in their later careers, and a mutual hurt, each thinking the other greedy or unkind. Muller lamented that so much of his time that should have gone into carrying out the experiments of his ideas had to wait or was taken over by his rivals, because he was not supported by Morgan and he had to teach English to immigrants or work as a runner in Wall Street to earn money for his own and his mother's needs. Sturtevant did not hesitate to lament that Muller had a "priority complex" and did not credit others for their own insights. Morgan sided with Sturtevant and Bridges; he took the view that ideas were cheap and commonplace and of little importance without the experiments to test them. It was Morgan who chose his own title, professor of experimental zoology. Morgan never felt embarrassed with his wrong ideas (he had many) because they fell by the wayside when he put them to test.

Although Muller finished his dissertation work in 1915, his degree was not awarded until 1916. His dissertation was on crossing over, a phenomenon first discovered in England and misinterpreted by Bateson as coupling and repulsion.[7] Bateson thought of Mendelian units almost like bipolar magnets that repelled or attracted each other. A different model impressed Morgan, one that stemmed from his reading of a paper by F. A. Janssens on meiosis, which showed chromosomes twisted around each other. Morgan speculated that these twisted threads could break and reunite, separating or bringing together segments of a paternal and maternal homologue. He called the process "crossing over."

Sturtevant used the data from Morgan's first X-linked mutations and constructed a map. Muller was in awe of Sturtevant's interpretation. (Sturtevant was still an undergraduate when he created the first map). Muller suggested using the ratio of crossovers to the total of crossovers and noncrossovers for determining the map unit. (Sturtevant had used the ratio of crossovers to noncrossovers). Muller then followed up the mapping of the mutations and determined that genes kept in heterozygous state for several generations did not lose their specificity. He also worked out a mechanism and interpretation, measured by what he called the coincidence and interference of crossing over for inconsistencies in map length. This resolved the observation that certain linked genes (those relatively close to one another) had a predictable distance when their individual internal distances were added, but those relatively far apart fell short of that predicted distance. In Muller's interpretation, genes close together rarely had intervening double crossovers and those farther apart usually did. This made the longer distances shorter than the sum of the distances of contiguous segments within that stretch between the two outer genes. Muller believed the first break led to a release of tension nearby and this prevented multiple crossovers in that region.

GENETICS AT RICE UNIVERSITY

Muller decided to leave Morgan's laboratory after a visit by Julian Huxley, who had been appointed as the founding chair of the Biology Department at the new Rice Institute. Huxley was impressed by the work of the Morgan school, and he asked Morgan to recommend a student; he recommended Muller for the job. Muller established a lifelong friendship with Huxley in those few years they had together at Houston. It made each a strong supporter of the genetic mechanism involved in the Darwinian natural selection. By 1917, however, Huxley felt compelled to return to England to enlist in the war. He left Muller in charge and Muller hired Edgar Altenburg to share the teaching while they carried out research on mutation rates and the mutation process. Muller also continued with Altenburg a long-term project started in 1912 that was not published until 1920: a study of what Muller called the gene-character problem.

On theoretical grounds Muller had argued that Darwinian variation has a genetic basis and there must be subtle modifier genes involved in the different expressions of a genetic trait. Morgan had found two such mutants, one called Truncate and the other called Beaded wings. In both cases the shape of the wing varied and neither stock could be made homozygous. They kept throwing off normal winged flies. Morgan had turned these over to Muller to play with and as the years went by, Muller accumulated the evidence that these were complex hereditary systems. In Beaded wings the dominant chief gene (Beaded) was made perpetually heterozygous with rare or no normal-winged flies. The stability of the heterozygote arose from Muller's determination that Beaded, while dominant for its visible effect, was recessive for a lethal trait. In the homozygous state it killed the embryo. In one line of Beaded a second recessive lethal arose, but it arose in a homologous chromosome, rendering the unrelated lethal perpetually heterozygous. This new lethal was stabilized by a repressor of crossing over (then called a C factor and later recognized as a chromosomal rearrangement, an inversion) (1918).

The analysis of Truncate wings (jointly done with Altenburg) was even more complex, with isolated and mapped modifier genes, which Muller called intensifiers and diminishers, affecting the expression of the dominant chief gene (1920,1). (Like Beaded, Truncate was a recessive lethal and dominant visible mutation.) There were also environmental modifiers, especially temperature: the mutant expression enhanced at higher temperatures and the normal phenotype at the lower temperature. Muller considered these experiments of supreme importance for the emerging neo-Darwinism, which attempted to bring classical genetics into Darwinian natural selection. In nature, he argued, genes evolve through systems of modifier genes that eventually become homozygous and stabilize the new trait. He also used his analysis to discredit DeVries's mutation theory. Muller argued that the new species DeVries obtained in *Oenothera* were actually complexes of chromosomal rearrangements that underwent occasional crossing over, chromosome doublings, or losses of chromosomes, leading to the expression of many changes in the plant's phenotype and rendering it incapable of breeding with the original type. *Oenothera,* he argued, was an aberrant mechanism of evolution and not in the Darwinian mainstream.

Muller's views, initially formulated in the debates with students in the biology club at Columbia, were initially based on theoretical considerations, and he clashed in print with Castle over Castle's interpretation of hooded rats and other variable traits in small mammals (1914). Castle argued that the genes themselves varied through contamination in the heterozygous state, a claim Muller challenged in his own dissertation work and that he could now demolish with the clear evidence of modifier genes and a reductionist explanation of DeVries's own competitive model of evolution. For Muller the gene was stable until it was itself mutated and that mutation rate, as he and Altenburg demonstrated, was relatively rare.

Muller left Rice to serve as an interim professor at Columbia while Morgan was away on sabbatical leave. Muller hoped to join the faculty there but Wilson felt it would not work out if Morgan came back, which he did for a few years before leaving to head and develop the new Department of Biology at California Institute of Technology. While at Columbia (1919-1921), Muller published several theoretical papers that charted a future course for his research (1920,2; 1921,1,2; 1926). He argued that mutation should be limited to changes in the individual gene and that they should not be lumped together with other hereditary changes such as nondisjunction, polyploidy, and chromosome rearrangements. He also believed genes should be analyzed through their mutations. He considered the gene as having a unique property in replicating its variations and that something basic was present in the gene, which made it unique to all life forms. He also recognized a similarity between genes and viruses, comparing viruses with "naked genes." He believed the gene would someday be accessible to chemical and physical analysis.

GENETICS AT THE UNIVERSITY OF TEXAS

Muller returned to Texas but not to Rice. Instead he chose a position at the University of Texas at Austin. He had a powerful influence on his colleagues, especially John Thomas Patterson and Theophilus S. Painter. Patterson was studying armadillo embryology and Painter was working on the cytology of spiders when Muller joined the faculty. After Muller showed the versatility of *Drosophila* as a tool for genetic analysis, both Patterson and Painter switched their organism of choice and became major contributors to *Drosophila* genetics. This was both a benefit and a difficulty. It increased the stimulation of discussions and approaches to work in classical genetics, but it also led to a renewed rivalry, with Muller feeling that much of his time and ideas were entering the work of his colleagues and his own work was suffering from neglect. He tried to solve this by working at night, not a very good idea for a married man, and soon his marriage was foundering and, of course, he had alienated Patterson and Painter.

Muller alienated his colleagues as well as the university in other ways. He became an *ex officio* adviser of the National Student League, named by the FBI as a communist student organization, and he became an underground editor of *The Spark*, a newspaper promoting socialist goals, including civil rights for African Americans, equality of opportunity for education and careers for women, unemployment insurance for the unemployed, social security for the retired, and other progressive legislation championed especially by the Communist Party, for which Muller had strong sympathies although he never joined.

It is remarkable that as Muller's personal life became more complicated with marriage to Jessie (who was fired from the faculty in Mathematics when she became pregnant) and conflicts emerged with his colleagues, he became more intensely involved in his major discovery. When he came to Texas he was hoping eventually to induce mutations. He had tried, unsuccessfully, a number of chemical approaches based on the finding that temperature increased the mutation rate in a way consistent with the Q10 of chemical reactions. In 1926 he reexamined the use of radiation. Morgan, Blakeslee, and Payne had tried radiation without success about 1910. Muller realized that a subjective search for mutations was not reliable. (Although Lewis J. Stadler used that method successfully with maize, his papers appearing several months after Muller's *Science* paper appeared [1927,1]). Instead Muller designed tools to isolate the most commonly occurring mutations, recessive lethals (first discovered by Morgan).

One of Muller's great contributions to genetics was stock design. He used complex rearrangements and combinations of recessive and dominant visible markers to identify the passage of chromosomes from parent to offspring. One such stock, called *ClB,* consisted of a recessive lethal, a crossover suppressor, and the dominant visible mutation called Bar eyes, all on the X chromosome. By irradiating normal or wild-type male flies and having the X chromosomes of their sperm individually rendered heterozygous with the *ClB* chromosome, Muller could test for the presence of an induced recessive lethal mutation by looking for the absence of that category among the progeny (the grandsons of the irradiated male). This gave Muller a quantitative measure of induced mutations, and he was surprised and elated by an abundance of induced mutations that were 150 times more plentiful than spontaneously arising mutations. He not only obtained the lethal mutations he expected but also visible mutations that were both new and allelic to spontaneous forms previously picked up over the prior 15 years in laboratories around the world.

Muller published his results (without data) in *Science* (1927,1) to establish his priority and that same year presented the data in great detail at the International Congress of Genetics in Berlin (1927,2). The publicity for Muller's report of artificially induced mutations was worldwide and Muller returned to the United States with international stature. The Berlin paper mapped the lethals and visible mutations, eliminated competitive models of genes as bean bags of particles, and revealed that a portion of the first generation of mutations was fractional or mosaic (a condition associated with the DNA double helix model and not successfully interpreted until the 1950s).

PERSONAL SUCCESS AND FAILURE AT TEXAS

Muller's troubles at Texas intensified, as he and Jessie considered separation and Patterson and Muller were no longer on speaking terms. Muller had also intensified his left-wing behavior by bringing two Soviet students to his laboratory, Solomon Levit and Isador Agol, on Rockefeller scholarships. Levit collaborated with Muller on human genetics, a field that Muller felt needed some basic science to improve its study of human traits. Muller had published a paper (1925) on identical twins raised apart arguing that very little was known of the genes involved in human behavioral traits and how they interacted with the environmental factors. He did believe such analysis, like his earlier work with Beaded and Truncate, was eventually feasible, and identical twins was one way to start.

In 1932 Muller's personal life began to collapse over his marriage, his discontent with Texas, the investigations of the FBI, veiled references to him in the local newspapers as a communist subversive on campus, and claims Stadler and others were making that X rays did not induce gene mutations (as changes in the individual gene) but instead induced chromosome rearrangements of various kinds and sizes. Muller disappeared from his laboratory and did not show up to class, and after his wife called anxious about his whereabouts, a search posse of faculty and graduate students went looking for him in the woods near the outskirts of Austin. He was found walking, muddied, and wrinkled by an overnight rain, and somewhat confused. He had slept off an overdose of barbiturates in a suicide attempt but returned to his class the next day as if nothing had changed.

The suicide attempt occurred just before he left to make presentations at the Third International Congress of Eugenics at the American Museum of Natural History in New York City and at the Sixth International Congress of Genetics in Ithaca at Cornell University. To the amazement of many who knew him, both were major papers that had significant impact on those who heard them. At the eugenics congress Muller presented "The Dominance of Economics over Eugenics" (1932,1). Although C. B. Davenport had attempted to block presentation of the paper, Muller prevailed in his denunciation of the American eugenics movement.

Muller argued that only in a socialist country, where all children, male and female, white and black, had equal opportunities for education, housing, and other social services, would there be an opportunity for a successful eugenics program. American eugenics, he argued, was based on the false premises that social traits such as pauperism, vagrancy, feeblemindedness, and recidivist crime were largely innate traits. Muller argued that this was unproven and probably false. It was a shock to readers of the *New York Times* (and newspapers around the world) to hear an appeal for the end to racial discrimination, to class-based claims of inferiority, and to the oppression of women. Muller's phrasing was Marxist and his sympathies with the Soviet Union as the only country where that potential existed (so he believed) was considered both outrageous and reckless for a professor from Texas.

At the Ithaca congress Muller presented a lengthy paper (1932,2), "Further Studies on the Nature of Gene Mutation," on what might be considered a capstone of classical genetics. Muller presented a theory of gene function in which he introduced the terms "hypomorph" (less than normal activity), "amorph" (no activity at all of normal function), and "neomorph" (brand-new traits that have no counterpart in their normal allelic source). Alleles like apricot or eosin in the white-eyed series were hypomorphs; white itself was an amorph; and the Bar mutation was a neomorph. Muller demonstrated these functions using deleted X chromosomes carrying extra doses of the gene being studied.

Muller also introduced a second discovery. He used fragments of X chromosomes to identify a special category of modifier genes that he called dosage compensators to explain a phenomenon he called dosage compensation in which most genes on the X produce the same outcome quantitatively and qualitatively for gene action whether present in two doses in the homozygous female or one dose in the hemizygous male. Many of those who heard Muller's presentations were stunned by his originality, his forcefulness in presenting his views, and the importance of what he presented. Others, aware of the rumors surrounding his mental collapse and suicide attempt, found the presentations so incomprehensible and incoherent that they could not take in the importance of what he presented.

Muller returned to pack up and leave for Berlin. He had been awarded a Guggenheim Fellowship to study at the Institute for Brain Research, part of the Kaiser Wilhelm institutes. There he collaborated with N. V. Timofeef-Ressovsky in studies on target theory and the expression of partial dominance by recessive lethals. He arrived in 1932 but the following year Adolf Hitler was elected chancellor and the institute was vandalized by Nazis, who looked with suspicion on the communist leanings of that unit. Muller left to accept an invitation from N. I. Vavilov to come to the Soviet Union and establish a genetics laboratory in Leningrad. At the time, Vavilov's position was similar to that of the secretary of agriculture in an American president's cabinet.

Muller's five years in the Soviet Union (1932-1936) were transforming. He had the best support for his research as corresponding member of the U.S.S.R. Academy of Sciences. He was also free of teaching duties. He built a laboratory first at Leningrad and then at Moscow, where he recruited several graduate students and research associates. The projects he initiated focused on gene function explored through position effect; gene evolution studied through the Bar case; and gene structure analyzed through what he called the left-right test.

In the first of these he noted that certain genes, like scute 19, could be shifted to another chromosome and still retain the original function. Other genes in the region of the tip of the X chromosome, when juxtaposed against heterochromatin showed classical position effect variegation or loss of function (1935). The genes themselves, as his students showed, could be isolated by crossovers and restored to normal function. In the Bar case Muller made use of Painter's discovery of salivary gland chromosomes (a discovery Muller considered so important that he nominated Painter for election to the National Academy of Sciences despite his personality clashes with him back at Texas (1936,1)).

With Alexandra Prokofyeva as his cytologist in the second project Muller showed that the Bar mutation was actually a duplication and he interpreted this as a primary unequal crossover. Once established, Bar tended to revert to normal or produce a triplication, called ultraBar. Muller called this secondary unequal crossing over. The first event Muller associated with extension of individual or small numbers of genes into chromosomes and genomes in the evolution of life from the first gene, and he modified the cell doctrine with what could be called a gene doctrine, which asserts that all genes arise from preexisting genes. Muller's insight into gene evolution was amply confirmed by the nests of duplicated genes associated with the human hemoglobin A and hemoglobin B genes, each a consequence of extensions by unequal crossing over.

In the third of these projects Muller used independent inversions with breaks, one near the scute region, and the other toward the centromere heterochromatin. These heterozygous inversions provided opportunities to combine the fragments of the yellow-achaete-scute region near the distal tip of the long arm of X chromosome. Muller's analysis revealed discrete breakage regions between genes, as if there were some inert or nonfunctional material between individual genes (1940).

Muller had ambitious plans for analysis of the gene through radiation-induced mutations and cytological studies. He also was consultant with Levit for the first medical genetics research laboratory. This was constructed in Moscow and included dozens of identical twins that were studied for their physical traits, susceptibility to tuberculosis and other diseases, behavioral responses to mechanical tasks, and similar studies that attempted to sort genetic from environmental factors. The institute published a journal of human genetics (four issues were produced). Muller looked on this pioneering effort as a prelude for his own eugenic ideals. He went ahead with the publication of a book he had started in 1919, which he called *Out of the Night* (1936,2). He asked Levit's advice on how a eugenics program could be launched in the Soviet Union, and Levit, a party member, advised him to go to the top.

Muller had the book translated and presented to Premier Stalin with a lengthy letter advocating his utopian dream of positive eugenics in a classless society. It was the wrong time and the wrong idea. At the same time as Muller was hoping to expand his genetic and eugenic programs, a countermovement was underway in Soviet science. Trofim D. Lysenko in Odessa was offering a different view of heredity based on his plant-breeding experiments. He felt that the heredity of a species was malleable if it was shattered by a provocative environment and retrained in the desired direction. Lysenko based his theory on the work of I. V. Michurin, a Russian Luther Burbank, who like Burbank believed Lamarckian transformations by the environment were assimilated by the plants he studied. Michurin's work was primarily in fruit trees and based on grafting experiments. Lysenko's work was primarily based on changes in cereal crops, with claims that cold shocks (vernalization) could transform winter into spring wheat or even wheat into rye or oats. Lysenko and his supporters extended their theories to all of heredity, and looked upon western genetics or Mendelism-Weismannism-Morganism as an imported bourgeois capitalist, pseudoscientific system intended to check progress, support racism, and promote fascism.

A bitter debate emerged, with growing support for the Lysenkoists, who successfully lobbied to prevent the 1937 International Congress of Genetics from being held in Moscow. Muller was drawn into the controversy, complicated by the 1936 purge that Stalin had begun through assassinations, arrests, staged trials, and imprisonments of those he considered untrustworthy. Both Agol and Levit were arrested, charged with being Trotskyites, and executed. Muller debated Lysenko in Moscow in December 1936, accusing him of practicing the equivalent of shamanism instead of science and called him a fraud. Muller was shouted down in the uproar at this mass meeting of 3,000 geneticists and collective farmers, about equally divided in their support for genetics or Lysenkoism. Muller realized there was little hope for continued research in the Soviet Union, and Vavilov advised him to find a safe way out. Muller chose to enlist as a volunteer in the Spanish Civil War and he joined the International Brigade, serving with the Canadian physician Norman Bethune doing physiological research on blood transfusion.

THE EDINBURGH YEARS

Muller stayed in Spain through the siege of Madrid and when the cause of the Republican Army seemed on the verge of defeat, he tried to find a place to go. He could not return to the Soviet Union, where he would be subject to intimidation, arrest, or execution. He could not return to Austin because he received notice that he would first have to stand trial in the faculty senate for violating a university policy as an editor of an unauthorized newspaper (university policy required signed editorials and columns on student publications). Muller hoped to find work in Paris with Joliot Curie or in Stockholm with Gunnar Lundberg, but they had no openings. Huxley heard of his difficulties and contacted F. A. E. Crew, the director of the Institute for Animal Genetics at the University of Edinburgh.

Crew arranged for Muller to be a guest investigator and Muller found himself once more with an opportunity to develop a graduate program. He arrived in 1937 and he quickly began research with some new problems to examine. He looked at the relation of radiation dose to mutation frequency and with S. P. Ray-Chaudhuri demonstrated that the same amount of mutation is produced by a given dose whether that dose is administered over a month (a protracted dose) or over 30 minutes (an acute dose) (1939). This led Muller to argue that even diagnostic doses of radiation were of concern for radiation protection and he advised practitioners of the danger possible in his annual report to the granting agency that supported his research. Physicians objected that Muller's views were injurious to patient confidence and inappropriate because the work was done on fruit flies and not human patients. It was the beginning of a skirmish on radiation safety that would persist for the rest of Muller's life.

Muller had two additional students whose collaboration proved fruitful. With Guido Pontecorvo, Muller worked out ways to use triploid *D. melanogaster* females and heavily irradiated *D. simulans* males to construct diploid surviving embryos that carried all *D. melanogaster* chromosomes except for a fourth chromosome from *D. simulans*. The analysis of these flies allowed Muller and Pontecorvo to argue that interspecific hybrids that fail to survive owe their failure to assignable genetic differences rather than to some vague mixing of incompatible cytoplasm. Muller and Pontecorvo also used irradiation and triploid flies to identify the mechanism of dominant lethals. These were aborted embryos produced by radiation exposure of sperm (1942), and Muller and Pontecorvo showed (independently of Barbara McClintock's work on maize) that dicentric chromosome formation (what McClintock called the breakage-fusion-bridge cycle) was the source of cell death leading to the aborted embryos.

The second student, Charlotte Auerbach, like Pontecorvo was a refugee from fascism. Crew assigned her to Muller. Muller suggested to her that a productive way to study the gene was through mutation and he recommended looking at chemical mutagenesis. Auerbach used pharmacologist Robson's suggestion to use mustard gas and the first demonstration of a potent chemical mutagen was successfully published (but had to wait until the end of the war because of secrecy laws imposed on agents that were used or could be used for warfare). Also at Edinburgh, Muller met and married his second wife, Thea (Dorothea Kantorowicz). Muller had the frustrating duty of being a leading planner of the aborted Congress of Genetics in Moscow and the transferred congress that met in Edinburgh on the eve of World War II. The outbreak of war put an end to basic research in Great Britain, as a fight for survival dominated all other issues. Muller was advised to move back to the United States.

The best Muller could salvage was an interim position at Amherst College. He did not have the financial support for research, and it was difficult to find assistants willing to work in jobs that were unrelated to the war effort. It was also a time for happiness and rediscovering family life with the birth of his second child, Helen. Muller's major activities at

Amherst were writing review articles. He also worked as a consultant on radiation genetics projects for the then-secret Manhattan Project, but those could not be published. It also meant a return to teaching but Muller's heart wasn't in teaching undergraduates. As the war came to an end Muller knew he would not be added to the faculty. He wrote in desperation to friends. McClintock said a letter to her was so alarming in his despair about continuing in academic life that she burned it. Fortunately, Indiana University heard of Muller's difficulties. Fernandus Payne, who admired Muller's work, sent Tracy Sonneborn to a meeting to explore Muller's interest in joining the staff. Muller was delighted, and in 1945 he moved his family to Bloomington.

THE INDIANA YEARS

Muller spent his happiest years in Bloomington. He felt warmly appreciated by his colleagues. He was generously supported by the Rockefeller Foundation and by Indiana University with grants to begin another graduate program. He taught at the graduate level (three courses a year), and he felt vigorous at the age of 55. In 1946 he was awarded the Nobel Prize, and that had a transforming effect on his position in the university and in national life. It was the third nomination for Muller. The rule of three prevented Muller (as well as Sturtevant and Bridges) from receiving the Nobel with Morgan in 1933. Lancelot Hogben was asked to write a nomination for Muller in 1939, but war broke out and Muller's candidacy was deferred. The bombings of Hiroshima and Nagasaki had changed the relation of science to society. An Atomic Age required public debate and Muller's prize was seen as a message to him and to science to steer society through potential abuse or calamity.

While Muller wore the mantle of elder statesman for science, he was also committed to his graduate program.

He studied a variety of projects in radiation genetics using neutrons and other particles, often in collaboration with facilities at Brookhaven National Laboratory. He also looked at new problems in human genetics. He shifted from twin studies as a tool to understanding and reexamined the survival of genes in populations. He made use of his Soviet-period research on the partial dominance of recessive lethals (work done with Kerkis) and extended it to population genetics, using a modification of equations first used by Danforth. Muller presented a new concept that he called genetic load (1950). He believed that spontaneous mutations accumulated in populations and reached an equilibrium with the amount of newly arising mutations matching those eliminated through their partial dominance. In human populations, he argued, the mutational load increases each generation because the pressure of natural selection is relaxed in an unnatural environment.

Muller and his students studied spontaneous mutation rates and used protracted and acute doses under varied physiological conditions (nitrogen- or oxygen-rich atmospheres) that might diminish or enhance chromosome breakage or gene mutation. He refined the tools for genetics and launched numerous stocks to improve the detection of lethal mutations (recessive and autosomal), sterility mutations, and visible mutations.

Throughout those years he was also embroiled in Cold War conflicts. He spoke out against radiation abuses. He was distrusted by those who misinterpreted Muller's concerns about radiation hazards in medicine, industry, and weapons testing as attacks on national defense and the survival of the West against Stalinist imperialism. He went public on the Lysenko affair after 1948 with an attack on Soviet genetics. At the International Genetics Congress in Stockholm the eastern bloc delegates walked out when Muller started listing the crimes against science he had witnessed in the Soviet Union. Muller was called to testify before the House Un-American Activities Committee. (That testimony is still immune from access). It was an era of fear. He and his wife burned thousands of items they had accumulated in his travels, including correspondence with known communists or communist sympathizers. He hoped to protect his students and colleagues, who like him erred in thinking that the Soviet experiment was democratic and just and free of prejudice.

