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A Biographical Memoir by
JAMES R. ARNOLD, JACOB BIGELEISEN, AND
CLYDE A. HUTCHISON, JR.

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BY JAMES R. ARNOLD, JACOB BIGELEISEN, AND CLYDE A. HUTCHISON JR.

HAROLD UREY WAS A SCIENTIST whose interests, accomplishments, and influence spanned the disciplines of chemistry, astronomy, astrophysics, geology, geophysics, and biology. Although he was meticulous in his attention to detail, his sights were always on broad questions at the forefront of knowledge. His unusual powers of concentration and capacity for hard work accounted for much of his success in exploring and opening up major new fields of research, including his discovery of deuterium and work on isotope chemistry, isotope separation, isotope geology, and cosmochemistry. Urey's approach to a new area began with his becoming thoroughly familiar with what was known about the subject of his curiosity and then the formulation of a theory to explain a large amount of uncorrelated material, which was then followed by carefully planned experiments. The latter frequently involved the design of new experimental equipment beyond the state of the art.

As a graduate student in physical chemistry in the early

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1920s, Urey realized that future progress in that discipline would require a knowledge of the quantum theory of atomic and molecular systems, which was undergoing a revolution in Europe. He supplemented his command of mathematics and physics by formal coursework prior to going to the Bohr Institute in Copenhagen in 1923. His exposure there led to his formulation of the concept of the electron spin concurrent with but less complete than the Goudsmit-Uhlenbeck discovery. After completion of his text with Arthur Ruark, *Atoms, Quanta and Molecules*, one of the first English texts on quantum mechanics and its applications to atomic and molecular systems, Urey became interested in nuclear systematics. This led to his discovery of deuterium. The conception of this search, the design of the experiment, the actual discovery, and its publication are a model for the planning and execution of scientific research. His discovery of the differences in the chemical and physical properties of deuterium compounds led to his broader interest in isotope chemistry and isotope separation. Here again he developed the theory that led to the prediction of the magnitude of isotope effects in the light elements. He followed this up with experiments to confirm the theory, and this led to his pilot plants that achieved the first concentration of ^{15}N, ^{13}C, and ^{34}S.

Urey's interest in democratic government and world affairs led to the sense of urgency that developed in the Manhattan Project late in 1941. His major contributions and dedication to the success of the program through his work on uranium isotope separation, heavy water production, and ^{10}B enrichment and his service on the various NRDC and OSRD committees related to the development of the atomic bomb have never been fully appreciated.

With the war behind him Urey conceived the isotope thermometer and its application to geochemistry. From there

he became interested in the moon, formation of the planets, meteorites, the abundances of the elements, and finally, the origin of life. He was a major supporter of the manned mission to the moon and was an active investigator in the program.

Harold Urey was a warm and generous person. He was warm in all his personal relations and generous with his time, attention, and resources. To have known him and worked with him were unequaled experiences for each of the authors of this memoir. None of us could have prepared this memoir alone.

UREY'S EARLY LIFE UP TO HIS ENTRANCE TO GRADUATE SCHOOL IN BERKELEY

Harold Clayton Urey was born in Walkerton, a small town in Indiana, on April 29, 1893. His father, a school teacher and a minister in the Church of the Brethren, died at the time Harold was just starting his elementary schooling. Upon graduation from grade school at age fourteen, Urey barely managed to pass the entrance exams for high school. But in high school he became interested in all aspects of his work, due, he said, to the excellent teachers he had there, and he immediately became the leader of his class in all subjects, a position he maintained throughout his high school years and in college.

When in 1911 at age eighteen he graduated from high school, Urey became a teacher in a small country school in Indiana with some twenty-five children in various grades. After one year he went to Montana, where his mother, stepfather, brother, and sisters had already gone, and taught in small elementary schools.

It was while teaching in a mining camp that the son of the family with which he was living decided to attend college, and this influenced Harold to do the same. He en-

tered the University of Montana in Missoula in the autumn of 1914. By carrying a heavy schedule of courses he was able to complete his college education in three years with a straight A record, except in athletics. He did this in spite of being required by his financial situation to wait on tables in the girls' dormitory and work one summer on the railroad being built there. Many years later in his Willard Gibbs Medal address he spoke warmly of the inspiration he received from the professors at the University of Montana and of the beginning of his interest in science due to their counseling advice, in particular the influence of A. W. Bray, professor of biology. Under Bray's guidance Harold majored in biology, and his first research effort was a study of the protozoa in a backwater of the Missoula River. His interest in the origins of life, a field in which he was to make a major contribution much later at the University of Chicago, originated with that earliest research. Bray also encouraged him to study chemistry, and he obtained a second major in that subject.

World War I began as Urey entered the university, and at the time he completed his work there in 1917 the United States declared war. He was urged by his professors to work in a chemical plant, chemists being badly needed at that time. During the rest of the war he worked at the Barrett Chemical Company in Philadelphia. In 1919 after the end of the war he returned to the University of Montana as instructor in chemistry.