Muller tried to separate the politics of the Cold War from the very real issues that he felt had to be addressed. What should the maximum exposure doses be for a lifetime of medical diagnosis? How should standards be set for maximum permissible doses into the environment or in the workplace for the nuclear power plants industries were planning? When should scientists support efforts to ban atmospheric and oceanic testing of nuclear weapons? Muller's views were complex and often misconstrued. He wanted both atomic and hydrogen bombs to be developed. He felt nuclear disarmament was not possible unless it was by mutual agreement in treaties with guaranteed scientific inspection to prevent cheating. He argued that fallout doses (except for the largest of the hydrogen bombs used) were too small to be a public health threat. He argued that diagnostic doses were individually low risk, but when given to hundreds of millions of people, did induce a predictable number of leukemias, solid cancers, and mutations. He argued that the Atomic Bomb Casualty Commission in Hiroshima and Nagasaki would find few mutations in the children of the exposed population because most mutations are recessive and they would not show up for many generations to come. Muller's views were often rejected by those who feared any dose of radiation however small and by those who dismissed low doses as harmless or even beneficial to the public (because they allegedly created a hybrid vigor in the offspring).

In 1957 Muller revived his eugenic ideals (1959, 1961). He felt the abuses of Nazi eugenics and the American eugenics movement were historical accidents not likely to be repeated in democratic societies. He urged in such societies the adoption of his idea, germinal choice, which should give to the user the decision making on what sperm or eggs to use for producing children. He hoped people would learn to separate sexual activity from the quality of children they desired, just as they had learned to separate sexual activity from reproduction by the adoption of artificial means of birth control. In addition to family planning based on thoughtful desires for children, he recommended a genetic enlightenment that would be most likely to produce healthier, wiser, and more caring offspring. He was criticized in editorials as being ignorant of the Holocaust and the excesses humans are cable of applying against humanity. Many thought he was trying to revive the old-line eugenics he had condemned. Muller realized as his health began to fail that no eugenics was better than bad eugenics, and he refused to endorse a planned sperm bank in California for germinal choice that was based on the values of old-line eugenics.

MULLER'S LEGACY

Muller led a flawed life. His political involvement in the uses of genetic knowledge made him vulnerable to controversy and negative assessment. It is difficult to speak out on important issues without experiencing rejection or being misconstrued. He told his students that it was their duty to bear witness and to speak out against the abuses of science in their generation. Most scientists have difficulty playing the role of a gadfly. They enjoy doing their science and not worrying about the way their findings will be used. Muller was not alone in taking public stands. Julian Huxley, J. B. S. Haldane, Linus Pauling, Joshua Lederberg, and James Watson are among the many scientists of stature who have spoken out against public policies that seemed injurious to the public. He argued that genetics was the most subversive science because it dealt with issues fundamental to human nature. A geneticist cannot expect to be ignored by those who reject natural selection and evolution. Geneticists are bound to encounter public controversies as their findings are applied to human reproduction. At worst, the government, as in the Soviet Union or in Nazi Germany, may endorse a spurious science to counter the findings developed by geneticists. Muller's views on eugenics are complex. In the long run he may turn out to be prophetic and genetic-load concerns in distant generations may lead to germinal-choice reproductive options to reduce that load. Muller served humanity well in promoting radiation safety and helping to curb the most egregious abuses of radiation in industry and health.

Muller's roles in contributing to classical genetics, in founding the field of radiation genetics, and in relating genetics to evolution are solid contributions that will endure. His influence on the careers of many of his colleagues and those who took his courses was profound. He had some successful students, including Bentley Glass, who was elected to the National Academy of Sciences. Both Pontecorvo and Auerbach became fellows of the Royal Society. Many of his students entered academic life and had productive careers. Many of his students in the Soviet Union were not so fortunate, and they spent years isolated from publishing, forced out of genetics, imprisoned, or executed. It is a tribute to Fernandus Payne that he recruited Muller. Payne dismissed the claims of psychosis, communism, and a personality likely to be disruptive to colleagues. Muller had his difficult moments at Indiana but more often than not he brought glory to the university; he respected his colleagues (refusing to teach less than they); and he deeply appreciated the gift of tranquility bestowed on him. Those students who worked with him will appreciate his kindness in encouraging their careers, helping them financially through hard times, and fighting passionately with them on every sentence they wrote in their articles with the conviction that what they did mattered and deserved the tough evaluation of his considerable knowledge.

HONORS AND DISTINCTIONS

In addition to his Ph.D. in 1916 at Columbia University, Muller was the recipient of five honorary degrees: D. Sc., University of Edinburgh (1940); D.Sc., Columbia University (1949); D.Sc., University of Chicago (1959); M.D., Jefferson Medical College (1963); and Ph.D., Swarthmore College (1964). He received numerous prizes and recognitions of his stature in his career: the Cleveland Research Prize, American Association for the Advancement of Science (1967); Nobel Prize in Physiology or Medicine (1946); president, VIII International Congress of Genetics, Stockholm (1948); Kimber Award in Genetics, National Academy of Sciences (1955); Virchow Medal, Virchow Society of New York (1956); vice president, International Congress of Radiation Research, Burlington, Vermont (1958); Darwin Medal, Linnaean Society, London (1958); Darwin Medal, Deutsche Akademie Naturforscher Leopoldina (1959); Alexander Hamilton Award, Columbia University (1960); Humanist of the Year, American Humanist Association (1963); and City of Hope Medical Center Research Citation (1964).

Muller was a member of numerous learned societies in the United States, including the National Academy of Sciences (elected in 1931); fellow, American Association for the Advancement of Science; American Society of Naturalists (president, 1943); American Philosophical Society; American

Academy of Arts and Sciences; American Society of Zoologists; Genetics Society of America (president, 1947); American Genetic Association (vice president, 1959); Society for the Study of Evolution (president, 1957); American Society of Human Genetics (president, 1949); Society for Experimental Biology and Medicine; American Humanist Association (president, 1956-1959); honorary member, American Institute of Biological Science.

Muller was also elected to the following foreign learned societies: the Royal Society, London; U.S.S.R. Academy of Sciences, corresponding member (1933, resigned 1948); Royal Danish Academy; Royal Society of Edinburgh; Royal Swedish Academy; Accademia Nazionale dei Lincei; National Institutes of Sciences of India; Akademie der Wissenschaften und Literatur, Mainz; Genetics Society, Japan; Genetical Society; Mendelian Society of Lund; Deutsche Akademie Naturforscher Leopoldina; Japan Academy; Zoological Society, Calcutta; Societa Italiana di Genetica Agraria; Rationalist Press Association; World Academy of Arts and Science (vice president, 1964).

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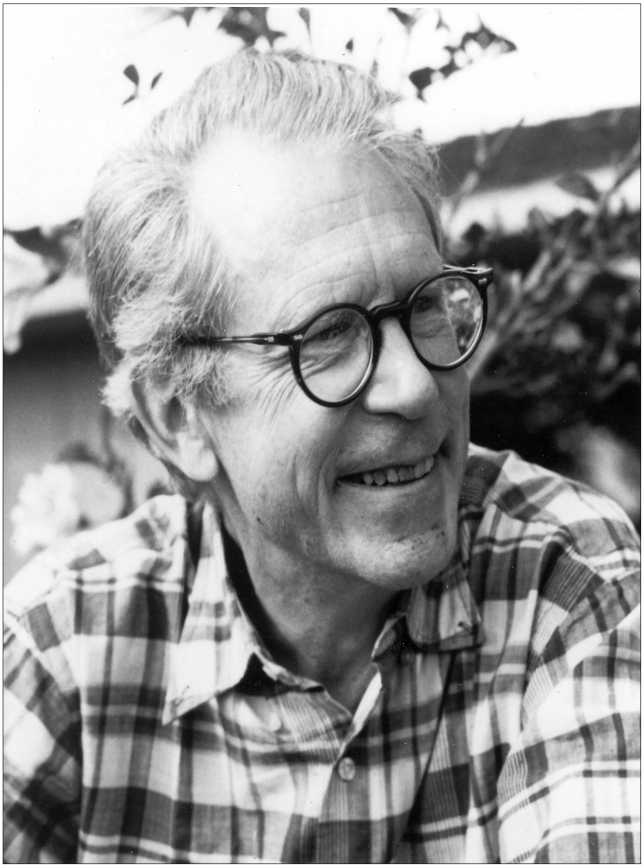
Photograph by Jonathan Wittenberg.

David D. Perkins

DAVID DEXTER PERKINS

May 2, 1919–January 2, 2007

BY ROWLAND H. DAVIS

David Perkins has a unique place in the history of fungal biology and genetics. His extensive contributions to the field began shortly after Beadle and Tatum presented clear evidence of the relation of genes and enzymes (1941, 1945). They used the filamentous fungus *Neurospora* as their experimental organism. While Beadle and Tatum popularized the use of microorganisms in the molecular revolution that followed, David Perkins assured the continuing status of *Neurospora* as a model organism used for many other types of study (Davis, 2000, 2003; Davis and Perkins, 2002). He did so largely through his extensive studies of its genetics, cytogenetics, population biology, and mating systems. In addition, his laboratory contributed, over a period of 55 years, many new techniques, compendia of all known mutant strains, updated genetic maps, and exchange of information among a global community of *Neurospora* researchers. As Charles Yanofsky said in a recent memorial tribute,[1] "Beadle and Tatum initiated research using this organism, but it was David who made certain that this interest would continue." The new field of fungal genetics and biology originated with the *Neurospora* community, and David can claim perhaps the greatest role in its origin.

PERSONAL HISTORY

David Dexter Perkins was born on May 2, 1919, to Dexter Perkins and Loretta Miller Perkins in Watertown, New York. His formative years were spent in the Great Depression, and his strong family imparted to David qualities of frugality, cooperation, hard work, and the ability to distinguish the important from the unimportant both in life and in science. He received his bachelor's degree at the University of Rochester in 1941. He joined the laboratory of Francis Ryan at Columbia University thereafter. His pacifism made him unwilling initially to join the war effort, but he decided soon that he must do so. David left Columbia to serve in military intelligence in England, studying aerial photos and briefing Air Force crews regarding bombing targets. After the war, he rejoined the Ryan laboratory and received his Ph.D. in 1949. He became a faculty member at Stanford University the same year and remained there for his entire professional life. Although he officially retired in 1989, he continued his laboratory work until his death on January 2, 2007.

David married Dorothy ("Dot") Newmeyer (b. 1922), a student of Edward Tatum, then at Stanford, in 1952. She worked for most of her professional life in David's laboratory and made many scientific contributions, both independently and jointly with David. They had one child, Susan, born in 1956. Dorothy Perkins, in poor health for many years, passed away just four days after David died.

David rode his bicycle to work at least six days a week, arrived before 6:00 a.m., took the stairs to his office even in his 80s, and rested on his back on the floor under his desk at noon daily so he would be fresh for a long afternoon's work. The stability of David's life—working this way at Stanford University for 58 years, married to Dot for 54 years, and dying four days before his wife in 2007—underlay a unique, global impact. David was remembered as much for his personal integrity, generosity, and outreach to others as he was for his considerable scientific contributions.

Hard-working and modest, David had time for others, listening to their problems, trusting them, and giving back as much as he could to help them. At the memorial website[1] many testimonials from his scientific associates speak to these traits. Alice Schroeder, one of his students, remarks,

David also understood how to train students. He felt that your professor pointed out interesting projects, advised and critiqued experiments but let you become the expert, making decisions and mistakes that made the project your own and one you could carry with you when you left the lab. …He hated to see an ounce of scientific ability wasted and understood the pressures that women faced if they were to be scientists and have a whole life.

Robert Lloyd, an underprivileged high-school graduate recruited as an assistant to work in the laboratory, speaks of David's and Dot's generosity:

I was working and going to Foothill Community College at night, but David suggested I quit work and go to school full time and explained that the economics made more sense that way. I didn't see how that was possible. "We have this money that is just sitting in the bank doing nothing," he said. "You can either repay the loan when you finish your education or you can help someone else." Because of the model Dot and David set for me, I was able to do both. I became an artist and professor of photography.

These anecdotes give an inkling of the way that Dot and David personally influenced generations of students, associates, and members of his scientific community. He remained in contact with everyone who wrote or asked questions of him. He remembered everything, and gave penetrating analyses of others' problems with a sly twinkle in his eyes—never harsh, never demeaning, always helpful. The same sense of community led him, often at Dot's urging, to donate much to civic and political causes, and later to take up political activities, as her frequent illnesses restricted her own.

While I knew David most of my professional life, I got better insight into David's and Dot's lives on my sabbatical in 1985 with Charles Yanofsky, whose laboratory lay across the hall from David's. I lived for the first half of my stay in a spare room in the Perkins's plain house while he traveled in Africa to collect *Neurospora* samples from the wild. In return, I could look after Dot, who was by then in poor health, care for the house and grounds, shop for food, and cook for both of us. It gave me a chance to know Dot's love of music, literature, and I might say, cooking. Our conversations ranged widely, from the mutability of histidine mutants to Bach partitas to *The Jewel in the Crown*, then playing on their black-and-white TV. I felt that she and David would have discussed all these things and more in their normal lives, an intellectual feast for two every night. I also noted that much of their music was on 78-rpm records, under a working turntable and simple amplifier, by that time ready for the Smithsonian—just another sign of the modesty of their personal lives.

David's long letters from Africa, filled with adventure, rivaled in interest most of the science that progressed in his laboratory at the time. The letters were a regular high point in the lab's life while he was gone. He returned, with visible and poignant affection for Dot, and within a day he resumed his lab work. The atmosphere of his lab swelled with bursts of discussion and visitors eager to welcome him back and hear more of his travels. Supremely interruptible, he listened, gave advice, and smiled as he turned back to help his research associate, Barbara Turner, sort his vast collection of African *Neurospora* samples.

At this time Charley Yanofsky, long immersed in the molecular biology of bacteria, had by then returned in part to studying the molecular biology of *Neurospora.* He had gathered a stable of students, postdoctoral fellows, and visitors like me. The two laboratories joined in a scientific and social collaboration that lasted for years, with regular lunches and coffee hours for all who could attend. David's invariant peanut-butter sandwiches at lunch and Charley's invariant cookies and cakes at 4:00 accompanied some of the most valuable modern crosstalk in the history of *Neurospora* biology. Molecular biologists learned the arcana of *Neurospora* genetics and microbiological techniques from David, and the Perkins laboratory collaborated on molecular biological research for the first time. This union of laboratories assured the renaissance of *Neurospora* research that culminated in the organism being the first filamentous fungus to have its entire genome sequenced. It remains a model for this group of organisms for that reason, and David can take no small credit for that.

SCIENTIFIC CONTRIBUTIONS

FORMAL GENETICS AND CYTOGENETICS OF NEUROSPORA

Three species of the filamentous fungus *Neurospora* were described after the discovery of the sexual phase of these organisms (Shear and Dodge, 1927). They belong to the group known as Ascomycetes. As in most fungi, their tubelike cells (hyphae) grow at their tips, branch extensively, and continuously fuse to make a reticulated mycelium. The cells contain many haploid nuclei in a common cytoplasm. Matings take place only between mycelia of opposite mating types (*mat A* and *mat a*), and the life cycle is completed by fusion, within a fruiting body, of two haploid nuclei, followed immediately by the two sexual divisions (meiosis) of the diploid nucleus. Meiosis yields the four haploid nuclei. In all three species the four products divide again to form eight nuclei. In two species, *N. crassa* and *N. sitophila*, each of the eight nuclei is enclosed in a single spore; in the third species, *N. tetrasperma*, two nuclei of opposite mating type are included in each of four spores, rendering the spores and the mycelium derived from them self-fertile. Thus in these organisms all the products of a single meiotic process are recoverable as ascospores in a single ascus.

Dodge, after working on the sexual cytology and patterns of segregation of the mating-type genes, recognized the potential of *Neurospora* for genetic research, and encouraged others to use the organism in their genetic research. By the time Beadle and Tatum were ready to seek a simple organism for their biochemical studies, Carl Lindegren had domesticated *N. crassa* and *N. sitophila* for laboratory work, having developed standard strains, some morphological and color mutants, and a first linkage map of one chromosome of the organism (Lindegren, 1936).

The novel ability to study genetic segregation and recombination in single meiotic cells (the asci) of a simple organism was an extremely attractive opportunity for geneticists at the time. David Perkins was by then at Stanford, where Beadle and Tatum had done their first *Neurospora* work (Beadle had left Stanford for Caltech in 1946). David devoted himself to intense genetic studies of *N. crassa*, because this organism, at the heart of the tidal wave research started by Beadle and Tatum, had to be fully characterized genetically. In doing so he not only proved that *Neurospora* obeyed all the rules of genetics of higher organisms, he also greatly extended studies of chromosome interference (1954), cytogenetics (1977, 1997), the cytology of the sexual system (Raju, 1992), and made linkage maps of all mutants he could obtain from most other *Neurospora* laboratories (1982). *Neurospora s*oon rivaled corn, *Drosophila,* and the mouse in the number of known genetic loci. Outside of yeast it remains genetically the best-known microorganism among eukaryotes (organisms with true nuclei).

Most mutant strains from the Beadle-Tatum era had been derived from X-ray and ultraviolet mutagenesis. These treatments often cause chromosomal breakage. David therefore discovered many chromosomal rearrangements of *Neurospora's* seven chromosomes during his gene-mapping studies. Strains carrying aberrations yielded characteristic patterns of ascospore abortion in crosses. These patterns were easily visible under a dissection microscope, and David constructed new and valuable strains for future experimental use (Kasbekar, 2007). These chromosomally aberrant strains could be used as experimental tools with little training by the increasing numbers of biochemists entering the field of biochemical genetics. For example, David developed a widely used strain called *alcoy* for quick mapping studies (1969). The strain had visible mutations (albino, colonial, and yellow), marking three compound chromosomes (arising as reciprocal translocations). When *alcoy* is crossed to a normal strain with a new mutation, the compound chromosome on which the new mutation lay could be determined by its linkage to one of the visible markers. Only one or two follow-up crosses would be needed to reveal the location of the mutation more precisely.

The work described above, to which Dot Perkins contributed continually, underlay the rapid progress in the field of biochemical genetics of fungi in laboratories around the world. In the 1950s another fungus, *Aspergillus nidulans,* took its place beside *Neurospora* as a model organism for genetic and biochemical study, owing to the work of Guido Pontecorvo and his group (Pontecorvo et al., 1953). David, who spent a sabbatical with Pontecorvo in the early 1950s, effectively urged cooperation between the two communities, which maintained increasing contact thereafter.

Bacterial and bacteriophage genetics began with the discovery of recombination in *Escherichia coli* (Lederberg and Tatum, 1946) and developed explosively after the discovery of the structure of DNA in 1953. These milestones diverted attention from the maturing field of fungal biochemical genetics. Although work on *Neurospora* continued vigorously, the number of laboratories working with the organism remained fairly small. But by that time the community was loyal to *Neurospora,* bound in part by the standard techniques and strains propagated by David's laboratory. Even in the 1950s the Fungal Genetics Stock Center had been established. The annual *Neurospora Newsletter* began in 1961, as did the biennial *Neurospora* information conferences. David, with Raymond Barratt and others, published the first compendium of *N. crassa* mutants, genetic maps, and descriptions of the variety of different mutations (multiple alleles) of well-known genetic loci (1954). He updated this compendium twice thereafter (1982, 2001), the last appearing as a book. These resources were invaluable to the *Neurospora* community over half a century.

David's greatest personal contribution in those years was his insistence on communication, lack of competition, sharing of resources, and regular, widely distributed reports of new techniques and strains for use by others. Beyond *Aspergillus* and several genera of mushrooms, the genetics of other fungi was in a primitive state. *Neurospora* thereby became recognized as a model organism through the use of mutational analysis of attributes well beyond biochemical pathways, ranging from sexual biology, cell-to-cell recognition, mitochondrial biogenesis, circadian rhythms, population genetics, gene regulation, and development (Davis, 2000). *Neurospora* studies were sufficiently mature by 1975 that the field could attract new students in the face of the understandable attraction of the single-celled yeast *Saccharomyces cerevisiae* (often called the eukaryotic *E. coli*) as it became domesticated for studies of biochemistry, metabolic regulation, recombination, and cell division after 1970. Without David's central role in tending *Neurospora* as a public garden of resources, the organism would almost certainly not have survived to contribute as it has in the molecular era.

POPULATION GENETICS AND EVOLUTION

We may ask whether *Neurospora* simply followed the trends now set by the technically more accessible organisms *E. coli* and *S. cerevisiae* after 1970. Most of the *Neurospora* community understandably pursued research programs that had few counterparts in bacteria or yeast. David, while maintaining his formal genetic and cytogenetic studies, began a large-scale, global collection of wild-type strains of *Neurospora spp.* in collaboration with his long-time research associate, Barbara Turner (1976). In his extensive travels and from his trusted colleagues in the United States and abroad, he gathered over 5000 samples of all conidiating species of the genus. His intent was to study the genetic diversity of the genus in terms of population genetics and evolution. The questions to be answered were whether the multiple samples from individual areas were genetically diverse, indicating largely sexual propagation, or whether they were clonal, propagated asexually after a few ascospores established a primary population. The question was biologically interesting, because conidia, the asexual spores, appeared in abundance after fires, readily recognizable as powdery orange tufts on scorched and fire-killed vegetation. In addition, almost no one had observed the sexual stage of the organisms (the perithecia that produce ascospores) in nature. Soon it became obvious that the species of the genus maintained their presence over time largely by sexual reproduction. Even in limited areas the individual colonies collected after the fires were genetically different, as shown by their different mating types, incompatibility genes, and isozymes (1988). Moreover, the genetic diversity in small geographical areas was comparable to samples of the worldwide collection (Speith, 1975).

The second question about where sexual reproduction took place was harder to answer, but was answered eventually: the vegetative phase (mycelium) grew under the bark of trees or hidden in plant remains, where mycelia of different origin and mating type could complete the sexual phase cryptically, producing ascospores that would remain dormant for many years until activated by fire or chemical derivatives of plant decay (Pandit and Maheshwari, 1996).

The third question was the relation of one species to another. Were they variants of a single, global species, or were the species originally described truly isolated reproductively from one another? The latter was shown to be the case; so much so that fertility became a highly dependable criterion of species identity. Indeed, this led to the discovery of a new species, *N. discreta* (1986). These population studies set the stage for many genetic and evolutionary investigations, first of the productiveness of interspecies matings; then of the comparative molecular attributes of mating-type genes; then the role of incompatibility genes that limit asexual fusions of mycelia; and finally of the relatedness and probable diversification of the different species from a common ancestor. David's collections and basic work with global collections generated a host of research programs that continue vigorously to this day (e.g., Dettmann et al., 2003).

A fourth project tied in with his genetic studies, namely the discovery, with Turner, of a spore-killer trait (Sk^K is the active form) in *N. sitophila* and *N. intermedia* (1979). This chromosomal factor, carried by some strains, leads to the death, after killer × sensitive matings (*Sk*^{K} × *Sk*^{S}), of the sexual spores that do not carry the *Sk*^{K} factor. This is a meiotic drive element comparable to those in *Drosophila* and the mouse that distorts segregation ratios and blocks recombination over a large portion of the chromosome carrying it. Despite 30 years' work on this factor (Raju, 2007), its molecular nature and its mechanism of action, now being pursued by others, are still quite obscure. David was greatly disappointed that it was not, by the time of his death, more fully understood (D. J. Jacobson, personal communication).

CYTOLOGY OF THE SEXUAL PROCESS.

For many years David collected variant strains of all kinds. Many of these affected the sexual process, either in the success of matings or in the morphology of sexual structures and their products. With this collection David and long-term research associate Namboori Raju were afforded an opportunity to analyze the genetics of the steps of the mating process, from the origin of the female fruiting structure to the formation and shape of ascospores. The sexual process is now one of the best-known series of events subjected to cytological studies in any fungus (Raju, 1992). But just as important are the fruits of this knowledge in other studies. Among these was the discovery that if DNA-carrying genes indispensable for the meiotic remains unpaired with its homolog during meiosis, the mating fails. This phenomenon, meiotic silencing of unpaired DNA (MSUD) reflects the action of an RNA-silencing phenomenon, (Shiu et al., 2001). MSUD is distinct from the RNA-silencing mechanism (quelling) discovered in vegetative cells of *Neurospora* (Cogoni, 2001). Major efforts to define this process, including new mutations that disabled MSUD, were accomplished by Robert Metzenberg (NAS), who after retiring from the University of Wisconsin, spent five years as a visiting professor in David's laboratory. Bob imparted further momentum to molecular studies of the *Neurospora* sexual cycle. His analysis of MSUD through meiotic mutations that had long perplexed the community, has defined the process as a fundamental phenomenon seen in higher organisms.[2] Another molecular study in the laboratory was the ability to detect histone gene activity, even during meiosis, through use of the green fluorescent protein (GFP) as a tag (Freitag et al., 2004).

THE GENOMIC ERA

David was not a biochemist or a molecular biologist. However, his coherent development and maintenance of data on *Neurospora* genetics and cytogenetics, and his published compendia of information on all *Neurospora* strains and mutants were indispensable factors in this organism's becoming the first filamentous fungus whose genome was sequenced (2003). Because of the extensive knowledge of chromosomal architecture, mutational landmarks, and the standardization of data, the genome was quickly annotated and compared with that of other microbes, particularly yeast and other fungi (Borkovich et al., 2004). The wealth of new genes, unshared by yeast but found in other filamentous forms, opened a major window onto the diversity of fungi, and confirmed *Neurospora crassa* as a model for the entire group. The last compendium of *Neurospora* mutants (2001) includes the molecular findings on the genes listed, and serves as a modern reference for workers on all filamentous forms.

The use of common molecular techniques by all workers on fungi transcended the diverse biology of this group of organisms, from industrial forms to plant pathogens. Because of a shared molecular language and many techniques, the *Neurospora*-*Aspergillus* community invited workers on all filamentous fungi to join it after 1985. The field of fungal genetics and biology was thus born and now rivals many other fields in its scientific vigor, diversity, membership, and promise. David had long advocated attention to diversity (1991), and the massive community he left behind was one of his greatest legacies.

BUILDING A COMMUNITY

The selected bibliography below emphasizes that David's major preoccupation was the fundamental genetics of *Neurospora*. Nowadays one would ask whether this simply followed in the traditions of Morgan with the fruit fly and of the early workers with corn. This was the case, and David called attention to the parallels among all these model genetic organisms. But it was not journeyman work. In contrast with the pioneer geneticists, David accomplished as much almost single-handedly. He was the first to explore so deeply and so broadly the genetic system of a haploid, eukaryotic organism. Not only did he establish the formal genetics of *Neurospora* but the cytogenetics, the sexual biology, and almost incredibly, the population genetics of the fungus as well. This pioneer work qualified him to point the way for three later generations of workers on the organism. All the time, he promoted *Neurospora* as a research tool, "keeping the machine running," as it were, while doing original work in many areas. The later papers show that he easily contributed to molecular studies of the organism as molecular approaches overtook all of biology.

Another aspect of his bibliography is not at all obvious. David never put his name on a paper to which he had not made a hands-on experimental contribution. He had only four graduate students but many visitors and several long-term research associates: his wife, Dorothy Perkins; Raju; Turner; and later, David Jacobson. Their work was supported by his research grants, one a 42-year continuing award from the National Institutes of Health and at the end, additional support for the remaining years from the National Science Foundation. (One reviewer of David's 2004 grant renewal application called his laboratory a "national treasure.") His full bibliography lists all work funded by his laboratory, running to over 430 titles. His intellectual contribution to many of these projects was substantial, and most major laboratory leaders nowadays would have claimed authorship on all of them.

David built his community in part by encouragement of young and foreign investigators, a process extending over many years. At meetings he would listen intently to conversations and poster sessions of those new to the group. This not only flattered these entrants to the field but also made them the targets, years later, of information and advice David would forward from obscure places in the literature or from the informal communications of workers in the field. He remained tirelessly up-to-date with the *Neurospora* literature, and could review and discuss perceptively almost any subject. Many foreign *Neurospora* researchers would automatically visit his laboratory when they were in the United States, and many began their most important work on *Neurospora* after spending longer periods with him. His loyalty to them and to others was surpassed only by his memory of the details of their work.

Because of his intellectual and historical presence in the field and at meetings, he was able to impart other characteristics to the community. These were integrity, cooperation, and noncompetitive behavior in research. He gave freely of his ideas, ideas later embodied in original research by others. Few workers were worried about being scooped, largely because David and the *Fungal Genetics Newsletter* (originally the *Neurospora Newsletter*) made clear who was working on various projects and their progress. This tradition was characteristic of much work in Mendelian genetics early in the 20th century, but David projected it upon his own field well into the molecular era. He never copyrighted or patented a single finding or device in his own name, nor did he seek to claim any invention or technique as his own.

Finally, frugality, cleverness, and a hard work ethic drove David's years of productivity. With respect to the first two of these traits, David's contributions to the community were substantial. Given his commitment to genetic analysis on a grand scale, he devised or improved on techniques that facilitated many other research programs. He regularly published these methods in the newsletter (calling them "Stanford methods" rather than "Perkins methods"). He developed a fast method of collecting and analyzing unordered tetrads (groups of meiotic products) shot onto the lids of Petri dishes. This obviated isolation and growth of ascospore cultures for his study of spore abortion patterns. His tests of silica-gel storage of stocks allowed many people to easily maintain stock collections beyond the dreams of *Drosophila* workers. His introduction of *alcoy* and other stocks rendered mapping of new mutations rapid and routine. His compendia of mutations and their multiple alleles at all known chromosomal locations made all workers aware of what had gone before, sparing the field countless hours of work and much unnecessary duplication of findings in the literature.

PROFESSIONAL HONORS AND SERVICE

David received a National Institutes of Health Research Career Award (1964-1989) and an NIH MERIT Award (1987-1996). He assumed the job of editor in chief of *Genetics* (1963-1967) and later became president of the Genetics Society of America in 1977. He was elected to the National Academy of Sciences in 1981, was named a Guggenheim fellow from 1983 to 1985, and was awarded the Genetics Society of America

Morgan Medal in 1994. The British Mycological Society made him an honorary member in 2005.

I WISH TO THANK THE following persons for help with critiques, corrections, and additions to this and an earlier account (Davis, 2007), the latter including the contributions of Dorothy Newmeyer Perkins: Namboori Raju, Charles Yanofsky, David Jacobson, Barbara Turner, Susan Perkins, and the late Robert Metzenberg. The photo of David Perkins was taken by Jonathan Wittenberg in 1978, and was provided to me by N. Raju. It is used, with thanks, by permission of Dr. Wittenberg.

NOTES

1. C. Yanovsky. In Memoriam: David Dexter Perkins 1919-2007. www.stanford.edu/group/neurospora/.
2. Sadly, Bob Metzenberg, another creative and influential geneticist of the Neurospora group, died in 2007 at the age of 77, as did David Stadler, whose contributions greatly illuminated the study of recombination and mutation in Neurospora. The Neurospora community is thus further and significantly diminished.

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Photograph Courtesy of The University of Illinois Archives.

C. Ladd Prosser

CLIFFORD LADD PROSSER

May 12, 1907–February 3, 2002

BY GEORGE N. SOMERO

CLIFFORD LADD PROSSER, AFFECTIONATELY known as "Ladd" by all who had the great fortune to interact with him during his seven-decade-long career, was a principal catalyst in the development of the broad field of comparative physiology. Born in Avon, New York, in 1907, Ladd either witnessed the early development or, indeed, fostered the very conception of many of the core research areas still being actively studied by biologists who describe themselves as comparative, integrative, or evolutionary physiologists. Workers in this broad area of biological investigation thus owe a major debt to Ladd Prosser, whose curiosity about nature led him to ask penetrating questions that continue to challenge and motivate us. Moreover, and very importantly, he helped to refine a philosophical context—the comparative method—that has enabled biologists to exploit the diversity of nature to elucidate the common, basic principles that characterize living systems.

This short biographical memoir of Ladd Prosser has two primary purposes. One is to provide a description of the scope and breadth of Ladd's scientific accomplishments, which were foundational for so many areas of physiological study. The second is to describe the character of this remarkably insightful and humane individual in hopes of explaining his success in motivating and energizing a large cadre of young scientists, many of whom have gone on to become leaders in their fields. We have much to learn from Ladd's formal contributions to physiology—his several books and over 200 research papers—and from the way he stimulated and nurtured others. His approaches to science and to scientists should continue to serve as a role model for others seeking to maintain the vitality of the comparative approach to biology that was the core of his scientific philosophy.

In developing these two themes I begin by acknowledging the assistance I've enjoyed from talking with former Ph.D. students and postdoctoral associates of Ladd and from reading the biographies they have published about their mentor (Hazel and Sidell, 2002; Mantel, 2002). Although I am not an alumnus of Ladd Prosser's laboratory, I had the privilege of working with him for several weeks on board the research ship *Alpha Helix* during the Bering Sea expedition in 1968. Following this we interacted frequently through visits to each other's laboratories, at meetings, and through correspondence. The qualities that accounted for Ladd's remarkable level of success in doing science and in stimulating others became apparent very rapidly. Perhaps the most striking aspect of Ladd's character was a true child-like curiosity about nature, which typically was appreciated at one's very first interactions with him. Ladd claimed that Sunday walks in the woods with his father triggered in him a love of nature and a desire to find out how it works—at all levels of biological organization. Several decades later this curiosity was still there, undiminished and, in fact, probably greatly amplified as he learned about more things to spike his curiosity. As his students have continually remarked, this enthusiasm was contagious for all of those lucky enough to be in the lab with him. On the *Alpha Helix* cruise I had the added privilege of sleeping in the top bunk above Ladd. I can testify that his enthusiasm continued long into the night, perhaps too long on some nights when my mental stamina (or my stomach's tolerance of a rocking ship) was not up to the standards of this scholar some three decades my senior.

The other remarkable aspect of Ladd's character that quickly became evident was his voracious love of knowledge. He not only was incessantly curious about nature but he also wanted to know what you knew or thought about a myriad of different topics. This desire to learn from others was coupled with capacities for filing away all that he had learned and being able to integrate and synthesize this information both horizontally among disciplines and vertically along the reductionist-holistic axis. Having grown up with many of the fields of physiology and, in some cases, serving as the originator of these fields, Ladd was successful in keeping up on the literature in a way that would be impossible in this day of fragmentation of knowledge and overwhelming output of papers. One of his Ph.D. students, Linda Mantel, commented in her tribute to Ladd, "Scientists with Ladd's vision and breadth of interests aren't made any more, and we are all poorer for that."

FORMATIVE INFLUENCES: FROM WALKS IN THE WOODS TO POSTDOCTORAL STUDY

Ladd's *Scientific Autobiography and Personal Memoir* (Prosser et al., 2001)," completed shortly before his death, provides many insights into the formative factors that led to his illustrious career. Ladd grew up in a small town in rural New York, where his father ran a general store. The family had hoped that Ladd would follow in his father's footsteps and become a merchant. However, the footsteps that truly mattered were those made along the forest trails, where Ladd's acquaintance with and curiosity about nature set in motion his desire to study biology. These experiences, along with encouragement from his seventh-grade science teacher, motivated Ladd to enter the University of Rochester, in 1925, as a biology major. A formative event during his undergraduate career was a course he took in physiological psychology, which convinced him that unraveling the underlying neurobiology of behavior could be done more effectively through studying "lower" animals such as invertebrates rather than mammals. This insight no doubt played an important role in the development of Ladd's philosophy about the comparative method. He graduated from Rochester University (Phi Beta Kappa) in 1929 and went directly to Johns Hopkins University to begin his doctoral studies.

At Johns Hopkins Ladd studied with a leading cellular physiologist, Professor S. O. Mast. Ladd's wide-ranging interests were manifested even at the onset of his research career. With Mast he studied amoeboid motion and published his first papers on this topic. He did parallel work on the behavior and nervous system of the earthworm, a reflection of his desire to exploit the comparative approach to behavior and neurobiology in his work.

After receiving his Ph.D. in 1932, Ladd moved to Harvard University for postdoctoral study under a Parker Fellowship that stipulated that he must remain celibate for the duration of the fellowship period. At Harvard Ladd studied principally with Hallowel Davis but also interacted closely with other giants in the field of physiology, notably Walter Cannon and Alexander Forbes. The Harvard period saw Ladd further develop his investigations of neurobehavioral phenomena, this time using crayfish as his model system. He discovered the existence of spontaneous, rhythmic neural activity in the central ganglia of this crustacean. In his autobiography Ladd states that this was "one of the most important discoveries I ever made" because of "the prevailing view that all behavior was initiated from outside the organism." Ladd was never hesitant to challenge the conventional wisdom when his data indicated that a change in perspective was needed. His studies of the crayfish nervous system yielded yet another surprise: the existence of a caudal photoreceptor that had not been anticipated.

An important component of Ladd's experiences during his periods at Harvard and Johns Hopkins was the summers spent at the Marine Biological Laboratory at Woods Hole, Massachusetts. There he furthered his interests in comparative biology, developed a love of summer retreats to this intellectual epicenter, and in 1934 met his future wife, Hazel Blanchard, who worked in the MBL library. Hazel, of course, could not become his spouse until the constraints of Ladd's Parker Fellowship no longer applied.

Ladd's fellowship support did allow him the opportunity to spend part of his postdoctoral period abroad. In 1933 he sailed to England for study at Cambridge and Oxford universities. At Cambridge he worked with Edgar Adrian, who had received a Nobel Prize in 1932. In Adrian's laboratory Ladd continued his work on earthworm neurobiology. In typical Prosserian fashion, he began to extend his comparative interests even more broadly. To this end he developed a collaboration with John Eccles (a future Nobelist) at Oxford University. As those who knew Ladd can attest, his physical energy matched his intellectual energy. Thus, his willingness to commute between Cambridge and Oxford by bicycle was right in character. At Oxford in Eccles's laboratory, Ladd recorded from the sympathetic ganglia of the cat, thus further broadening his expertise in neurobiology.

ACADEMIC POSITIONS AND CAREER HIGHPOINTS

Ladd returned to the United States in 1934. His restrictive fellowship was over. Hazel met the boat, and she and Ladd were married the following day. That same year Ladd accepted his first faculty position, at Clark University, where he remained through 1939. He received support from the Rockefeller Foundation to develop a research program, which extended his focus on spontaneous nervous activity. Summers were still spent in Woods Hole, where Ladd worked with John Z. Young on the new study system involving the squid giant axon.

Although Ladd prospered during his time at Clark University, he sought a position at a larger university where he could more fully realize his potential in research. Such an offer materialized in 1939: a position at the University of Illinois in Champagne-Urbana, where Ladd was to spend the remainder of his long scientific career. This offer involved a significant piece of negotiation. Ladd's contract specified that he was to teach what would today be called a "service course" for agricultural students. Ladd insisted that he also be allowed to teach a new course of his own design in comparative physiology. This course was to serve as a focus for his development of a synthetic view of comparative physiology that resulted in the publication of his *Comparative Animal Physiology*, which evolved through four editions (1950, 1961, 1973, 1991).

Ladd's career at Illinois was interrupted by the Second World War. Perhaps with some sense of foreboding, he arrived by car at Champagne-Urbana on the day that Hitler invaded Poland. In 1942 Ladd was recruited for the Manhattan Project, and moved his family (now with a young daughter) to Site B of the project, an old brewery in Chicago that the federal government had obtained for research on the effects of radiation on organisms. Ladd organized a group of 150 scientists to work on this poorly understood, yet vitally important issue. His group showed that particular dangers arose from bone-seeking materials like strontium-89. Ladd was one of 69 signatories to the famous Szilard-Einstein letter to President Truman, dated July 17, 1945, which cautioned on the use of the atomic bomb as a weapon. After the war, Ladd worked briefly on Bikini atoll, where further studies of the effects of radiation were conducted. He also was instrumental in preparing 12 reports on the effects of radiation. This pathbreaking work was an important foundation for the radiation safety standards later developed by the federal government.

Ladd returned to the University of Illinois and picked up the pace of his research program. He also put major efforts into fostering an improved research environment at the university. From the start of his career at Illinois Ladd had worked hard to develop a world-class physiology program at the school. He sought to ensure that physiology was broadly focused and included the study of diverse species, not just mammals. His efforts led to the establishment of the Department of Physiology in 1949 and later to a combined Department of Physiology and Biophysics, which he chaired from 1960 through 1969. He also was a prime mover in developing the neurosciences program at Illinois, for example, through helping initiate the Neural and Behavioral Biology Program.

Despite the altered focus of his scientific work during the Second World War, Ladd's interests in comparative physiology remained strong. He began to envision a textbook in comparative physiology that would serve a number of purposes. It would not only be a comprehensive review of what was known in the field but also would document the importance of examining biological function from a wide evolutionary and environmental perspective. The initial volume of this classic text (1950), which Ladd prepared with the help of four coauthors, succeeded brilliantly and quickly became the standard book in its field. The book was a reflection of Ladd's intellectual strengths, especially his skills in organization and synthesis. Ladd loved to tell the story about how the organization of the literature for the book involved sorting relevant papers among bushel baskets he kept in the basement of his house. Each basket contained the nucleus of a different chapter. Those of us who visited Ladd at his office in Burrill Hall witnessed the evolution of this filing system: Later editions (1961, 1973, 1991) were generated from enormous stacks of reprints covering every square inch of flat surface in his office. Remarkably, this filing system was very low in entropy. I recall sitting with Ladd in his office and asking him about a specific paper. Without hesitation he went straight to the appropriate stratum in one of the many stacks of reprints and extracted the exact document I wanted to see. This capacity for accumulating the world's literature was paired with an ability to synthesize what it contained. All four editions of *Comparative Animal Physiology* present not only an enormous set of "trees" but also a synthetic view of the "forest" at large. This was Ladd Prosser at his best: learning everything he could and then returning to the community a beautiful synthesis of what to many might seem a bewildering array of unrelated facts. Among the forms of recognition he received for his scholarship was election to the National Academy of Sciences in 1974. He was elected to the American Academy of Arts and Sciences in 1957 and was awarded a Guggenheim fellowship for 1963-1964.

The diversity of interests found in *Comparative Animal Physiology* was a reflection of the breadth of Ladd's research program, which flowed in several productive channels. His studies included major contributions to the following areas: invertebrate neurobiology; the comparative physiology of muscle, especially the electrical activity and rhythmicity of smooth muscle; temperature physiology, notably the phenotypic plasticity that marks acclimation to different temperatures; and the general theory of adaptational physiology. Ladd published more than 150 original papers and wrote more than 50 synthetic reviews in these fields. Ladd's 1986 volume, *Adaptational Physiology: Molecules to Organisms*, represented his effort to provide an overview of physiology that vertically integrated information from the ecological to the molecular level. This grand synthesis was the publication that Ladd was proudest of in his huge corpus of work.

In my own area of specialization, thermal physiology, the publications that Ladd wrote with graduate student and postdoctoral colleagues like Andrew Cossins, Jeff Hazel, and Bruce Sidell are landmark papers that helped to define the field. His lab truly was the epicenter of the study of thermal acclimation, and much current work on the molecular details of this process can trace its origins to the questions and publications that originated in Ladd's laboratory at the University of Illinois.

The contributions that Ladd made to physiology include extensive service as an editor of leading journals. He served as editor of *Physiological Zoology* (now *Physiological and Biochemical Zoology*) from 1976 through 1988, bringing this journal to high ranking within the literature of physiology. He also served on the editorial boards of several other journals, including *The American Journal of Physiology*, *The Journal of Comparative Physiology*, and *Comparative Biochemistry and Physiology*. His broader public service included a large number of committee memberships for the National Science Foundation, National Institutes of Health, and National Research Council of the National Academy of Sciences.

RETIREMENT YEARS

Ladd retired from his faculty position at Illinois in 1975 at age 68. Fortunately for the physiological community, his retirement on paper didn't mean much in terms of what actually transpired in the laboratory. Ladd maintained an active program for an additional 20 years; his final graduate student, William Seddon, obtained his Ph.D. in 1994. During his retirement, Ladd completed a further edition of *Comparative Animal Physiology*, in 1991, and his synthetic book, *Adaptational Physiology: Molecules to Organisms*, in 1986. He continued to be a stimulating presence at scientific meetings, notably the annual meeting of the American Society of Zoologists (renamed the Society for Integrative and Comparative Biology), for which he served as president in 1961. Ladd's withdrawal from laboratory work was necessitated in 1997 when he fractured a hip. Nonetheless, he remained a voracious consumer of the scientific literature, continuing to exchange ideas with colleagues during their visits or through the mail. I received letters from Ladd into his nineties, asking me about this and that, what I thought of a new idea he'd had, and so forth. Ladd's enthusiasm for life was never lost, even with the death in 1998 of his wife, Hazel.

We can be grateful that Ladd's spirit and energy enabled him to complete his autobiographical sketch, which saw publication in 2001. From his own words found in this volume we come to better understand what accounts for his drive, his level of achievement, and his pleasure from the study of nature. In his "Epilogue and Credo" Ladd summarizes his philosophy of life for the congregation of the Unitarian Church that he and Hazel were members of for many years. Ladd emphasized that his atheism is paired with a love of the natural world and a desire to better understand humankind's place in the universe. This deeply humanistic man stated that "evolution is not a theory but a proven fact. Humankind is part of the continually evolving and beautiful web of life. My goal has long been to develop a unified philosophy." The broad comparative focus of Ladd's science can be appreciated in part as an attempt to place our species into its proper evolutionary context. His pleasure in studying evolution reflects Charles Darwin's belief that there is indeed "grandeur in this view of life." The view of life that Ladd presented to us taught us enormous amounts about physiological evolution and about the human qualities that most ensure the enjoyable and productive exploration of nature's marvelous diversity.

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Theodore T. Puck

THEODORE THOMAS PUCK

September 24, 1916–November 6, 2005

BY DAVID PATTERSON

THEODORE PUCK WAS ONE OF THOSE rare scientists who essentially created a new discipline, somatic cell genetics. His work made possible much of modern mammalian cell molecular genetics. He devised the first practical method to accomplish single-cell plating of mammalian cells with a high (indistinguishable from 100 percent in some cases) plating efficiency (1955). What is not so widely recognized are his contributions to the more technical aspects of this discipline; for example, he and his colleagues designed and built the first really practical CO_2 incubators for growing mammalian cells as individual colonies (1962,2). The incubators we all currently use, although technologically much different from Ted's original design, are based on the principles that he established.

Ted's lab was still building incubators when I arrived in 1971, and in my experience these incubators worked better than anything available to this day. He recognized early on the importance of devising well-defined, and hopefully completely defined, growth media for mammalian cells. He was certainly not the first to come to this realization, but he and his colleagues, especially Richard Ham and Gordon Sato, were among the most successful (Ham 1965; Barnes and Sato, 1980). One result of these studies was Ham's F10 and later Ham's F12 media, which are still widely used. He established and characterized the Chinese hamster ovary cell line K1 (CHO-K1), which remains a mainstay of modern mammalian cell genetics and is widely used in academic labs and in many biotechnology companies because of its favorable growth characteristics and ease of use for many different kinds of experiments (Puck, 1985). These innovations were critical for the success of somatic cell genetics.

Shortly after devising the single-cell plating technique, Ted and his colleagues determined the mean lethal dose of X irradiation required to kill mammalian cells (1956,2). This experiment is widely recognized as one that revolutionized the field of radiation biology. It is also recognized as having a revolutionary effect on the use of radiation to treat cancer. Another early contribution involved the definitive proof that humans have 46 chromosomes. Clearly this was first shown by Tjio and Levan, but their results were not easily accepted (Tjio and Levan, 1956); for example, in 1958 a suggestion was made that humans could have 46, 47, or 48 chromosomes, and that Caucasians and Japanese might differ in this regard (Kodani, 1958). Ted's immediate recognition of the outstanding nature of Tjio and Levan's cytogenetic work led him to invite Tjio to join the laboratory, where they made important contributions firmly demonstrating that 46 is indeed the correct number of human chromosomes (1958,1). He organized a seminal meeting in 1960 in Denver that established the Denver system of chromosome classification that is the basis for the methods still used today (1960,2).

Ted and his colleagues developed the first practical method for isolating auxotrophic mutants of CHO-K1 cells (1967,2; 1968). His laboratory was one of the first to use somatic cell hybridization to map genes onto human chromosomes and the first to identify different complementation groups among auxotrophic mutants with the same nutritional requirements (glycine in this case) (1969,1,2). His laboratory was also one of the first to apply techniques of molecular biology to extend the resolution of mapping human chromosomes theoretically to any degree of resolution desired (1982). He was one of the first to recognize the relationship between structural components of the cell and regulation of gene activity, a phenomenon he called "gene exposure" (Ashall et al., 1988). He also devised what may be the most sensitive assay for mutation using mammalian cells in existence (1997, 2002). He looked upon this as one of his most important scientific endeavors, one that he continued to pursue until his death.

Ted was elected to the National Academy of Sciences in 1960 and the Institute of Medicine in 1974. He garnered numerous awards in his life, including the Lasker Award in Health Science in 1958, a Distinguished Professorship of the American Cancer Society in 1966, the E. B. Wilson Medal of the American Society for Cell Biology in 1984, the Bonfils-Stanton Foundation Award in Science in 1984, and many others. Unfortunately he was never awarded the Nobel Prize, which I and many others are convinced he deserved.