After two years of teaching he realized that if he was to advance academically he would need to obtain a Ph.D. degree. The head of the Chemistry Department at Montana sent a letter of recommendation to Professor Gilbert N. Lewis of the Chemistry Department of the University of California, Berkeley. A fellowship was offered to Harold,

and so in 1921, at the age of twenty-eight, he entered the University of California as a graduate student.

FROM CHEMICAL PHYSICS TO ISOTOPE GEOLOGY

The educational facilities, opportunities, and philosophy of Berkeley's Chemistry Department matched Urey's interests. The department stressed exploration of new ideas through original research and its weekly seminars. There were a minimum of formal requirements. Urey, nevertheless, took the opportunity to enroll in courses in mathematics and physics, which he deemed essential for his education as a chemist. In an unpublished autobiography (ca. 1969), Urey described his two years as a graduate student as "among the most inspiring of any of my entire life." His thesis was self-generated. The first part was an outgrowth of his unsuccessful attempt to measure the thermal ionization of cesium vapor. Bohr, Herzfeld, and Fowler had shown earlier that the ideal gas approximation leads to a dissociation instability for an atom with an infinite number of states below the dissociation or ionization limit. Its partition function is infinite at all temperatures. Urey and later Fermi showed that the correction of the ideal gas approximation for the excluded volume of the dissociating species leads to a convergence of the partition function of the atom or molecule. Urey's result was published in the *Astrophysical Journal*. When he became interested in the moon and planets, Urey was wont to tell his younger astronomy colleagues that he published a paper in the *Astrophysical Journal* before they entered the field. The second part of his thesis was of lesser long-term significance. He attempted to calculate the heat capacities and entropies of polyatomic gases before the correct description of the rotational energy states of molecules had been established by quantum mechanics.

When Urey received his doctorate in 1923, he realized

that there was much he needed to learn about the structure of atoms and molecules. He received a fellowship from the American Scandinavian Foundation and went to the Institute of Theoretical Physics, Bohr Institute, in Copenhagen. The institute under Bohr's leadership was a major center in theoretical physics, particularly the development of the new quantum mechanics and its application to atomic and molecular structure. There Urey became acquainted with Heisenberg, Kramers, Pauli, and Slater and the biochemist Hevesy. Before Urey returned to the United States in 1924, he attended a meeting of the German Physical Society where he met Einstein and James Franck, who later became lifelong friends.

On his return to the United States Urey took a position as associate in chemistry at Johns Hopkins University. There he continued his association with physicists, including Ames, Herzfeld, and Wood of Hopkins; Brickwedde, Foote, and Meggers of the Bureau of Standards; and Tuve of the Carnegie Institution. His research at Hopkins ranged from speculations on the spin of the electron to cooperative experiments with F. O. Rice on the disproof of the radiation hypothesis of unimolecular reactions. With Arthur Ruark, Urey wrote *Atoms, Quanta and Molecules*. He had established himself as one of the new generation of chemists who applied the new quantum mechanics of Heisenberg and Schrödinger to chemistry.

In the fall of 1929 Urey joined the Columbia faculty as associate professor of chemistry. He initiated both experimental and theoretical research. In the former area his work was mainly in spectroscopy—ultraviolet spectra of triatomic molecules and vibrational spectroscopy. He and his student Charles Bradley measured the Raman spectrum of silico-chloroform, a tetrahedral molecule. They found that none of the molecular force fields in use at the time could

reproduce the spectra of tetrahedral molecules. They introduced a new force field, the Urey-Bradley field, which is an admixture of valence bond and central force fields. The Urey-Bradley field remains in use in the analysis of the vibrational spectra of tetrahedral molecules. Urey's theoretical work at that time was directed to nuclear stability and the classification of atomic nuclei.

In 1931 Urey had on the wall of his office a chart of atomic nuclei. On the ordinate his chart was labeled "protons"; on the abscissa he plotted "nuclear electrons." This was prior to the discovery of the neutron. The number of nuclear electrons is the number of neutrons in the nucleus. The atomic number or nuclear charge is the number of protons minus the number of nuclear electrons. For the light elements Urey's chart showed the stable nuclei ${}_{1}^{1}\mathrm{H}$, ${}_{2}^{4}\mathrm{He}$, ${}_{3}^{6}\mathrm{Li}$, ${}_{3}^{7}\mathrm{Li}$, ${}_{4}^{9}\mathrm{Be}$, ${}_{5}^{10}\mathrm{B}$, and ${}_{5}^{11}\mathrm{B}$. From nuclear systematics, Urey and others postulated the existence of ${}_{1}^{2}\mathrm{H}$, ${}_{1}^{3}\mathrm{H}$, and ${}_{2}^{5}\mathrm{He}$. No isotopes of hydrogen or helium other than ${}_{1}^{1}\mathrm{H}$ and ${}_{2}^{4}\mathrm{He}$ were known in 1931. From atomic weight considerations, to be discussed below, it was estimated that, if a stable isotope of hydrogen of mass 2 existed, its natural abundance would be less than 1:30,000 parts of ${}_{1}^{1}\mathrm{H}$.