Ted was born in 1916 in Chicago. He remained in Chicago—with the exception of a one-year stay in Gary, Indiana, during his childhood—throughout his education, including his Ph.D. training with James Franck, a Nobel laureate in physics, at the University of Chicago. Early in his life he and his brother Bernard helped their father install asbestos insulation. At that time the dangers of asbestos were not known. Tragically his brother died of mesothelioma, a form of lung cancer provoked by exposure to asbestos fibers. Ted credits his brother's death, which occurred after he started working on cancer, with giving him a more personal stake in his research. This experience probably also contributed to Ted's lifelong interest in environmental mutagens.

Indeed, he was working on methods to detect environmental mutagens until he died. His goal was to develop a rapid, accurate, inexpensive, and simple method for detecting environmental mutagens so that exposure to them could be eliminated or minimized.

Ted was a dedicated family man who was extremely proud of the accomplishments of his wife, Mary, who coauthored many publications on sex chromosome disorders with Arthur Robinson. They were married at the Taos Pueblo in New Mexico, and had a home in Santa Fe, where his wife now resides. He was especially proud, and rightly so, of his three daughters, all M.D.s: Jennifer, Stirling, and Laurel. He died of complications from a fall on November 6, 2005.

James Franck had a major influence on Ted's career. Ted used to tell us that during World War II, he had been recruited (it seemed more like drafted to him at the time) to work on the Manhattan Project and that Professor Franck not only advised against it but made sure that Ted was not forced to work on the project. Franck, who was involved in the Manhattan Project, eventually chaired a committee that issued what became known as the Franck Report, the official title being "Report of the Committee on Political and Social Problems Manhattan Project 'Metallurgical Laboratory' University of Chicago, June 11, 1945" (http://www.dannen.com/decision/franck.html). In this report the committee, which also included Leo Szilard (see below), urged the demonstration of the atom bomb at an uninhabited site rather than its use against Japan and predicted the arms race that later occurred. Since this was a classified report, it is not likely that Ted knew about it until later, but one cannot help but speculate that Ted's relationship with Franck helped shape his lifelong interest in the role of science in human society.

During World War II, Ted stayed at the University of Chicago, where he worked in the laboratory of O. H. Robertson in the Department of Medicine on problems related to aerosols and the spread of bacterial and viral infections through the air and on dust particles. He was also a member of the Commission on Air-borne Infections, Army Epidemiological Board, Office of the Surgeon General, and his work had relevance to the war effort. In this capacity he was remarkably productive, publishing over 30 manuscripts. This period heightened his interest in biological sciences, and he applied for and obtained a postdoctoral fellowship in the laboratory of Max Delbruck at the California Institute of Technology, where he developed his interest in genetics and in the application of physical principles to biological problems. He remained in Delbruck's lab for only one year. At that point he was successfully recruited to establish and chair the Department of Biophysics at the University of Colorado Medical School. He remained affiliated with the University of Colorado for the rest of his life. He continued his work on bacteriophage until 1954, publishing 14 papers, many of which made important contributions to understanding of phage-host interactions. At that point his career shifted into mammalian cell tissue culture and somatic cell genetics.

At this point I would like to comment on Ted's relationship to Leo Szilard because this is a matter of some sensitivity. It is true that the nature of Szilard's involvement in the development of the feeder layer technology for single-cell growth of mammalian cells is a matter of some discussion, with the exact nature of the contribution being somewhat unclear (1994; Marcus et al., 2006). What is often overlooked in these discussions of events in and around 1954 and 1955 is that the evidence shows that Ted retained a deep respect and admiration for Szilard; for example, Szilard won the 1960 award as Humanist of the Year from the American Humanist

Association. Ted presented the award to Szilard on behalf of the association and wrote an eloquent article extolling Szilard's scientific accomplishments as well as his accomplishments as a humanitarian (1960,1). This piece closes with the statement, "If humanity is to survive this most threatening crisis of its history, something of Szilard's philosophy will have to become an accepted part of the universal attitude of mankind." It is also a matter of public record that Ted campaigned for Szilard to receive the Fermi Award, a presidential award for lifetime achievement in science.

In 1967 and 1968 Ted and his colleague Fa-Ten ("Louie") Kao published their classic method for isolating auxotrophic mutants of Chinese hamster ovary (CHO-K1) (1967,2; 1968). David Gillespie, my Ph.D. thesis adviser at Brandeis University, thought that these were seminal papers in genetics and insisted that his students read them. Shortly after this, Ted gave a seminar at Harvard that I attended. Ted's way of thinking about somatic cell genetics, his enthusiasm for science, and his optimism about the role of science in human society were incredibly impressive. David Gillespie strongly urged me to write to Ted about the possibility of joining his laboratory as a postdoctoral colleague, which I did.

A few weeks later Ted called me on the telephone and said he would like to meet with me. When I asked him where he was, he said, "I'm in a Radcliffe dormitory room." At the time I didn't know, of course, that one of his daughters was attending Radcliffe, and I was a little taken aback by this response. It was my first encounter with Ted's remarkable sense of humor. He was in town to give a talk at MIT. We met before his seminar and discussed possible projects.

We agreed that my project would be to isolate temperature-sensitive mutants of CHO-K1 cells, in the end a successful but somewhat limited accomplishment since at that time it was exceedingly difficult to determine the functional defects in such mutants (Patterson et al., 1976). Meanwhile, Ted and his colleagues Louie Kao, Larry Chasin, Bob Johnson, and others were developing crucial genetic methods of somatic cell genetics, most notably the use of various mutagens to induce mutations, somatic cell hybridization for complementation analysis of mutants defective in the same biochemical pathway, and mapping genes to human chromosomes (1969,2; Kao and Puck, 1971). Ted encouraged me to take part in this ongoing endeavor, which was a turning point in my career (1974).

Ted suggested that I should study purine-requiring auxotrophs of CHO-K1, a suggestion which in hindsight was a remarkably good one. This pathway had been well defined enzymatically largely by James Buchanan, Joseph Gots, and their colleagues, and consisted of 10 enzymatic steps. Ted and his colleagues had isolated two complementation groups of CHO-K1 purine auxotrophs, named AdeA and AdeB. Following the general strategy for somatic cell genetics defined by Ted and his colleagues, I set out to isolate additional complementation groups and to characterize the biochemical nature of the defects in the mutants. Ted and I had numerous discussions about this project, often on Saturday mornings, a time during which the distractions of the week were markedly reduced and a really good time to engage Ted in scientific discussions. One particular experiment was to determine which intermediates in the purine biosynthetic pathway accumulated in mutants representative of each complementation group by separating radioactively labeled intermediates using thin-layer chromatography. This experiment resulted in unambiguous ability to discriminate each complementation group biochemically, except ones so early in the pathway that they did not accumulate intermediates.

On the Saturday morning after I had obtained this result I brought it to Ted, who instantly not only grasped its scientific significance, but also its significance for my career. One comment he made was, "This experiment can make your career." In many ways he was absolutely right. He suggested several additional experiments before believing that the results were suitable for publication, for example, inclusion of additional mutants that required a purine, a pyrimidine, and glycine (GAT^- mutants) or that required a purine and a pyrimidine (AT^-). While this was frustrating at the time, it proved to be excellent advice, and greatly improved the resulting manuscripts.

The first manuscript was published in the *Proceedings of the National Academy of Sciences* with Ted and Louie Kao as coauthors (1974). After this publication, Ted insisted that I publish on my own or with students and postdocs from my own laboratory or other faculty members, even though including him as an author would have been clearly justified. Ted's action allowed me to establish my scientific independence, an especially difficult task since Ted offered me a position at the University of Colorado, which I accepted.

Ted saw that my interests were becoming biochemical and introduced me to some of his colleagues in that area, including Seymour Cohen and Ernest Borek, both of whom had distinguished careers studying aspects of purine metabolism. Seymour Cohen introduced me to HPLC, which at that time stood for "high pressure liquid chromatography," but now stands for "high performance liquid chromatography." It was quite a new technique at the time, and remains a mainstay of modern biochemistry.

Ted also insisted that I audit a medical genetics course taught by Arthur Robinson to medical students at the University of Colorado. Arthur was a pediatrician in private practice in Denver in the 1950s who took care of Ted's daughters. Soon

Ted had him working in the lab one day a week, and then giving up private practice to join the faculty of the Department of Biophysics (Robinson, 1990). Shortly after joining Ted's department, Ted, Jo Hin Tjio, and Arthur published one of the first manuscripts describing a sex chromosome abnormality in humans (1959). In 1964 Ted and Arthur published a method for sex chromatin determination in newborns (Robinson and Puck, 1964). These publications enabled Arthur to undertake a long-term analysis of the effects of sex chromosome anomalies on human development. Arthur's collaborator on 10 of these manuscripts was Ted's wife, Mary Puck. As Ted often did, he chose not to coauthor these manuscripts, although it would have been well justified. This project continued until 1998, just two years before Arthur's death in 2000. Ted and Arthur published numerous manuscripts together and were colleagues and friends for almost 50 years.

Auditing Arthur's class was an incredibly valuable experience as I got to know Arthur. We remained close friends until his death. Scientifically, these lectures led to another major change in my career. As part of my studies on purine synthesis, I undertook to map each of the genes encoding enzymes of the pathway to human chromosomes using somatic cell hybridization. One of these turned out to be on human chromosome 21. Through Arthur's course I had become familiar with Down syndrome, or trisomy 21, and it became clear that the mapping of one of the complementation groups, AdeC, to chromosome 21, offered a major opportunity to attempt to define the genetic content of chromosome 21 and to try to understand Down syndrome. Ted, who had published work on Down syndrome in 1965, enthusiastically supported this effort. In the early 1980s I applied for a project grant on Down syndrome and was successful. However, the site visit report recommended that Ted's component be deleted.

Though I dreaded the unpleasant task of telling Ted the news, he took the news with equanimity and promised to remain available as an adviser to the project, which he did.

Ted assigned me to run the Eleanor Roosevelt Institute Seminar Series. This became one of the premier seminar series at the medical school, largely because most of the speakers were Ted's friends and included many of the luminaries in molecular biology and biomedical research, including Max Perutz, Fred Sanger, Francis Crick, Marshall Nirenberg, Paul Berg, Ruth Sager, Lou Siminovitch, Phil Marcus, and others of that stature. Ted insisted that I act as their host, so I had many private interactions with them, much to my benefit and inspiration. Importantly, Ted also encouraged me to invite some of my scientific colleagues to visit. One in particular stands out, and that was David Housman, a colleague of mine in graduate school. This visit turned out to be quite significant. It led to a collaboration between Ted and David resulting in a series of three publications establishing methods to extend the resolution of somatic cell hybrid mapping to the molecular level and for isolation of clones of human DNA from human and hamster hybrid cells (1979, 1980, 1982). This method was widely used by many others, notably Carol Jones, who continued a fruitful collaboration with David that lasted until 1993. Again, Ted chose not to be a coauthor after the first publications even though it would have been appropriate—to help Carol establish her own independent laboratory.

For his entire career Ted believed that what we do as scientists is central to the human endeavor. Not only did he believe this, he acted upon his beliefs in countless ways, sometimes at considerable risk to his career and reputation; for example, in 1955 Ted along with Ray Lanier issued a public statement stating that aboveground radioactive testing being carried out at that time in Nevada was resulting in radioactive fallout in Colorado that posed a health hazard. At this time Ted had funding from the Atomic Energy Commission, which rejected his claims about the dangers of radiation. A telling quote from Ted was, "The trouble with airborne radioactive dust is that we breathe it into the lungs, where it may lodge in direct contact with living tissue." (*Los Angeles Times,* Mar. 13, 1955, p. 20). Lanier, then director of the University of Colorado's radiology department, pointed out the absence of any "safe minimum below which danger to individuals or their unborn descendants disappears." This statement is often credited with introducing the concept that there is no safe minimal dose of radiation. It was a prescient one, for decades later Ted worked on this exact question, namely, demonstration that extremely small doses of X irradiation, perhaps only one or two times above the dose all of us receive from background radiation, can cause mutations (1997). At the time this controversy was occurring the debate focused on X rays or gamma rays. Ted stressed that alpha and beta rays, especially from inhaled particles, might also cause health problems. This was another prescient statement. It is interesting that at that time Ted was most likely working on the experiments resulting in his publication with Philip Marcus of the true lethal dose of X rays. Many of Ted's early publications had dealt with aerosols, the spread of infections through the air and in dust, and ways to prevent these. He knew what he was talking about.

The governor of Colorado, Edwin C. Johnson, responded by saying that Puck and Lanier "should be arrested" and that "the statements are part of an organized fright campaign" (Miller, 1991). The Atomic Energy Commission also weighed in, claiming that there was no cause for concern from fallout. Of course, this position later changed, and atmospheric testing was stopped.

Later it became clear that even low doses of alpha radiation, the form released by radon, are causal for human cancers. Almost 50 years later Puck and his colleagues published a paper in the *Proceedings of the National Academy of Sciences* demonstrating the detection of mutations by extremely low doses of alpha radiation (2002). This manuscript also engendered some public response, although not nearly so strident, and much of the response had to do with the demonstration in this manuscript that caffeine inhibited repair of alpha-radiation-induced DNA damage. In this manuscript Ted warned against caffeine ingestion. Personally, Ted acted upon his findings and curtailed his intake of caffeine, something that he tried to convince me to do without any success. In that manuscript he also expressed his concept that mutation screening at low doses could be used to screen the environment for mutagenic agents, which then could be removed from the environment or protected against. He compared this to the use of sanitation to prevent infectious disease. He was fond of pointing out that sanitation was more important in reducing infectious disease deaths than the use of antibiotics.

Ted, as founder and chair of the Department of Biophysics at the University of Colorado Medical Center, instituted as one of the requirements of the Ph.D. degree that students demonstrate "some appreciation of the social, humanistic, and philosophical implications of the scientific and technological explosions which are occurring in our time" (Doctoral and Postdoctoral Training in Biophysics, University of Colorado Medical Center). This requirement was certainly still in force for many years after I arrived in Denver, and was taken quite seriously. If anything, Ted's belief in the crucial role of science in human history deepened as he grew older. He believed that the developed countries had an obligation to share the results of their scientific, and especially medical, research with emerging countries and that this would be a powerful force for world peace. Some considered this sentiment to be a bit of an overstatement, but not all. In an article in the *Economist* in 2002, "Sustaining the Poor's Development," the argument was put forward that Western leaders could contribute to helping developing countries by focusing their aid on "the issue that is still most difficult for poor countries to deal with themselves, disease" (*Economist,* Aug. 31, 2002, p. 11).

In the late 1950s and early 1960s Ted spearheaded the formation of the Eleanor Roosevelt Institute for Cancer Research. The concept was that to understand and eventually cure cancer would require a multidisciplinary approach not constrained along disciplinary lines. In this endeavor he was aided immensely by the support of Matthew Rosenhaus, president and chair of J. B. Williams Company. The name came about because the Rosenhaus and Roosevelt families had been acquainted for many years, and Matthew asked for and received Mrs. Roosevelt's permission to use her name. For many years the institute consisted of a single laboratory, Ted's. By the early 1970s Ted's success and changing conditions allowed the expansion of the institute to include other laboratories. I was one of the first of the new faculty members of the institute.

With Ted's support I also became an assistant professor in the Department of Biophysics and Genetics. As new faculty we were expected to apply for and obtain grants to fund our work, although initially we were all funded from Ted's resources. Ted suggested that I should apply for an R01 from the National Institute on Aging, which was formed in 1974. I was successful in this and received what I believe to be one of the first grants awarded by the NIA. Ted also urged me to apply for a Research Career Development Award, which was also successful. He suggested that I apply for a Basil

O'Connor starter grant from the March of Dimes. I applied for this award and was invited for an interview in Chicago. At that time I had never flown and was terrified of the prospect. At the interview I was told that I was ineligible for the award because my salary came from Ted's grant funds. I remember the flight back and landing in a raging thunderstorm at 10 at night being sure that I was going to die. I went to Ted the next day and told him about my experience, and after some thought, his solution was that I should convert the Basil O'Connor grant to an investigator-initiated research grant from the March of Dimes. I was really skeptical of this approach but went ahead, and was successful with this application as well. This would not have happened without Ted's encouragement and sage advice. So by 1974 because of Ted's encouragement and advice, I had my own well-funded laboratory at the Eleanor Roosevelt Institute and the Department of Biophysics and Genetics at the University of Colorado Medical School.

Funding was not always so abundant. I remember well Ted's attitude when the young (or sometimes not so young) faculty of the institute or department would complain about lack of funds. He consistently had two comments: "Anyone can do science with money" and "When all else fails, the Lord will provide." After hearing the first comment for several years, I finally responded, "I would like to try it that way." As I recall, I never heard that comment again. With regard to the second comment, it was usually not the Lord that provided funding when times were tight, but Ted.

Ted gave his students and fellows a remarkable degree of freedom. Some interpreted this as disinterest in their work, but my own experience was much different. Ted always had not only sage advice regarding research directions but also insights into experimental details and interpretation. Even before I had faculty status, I essentially had my own laboratory, with a great deal of freedom. Having my own lab brought some interesting responsibilities. One day in the spring of 1973 Ted introduced me to a young medical student and asked me to discuss with him the possibility of his spending a summer doing research in my lab. I thought he was very bright and so he joined the laboratory. The young man was Bob Nussbaum. Bob learned the basic methods of tissue culture and somatic cell genetics during that summer and fall. Later he described to me a lunch with Ted at Hoover's restaurant, then across the street from the lab, at which Ted explained to Bob how to derive kill curves of cultured cells based on the Poisson distribution. Bob remembers Ted saying, "You know what that is, don't you?" As Bob told me, "Of course the entire episode was rendered even more exciting by the fact that I ultimately intended to make him (Ted) my father-in-law and I realized quite early that if I flunked the Poisson distribution test, I was very likely never to attain that goal." Bob is now Holly Smith Distinguished Professor in Medicine and chief of medical genetics at the University of California, San Francisco, and the husband of Jennifer Puck, M.D., Professor, Department of Pediatrics and Institute for Human Genetics, UCSF. That summer in my lab was Bob's first experience in biomedical research, but at least as influential were his interactions with Ted, even discounting the role of future father-in-law. Incidentally, Ted was the only person I knew who could sign for lunch at Hoover's, a restaurant where we often had lunch to discuss science, education, politics, and basically any topic. Unfortunately Hoover's, long a landmark in the medical school area of Denver, no longer exists.

I certainly resonated with Bob's experience. On one of my first days in Ted's laboratory he took another postdoc and me to coffee, not at Hoover's but in the University of Colorado Medical School cafeteria. As soon as we sat down after getting our coffee, Ted started the discussion by asking, as far as I could tell out of the blue, "Do either of you know how a microwave works?" Fortunately the other postdoc answered immediately and correctly, because I had no idea. Of course, I was not courting one of Ted's daughters, so perhaps the stakes weren't so high for me.

As Ted and I grew closer I was able more and more to observe his administrative and development skills as director of the institute. It was fascinating to learn from Ted about scientific administration, leadership, and fund raising, until one day in 1978 Ted asked me to assume the responsibility of the associate directorship of the Eleanor Roosevelt Institute. I was somewhat shocked at this, having had absolutely no formal training as an administrator or fund raiser; nevertheless, I accepted, feeling that it would be an honor and privilege to work with Ted in this capacity. It was another turning point in my career. Ted arranged for me to meet many of his friends from outside the world of science, including the Roosevelts and other members of the board of the institute, including Matthew Rosenhaus, Emmett Heitler, at that time CEO of Samsonite, and others. I still maintain contacts with the Roosevelt family members, two of whom are now on the board of the institute, and with Bruce Heitler and Mattie's son Albie, although not as frequently as I would like. By 1984 I had assumed the position of president of the institute, succeeding Ted, and before him, James Roosevelt Sr., the oldest son of Eleanor and Franklin Roosevelt. In 1988 I became scientific director of the institute as well. I like to think that my assumption of these duties allowed Ted to devote himself more completely to his first love, biomedical research.

As chair of the new Department of Biophysics at the University of Colorado Medical School, one of Ted's responsibilities was teaching. As usual he threw himself into this effort and was very successful. To this day I run into people, often physicians who graduated from the medical school decades ago and who, when they learn of my relationship with Ted, comment with great enthusiasm that he was revered and inspiring as a teacher. Some of them were in Ted's first class of medical students.

Ted thought deeply about education throughout his life, both in medical schools and in other venues. In 1962 he published an article entitled "Special Responsibilities of the Medical School in View of the Biological Revolution" (Puck, 1962). The current dean of the University of Colorado at Denver School of Medicine credited Ted with helping to inspire a revision of the medical school curriculum in the early 1990s. He was deeply concerned about the teaching of science, and indeed of teaching in general, from elementary school to the postgraduate level. He was a member of the Paideia Group, organized by philosopher Mortimer Adler, which considered the state of primary (kindergarten through high school) education in the United States, and in 1984 published *The Paideia Program,* an educational syllabus in which Ted in collaboration with Donald Cowan published an essay on the teaching of science in primary and secondary school (Cowan and Puck, 1984). The ideas expressed by this group are still influential in education today. He was a member of the Editorial Board of the *Encyclopedia Britannica*. Ted continued to develop his new ideas on the state of science and medical education, and general education in the weeks before his death.

Ted loved the outdoors and the mountains of Colorado, and often spent many weeks during the summer in Aspen, where he played an active role in the Aspen Institute, originally known as the Aspen Institute for Humanistic Studies. Ted helped establish the Given Institute, originally the Given Institute for Pathobiology, a conference center in Aspen, Colorado, in 1972. It was originally dedicated to biomedical science conferences, although its mission has expanded since then. Ted organized some of the first scientific meetings at the Given Institute in Aspen and made sure that no scientific sessions were scheduled in the afternoons. During this "free" time, he organized hikes in the mountains around Aspen for the conference participants and their families. Many of the world's most renowned scientists took advantage of Ted's abilities as a hiking guide. Often the most advanced scientific ideas and hypotheses of the day were discussed and refined on these hikes. I had the good fortune to participate in many of these activities, and they were a great inspiration.

At my very first Aspen Conference, however, the tone was actually very depressing. Many of the speakers, renowned experts in their fields, seemed almost burned out, not knowing where their field was headed. Ted's response was "be bold, creative, think more broadly. This is the most exciting time in science." He never lost this attitude of infectious optimism about science. He worked with unflagging enthusiasm until the very end of his life. Only a few days before his death Ted, Sharon Graw, and I began a new collaboration based on his method to detect extremely low doses of environmental agents that can cause mutations leading to cancer. This project is completely consistent with his early, successful efforts to find the true lethal dose of radiation and to warn of its harmful effects. Ted's thoughts on this new collaboration were representative of the way he lived his entire scientific life of over 60 years: "This is the most exciting time in science. There is so much to do!"

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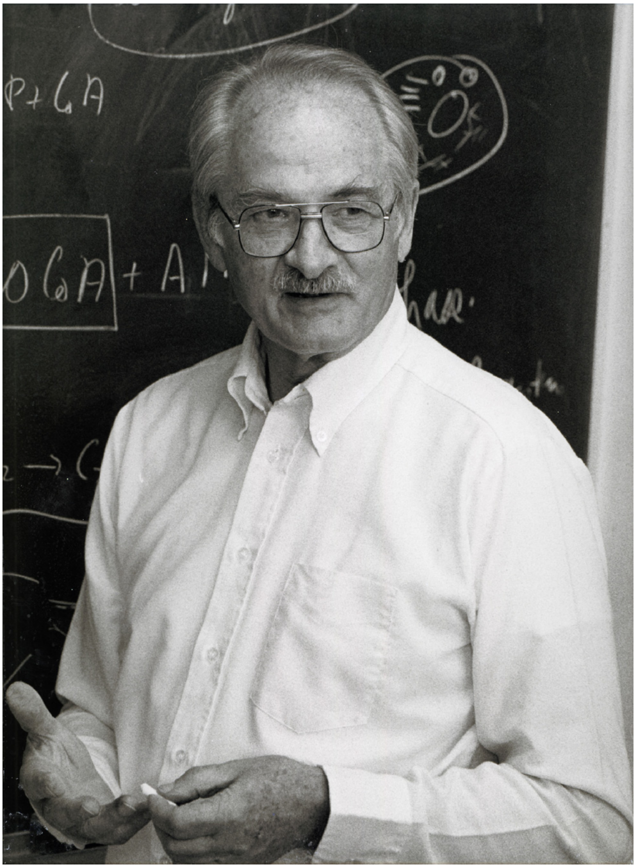

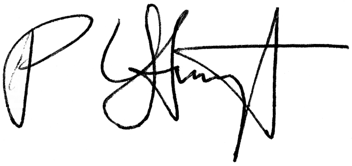

PAUL KARL STUMPF

February 23, 1919–February 10, 2007

BY ERIC E. CONN

PAUL STUMPF WAS A WORLD LEADER in the field of plant biochemistry, especially in the subject of plant lipid metabolism. Others have written,

> His research accomplishments elucidating the synthesis and metabolism of fats and lipids are too numerous to list here. His discovery in plants of the pathway for degrading fatty acids by alpha-oxidation stands as a singularly important achievement as it provided the understanding of the biochemical basis for a class of human genetic defects, including adult Refsum disease, an inability to metabolize phytanic acid. His research provided a basic understanding of fatty acid and lipid biosynthesis in plants. Paul's approach to science was perhaps reflected by his admiration of Impressionist paintings. He was interested in providing the science that formed the picture, without concern for minute detail. He saw research as Nobel Laureate Albert Szent-Gyorgyi did, whose quotation we all saw when facing Paul in his office: 'Research is seeing what others have seen, but thinking what no others have thought.' Paul was a visionary, foreseeing the impact of genetic engineering on plant research back in the 1970s, when gene cloning was still a decade in the future. Research results produced in his lab laid the foundation for genetic modification of oilseeds to alter their fatty acid composition.[1]

Paul was born in New York City on February 23, 1919, under rather tragic circumstances. Two months before Paul's birth his father, Karl S. Stumpf, committed suicide in New York City, leaving his mother without her husband, and her one-year-old child, Felix, without a father.

Paul's father was born in 1884 in the small village of Blankenberg, Germany. For many years his family had worked in the local paper mill. Because of an emerging musical talent, Karl Stumpf left Blankenberg at the age of 14 to study his chosen instrument, the clarinet. As a professional musician—his instrument was the bass clarinet—Karl was for some years a member of the Staatsoper Unter den Linden in Berlin. In 1907 Karl Muck, conductor of the Boston Symphony Orchestra, invited Paul's father, at that time only 23 years old, to join the orchestra as its bass clarinetist. His father accepted the position and together with 17 other German orchestra members became an integral part of the Boston Symphony. Karl Stumpf made frequent professional and personal trips to Germany. On October 2, 1914, en route to New York on the SS *Noordam* he met his wife-to-be, Annette Schreyer, who was a passenger on the same ship. The young couple entered into a shipboard romance, which culminated in their marriage in Boston on November 7, 1914.

When the United States entered World War I in 1917, a violent anti-German antagonism spread throughout America. One of its victims, Karl Muck, was arrested and jailed as a German spy on March 27, 1918, and by June 24, 1918, eighteen German members of the Boston Symphony Orchestra, including Karl Stumpf, were dismissed. Without an income and any hope of securing a similar position with an American orchestra, and with a growing family (Paul's brother Felix had been born on February 10, 1917, and Annette Stumpf was pregnant with Paul) his father became withdrawn and greatly depressed until on the evening of December 13, 1918, he committed suicide. Ironically, on December 14 his mother received a letter addressed to her husband from Walter Damrosch, then conductor of the New York Symphony Orchestra, inviting Karl Stumpf to join the orchestra as its bass clarinetist.

In 1920 Annette Stumpf decided to move her family to Germany in an attempt to escape from the anti-German feelings that still existed in Boston. She planned to raise her sons in the close presence of her husband's parents and the more familiar environment of postwar Germany. However, after three years in Blankenberg, she decided to return to America since she believed that the future of Germany was bleak.

Having inherited a considerable amount of money from an uncle, Annette unfortunately invested part of it in stocks prior to the disastrous collapse of the stock market in 1929. In the fall of 1930, with the remains of her inheritance, she purchased a small summer resort hotel near Bridgeton, Maine, where her sons attended the local high school, which had a total enrollment of 126 students. With little competition Paul achieved high grades in his classes. In 1932 he read *Microbe Hunters* by Paul De Kruif, a small volume of 12 chapters that briefly described the lives of such giants of microbiology as Pasteur, Koch, Lister, and other scientists of the 19th and early 20th centuries. Reading these chapters was indeed inspirational to a 13-year-old boy and led Paul to decide that he must set as a future goal an advanced degree in the biological sciences.

In the fall of 1934 Paul's brother, Felix, was accepted as a student at Harvard College in Cambridge, Massachusetts. Consequently Annette Stumpf sold the resort hotel and established a boarding house in Cambridge. After the family moved, Paul entered Cambridge High and Latin School with an enrollment of more than 4,000 students. What a contrast from a student body of 126 to one of over 4000! He survived, won a competitive Silver Medal and Science Prize in his senior year and after graduation spent one additional year at the high school to round out his education. He then entered Harvard College in 1937.

At Harvard, Paul Stumpf had three exceptional experiences. The first was taking Chemistry 2, the highly regarded organic chemistry course taught by Professor Louis Fieser, a gifted lecturer. Paul did very well in the course and also won the competitive laboratory contest called the "Martius Yellow Competition," in which the top 20 students in Fieser's lecture course had to synthesize seven compounds from α-naphthol in five hours. The second highly rewarding experience was taking Biology 3, General Physiology, taught by Professors A. Redfield and George Wald. This course gave Stumpf greater insight into the structure and functions of living systems. The third experience occurred when, as a senior honors student, Paul was required to carry out a research project.

He had become interested in the new field of enzyme chemistry although at that time Harvard College had no one who could be identified as an enzyme chemist. In the fall of 1940 Stumpf made an appointment to see Professor A. Baird Hastings, chair of the Department of Biological Chemistry at Harvard Medical School, who had published several papers in this field. At the appointment Hastings informed Paul that he no longer was active in this field, but he said that a young, bona fide enzyme chemist had just arrived from England to spend a year in the department; Hastings suggested that it would be worthwhile for Paul to meet him. Hastings took Stumpf downstairs to meet David E. Green, who was occupying a small, dark laboratory with a single wooden bench and enormous soapstone sink, essentially devoid of any equipment. After a short series of questions, Green accepted Paul and put him to work purifying a new enzyme, potato starch phosphorylase. His research resulted in a publication with Green (1942). Paul graduated from Harvard College in June 1941 with an A.B. magna cum laude.

In the fall of 1941 Green was appointed assistant professor in the Department of Medicine in the College of Physicians and Surgeons at Columbia University in New York City. Stumpf applied for graduate work in its Department of Biochemistry with the understanding that his mentor would be Green as he began his research for a doctoral degree, a study of the oxidation of pyruvic acid by *Proteus vulgaris*. At first Green and his coworkers occupied a small laboratory, but in short order he acquired another laboratory adjacent to his research space that Sarah Ratner, her technician Marian Blanchard, and Paul occupied. During the period from 1941 to 1946, Stumpf participated in research that led to eight publications, and had the good fortune to meet giants (or giants-to-be) in biochemistry: Ochoa, Lipmann, Meyerhof, Racker, Bloch, Shemin, K. Meyer, Gunsalus, Leloir, and Heidelberger.

At that time Lipmann had isolated a pyruvic dehydrogenase from *Lactobacillus delbrueckii* that oxidized pyruvic acid in the presence of inorganic phosphate to carbon dioxide and acetyl phosphate, a potentially active acetate compound. The enzyme that Stumpf had isolated possessed none of these properties since it did not require inorganic phosphate, and the end product was free acetic acid. Although Lipmann's discovery aroused considerable interest, numerous biochemists searched for similar systems in animal tissue with no success. With the discovery of the nonphosphate-requiring system in *Proteus vulgaris*, it became clear that the Lipmann system was unique to the *Lactobacillus* species.

In the spring of 1944 Stumpf was ordered to enlist in the U.S. Navy in Boston. Green immediately contacted Washington and requested that Paul be deferred since he was conducting research on an important Quartermaster Corps contract that Columbia University had with the U.S. Army. The research involved determining the action of chlorine on bacteria, a rather critical study since the routine practice in the armed forces was to add chlorine tablets to questionably safe drinking water. With only one day left before Paul had to return to Boston, Green received a telegram from the Quartermaster Corps officials in Washington deferring Paul for the rest of World War II. Thus he was able to complete his thesis and receive his Ph.D. in 1945. The research in Paul's Ph.D. thesis was entitled "Pyruvic Oxidase of *Proteus vulgaris*" (1945).

By then Stumpf was eager to establish his own research career and was exploring several opportunities, although still associated with David Green. Professor Severo Ochoa asked Stumpf to join his group, which he did not accept since it would have meant working on Ochoa's problems. Professor Fritz Lipmann also offered Paul a position in Lipmann's laboratory in Boston, but he did not accept that offer for the same reasons. A friend of Stumpf's in New York City indicated that her uncle, Professor Thomas Francis, chair of the Department of Epidemiology at the University of Michigan, Ann Arbor, was interested in appointing a biochemist. Paul contacted Professor Francis to indicate he would be interested in the opening and would initiate research in the biochemistry of virus reproduction. On July 1, 1946, Stumpf accepted Francis's offer and, as a bachelor, moved to Ann Arbor to establish his laboratory. He soon met a colleague in the department, Jonas Salk, who was carrying out research on the influenza virus at that time.

A few months after Paul arrived in Michigan, he received a letter from Professor H. A. Barker, who at that time was located in the Department of Plant Nutrition in the College of Agriculture at the University of California, Berkeley. Barker's letter explored the possibility of Paul's joining the department as a plant biochemist but noted that there was no position open at that time. Barker did inquire whether Stumpf would be interested if a position developed. Paul replied stating that he had just accepted the Michigan position and felt duty bound to remain in Ann Arbor for a year.

But, if a position did open up in Berkeley, Paul said that he would be interested in exploring the position further, in part because at that time his mother and his brother were living in San Francisco. In December 1946 Stumpf traveled to the Bay Area and met with Professors H. A. Barker and William Z. Hassid on the Berkeley campus.

In July 1947 Paul received another letter from Barker stating that a position was now available for the academic year of 1947-1948. Although his research in virus biochemistry was proceeding fairly well, Stumpf realized that the role of enzymes, as they related to viral growth and function, was very primitive at that time and that success in this field would be difficult. In the meantime Paul had met Ruth Rodenbeck, who was working for a master's degree in chemistry and was also an assistant housemother at a women's dormitory on the campus. They were married on June 13, 1947, with one of their ushers being Salk. The couple briefly struggled with the prospect of moving to Berkeley, and in September 1947 Paul informed Professor Francis of his decision to accept an assistant professorship on the Berkeley campus. Francis accepted the decision but was annoyed at Stumpf's departure from Ann Arbor. After 18 months in Michigan, the Stumpfs purchased a second-hand Plymouth and late in December 1947 left wintry Ann Arbor for the delightful climate of the Bay Area, where Paul began his career as a plant biochemist on the Berkeley campus of the University of California.

Stumpf was delighted with his new colleagues as well as the challenge of initiating a program in plant enzymology. Although he had neither a laboratory nor an office, he did receive $5,000 in start-up funds as well as bench space in Professor Barker's laboratory. Within six months of his arrival Paul completed and published a paper on the purification and characterization of pea aldolase (1948). This was the first paper in a series on glycolytic enzymes in plant tissues.

Their appearance resulted in an invitation to write his first review in *Annual Review of Plant Physiology* (1952). Nearly half a century later a former postdoctoral scholar with Paul, Curtis Givan, authored a review of glycolysis and dedicated it to Stumpf (Givan, 1999).

In 1950 James Bonner's first edition of *Plant Biochemistry* (Bonner, 1950), with its very brief chapter on lipid metabolism, made an impression on Paul. In that chapter Bonner noted that little was known about the β-oxidation of fatty acids in plants, and that nothing was known about the biosynthesis of fatty acids. He strongly urged that research be initiated to improve on the contents of this chapter in future editions. Stumpf undertook this challenge and his resulting research proved so successful that he authored the chapters on plant lipids in the second and third editions of Bonner's book.

In 1951 Professor Barker, whose research interests were in the field of microbial metabolism, accepted Eldon Newcomb (elected to the National Academy of Sciences in 1988) to do postdoctoral work in his lab. Barker soon realized that Newcomb, a Guggenheim fellow from the University of Wisconsin, was more interested in plants than microbes, and therefore suggested he work with Paul Stumpf.

During his research, Barker had accumulated a number of ^{14}C-labeled fatty acids including (1 and 3-^{14}C)-palmitic acids, which he made available to the Stumpf laboratory. Newcomb and Stumpf decided to initiate their study on lipid metabolism with a very simple experiment. Using a Warburg flask, which allowed them to trap any $^{14}CO_2$ formed, they added a homogenate of cotyledons of germinated peanut seeds to a mixture of (1-^{14}C)-palmitic acid and all the components required for the β-oxidation of fatty acids in animal tissues. A second flask, which contained only (1-^{14}C)-palmitic acid and the homogenate, was prepared as a control. After the reactions had gone to completion, the trapped carbon dioxide was recovered from both flasks and counted for radioactivity. To their great surprise and pleasure a considerable amount of $^{14}CO_2$ was found in both cups. The $^{14}CO_2$ observed in the control could not be due to β-oxidation system since the system was independent of the usual cofactors required for β-oxidation. Because the system they had discovered was unknown in the literature, this first experiment eventually led to the elucidation of what they termed an "α-oxidation system." Although the system turned out to be a minor mechanism for fatty acid oxidation in plants, 50 years later the system is now known to be involved in plant pathogen responses (Hamberg et al., 1999).

The existence of the α-oxidation system for fatty acids in humans became very important in understanding the mechanism of Refsum disease, a rare, inheritable disease in humans. Normally phytanic acid, formed by the oxidation of phytol, an alcohol component of chlorophyll found in all leaf tissues, is further oxidized by the liver to CO_2 and water. Patients with Refsum disease lack the α-oxidation enzyme system and thus accumulate high levels of phytanic acid in their serum lipids. The discovery of the α-oxidation system in plants has helped to explain the absence of this critical enzyme system in livers of patients with this disease.

In the early work on the α-oxidation system Newcomb and Stumpf observed that dialysis of the homogenate resulted in a total loss of activity but that the addition of boiled homogenate to the dialyzed system fully restored the activity. Paul Castelfranco was given the task of identifying the nature of this unknown compound as his Ph.D. thesis. He soon discovered that the unknown material was glycolic acid, a well-known organic acid in plants. Glycolic acid is rapidly oxidized to glyoxylic acid and H_2O_2 by the enzyme glycolic acid oxidase. By adding any enzyme-substrate combination that could generate H_2O_2, such as glucose and glucose oxidase, the requirement for glycolic acid disappeared, and suggested that any system that generated H_2O_2 should function in the α-oxidation process. However, the direct addition of H_2O_2 was inactive.

The final part of the story was the characterization of a possible intermediate in the system. While spending his first sabbatical leave in Bernie Horecker's laboratory at the National Institutes of Health in 1954-1955, Stumpf visited the laboratory of William McElroy at Johns Hopkins University. McElroy had studied in some detail the enzyme luciferase obtained from *Achromobacter fischeri*. In the presence of flavin phosphate and a long chain aldehyde, fluorescence occurred as a function of the concentration of the long chain aldehyde. Here then was a specific system for measuring the amount of a long chain aldehyde that might be formed in Stumpf's α-oxidation system. When this fluorescence assay was employed in the α-oxidation system, Stumpf observed that a long chain aldehyde was indeed formed. In 1974 further work on the α-oxidation of plant fatty acids by W. E. Shine in Stumpf's laboratory greatly clarified the mechanism of a rather complex series of reactions (1974).

In 1954 I was privileged to join the Berkeley Department of Agricultural Biochemistry, which now housed Paul Stumpf, H. A. ("Nook") Barker and W. Z. ("Zev") Hassid as its only members in the Biochemistry and Virus Laboratory. Nook and Zev, both members of the National Academy of Sciences (in 1952 and 1958, respectively), were wonderful, supportive colleagues but very senior to me, and so it was not surprising that Paul and I became especially good friends and colleagues. I recall one early conversation in which Paul remarked that if one continued to work in the same area that had been their thesis research, it would be difficult for outsiders to discern their accomplishments from those of their Ph.D. supervisor. While I knew that opinion was friendly advice to me as a young assistant professor in the department, it may also have been a factor in his changing his research area a few years earlier.

In 1955 when Stumpf returned from his sabbatical leave at NIH, Harry Wellman, at that time vice-president of the University of California, proposed to Paul the possibility of transferring to the Davis campus in 1958. A new building, Hoagland Hall, was under construction on the Davis campus, and he and colleagues could easily be housed in that facility. The reason for this move was an urgent need to form a new Department of Biochemistry on that growing campus. Paul discussed this possible move with his wife as well as with his colleagues in the small Department of Agricultural Biochemistry in the College of Agriculture on the Berkeley campus. Neither Barker nor Hassid indicated that they would like to move to Davis, but I was very enthusiastic about the move. It seemed an ideal environment for two plant biochemists to take root, and it offered a great opportunity to get to know faculty members in fields other than science. The campus had only 2,300 students at that time; the population of Davis was 5,000. Our experience over the subsequent years has never given us reason to regret the move.

An agreement was soon reached that both Paul and I would move to Davis by late summer of 1958 in order to offer an introductory course in biochemistry. The move was accompanied by the addition of two new faculty hires, Lloyd Ingraham from the Western Regional Research Lab in Albany, and Sterling Chaykin, who had just completed a postdoctoral position with Konrad Bloch at Harvard University.

By this time Paul had initiated his studies on the β-oxidation of long chain fatty acids that had been demonstrated early on with labeled substrates. This work then logically evolved into an examination of the biosynthesis of fatty acids in plants, including the formation of the physiologically important unsaturated fatty acids, for which there is no analogous work in animal systems. Ohlrogge and I have written:

These studies included the identification of the many component enzymes, their subcellular localization, and the discovery of the prokaryotic nature of enzymes of fatty acid synthesis and of the chloroplast acetyl-CoA carboxylase. The discovery of acyl-ACP thioesterases led to a description of "CoA track" versus "ACP track" reactions that was a conceptual precursor to the prokaryotic and eukaryotic two-pathway hypothesis that has underpinned much of modern plant lipid research.[2]

We've also written,

In addition to his fundamental research contributions, results from the Stumpf lab laid the foundation for the genetic modification of oilseeds to improve their fatty acid composition. Paul was a key early advisor and consultant for Calgene, the successful biotech company founded in Davis. Much of the early success of Calgene in transgenic modification of the fatty acid composition of canola rested on the groundbreaking characterization and purification of acyl-ACP desaturases and thioesterases that were carried out in Paul's lab.[2]

Paul has written an interesting "autobiography" describing his perspectives on his 50 years of active lipid research (Stumpf, 1994).

We have also written,

Professor Stumpf trained more than 60 students, postdocs and visiting scientists, many of whom went on to become leaders in plant biochemistry research. Throughout his career he maintained a close connection with bench work. He trained every new arrival in the lab on the use of the gas chromatographs and their radioisotope detectors, and when an instrument needed maintenance, Paul provided hands-on repairs. He was also creatively engaged in each research project, making many suggestions for experiments, while allowing students and postdocs the freedom to follow their own intuition. Many of those who trained with Paul have fond memories of the atmosphere in the Stumpf lab as an excellent place to do science, and of the relaxed social interactions that included trips to the Stumpf cabin near Lake Tahoe.[2]

Turning from his research, Professor Stumpf was a highly respected citizen of the UC Davis community, and a strong advocate for faculty governance at the University of California. He served on and chaired numerous academic senate committees, including the Committee on Committees, the Committee on Privilege and Tenure, the Academic Freedom Committee, and the Budget Committee. Paul was especially interested in campus planning and served many years on the Academic Senate Campus Planning Committee and the Physical Planning Advisory Committee under Chancellor Mrak. With an extensive collection of photographs, Paul carefully documented the buildup of the campus from a student enrollment of 2,300, when he came to Davis in 1959, to over 19,000 at the time of his retirement in 1984. Paul was also the founding president of the UC Davis Emeriti Association, chair of the UC Davis Academic Senate Emeriti Committee, and secretary of the Council of University Emeriti Associates. Paul also served on numerous review and advisory panels for the National Institutes of Health, the National Science Foundation, and the U.S. Department of Agriculture.

Paul and I coauthored five editions of our popular textbook *Outlines of Biochemistry* from 1963 to 1986. The use of Outlines in the name of our text was a follow-up from another textbook, *Outlines of Enzyme Chemistry*, that he had coauthored with John B. Neilands in Berkeley, which ran for two editions. Both were published with John Wiley and Sons. Paul and I also coedited the 16-volume treatise of *Biochemistry of Plants* (Stumpf and Conn, 1980-1990). He frequently cited this treatise as an example of the rapid growth of the literature in plant biochemistry over the 30 years since James Bonner's first edition of *Plant Biochemistry* appeared in 1950.

Professor Stumpf was elected to membership in the National Academy of Sciences in 1978 and would sometimes recall the way he was informed of this honor. He was lecturing to approximately 450 students in general biochemistry on a Tuesday in late April 1978. Just before going to class his secretary reported that someone was trying to reach him by telephone. He said that he would return the call after class and hurried over to the classroom. He had begun his lecture when Professor Emanuel Epstein entered the classroom and asked permission to make an announcement. Paul gave him permission and Emanuel announced that Paul had just been elected to the National Academy of Sciences. Whereupon Paul responded by saying, "Well, I'll be ______," much to the delight of his students. Only later did Paul learn that Professor Epstein had also been elected that morning.

Paul Stumpf served as president of the American Society of Plant Physiologists in 1980 and chaired its Board of Trustees from 1986 to 1990. He had earlier received the Stephen Hales Prize from the society in 1974. Then in 1992 he was awarded the Charles Reid Barnes Life Membership by the society. Another award was the Lipid Chemistry Prize from the American Oil Chemists Society in 1974. His sabbaticals were facilitated by a Senior Scientist Award from the Alexander von Humboldt Foundation of Germany (in 1976) and two Guggenheim Foundation Fellowships (in 1962 and 1969). Paul was elected to the Royal Danish Academy of Sciences in 1975.

A few years after Paul Stumpf retired, the Board on Agriculture of the National Research Council issued a report in which it analyzed the persistent inadequate funding of research in the agricultural sciences and recommended that Congress greatly increase its annual appropriation from $40 million to $500 million. J. Patrick Jordan, administrator of the Competitive State Research Services of the U.S. Department of Agriculture, asked Paul to help develop its existing Competitive Grants Program into the National Research Initiative Competitive Grants Program. In 1988 Paul was appointed chief scientist responsible for the conversion. (Although this appointment was normally for one year only, Stumpf was reappointed two more times and served until 1991.) The Board on Agriculture report was supported by the George H. W. Bush administration and enacted into law by Congress in September 1990. It then became Paul's responsibility to oversee the implementation of this report and its new funding in 1990 to the level of $70 million, and to $100 million in 1991. Unfortunately, even now the recommended support of $500 million has not been achieved.

During those three years in Washington, Stumpf thoroughly enjoyed the drastic change from being an academic to heading an important federal organization, which provided research funds for many agricultural scientists. Besides dealing with scientists and solving minor problems that would arise in the program, Paul also had the responsibility of explaining the programs to members of Congress. He frequently commented to his colleagues in Davis that for a significant number of Congress persons, their efforts were mainly divided between funneling funds to their districts and getting reelected. On one occasion Paul made a courtesy call to a powerful Congressman from Massachusetts and during their conversation the Congressman asked what district Paul was from in Massachusetts. When Stumpf replied that he was not from Massachusetts but from California, the courtesy call ended abruptly.

Ohrogge and I have noted:

Twenty two years of retirement permitted the Stumpfs to enjoy numerous trips around the world. They loved to travel and participated in approximately 50 Elderhostel programs, including one to Antarctica. Golf bags were frequently packed on these trips, as Paul had what one daughter has described as a 'hate-love' relationship with that sport.[2]

In order to support education and research in the department he had founded, he and his wife, Ruth, endowed the Paul K. and Ruth R. Stumpf Professorship in Plant Biochemistry in the Section of Molecular and Cellular Biology at UC Davis in 1999.

Paul Stumpf died February 10, 2007, at his home in the University Retirement Community at Davis. He was 87 and had been ill for some time. He is survived by his wife of 59 years, Ruth Stumpf, five children, and their spouses: Ann Shaw (Michael), Kathryn Fruh (Bill), Margaret Noonan (Mark), David Stumpf (Susan), and Richard Stumpf (Patrice), 11 grandchildren, and one great grandchild.

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2. E. E. Conn and J. B. Ohlrogge. Paul Karl Stumpf obituary. *Am. Soc. Plant Biol. Newsl.* 34(3)(2007):23-24.

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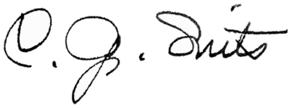

CHAUNCEY GUY SUITS

March 12, 1905–August 14, 1991

BY ROBERT J. SCULLY AND MARLAN O. SCULLY

GUY SUITS, DECORATED SCIENTIST AND inspired administrator, is proof that giants once strode the earth. Here is a man who put the stamp of excellence on everything he touched. He cared deeply about science and his fellow man. He helped us all to be better than our best.

In the present era of "How does it impact the quarterly earnings?" it is important to pause and consider one who always kept the big picture in focus and in so doing helped his company and his country become more prosperous and more productive. As the chief scientific executive officer of General Electric (GE), Guy put many far-reaching programs in motion. For example, one of us (M.O.S.) had the privilege of interacting with Guy Suits as a General Electric-Rensselaer Polytechnic Institute (RPI) scholarship student. Indeed, during the early 1960s, GE had many special programs in which students from around the country were brought to RPI on generous scholarships.

M.O.S. recalls with pleasure a conversation with Guy at that time. As a naïve young student M.O.S. asked, "How does GE justify spending so much money on undergraduate scholarships?" Guy's reply was memorable: "We draw from the body of knowledge and we feel we should add back in like measure."

The scientific community is indeed fortunate to have had such a truly unique and high-minded leader as C. Guy Suits. In addition to being an eminent scientist, Guy embodied commitment and dedication to excellence and leadership. Under Guy Suits direction the different units of the company paid into a central pool such that approximately two-thirds of the GE research lab budget was funded by internal money with only one-third coming from external grants and contracts. Unfortunately, over the next decades the ratio of internal to external research funding went from roughly two to more like one-half. Fortunately, the ratio is now heading back in the direction of the Guy Suits paradigm, with more of the research funding provided internally.

In life one's values determine one's actions (i.e., our response to stimuli is conditioned by our training and background). Science in some ways is just the opposite. The process of discovery, invention, and development naturally result in insights and technologies that redefine what is important. So it is that scientists play a vital role in shaping our society. It is thus important to analyze and study the lives of leaders who made the United States the scientific powerhouse that it became after World War II.

Guy was born in Oshkosh, Wisconsin, the son of a pharmacist. He grew up around Medford, Wisconsin, and completed his schooling there. During his graduate work, Guy worked as a consultant for the U.S. Forest Products Laboratory. One of his biggest contributions there was the development of new methods for measuring the moisture content of wood. Guy's innovative achievements began turning heads, and in 1929 he was awarded an exchange fellowship at the Swiss Federal Institute of Technology. He found the requirements for graduate work to be somewhat lax in Europe. To earn a Ph.D. degree at that time one needed only to produce a research work and pass a verbal exam. He earned his doctor of science degree there in 1929 but returned to the University of Wisconsin to further his training. The summer of the same year he worked for GE as an intern, returning early in 1930 to begin his lifelong career at GE. Guy began work as an assistant to Albert Hull, the inventor of the first electron tube. Hull had an undergraduate degree in Greek, and he gave Greek names to the electron tubes he invented, the Thyratron tube being the most famous.

General Electric has always been fond of saying, "Our people are our most important product," a motto they still adhere to. Obviously, they've benefited from this, perhaps never more so than when they hired the young Mr. Suits, who at the age of 39 became GE's youngest officer. The nature of his experiments, discoveries, and patents were always innovative and useful. During his years as director of research at GE, Guy led teams who developed a literal treasure chest of technologies. One such example was the first engineered diamonds as well as a patented process for their large-scale production. He later guided his research teams in the development of Borazon® (cubic boron nitride), a synthetic material nearly as hard as diamond. Another "home run" was the multivapor lamp, today's leading high-efficiency light source. On the chemical side of things, Lexan® and Noryl® resins were developed. These last two are world-class polymers with excellent resistance to heat, cold, and water penetration. They have replaced many metals in today's products and comprise everything from compact disks to motorcycle helmets. They are increasingly being used in computers, surgical instruments, and automobiles.

When Guy began his career at GE he was interested in the emerging field of electronics. The word "electronics" was just becoming a household word, and the company was making basic contributions to its study. He was particularly interested in the electric arc and high-temperature plasma phenomena, today the backbone of any industrial assembly plant. As with many effective leaders and developers, he broke down the complicated into simple terms. He described electrical arcs as follows:

> Electrical machinery, as a manner of speaking, is made up of copper wires, electric fields that form coils, circuit breakers, and the like. The principal unknown was what went on in the circuit breaker, involving electric arcs. The engineers in the company were very pleased when I got into this field, because it was a field where they had felt very baffled and completely lacking in any sense of design organization in phenomena involving electric arcs.

Suits eventually acquired 79 patents in his name. His experiments included high-temperature arcs in which arc temperatures of 18,000 degrees Fahrenheit were achieved. And he invented original methods of measuring those temperatures. Industry today uses much of the comprehensive theory developed by Guy Suits in the understanding of switch, light, and welding arc applications.

He enjoyed his first 10 years with the company as a researcher, but GE is a company with demand for the unique resource of good leadership. Guy found this out in 1940 when he was made an assistant director of research. As with most highly qualified managers, management was the last thing he wanted to get into. He even said that it was the remotest thing from his mind.

World War II interrupted the course of research and development for Guy. The company assigned him permanently to the Washington office of war-related R&D in the spring of 1942. We were jumping into the war effort with both feet. Every individual was expected to contribute, and Suits contributed mightily, leading a massive U.S. effort on radio and radar countermeasures. Experts later determined that those countermeasures (jamming) saved some 450 planes and 4,500 lives. In addition, a $2 billion Axis radar system was rendered ineffective.

During the war, Guy moved further away from research and went more heavily into management. He was on the run most of the time, coordinating the efforts of 30 GE laboratories, which were deliberately spread out to increase competition among fields of opposing interests. For example, radar measures and radar countermeasures are competitive fields. Having them work under the same roof would have been less productive, as competition would have been unlikely.

That security restrictions during the war were stringent could not be argued. Most scientists felt they were too stringent. Despite his frustrations he stayed focused on prioritizing his teams' efforts to meet Allied demands. In the face of almost zero information exchange, as well as the difficulty of obtaining accurate details of the technology needed at the front, Suits was able to stay focused on the needs of the military.

This was accomplished by holding meetings with a group of naval, air force, and army officers who could tell him what was happening at the front. Such pressing times made for lifelong camaraderie. For the rest of his life the group held annual reunions, a testament to Suits's belief in the development and training of personnel as the best way to advance any organization. In fact, upon his return to duties at GE he felt that the greatest asset he brought back to the company was his acquaintance with some very capable people.

At war's end Guy oversaw the postwar expansion of GE's powerful research arm. A 600-acre site was selected in Niskayuna, New York, as the home for a new research laboratory. Not only would Guy be the new director of research, but the director of the lab as well. He insisted on impeccable design and construction features. The result of this today is that the facility is still an excellent working laboratory.

As Guy made an impression on the international scientific community, his list of awards and honorary memberships grew large enough to constitute a report in itself. To name a few: in 1940 he was chosen to be one of the outstanding young men in Durward Howes's "America's Young Men." He was elected to the National Academy of Sciences in 1946 and the National Academy of Engineering in 1964. In 1958 he was awarded the Procter Prize of the Scientific Research Society of America. Always an advocate for improving on the metallurgical skills in heavy industry, Guy received the Industrial Research Institute Medal as its highest honor for contributions to the management of industrial scientific research. He was awarded the Advancement of Research Medal of the American Society for Metals in 1966, followed by the Frederik Philips Award from the IEEE in 1974, the King's Medal for Service in the Cause of Freedom (U.K.), and the Presidential Medal for Merit (U.S.).

Guy retired in 1965. He remained active in numerous scientific and research societies, never losing his drive for invention and progress. He designed skis and boomerangs, made his own furniture, and rewove oriental rugs. His house was largely decorated and furnished with his own creations—just what one might expect from an inventor. He was a self-taught clarinetist, a pilot, a skin diver, a hunter, and an exceptional photographer who made his own beautiful leather camera cases.

From an engineering standpoint Guy was never one to be satisfied with the most conventionally accepted technologies. From skis to boomerangs he always envisioned ways to improve even on the most traditional developments. This engineering creativity extended to telescopic rifle sights, which he found to be inefficient and unreliable—so he built his own. But such ambition did not keep him from being down-to-earth and approachable. Early in his marriage he often had to assist his wife with cooking instructions. His son recalls that once his mother called Guy to ask how long she should boil potatoes. To this he responded sharply, "What do you mean, 'How long do you boil potatoes?' You boil them till they're done." Surely there was much wit and charm in a family as creative as that of Guy Suits's. When he passed away that family included his wife, Laura; sons, David and James; five grandchildren; one great-grandchild; and several nieces and nephews.

Guy Suits's great contributions were not only what he did for scientists but the general population as well. The life of Guy Suits is a beacon illuminating the attributes of a successful life, for him they featured:

- Loyalty—He stayed with one employer his entire career.
- Selflessness—He sought to better the lot of peers, his subordinates, and the world's future scientists.
- Dedication—His love of science did not blind him to other duties. When his country called, he answered, saving thousands of lives.
- Success—He was a highly regarded scientist who rapidly rose through the ranks of a very competitive company.

Here indeed was a giant of a man.

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ROGER JOHN WILLIAMS

August 14, 1893–February 20, 1988

BY DONALD R. DAVIS, MARVIN L. HACKERT,
AND LESTER J. REED

ROGER J. WILLIAMS WAS A NOTED organic chemist who became an internationally acclaimed biochemist and a pioneer in the study of vitamins, promoting the importance of nutrition and the concept of biochemical individuality. Roger and his students and collaborators discovered or characterized the B vitamins pantothenic acid, folic acid, pyridoxal and pyridoxamine (forms of vitamin B_6), as well as lipoic acid and avidin. They also developed a commercial synthesis of vitamin B_{12}, and did pioneering work on inositol. Roger was the author or coauthor of nearly 300 articles and 21 books. He devoted much of his later years to educating the public about the benefits of complete and proper nutrition to good health, presented in such popular books as *Nutrition Against Disease*, *Biochemical Individuality*, *The Wonderful World Within You*, and *The Prevention of Alcoholism.*

Roger received his bachelor's degree from the University of Redlands in 1914, and master's and Ph.D. degrees from the University of Chicago in 1918 and 1919, respectively. After several years at the University of Oregon and Oregon State College, he joined the University of Texas at Austin in 1939. There he founded the Biochemical Institute in 1940 with funding provided by Benjamin Clayton and the Clayton Foundation. He believed that the best approach to understanding biochemical issues related to health and disease would come from elucidating the biochemistry of normal cells. Toward this goal he fostered the efforts and collaborations of individual investigators to succeed in their particular areas and contributed to the fundamental understanding of such substances as amino acids, proteins, enzymes, vitamins, and nucleic acids. He was elected to the National Academy of Sciences in 1946 and was president of the American Chemical Society in 1957. He died on February 20, 1988, at the age of 94.

PERSONAL HISTORY

Roger John Williams was born on August 14, 1893, in Ootacumund, India, as the youngest of six children of American missionary parents. His parents brought him to the United States at two years of age by ship across the Pacific. After a brief stop in Oakland, California, his father acquired an 800 acre ranch in Greenwood County, Kansas, about 12 miles outside Eureka. At four years of age Roger attended a one-room country school about 2 miles from their home. The family soon moved to California, where his father took a small pastorate at Otay, near the Mexican border. They moved back to their ranch about four years later, and eventually they settled in nearby Ottawa, Kansas.

In school Roger was a good student. It was discovered later that he had severe aniseikonia, an eye condition that caused eyestrain whenever he read for more than a short time. Aniseikonia was unknown until about 1930. His condition was corrected with only moderate success by special glasses in 1941, but by that time he was already nearly 50 years old. Rather than feel sorry about his condition, Roger would state that his affliction gave him more time to think when others might be reading.

Roger's interest in chemistry was due to the influence of his oldest brother, Robert, whom he always admired and who also became a member of the National Academy of Sciences. Robert, who was eight years older than Roger, saw firsthand the effect of nutritional deficiency diseases while growing up in India. In 1910 when Roger was just 17 years of age, Robert learned that a syrup made from rice polishings could cure beriberi. Twenty-six years later Robert isolated and synthesized vitamin B_1, which he named thiamine.

During Roger's undergraduate years focused on chemistry, he also considered a future in medicine, the ministry, and writing. As a junior in college he was elected editor of the monthly college publication. His talents as a future author were recognized in his senior year, when he won a $20 essay prize that was open to all students. Although he had the "writing bug," he had the practical sense to know that writing would probably not afford him a decent livelihood.

Roger received his B.S. degree from the University of Redlands in 1914 and a high school teacher's certificate in 1915 from the University of California, Berkeley. However, his first job offer out of college was as the foreman of a small nursery. He was qualified for the nursery job because he had worked as a "nursery rat" while living in Ottawa. He learned to bud deciduous fruit trees; two thousand young trees per 10-hour day to earn one dollar. However, at the last minute a teaching job opened up at Hollister, California. Roger would later refer to this as "the hardest work I ever did." He taught three subjects—chemistry, physics, and general science—besides acting as a guardian and referee in several study halls. He came home thoroughly exhausted each evening.

During his first year of teaching, on August 1, 1916, he married his college sweetheart, Hazel Elizabeth Wood. They were married for 35 years, until her death in 1952.

They had three children: Roger John Williams Jr. (b. 1918), Janet Elizabeth Williams (b. 1920), and Arnold Eugene Williams (b. 1927). They decided to make a break after his next year of teaching and continue his graduate work at the University of Chicago, where his three older brothers had graduated. At Chicago, Roger met Prof. Julius Stieglitz, who greatly influenced his thinking in the field of organic chemistry. His undergraduate studies in organic chemistry left him discouraged about his potential as a chemist. He would later say that Stieglitz "lifted organic chemistry out of the hopeless state (for me) of being merely something to memorize" to exciting science. This epiphany led nine years later to Roger's first textbook on organic chemistry. It was ultimately adopted by nearly three hundred universities and colleges and published in five editions.

Roger was awarded his M.S. degree in 1918 and his Ph.D. (magna cum laude) in 1919 for his work in biochemistry with F. C. Koch. His thesis was titled "The Vitamine Requirement of Yeast," and it attracted more than an average amount of attention. Roger would later say that he was always very glad that he was attracted to the rich field of biochemical investigation at that time. After the University of Chicago, he worked as a research chemist for the Fleischmann Co. He then took an academic position as an assistant professor in 1920 at the University of Oregon, rising in the ranks to professor in 1928. From 1932 to 1939 he was a professor at Oregon State College.

During his time in Oregon, he discovered and characterized pantothenic acid. This proved to be a major scientific contribution. Patents on its synthesis were given to Research Corporation. Royalties amounted to many hundreds of thousands of dollars per year, a substantial portion of which was ploughed back into scientific research each year. Roger felt it was fortunate when investigators could concentrate on their scientific work and forget about "the million dollars" their contributions were going to yield.

In Oregon, Roger learned to trout-fish. He spoke fondly of wading in clear mountain streams, even if there were no fish to catch. Music was another important aspect of Roger's life. He had absolute pitch and could tune and play the violin or piano by ear. While at Oregon, Roger also took up golf and found that it suited his temperament well. He recalled that the first time he broke 80 on a regulation golf course was on Friday, the 13th of September 1940. On that memorable day the Clayton Foundation began support of his work at the University of Texas at Austin. He shot a 76 that day.

As noted earlier, in 1939 Roger accepted a professorship in the Department of Chemistry at the University of Texas at Austin. The following year, with the support of Benjamin Clayton, he founded the Biochemical Institute (later named the Clayton Foundation Biochemical Institute) and served as its director for 23 years. After Hazel died in 1952, Roger married Mabel Phyllis (Foote) Hobson the following year. Phyllis had been widowed in 1950 and had one son, John Wallace Hobson (b. 1929). Roger and Phyllis traveled extensively nationally and internationally. Even after 80 years of age, he presented over 80 lectures and seminars on his concepts of biochemical individuality and the importance of nutrition in health and medicine. Phyllis Williams died in June 2004.

VITAMINS AND NUTRITION

In his early research work at the University of Chicago, University of Oregon, and Oregon State University, Roger developed the methodology for the quantitative determinations of chemicals essential for the growth of yeast. Of greater importance was his concept of the universality of the basic biochemistry of all living organisms. This concept led him to apply his research on the nutritional requirements of yeast to the search for vitamins, which were then unknown animal nutritional factors being related to human disease. He pursued his research with yeast despite attempts by outstanding biochemists of that period to persuade him to work with animals. This persistence led to his discovery of the yeast growth factor, pantothenic acid, which he announced in 1933, and led to the subsequent acceptance of this compound as an essential cog in the biochemical machinery of all cells and as a vitamin for many species. The consequence of this work, and related work of one other laboratory, was that microbial systems attained a leading role in the discovery of new nutritional factors as well as in the development of biochemical genetics and intermediary metabolism.

This major transition to the use of microbial systems in biochemistry was occurring at the time Roger moved to the University of Texas at Austin. In developing his new Biochemical Institute he provided a working atmosphere in which young scientists and faculty associated with the institute had the opportunity to develop their full potential. Some of his early colleagues in the institute were Robert Eakin, Esmond Snell, William Shive, and Lester Reed. The success of the Biochemical Institute and the faculty associated with it is evidenced by the awards and honors bestowed upon them, and a legacy of vitamins and biochemicals whose names were created by them (e.g., pyridoxal, pyridoxamine, folic acid, lipoic acid).

Roger was highly regarded not only for his intuitive concepts but also for his ability to communicate. An interesting sidelight with respect to his scientific work was his flair for coining words. He had written successful textbooks in organic and biochemistry courses. When new names or words were needed, he simply created them. He had named his new vitamin pantothenic acid (Greek *pantothen*, from all sides). He continued with folic acid, the vitamin isolated from spinach leaves (Latin *folium*, leaf), and avidin, the egg-white protein that tenaciously binds to biotin (Latin *avidus*, to covet). Other words created by Roger for concepts are nutrilite, isotelic, propetology, and genetotrophic. The word nutrilite came to him in a dream. Roger and his brother were discussing (in his dream) the need for a word comparable to vitamin that would apply in plant as well as animal physiology. In his dream he proposed the word nutrilite. In the dream his older brother Robert agreed that it sounded good, and with his dream approval, Roger published the suggestion in *Science* (67[1928]:607).

Honors and awards accorded Roger include the Mead-Johnson Award of the American Institute of Nutrition (1941), Chandler Medal from Columbia University (1942), Southwest Regional Award of the American Chemical Society (1950), Arthur M. Sackler Foundation Award for Nutrition (1983), and honorary D.Sc. degrees from the University of Redlands (1934), Columbia University (1942), and Oregon State University (1956). Roger Williams was a member of numerous scientific organizations, including the National Academy of Sciences, American Chemical Society (president, 1957), American Society of Biological Chemists, American Institute of Nutrition, Biochemical Society of London, American Association for Cancer Research, American Association for the Advancement of Science (fellow), Society of Experimental Biology and Medicine, and American Association of Clinical Chemists. He was a member of many honor and professional groups, such as Phi Beta Kappa (honorary), Phi Kappa Phi, Phi Lambda Upsilon, Alpha Chi Sigma, and Sigma Xi. He served on many editorial and advisory boards and committees, such as the Food and Nutrition Board of the National Research Council (1949-1953); Committee on Problems of Alcohol; President's Advisory Panel on Heart Disease (1972);

and Editorial Board, Archives of Biochemistry, as well as on research and medical advisory boards or committees of the National Multiple Sclerosis Society, Muscular Dystrophy Association of America, and the American Cancer Society. He was one of the founders (with Albert Szent-Gyorgyi, Linus Pauling, and Arthur M. Sackler) of the Foundation for Nutritional Advancement.

LASTING CONTRIBUTIONS

Although many will remember Roger J. Williams as the discoverer of pantothenic acid and as a contributor to knowledge about the B vitamins generally, Roger himself felt that his most important and far-reaching contributions were embodied in his books *The Human Frontier*, *Free and Unequal*, and *Biochemical Individuality: The Basis for the Genetotrophic Concept*.

Roger's concepts of biochemical individuality began to develop when postoperative morphine had an "opposite-than-expected" effect on him. His initial scientific studies along this line were concerned with individual differences in the taste of creatine, a work published in 1928. Later work in this area demonstrated the broad role of inborn differences in creating unique biochemical individuality. In several books he stressed that these inborn individual differences are widespread, of varying magnitude, and are crucial to the understanding and solving of most human problems: *The Human Frontier*, 1946; *Free and Unequal*, 1953; *Biochemical Individuality*, 1956; *You Are Extraordinary*, 1967; and *Rethinking Education: The Coming Age of Enlightenment*, 1986.

Roger was a leading advocate of the role of biochemical individuality in various disease processes and of the merit of using nutritional science in both traditional medicine and preventive medicine: *What To Do About Vitamins*, 1945; *Alcoholism: The Nutritional Approach*, 1959; *Nutrition in a Nutshell*, 1962; *Nutrition Against Disease: Environmental Prevention*, 1971; *Physician's Handbook of Nutritional Science*, 1975; *The Wonderful World Within You*, 1977; and *The Prevention of Alcoholism Through Nutrition*, 1981.

His concepts have received recognition throughout the world, and several of his books have been translated into other languages. His ideas helped inspire the founding in 1975 of a medical clinic in Wichita, Kansas—Center for the Improvement of Human Functioning International—where all new patients receive a copy of *The Wonderful World Within You*.

His basic idea was that human differences (differences between individual human beings) are widespread, often of great magnitude, and demand careful and extended study and attention, in order that human understanding may progress and better human relations be accomplished. These differences, he contended, are politically extremely important because they are the basis for our love of freedom. Their appreciation is essential to goodwill, tolerance, human communication, and human understanding. In medicine, preventive medicine and psychology are important because many medical and other human problems can never be solved until these differences are carefully studied and fully appreciated.

Roger felt strongly that biochemistry was destined to play the crucial role in ushering in a new era of human understanding. He believed that the many measurable biochemical differences between individuals will prove to be the keys to the solution of a host of human problems. Once the principle was established that genetically determined human differences are highly important and required study, he believed the results of such studies would carry conviction and that the resulting momentum would guarantee that these subjects would be pursued with vigor. These differences (biochemical and otherwise) are substantial. He felt that medicine had been inordinately concerned with how "the human body" functions, and had paid very little attention to the striking genetic and individual metabolic pattern differences.

Roger, in summing up his scientific work, recognized that it was of a diverse nature and called himself one of those specialists who specialize in having broad interests. He listed biochemistry, genetics, pharmacology, nutrition, psychology, anthropology, social science, and medicine among his broad interests. He also listed other things of great importance: his family, his friendships, his colleagues, his students, religion, and enjoyment of music, shows and other recreations, fly fishing, golf and card playing. Roger was an accomplished storyteller and the anecdotes he loved to tell will be long remembered. He felt that it was appropriate for a scientist to attempt to understand the whole world. His intuitive ideas and concepts broadened the frontiers of science and education, and the rich legacy of his writings, from which there is still much to be gleaned, will long endure.

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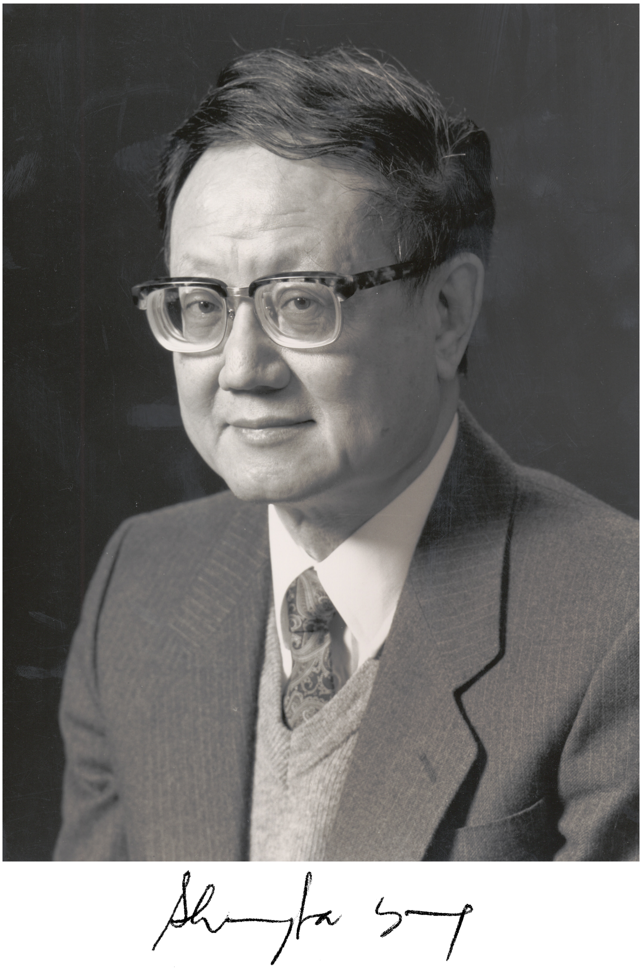

SHANG FA YANG

November 10, 1932–February 12, 2007

BY KENT J. BRADFORD

SHANG FA YANG WILL BE REMEMBERED as the plant biochemist who elucidated the pathway for the biological synthesis of ethylene, a plant growth-regulating hormone. Ethylene, a simple compound containing two carbon and four hydrogen atoms, was known already in the early 20th century to elicit abnormal growth in plants and to hasten the ripening of fruits. This remained somewhat a curiosity associated with leaking gas mains until it was shown in the 1930s that plants produce ethylene and that it is broadly involved in regulating plant growth and development, including seed germination, root and shoot growth, responses to environmental stresses, flowering, fruit ripening, and senescence or death of plant tissues and organs. The fact that ethylene is a gaseous compound made it unique among plant hormones, and it was of considerable interest to understand the mechanism and biochemical pathways by which plants produced the compound.

At the time Yang entered the field there was some evidence that the amino acid methionine could be a precursor for ethylene production, and various in vitro systems were being explored to convert this and other potential precursors into ethylene. Yang contributed significantly to these studies, using his knowledge of chemistry to explore different reaction mechanisms. Eventually, however, it was in vivo studies that led to the breakthrough when Yang's group supplied apple fruit tissues with ^{14}C-labeled methionine and observed that a labeled compound accumulated under anaerobiosis, a condition that prevents ethylene synthesis. They identified the compound as 1-aminocyclopropane-1-carboxylic acid, or ACC, and showed that it could be readily converted to ethylene by plant tissues under aerobic conditions. This discovery led to the identification of the enzymes responsible for production of ACC from S-adenosylmethionine (SAM) and for conversion of ACC to ethylene, their eventual cloning and characterization, and opportunities for the genetic and chemical manipulation of ethylene biosynthesis and action in plants. Yang also discovered that the methylthioribose group from SAM is recycled back into methionine formation following ACC synthesis to sustain high rates of ethylene production, which has been termed the "Yang Cycle." In addition, he studied cyanide metabolism (after discovering that cyanide is a by-product of ethylene biosynthesis) and the effects of sulfur dioxide on plant cellular processes.

The elucidation of the ethylene biosynthetic pathway and the enzymes involved stimulated many studies of how this pathway is regulated in plants by both internal and external factors. The specific roles of different gene family members encoding the ethylene biosynthetic enzymes have been elucidated for a number of important plant growth stages. Yang's work focused attention on ethylene in plant biology, subsequently resulting in the first identification of a plant hormone receptor and a detailed understanding of the molecular signaling pathways by which ethylene is perceived in plant cells. Yang also contributed to the development of ethylene-releasing compounds that allow the convenient application of ethylene to promote fruit ripening or to facilitate harvest. He helped to develop chemical antagonists that block ethylene action and are now used commercially to extend the life of cut flowers. This dual focus exemplifies Shang Fa Yang's scientific contributions: rigorous studies of fundamental mechanisms in plant biochemistry and the simultaneous application of that knowledge to solve problems in crop production and storage.

Shang Fa Yang did not know the exact date of his birth in Taiwan but deduced that it was in late October or early November of 1932; he celebrated his birthday on November 10. He was the youngest child of a large family (five elder brothers and six elder sisters). His father, Chian-Zuai Yang, was a businessman involved in the production of maltose from sugarcane in Tainan City, but the factory was sold in 1936 and his father became a miller. Yang entered primary school when he was seven years old and began to learn Japanese, as Taiwan was under Japanese control at that time. Every morning, students assembled for a speech by the Japanese head of the school, who exhorted them to grow strong to beat the Americans. Yang's mother died when he was in the fifth grade, and his third eldest sister took care of him afterward. Near the end of 1944 American aircraft began to bomb Taiwan and air raid alarms were frequently heard. Yang noted that he particularly remembered March 1, 1945, when the city was bombed and houses were burning. He took refuge in a bomb shelter with friends and emerged later to discover that a 500-kg bomb had created a crater only about 500 meters from their shelter. He considered himself very lucky to have escaped injury. His family then moved to the countryside but returned to Tainan City after the Japanese surrender in August 1945. However, life was very difficult due to food shortages. Rice was expensive and hard to find, and their main food was radishes. He resumed middle school in 1946, still using Japanese and starting to learn Chinese. Armed soldiers from mainland China were in the streets and the situation in Taiwan remained dangerous for several more years. When he was in senior high school, Yang also lost his father.

Yang entered National Taiwan University in 1952 and studied agricultural chemistry because he wanted to help the farmers and thus promote agriculture in Taiwan. Although food remained in short supply and his nutrition was poor, Yang was active as a leader of student organizations. He received his B.S. degree in 1956 after writing a 40-page research report in English, and subsequently completed an M.S. degree, also in agricultural chemistry from the National Taiwan University, in 1958. Although funding for research was limited, Yang was highly motivated and initiated independent studies with the support of his professors. When he discussed his research with his colleagues and professors, he used a mixed language of Japanese, Taiwanese, and Mandarin. Reference books from Japan were much cheaper than the books from the United States, so he read many biochemistry journals in Japanese. Yang remained sufficiently fluent to present seminars in Japanese when he was invited to Japan in his later years. He also was asked to learn German before he got his M.S. degree. He studied hard and passed his exam, but did not claim to be good in German. He planned to study abroad, and in consequence was also required to undertake four months of basic army training before being allowed to leave.

Yang received a scholarship to study in the United States, where he attended Utah State University and worked with G. W. Miller on the effects of fluoride on plant biochemistry and metabolism (Yang and Miller, 1963), receiving his Ph.D. in 1962 in plant biochemistry. He then pursued postdoctoral research with Paul K. Stumpf at the University of California, Davis, where he studied lipid biosynthesis in avocado fruits (Yang and Stumpf, 1965). He then accepted a postdoctoral fellowship with B. N. LaDu at New York University Medical School in 1963-1964 but determined that he did not like working with animals or living in the city. However, not all was lost, as he met Eleanor Liu, an accounting student at NYU, who subsequently became his wife in September 1965. He returned to California and to plant biochemistry in 1964 as a postdoctoral scientist with Andrew A. Benson at Scripps Institution of Oceanography in La Jolla. In 1966 Yang was hired as an assistant biochemist in the Department of Vegetable Crops at the University of California, Davis, and immediately began studies of the plant hormone ethylene that would be the focus of his scientific career.

Yang was hired to conduct research on the postharvest biochemistry of fruits and vegetables, a topic of considerable importance to the horticultural industry in California, which ships fresh produce long distances to markets in the eastern United States and internationally. He initially shared a laboratory in the newly constructed Mann Laboratory with Harlan K. Pratt, a pioneering researcher in the ethylene physiology of fruits. The gaseous compound was known to hasten the ripening of fruits and to cause growth distortions in growing plants, and had been shown in 1934 to be produced by ripening fruits. Pratt had constructed a gas chromatograph that could measure the parts-per-million concentrations of ethylene produced by plants and had demonstrated the close relationship between ethylene and fruit ripening. However, little was known about the pathway of ethylene biosynthesis in plants, and Yang set out to apply his biochemical expertise to this aspect of plant ethylene biology.

The modern search for the ethylene biosynthetic pathway began in 1965 when Morris Lieberman's group discovered that both plants and in vitro chemical systems converted methionine into ethylene. Yang's first paper on ethylene (1966) explored the intricacies of an in vitro model system for the generation of ethylene from methionine, and he utilized both in vitro and in vivo approaches to explore potential intermediates of and mechanisms for the conversion of methionine to ethylene (1972; Baur and Yang, 1969). He also studied the mechanism of formation of ethylene from 2-chloroethylphosphonic acid in plant tissues (Yang, 1969; Yamaguchi et al., 1971). This compound, under the generic name of ethephon, enabled the commercial application of ethylene for agricultural purposes, as it is taken up by plants and converted into ethylene. It has been widely used as a fruit-ripening agent, to loosen fruits for harvest and for defoliation of cotton before harvest.

Continuing studies on the biogenesis of ethylene showed that methionine was converted to S-adenosylmethionine (SAM) and that SAM was a precursor of ethylene. In addition, under anaerobic conditions that prevented ethylene formation, a labeled compound accumulated in tissues supplied with ^{14}C-SAM (1977). This discovery stimulated active competition among various groups to identify the unknown intermediate between SAM and ethylene. This intensive effort culminated in the identification of 1-aminocyclopropane-1-carboxylic acid (ACC) as the immediate in vivo precursor of ethylene (1979[1]). Yang quickly developed a sensitive assay for ACC via its chemical conversion to ethylene (1979[2]), which facilitated wide-ranging studies by his group on the regulation of ACC and ethylene biosynthesis in plant growth, fruit ripening, and stress responses (1980[1,2]; Hoffman and Yang, 1980; Yang et al., 1980).

With the identification of the two key steps in ethylene biosynthesis, the conversion of SAM to ACC and of ACC to ethylene, Yang turned his attention to the identification of the enzymes responsible for them. His group and that of Hans Kende at Michigan State University soon reported the isolation of 1-aminocyclopropane carboxylate synthase, the enzyme responsible for the conversion of SAM to ACC (1979[4]; Boller et al., 1979). Isolation of the ethylene-forming enzyme, or ACC oxidase, was a more difficult task, and many studies of ACC metabolism were conducted in conjunction with this search. Yang's group determined that ACC could be malonylated and that this pool of conjugated ACC was largely unavailable for conversion to ethylene (1982[1]). Light, carbon dioxide, oxygen, and water stress all influenced the conversion of ACC to ethylene (reviewed in 1984[1]). The conversion of ACC to ethylene was also stereospecific, as demonstrated by the differential conversion of stereoisomers of 1-amino-2-ethylcyclopropane carboxylic acid to 1-butene (1982[2]). This provided an important test for the enzymatic conversion of ACC to ethylene versus non-specific chemical conversion (McKeon and Yang, 1983) that was important in the subsequent isolation of ACC oxidase (1992). Additionally, Yang's group demonstrated that cyanide was a by-product during the enzymatic conversion of ACC to ethylene (1984[2], 1988). Under conditions involving high rates of ethylene synthesis, such as following induction by auxin or during fruit ripening, the cyanide detoxification enzyme L-3-cyanoalanine synthase removes cyanide resulting from the oxidation of ACC, thereby maintaining a safe level of cyanide in these tissues (1988).

These metabolic studies also revealed that methionine pools were too low in plant tissues to sustain the observed rates of ethylene synthesis. Yang's group demonstrated that after ACC is released from SAM, the methylthioribose moiety from the remaining methylthioadenosine is recycled to replenish methionine levels and sustain ethylene biosynthesis (Miyazaki and Yang, 1987). Some had speculated that the methylthio group would simply be attached to an existing homocysteine molecule to form a new methionine molecule, thus recycling only the methylthio group. Yang's research confirmed that both the methylthio group and ribose carbons from methylthioadenosine became part of the newly formed methionine. The reactions of this recycling pathway (Yang et al., 1990) were known in other systems (bacteria, yeast, rat liver extracts), but Yang's group was the first to demonstrate that it was active in plants, and it has been christened the "Yang Cycle" in plant biochemistry textbooks.

As the tools became available for cloning and characterizing the genes responsible for the steps in ethylene biosynthesis, Yang contributed to many studies of the regulation of those genes in fruit ripening, plant growth, wounding, and stress responses (1991; 1994; Yip et al., 1992; Shiu et al., 1998; Yang and Oetiker, 1998). Particularly noteworthy were his studies of the active sites and mechanisms of action of ACC synthase and ACC oxidase (1990; Li et al., 1996; Shaw et al., 1996; Zhou et al., 1999; Charng et al., 2001). Yang wrote numerous highly cited reviews and book chapters that defined ethylene biosynthesis and its role in plant biology for a generation of students and researchers (Yang, 1980, 1984[1], 1985).

While most widely known for his work on ethylene biosynthesis and action, Yang also maintained active research programs in other areas of plant growth and metabolism, including on auxin metabolism and action (1979[3]; Aharoni and Yang, 1983), cytokinin action (Lau and Yang, 1973; Chen et al., 2001), and the biological effects of sulfite and sulfur dioxide (Peiser and Yang, 1979, 1985).

In all his work Yang continually linked his discoveries to practical applications in postharvest biology and plant growth regulation. He applied his knowledge of chemistry and physiology to learn more about ethylene biosynthesis, and he utilized this information to improve postharvest storage conditions and product quality (Hyodo et al., 1978; Yang, 1987; Yang and Oetiker, 1998). His knowledge of the literature related to ethylene and plant hormones was encyclopedic, and he was continually testing and retesting fundamental mechanisms and interactions as new data became available. Yang was a rigorous but positive mentor and role model to his graduate students and postdoctoral associates and actively encouraged and assisted them in obtaining positions and advancing their careers.

Shang Fa Yang traveled extensively, including sabbatical leaves in Germany, Taiwan, the United Kingdom, and Japan. He figured prominently at many national and international research conferences and served on the editorial boards of leading journals and as a member of several professional and scientific societies. He won many awards and honors, including the Campbell Award of the American Institute of Biological Sciences in 1969, a Guggenheim Fellowship in 1982, the International Plant Growth Substances Association Research Award in 1985, and the Outstanding Researcher Award from the American Society for Horticultural Science in 1992. Yang was named the University of California, Davis, Faculty Research Lecturer in 1992, the highest honor given by this institution for excellence in research. He was elected to the National Academy of Sciences in 1990 and to the Academia Sinica in Taiwan in 1992. In 1991 Yang received the prestigious international Wolf Prize in Agriculture, often considered to be the "Nobel Prize" for agricultural research.

After an outstanding career as professor and biochemist at the University of California, Davis, Yang took early retirement in 1994 to accept a Distinguished Professorship at the Hong Kong University of Science and Technology (HKUST), where he established an active plant research group in the Department of Biology. He continued his work on ethylene, particularly on the characterization of the structure and function of ACC synthase (2000; Li et al., 1996; Zhou et al., 1999). He also coedited a book on *Discoveries in Plant*

Biology with his colleague S. D. Kung at HKUST (Kung and Yang, 1998). In 1995 Yang was recognized as a Distinguished Research Fellow by the Institute of Botany of the Academia Sinica in Taipei, Taiwan. He returned to Taiwan in 1996 to serve as the director of the Institute of Botany and subsequently as vice president of Academia Sinica from 1996 to 1999. In this position he directed its numerous research institutes, including the establishment of the new Institute of Agricultural Biotechnology (subsequently renamed the Agricultural Biotechnology Research Center). He also served as the first chief director of the National Science and Technology Program for Agricultural Biotechnology, which funds and oversees research and development projects related to agricultural biotechnology in Taiwan. Yang played important leadership roles in advancing plant biology and agricultural biotechnology in both Hong Kong and Taiwan. Following his service with Academia Sinica, Yang retired to Davis, California, although he continued to publish additional scientific work.

Shang Fa Yang passed away suddenly and unexpectedly from complications of pneumonia on February 12, 2007, at the age of 74. He is survived by his wife, Eleanor, and two sons, Albert and Bryant, who have pursued careers in engineering and chemistry, respectively. Prior to his death, Yang was planning to endow a program to foster greater scientific exchange between the University of California and the Academia Sinica. This initiative was continued by Eleanor, Albert, and Bryant, and culminated in the establishment of the Shang Fa and Eleanor Yang Scholarly Exchange Endowment in October 2007. This endowment will support visits of scholars in the agricultural, biological, and chemical sciences between the University of California, Davis, and Academia Sinica, Taiwan, two institutions that Shang Fa Yang loved and served with distinction.

Shang Fa Yang's contributions to plant biology, including his discovery of the pathway for ethylene biosynthesis, the elucidation of the Yang Cycle and his work on auxin, sulfur, and cyanide metabolism, are described in more than 225 journal articles and book chapters that he published during his career. He has earned a significant and enduring place in the modern history of plant biochemistry. His humor and humanity endeared him to students, colleagues, and friends, who also appreciated and benefitted from his remarkable intelligence. His untimely death prematurely ended a life devoted to scholarship, teaching, and service.

The author is grateful to Eleanor, Albert, and Bryant Yang for sharing personal information and reviewing this contribution for accuracy. Yee-Yung Charng, Neil E. Hoffman, John H. Miyazaki, Galen D. Peiser, and Wing Kin Yip reviewed the draft and contributed additional information to the final document. Details from Yang's early life in Taiwan were translated from Chinese by Lianhai Fu from *The Collections in Commemoration of Dr. Shang Fa Yang's Retirement* prepared in 1999 by the Institute of Plant Research of the Academia Sinica and edited by Yu Qin Huang.

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VERNON ROBERT YOUNG

November 15, 1937–March 30, 2004

BY NEVIN S. SCRIMSHAW, ARNOLD L. DEMAIN,
AND NAOMI K. FUKAGAWA

VERNON ROBERT YOUNG WAS THE world's leading expert on human protein and amino acid requirements and metabolism at the time of his death of complications of renal cancer at the age of 66. He was a key investigator in the series of studies that revealed the inadequacy of the 1973 Food and Agriculture Organization/World Health Organization Recommended Allowance for human protein requirements and the research that corrected this serious error. Later his innovative use of stable isotopes showed that the estimated essential amino acid requirement levels universally accepted since the 1940s were much too low. These erroneous values had been endorsed by a series of FAO/WHO committees, including one that met as late as 1985.

With confirmation in Indian subjects from his collaborator Anura Kurpad in Bangalore, Young proposed a new "MIT" pattern that was adopted, with minor changes, by the 2003 FAO/WHO/UNU Expert Consultation. It recognized that adult essential amino acid requirement estimates per gram of protein needed to be increased by a factor of 2 to 3, depending on the specific amino acid. He reported this work and a great deal of other groundbreaking research in over 600 full-length articles and book chapters, approximately one per month for 42 years. His contributions to human nutritional science were exceptional.

Vernon Young was born in Rhyl, North Wales, in 1937 but lived in Cardiff from an early age. His interest in agriculture developed from visits to an uncle's farm in Nottinghamshire. He obtained his B.Sc. from the University of Reading in 1959 and a postgraduate diploma from Cambridge University in 1960. He then moved to the University of California, Davis, and obtained his Ph.D. in 1965 with a thesis on calcium and phosphorus homeostasis in sheep. He came to the Department of Nutrition and Food Science of the Massachusetts Institute of Technology as a postdoctoral fellow in the same year and was promoted to assistant professor the following year. He rose rapidly through the ranks and became a full professor in 1977.

Soon after arriving at MIT he met his future wife, Janice Harrington, of Wellesley, Massachusetts, a suburb of Boston, who was then executive secretary of the department. They were married in 1966 and settled permanently in Wellesley. Vernon Young's life was dedicated to his research at MIT and with many collaborators in other institutions and countries, but he was also devoted to his wife, his four sons—Christopher, Andrew, Michael, and Richard—and his daughter, Patricia. They were a happy and devoted family. Vernon did much of his writing at his home in Wellesley when he was not in his MIT office or traveling. A twin sister, Sylvia Young Price, lives in Council Bluffs, Iowa.

ADDITIONAL ACADEMIC APPOINTMENTS

In addition to his professorship at MIT Young served as associate program director of the MIT Clinical Research Center, 1985-1987, and director of research for the Shriners Burns Institute, 1987-1990. Additional appointments in Boston at the time of his death included lecturer in surgery, Harvard

University, and senior visiting scientist, U.S. Department of Agriculture Human Center on Aging, Tufts University, 1988-2004.

Young also held appointments as visiting professor at the University of California, Los Angeles, 1983; University of Southern California Medical School, March 1984; University of Illinois, Urbana, March 1986; University of Michigan, Ann Arbor, March 1986; University of Iowa, Iowa City, May 1986; University of Florida, Gainesville, December 1987; Dartmouth Medical School, Hanover, January 1988; Case Western Reserve School of Medicine, Cleveland, September 1988. He served as visiting research fellow, Merton College, Oxford, U.K., April-June 1994; visiting scholar, University of Texas Health Sciences Center at San Antonio 1996; and visiting professor, the Universities of Wageningen and Maastrich, The Netherlands, 2000; and visiting research fellow, University of Ulster, Coleraine, Northern Ireland, 2002.

NATIONAL AND INTERNATIONAL LECTURESHIPS AND COMMITTEES

Young's named lectureships included the Vickers Lecture of the British Neonatal Health Science Center, San Antonio, 1986; the American Society for Nutritional Sciences McCollum Award Lecture, 1987; Burns Lecture, Royal College of Physicians and Surgeons, Scotland, 1990; Brackenridge Lecture, University of Texas Health Science Center, San Antonio, 1996; Bruce and Virginia Street Lecture in Preventive Nutrition, University of North Texas, Fort Worth, 1996; Ninth Annual Malcolm Trout Lecture, Michigan State University, 1997; Rudolf Schonheimer Centenary Lecture, Nutrition Society of U.K.,1998; first David Murdock Lecture in Nutrition, Mayo Clinic, Rochester, 1998; Jonathan Roads Lecture, American Society for Parenteral and Enteral Nutrition. 1999; Hans Fischer Lecture, Rutgers University, New Brunswick, 1999; and W. O. Atwater Lecture and Award, USDA Agricultural

Research Service, 2001. Editorial boards on which he served included the *American Journal of Clinical Nutrition*, 1976-1978; *Nutrition Research*, 1980-1984; *Advances in Nutrition Research*, 1976-2004; *Pediatric Gastroenterology and Nutrition*, 1981-1985; *Age*, 1977-1982; *Growth*, 1986-1990; *Journal of the Nutrition Society of Nigeria*, 1990-1994; and *Nutrition Today*, 1997-2004.

The preceding description of Young's positions, honors, named lectures, and editorial boards does not capture the extent of his influence in Boston, nationally and throughout the world. Within MIT he served as a member of the important Committee on the Use of Humans as Experimental Subjects from 1978 to 1984 and the Committee on Radiation Safety. Within the Department of Nutrition and Food Science he served as an undergraduate and graduate adviser and on the Curriculum Committee, the Nutrition and Metabolism Doctoral Committee, Doctoral Examination Committee, and Executive Committee. Nationally he served as a member of the Food and Nutrition Board of the Institute of Medicine, 1992-1998. He chaired the Nutrition Implementation Committee, National Cancer Institute Division of Cancer Prevention from 1998 until 2004.

Among the many national committees to which Young contributed were the Food and Drug Administration Board of Inquiry on aspartame, 1978; the National Institutes of Health Nutrition Study Section, 1981-1985, and numerous ad hoc study sections; the NIH Consensus Panel on Health Risks, the Children's Nutrition Research Center at Baylor University, Houston, 1985; National Academy of Sciences Committee on Diet and Health,1986-1987; USDA Council of Scientific Advisors, Houston; Scientific Advisory Committee, Pennington Medical Center, Baton Rouge, 1991-1998; National Dairy Council, 1994-1998; and the Basic Science Implementation Subcommittee, National Cancer Institute Division of Cancer Prevention, 1998-2004. Internationally he was a member of the Committee on Human Protein-Energy Requirements, International Union of Nutrition Sciences, 1991-1997; FAO/WHO/UNU Expert Group on Protein and Energy Requirements, 1981; Visiting Group of the Rowett Research Institute, Aberdeen, 1992; Scientific Advisory Committee, Deutsches Institut fur Ernahrungsforschung, 1995-1998. In honor of his nutrition expertise he was elected in 2001 to the Board of the Nestle Company, Vevey, Switzerland.

INITIAL RESEARCH AT MIT

Brilliance and exceptional scientific intuition characterized his research career from the beginning. Young published several papers on the effects of dietary protein, infection, and hormones on the translation step of protein synthesis by muscle ribosomes with special attention to initiation and elongation. This led to work on in vivo protein degradation, specifically targeted to certain muscle proteins containing methylated amino acids such as 3-methylhistidine occurring in actin and various types of heavy chain myosin. Stimulated by studies of Hamish Munro in rats, Young was the first to demonstrate in humans that urinary 3-methyl histidine is a direct indicator of muscle proteolysis and hence muscle mass. The discovery was of such great interest that the paper describing this finding became a Citation Classic in 1992. He also prepared and examined ribosomal fractions from skeletal muscle with respect to their capacity for protein synthesis in response to insulin and other factors.

HUMAN OBLIGATORY NITROGEN LOSSES AND PROTEIN REQUIREMENTS

Together with Nevin Scrimshaw and their many graduate students, Young guided a series of studies that measured the variations in adult obligatory nitrogen losses as the basis for predicting adult protein requirements. This work, complemented by that of Doris Calloway at the University of California, Berkeley, became the basis for the 1973 FAO/WHO report. Unfortunately this committee failed to take into account the lower utilization of protein at requirement levels and arrived at an erroneously low recommendation that was later corrected by Young and Scrimshaw as described below.

PROTEIN QUALITY STUDIES

An extended series of studies explored nitrogen absorption and retention in human subjects and yielded improved procedures for the use of nitrogen balance in the assessment of the quality of dietary proteins. The initial work on cereal proteins demonstrated the progressively higher percent retention of absorbed nitrogen as protein intake decreased below requirement levels. This led to extensive studies of the protein value of soy protein isolate. These demonstrated that this soy protein as a sole source for human feeding had a protein utilization comparable to that of animal protein.

Based on the internationally accepted essential amino acid pattern at the time, it should have been possible to "dilute" good quality protein with inexpensive nonessential amino acids without lowering its protein quality. Nitrogen balance studies with milk protein fed to adult subjects failed to confirm this expectation, suggesting that the accepted reference pattern was too low in the proportion of essential amino per gram of nitrogen. There was then no methodology available to explore this further.

PROTEIN REQUIREMENT STUDIES

Young and Scrimshaw's graduate student Cutberto Garza showed in three successive nitrogen balance studies with MIT students that the level of protein intake recommended by the 1973 FAO/WHO Expert Group resulted in loss of lean body mass, lower serum albumin, and in some subjects, signs of liver pathology. They developed a unique 15-day day multilevel N balance approach to determining protein quality and applied it in a United Nations University-sponsored uniform field trial in 15 countries. On the basis of the data obtained they proposed a recommended protein intake one-third higher than the 1973 international Recommended Protein Allowance. This level proved adequate when fed to subjects for 50-90 days in six countries. With minor adjustment the value that they proposed was adopted by the 1985 FAO/WHO/UNU Joint Expert Consultation on Protein-Energy Requirements with a profound effect on estimates of protein deficiency in developing countries and on nutrition, agriculture, and health policy.

REVISION OF HUMAN AMINO ACID REQUIREMENTS

Young's most recent and most important work was to pioneer the use of stable isotopes in studies of human nutrition, leading to the development of methods to build on and replace nitrogen balance as the approach for the assessment of protein and amino acid requirements. In a very productive collaboration with Dennis Bier at Washington University, St. Louis, Young showed that whole-body amino acid flux, protein synthesis, breakdown, and amino acid oxidation in humans respond to the content of meals and that these responses are modulated by the protein, amino acid, and energy components of the diet. At the beginning of these studies his graduate students went to Bier's laboratory in St. Louis to analyze their samples, using a mass spectrometer available there. His output was increased when Young established mass spectrometry facilities, including isotope ratio and gas chromatography mass spectrometry and later liquid chromatography mass spectrometry at MIT. The pace of the work accelerated because students used both facilities.

The qualitative importance of both protein synthesis and breakdown in premature infants was first demonstrated in Young's studies using ^{15}N as a tracer. Using ^{15}N-labeled amino acids in adults, he reported a redistribution in the pattern of whole-body protein metabolism with advancing age.

The various ^{15}N tracer studies of amino acid (N) and protein metabolism changed our fundamental understanding of mammalian protein and amino acid nutriture. This was further extended to demonstrate enhanced rates of protein synthesis and breakdown in children suffering from burns that provided a metabolic explanation for the greatly increased protein requirement of a burned patient.

Using different stable isotope probes, Young and his colleagues elucidated the age- and disease-related changes in amino acid, glucose, and fatty acid metabolism. Using a novel approach, Young developed a stable isotope method using ^{15}N glycine tracers to explore changes in albumin synthesis with advancing age. His findings indicated that albumin synthesis is regulated by amino acid intake at a lower set-point in the elderly than in young adults.

Together with one of his last students, Naomi Fukagawa, Young demonstrated for the first time in humans using stable isotope tracers and the euglycemic insulin clamp technique that insulin's primary role was to inhibit proteolysis and that stimulation of protein synthesis necessitated amino acid availability. He also played a major role with M. Janghorbani in developing and applying new stable isotope techniques for studying the metabolism of trace minerals such as zinc, copper, iron, selenium, and calcium in human subjects. This involved their analyses in blood, urine, and feces during metabolic studies. He was the first to compare intrinsic and extrinsic probes for measurement of dietary zinc and selenium bioavailability in humans.

Using the facilities of the MIT Clinical Research Center, Young introduced stable isotope studies of amino acid oxidation as an index of amino acid balance. His work on ^{13}C-labeled amino acids, especially leucine, valine, lysine, and threonine, replaced nitrogen balance techniques and led to a new approach for estimating amino acid requirements. This involved infusing a ^{13}C-labeled test amino acid over several hours after intakes of the amino acid were varied over a wide range to determine the intake level associated with increased oxidation, measured as $^{13}CO_2$ content in expired breath.

With his students Young successfully quantified the requirements for leucine, lysine, threonine, methionine, and cystine using this approach. Later he very ingeniously reversed the procedure by measuring the level of intake of a specific amino acid at which oxidation of another labeled essential amino acid (e.g., leucine) was reduced. Using this approach over a 24-hour period during the postabsorptive or fed states, he was able to determine the requirement of amino acids and the levels of intake that would achieve amino acid balance. He is credited with developing the 24-hour indicator amino acid balance approach widely used today to determine amino acid requirements in health and disease.

Using stable isotope approaches, Young also explored the metabolism of dispensable amino acids, such as glycine, and developed a new approach for quantifying the whole-body synthesis rate of dispensable amino acids, particularly alanine, glycine, praline, and arginine. This was the first time this approach had been used, and it enabled him to demonstrate the sensitivity of whole-body alanine synthesis to changes in the availability of carbon, hydrogen, and nitrogen moieties. Young deftly adopted compartmental and noncompartmental models to further expand his use of stable isotope probes and later included multiple tracers to permit the dissection of complex metabolic processes in vivo in humans.

Young's last completed work was a series of elegant multi-tracer studies using both isotopomers and isotopologues of arginine and related urea cycle intermediates (arginine, ornithine, and citrulline) together with a labeled essential amino acid, such as leucine, to explore the nutritional significance of arginine and its role in metabolism and nitric oxide synthesis. He suggested that arginine homeostasis is achieved by a balance between intake and breakdown with de novo arginine synthesis playing only a minor role. Young was also at the forefront of using short-lived isotopes (^{11}C) and PET scanning in experimental nutrition studies.

True to his visionary spirit Young embraced the advances made in molecular biology and saw opportunity in the postgenome era to explore the dynamics of nutrient-gene interactions. However, he always reminded one of the importance of probing at a higher level of biological complexity and that integrative science or systems biology would be key to advancing nutritional science together with modern technology in the 21st century.

FINAL COMMENTS

Vernon Young was recognized as a major force in human nutrition. He was elected to the National Academy of Sciences in 1990 and the Institute of Medicine in 1993. Other awards and honors include the 1973 Mead Johnson Award and the 1983 Borden Award from the American Institute of Nutrition; the 1987 McCollum Award for Distinguished Achievement in Nutrition Research from the American Society for Clinical Nutrition; the 1997 Rank Prize in Nutrition, U.K.; the 1991 Gopalan Oration and Gold Medal, Nutrition Society of India; the 1995 the Bristol-Myers Squibb Award for Distinguished Achievement, U.S.; the 1996 Roger Williams Award in Preventive Nutrition; the 1997 Danone International Prize for Nutrition (France); the W. O. Atwater Award (U.S.); the

1998 International Award for Modern Nutrition, Switzerland; the 1999 Jonathan E. Rhoads Award, American Society for Parenteral and Enteral Nutrition; the 2001 W. O. Atwater Lecture and Award, USDA Agricultural Research Service; and the 2003 Conrad Elvehjem Award, American Society for Nutritional Sciences. In 1997 he received a doctor of medicine *honoris causa* from Uppsala University, Sweden, and the 1999 Award of Excellence from the Alumni Association of the University of California, Davis.

Since its founding in 1982, Young was a board member and officer of the International Nutrition Foundation, which has extensive international fellowship programs and publishes the *Food and Nutrition Bulletin* on behalf of the United Nations University. He served as vice president from 1989 to 1991 and president from 1991 to 1992 of the American Institute of Nutrition. During 1996-1998 he served as the first chairman of the Food and Nutrition Board Committee, responsible for the new and greatly expanded Recommended Dietary Allowances (RDAs) and skillfully guided the group to a consensus. He served as a director of the American Board of Nutrition, 1979-1990.

While words can describe his scientific achievements and his national and international reputation, they cannot adequately convey his ebullient Welsh personality, sly humor, and unusual charisma. Young would tease outrageously anyone at any level of society, and they loved it. He was widely and greatly admired as a teacher, researcher, colleague, and exceptional human being. He was brilliant, completely dedicated, and exceptionally considerate of others at all professional and social levels. He was an outstanding mentor whose infectious curiosity stimulated inquiry and new discoveries. There are few persons who have been so universally liked throughout the world or who have contributed as much to nutritional science. In one of his last lectures he quoted Arthur M.

Sackler, who established the *Medical Tribune* newspaper: "Art is a passion pursued with discipline and science is a discipline pursued with passion. Passion is the engine that drives creativity." This latter well describes Vernon.

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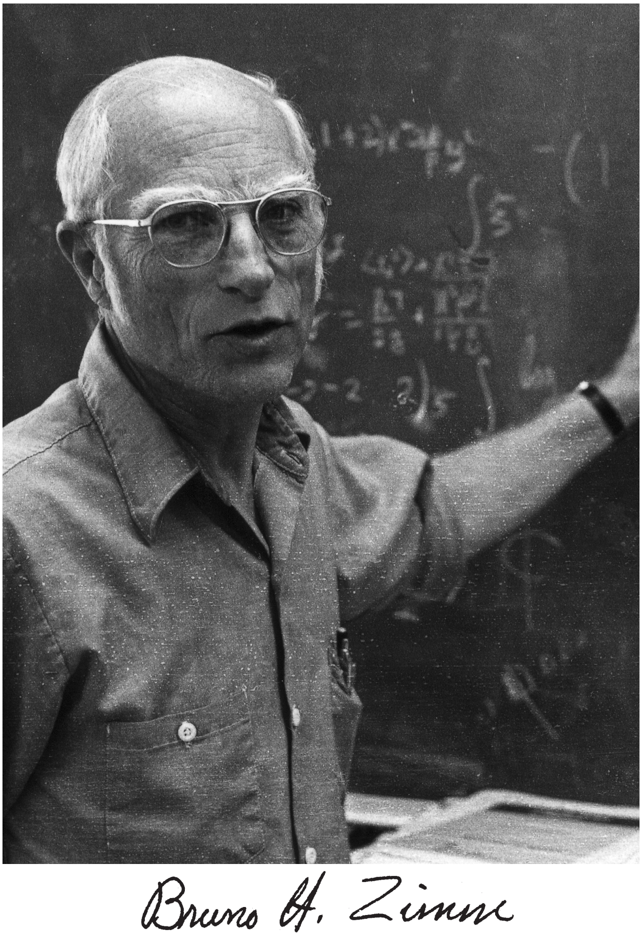

BRUNO HASBROUCK ZIMM

October 31, 1920–November 26, 2005

BY CAROL BETH POST AND RUSSELL F. DOOLITTLE

PROFESSOR BRUNO H. ZIMM, distinguished scientist and premier polymer chemist, died on November 26, 2005, in La Jolla, California, at the age of 85 after a hard-fought battle with Parkinson's disease.

FAMILY AND CAREER

Bruno Zimm was born in 1920 in Woodstock, New York, to Bruno Louis Zimm, an acclaimed sculptor, and Louise Zimm, a writer. He married Georgianna Grevatt in 1944, and together they raised two sons, Louis Zimm and Carl Zimm.

Zimm's pioneering contributions to polymer chemistry laid the groundwork for modern research on biological and synthetic macromolecules, and established him as a founding father of the physical chemistry of macromolecules. His lasting impact on not only science but also on researchers is unparalleled. Zimm's scientific preeminence was recognized early, and he was elected to the National Academy of Sciences in 1958 at the young age of 38, with many of his celebrated works yet to be published.

Bruno Zimm received his B.S. in chemistry from Columbia University in 1941, and in 1944 earned his Ph.D. under the tutelage of Joseph E. Mayer, also at Columbia. As this was the time of World War II, Zimm spent a summer working on a war-related project under the direction of Victor K. LaMer. He and a fellow graduate student, Paul Doty, studied light scattering theory to investigate the optical properties of smokes. This summer's project, therefore, was Zimm's introduction to light-scattering theory. In 1944 he moved across town to the Polytechnic Institute of Brooklyn as an instructor and research associate with Professor Herman Mark. His research there from 1944 to 1946 was the start of a lifelong fascination with biological and synthetic macromolecules. Bruno accepted a faculty position at the University of California, Berkeley, in 1946, and began developing the method of using light scattering for analysis of high-molecular-weight polymer solutions. As a young assistant professor he invented the famous Zimm plot for simultaneous determination of three fundamental macromolecular parameters: radius of gyration, the second virial coefficient, and molecular weight. Three of the publications reporting this pioneering work in 1948-1949 have been cited more than a thousand times, and Zimm is sole author on two of these (1948,1,2; 1949). While associate professor at Berkeley (from 1950 to 1952), he took leave to be a visiting lecturer in chemistry at Harvard University. In 1951 Zimm moved to the General Electric Research Laboratory in Schenectady, New York, where he spent nearly 10 years and made the transition from synthetic polymers to biological ones of polypeptides and DNA. At GE, Zimm published his two most highly cited papers: the report of the Zimm-Bragg theory of the transition between helix and coil in polypeptide chains (1959) and the theoretical description of polymer solution viscoelasticity and flow birefringence (1956). That second paper is regarded as the fundamental description of polymer dynamics with nearly 2000 citations. He returned to academia in 1960 and after a brief stint as a visiting professor at Yale, he moved to the University of California, San Diego, (UCSD), where he remained for the rest of his life. It was at UCSD that Zimm designed the Cartesian-diver viscoelastometer with a freely floating inner cylinder to generate very low shear rate for measuring chromosomal-size DNA, and contributed theories of DNA melting and gel electrophoresis. He became Professor Emeritus of Chemistry and Biochemistry in 1991. He continued his work as long as he was able, and never tired of talking about science and how the world works. After becoming Professor Emeritus and no longer formally mentoring students and postdocs, Zimm published, each year until 2000, one or more papers, several single-authored and some from new collaborations.

Zimm was the recipient of the National Academy of Sciences Award in the Chemical Sciences in 1981, the citation reading:

> For his contributions and influence in theoretical and experimental polymer chemistry, notably his work on polymer interactions, polymer visco-elasticity, the helix coil transition in bio-polymers, the theory of light scattering, and the study of extraordinarily large DNA molecules.

Zimm was also a fellow of the American Academy of Arts and Sciences (since 1969), and a member of several scientific societies. Among the numerous awards and honors he received are the Baekeland Award, North Jersey Section, American Chemical Society in 1957; Bingham Medal of the Society of Rheology in 1960; the American Physical Society High-Polymer Physics Prize in 1963; Kirkwood Medal, New Haven Section, American Chemical Society in 1982. He wrote over 165 scientific publications. Special journal articles and issues highlight many of his substantial accomplishments.[1]

SCIENTIFIC ACHIEVEMENTS

Zimm's lifelong interest in understanding the nature of polymeric solutions was ignited at Columbia University, where he met Walter Stockmayer, his lifelong friend and collaborator. Both Zimm and Stockmayer were inspired by Joseph E. Mayer, Zimm was a doctoral student of Mayer and Stockmayer took a job as an instructor at Columbia with the aim of being near Mayer. Zimm also claimed that Charles O. Beckmann was mostly to blame for his and Stockmayer's conversion into polymer chemistry, but one cannot discount the seminal work of Paul Flory—which drew Zimm's attention as a young physical chemist to the burgeoning field of polymer science—and Paul Doty, a fellow doctoral student with Mayer. Together, Zimm and Doty contemplated how to measure the absolute molecular weight of polymeric molecules and distributions, a curiosity that eventually lead Zimm to measure molecular weights of the enormous chromosomal DNA molecules. In the early 1940s obtaining molecular weights of synthetic polymers was of fundamental interest but all methods known at the time had difficulties. In 1944 Zimm and Doty, both at Brooklyn Polytechnic Institute, heard that P. J. W. Debye had developed an unpublished method to measure polymer molecular weights from light scattering. With insight from their earlier discussions on the Einstein-Smoluchowski theory of light scattering related to density fluctuations of smokes, Zimm and Doty recognized immediately that combining Smoluchowski's equation with Raoult's Law gave an estimate of absolute molecular weight from two measurements: turbidity and the refractive index increment of a solution. In collaboration with Herman Mark, Zimm and Doty conducted experiments that showed the new method worked, and published the results (1944, 1945) soon after P. J. W. Debye's paper appeared in the *Journal of Applied Physics* in 1944.

Zimm's curiosity about polymers endured and as an assistant professor at Berkeley he continued to work on polystyrene, his favorite molecule, for about 15 years. Bruno was exceptional, equally brilliant and proficient with theory and experiment. Even early in his career he combined his deep physical insight with a solid practical sense to develop a now famous experimental method to simultaneously analyze the radius of gyration, second virial coefficient, and molecular weight of a polymer (1948,1). The analytical method, a primary tool utilized by polymer chemists today, is called a Zimm plot. That is, it is called a Zimm plot by everyone except Bruno. While a student of Bruno's, one of us (C.B.P.) used this method to study DNA condensation, and in the first draft of the manuscript called the resultant figure a Zimm plot, as you would expect. In the next draft, and in the 1982 publication of the work, the figure is found described as a "reciprocal-intensity light-scattering plot" as a result of Bruno's editing.

The method of analyzing macromolecular properties from light-scattering intensities is not all that appeared in the highly cited 1948 paper first reporting the Zimm-plot (1948,1). Testimony to Zimm's true brilliance, the paper also describes the design of a new photometer, including electronic circuit diagrams and details on performing measurements at different angles. The instrument measured the intensity of scattered light more accurately than other instruments available at the time. It should not be overlooked that Zimm also contributed the theoretical treatment for the angular dependence of scattered light in the preceding article of the same issue of the *Journal of Chemical Physics* (1948,2). Those who emphasize the interdisciplinary nature of modern scientific research should take note of these extraordinary single-authored, back-to-back publications addressing instrumentation, experiment, and fundamental theory, each with more than 1000 citations. Application of Zimm's plotting method yielded plenty of data revealing interesting new behavior of polymer solutions (1950) on which to build future theories.

Equilibrium statistical properties of polymers held Zimm's attention for many years. He applied his incisive understanding of statistical mechanics to numerous problems related to polymer configurations and phase transitions. Early in his career he developed a statistical treatment of solutions of large molecules based on Mayer's theory for nonelectrolyte solutions to obtain a general expression for the second virial coefficient of macromolecules (1946). This work was soon followed by an investigation of the dimensionality of polymer chain molecules. An elegant description of the calculation of the radii of gyration for branched polymers accounting for the effects of excluded volume appeared in a classic paper written with his close friend Stockmayer, or "Stocky" (1949). In the 1950s when the interests of some polymer scientists started to turn toward biomolecules, Zimm published another of his celebrated works: the Zimm-Bragg theory to describe the helix-coil transition of a polypeptide chain (1959,1), accompanied by careful experimental investigations to try out the theory (1959,2). The Zimm-Bragg theory of helix-coil transition remains today an integral component of the theoretical prediction of protein folding and design, as well as experimental measurement of protein structural stability.

Zimm's lasting influence on polymer science also includes his numerous contributions to understanding nonequilibrium properties of polymers. His most highly cited paper (1956) is an insightful and practical treatment of flexible chains to obtain expressions for viscoelasticity, birefringence, and dielectric relaxation. It is a beautiful example of Zimm's ability to recognize how to formulate a problem and apply the right mathematical tools to find a useful outcome (1956; 1959,3). He also investigated dynamic properties of DNA. Some of the problems that captured his attention were how to explain the melting of double-stranded DNA (1960) and the fluorescence depolarization of DNA (1979). In his pursuit of understanding the behavior of polymers, Zimm eagerly adapted new technologies that would help him to investigate these large molecules. He made good use of computational methods, such as Monte Carlo, to test certain assumptions made in deriving analytical theories for sedimentation and other transport properties (1981, 1980), as well as equilibrium counter-ion distributions (1984). More recently Zimm became interested in polyelectrolyte behavior of DNA and developed a rigorous description for the snaking of long DNA chains through a gel under the force of an applied electric field (1985).

Coincident with his arrival at UCSD Bruno embarked on a long-term experimental project to find the true lengths of native DNA molecules. At the time, the true size of DNA molecules was unknown but there was evidence from autoradiography and electron microscopy that these molecules were incredibly long, and therefore fragile. Again, Bruno's creative genius appears on the scene not only conceptually but also in designing and constructing the scientific tools to explore new concepts. Bruno conceived one of his many ingenuous ideas: to operate a rotating viscometer in an inverse design where the inner cylinder rotates while the outer one is fixed (1962). This simple reversal results in the shear stress being fixed and the shear gradient being measured, a condition that removes stress and reduces the chance of breaking a long DNA molecule. Oddly, but true to the nature of this great thinker, the idea occurred to Bruno during the mundane task of washing dishes, when he observed a drinking glass rotating in the dish water and realized from the swirling motion the advantage of rotating the inner cylinder. The culmination of these elegant studies was the measurement, by Bruno and biologist Ruth Kavenoff, of full-length DNA molecules from chromosomes of the fruit fly (*Drosophila*)

(1973). The use of chromosomes from different species of fruit fly that had morphologically different-size chromosomes gave them an external gauge that verified their measured values, which ranged from 20 billion to 80 billion Daltons. As a *coup de grace* they used DNA from a mutant strain of fruit fly that had a shortened chromosome, the measured length of which correlated perfectly with what was observed in the microscope. Thus, they had conclusively shown that each chromosome is composed of a single molecule of DNA.

REFLECTIONS

Bruno Zimm was universally liked and respected as an exemplary scientist, teacher, and person. For the National Academy of Sciences Award in the Chemical Sciences, he was cited for research "that in the broadest sense contributes to better understanding of the natural sciences and to the benefit of humanity." He was not a man enslaved by professional ambitions. His wife, Georgianna, once said, "He didn't think about those things [awards and recognition]. He did it [research] because he enjoyed it." He pondered the world and delighted in the joy of discovery and the understanding of how the world works. As Stocky noted in celebrating Bruno's 65th birthday,

> [His modesty] turned into perhaps the most admirable and certainly the most lovable of all of Bruno Zimm's gifts: he simply is unaware of his stature, and treats all creatures, anthropoid or not, always patiently and affectionately as no less than complete equals.[1]

The photograph at the beginning of this memoir captures Bruno at one of his most loved activities: working on an analytical theory (and probably teaching a student) at the chalk board. He preferred working with a small group and rarely took on more than two or three graduate students and a few postdocs at a time. Nonetheless, he left a remarkable legacy of students and postdocs, with the overwhelming majority having careers in academia. Two members of his first group of students and postdocs at UCSD, Don Crothers and Vic Bloomfield, wrote a seminal text on DNA physical chemistry. He kept his group small because he was always thoroughly involved with his students and needed time to be available to listen to their ideas and to hear out their problems. Not only was Zimm one of the world's great thinkers and most gifted scientists, he was also a wonderful listener. He listened to the thoughts, ideas, and questions of colleagues and students, as well as to their problems. But he also wanted time to think for himself. Many of his papers throughout his career are single authored.

Consistent with his proficiency in both experiment and theory, Bruno enjoyed working with his hands as much as he did thinking. Research and the lab were more like a hobby than a job to Bruno. His lab at UCSD included a shop filled with tools he collected over the years: equipment for glass blowing (a diamond saw, torches, gas cylinders), electronics (soldering guns, 'scopes, circuit boards), and a lathe, one of Bruno's favorite toys. Naturally, Bruno was glad to have any student who shared his fondness for tinkering make use of the shop. One, however, was seldom left alone in the shop, as the entrance was across from Bruno's office, and it was difficult to enter unnoticed. Once the lathe, for example, started to turn, Bruno would appear—with a smile—to ask, "Wouldn't you like some help with that?"

From his father, a sculptor, and his mother, a writer, Bruno inherited a deep appreciation of art and music. He loved to play the clarinet. For a number of years Friday lunchtime was spent playing duets in his office, and in the lab one could hear the sounds of Bach emanating from across the hall. He also read extensively in German and English. He learned the theory of light scattering as a student at Columbia by reading the original German edition of *Optik* by Max Born (published in 1933). Bruno once noted that after the exercise of reading Born's elegant German, he found other German readings easy. He later translated the memoirs of Ludwig Boltzmann (of whom Bruno was a fan) for the simple satisfaction of doing it. He was also an avid sailor and doted on his beautiful wooden Norwegian sail boat, *Altair*. He took pride in maintaining it, and occasionally enlisted the help of a student or two for the required painting and refinishing, always followed by an invitation for a sail.

Beyond these tangible contributions to science, the many lives Bruno has influenced cannot be determined. Being mentored by Bruno was a priceless gift, a joy, and a lifelong honor. Many felt privileged to have been taught by someone with such extraordinary insight and depth of thinking, but one who could reduce complex systems to simple ones. Yet, this unassuming master of science was exemplary in his humanity, teaching, patience, and support. He was a role model in a manner that, sadly, in modern scientific times is difficult to follow.

NOTE

1. W. H. Stockmayer. Bruno H. Zimm on his 65th birthday. *Macromolecules* 18(1985):2095-2096. Special Issue: Honor of B Zimm. *Biopolymers* 31(1991):1459-1667.

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