DISCOVERY OF DEUTERIUM

As early as 1919 Otto Stern reported an unsuccessful search for isotopes of hydrogen and oxygen, other than the ones of masses 1 and 16, respectively. In 1929 two Berkeley chemists, W. F. Giauque (who had been a graduate student contemporary of Urey) and H. L. Johnston, discovered the stable isotopes of oxygen, $^{17}\mathrm{O}$ and $^{18}\mathrm{O}$. Their natural abundances are 0.04 and 0.2 percent, respectively. The chemical atomic weight scale was based on the assumption that oxygen had only one isotope, mass 16. The atomic weight of hydrogen was based on the relative densities of hydrogen and oxygen

gases and the atomic weight of natural oxygen equal to 16. Aston had determined the atomic weight of hydrogen based on $^{16}_{8}$ O = 16. The chemical value of the atomic weight of hydrogen was 1.00777 ± 0.00002. Aston's mass spectrograph value, 1.00778 ± 0.00015, reduced to the chemical scale using the 1931 values for the abundances of ^{17}O and ^{18}O was 1.00756. To reconcile the physical and chemical atomic weights of hydrogen, Birge and Menzel postulated the existence of a stable isotope of hydrogen of mass 2 with a natural abundance of 1:4500.

Urey read Birge and Menzel's communication in *Physical Review* in August 1931. Within days he decided to look for an isotope of hydrogen of mass 2 and outlined his plan of attack. He would need a method of detection, and it would be desirable to prepare samples enriched in this isotope. The design of the experiment was a model of how one should conduct a search for a small effect. It was the prototype of the characteristics of Urey's work for the next two decades. As a method of detection, Urey and his assistant George Murphy chose the atomic spectrum of hydrogen. An isotope of hydrogen of mass 2 should have red shifted lines in the Balmer series. The shifts could be calculated from the Rydberg formula for the energy levels in the hydrogen atom after taking into account the relative masses of the electron and nucleus. They amounted to 1.1 to 1.8 Å in four lines in the visible part of the spectrum. These could readily be resolved with the 21-foot grating spectrograph that had just been installed at the Pupin Laboratory of Columbia University. The latter had a dispersion of 1.2 Å/millimeter in the second order. To enrich the heavy isotope, Urey and Murphy chose the distillation of liquid hydrogen. They estimated the fractionation factor for $^{1}_{1}H^{2}_{1}H$ from $^{1}_{1}H_{2}$ in the range between the freezing and boiling points from a Debye model for liquid hydrogen. Their esti-

mated fractionation factor was 2.5. To achieve an overall enrichment of 100 to 200 above natural abundance would require evaporating 5 liters of liquid hydrogen to 1 ml. The heavy hydrogen should be in this 1-ml residue. There were but two places in the United States capable of producing 5 liters of liquid hydrogen in 1931. They were Giauque's laboratory at the University of California and the low-temperature laboratory at the National Bureau of Standards in Washington, D.C. The NBS cryogenic laboratory had been established by Hopkins physics graduate F. G. Brickwedde, who overlapped with Urey at Hopkins. It is not difficult to understand why Urey chose to collaborate with Brickwedde.

During the period when Brickwedde was preparing the enriched sample, Urey and Murphy determined the optimum conditions for excitation of the atomic spectrum of hydrogen and suppression of the molecular spectrum. They did in fact find the lines to be expected for ${}^{2}_{1}\mathrm{H}$ in the atomic spectrum of natural hydrogen. They delayed publication until these lines could be shown to increase in intensity in an enriched sample. In particular, it was necessary to rule out any possibility that the ${}^{2}_{1}\mathrm{H}$ lines were artifacts (e.g., "ghost" lines from periodic errors in the ruling of the grating or lines from the molecular spectrum). The first of Brickwedde's samples showed no increase in the intensities of the lines attributed to ${}^{2}_{1}\mathrm{H}$. A less persistent person than Urey would have dropped the search. Brickwedde then prepared two more samples each by evaporation of a 4-liter batch of liquid hydrogen, this time close to the triple point, where the enrichment factor is somewhat larger than at the normal boiling point. Spectroscopic examination of these samples on Thanksgiving Day of 1931 confirmed the discovery of hydrogen isotope of mass 2, subsequently named deuterium. Urey reported his success to his wife, Frieda,

when he returned to his home in Leonia, New Jersey, hours late for Thanksgiving dinner.

For the discovery of deuterium, Harold Urey received the Nobel Prize in chemistry in 1934. Urey was the third American to receive a Nobel Prize in chemistry. He was young in comparison with most Nobel laureates in chemistry prior to or since 1934. He was the first of the California school to receive a Nobel Prize. He valued the contributions that his associates made to the discovery for which he received the recognition and shared one-quarter of the prize money with F. G. Brickwedde and G. M. Murphy.

In Urey's Nobel lecture, delivered on February 14, 1935, he called attention to the fact that Aston had just redetermined the physical atomic weight of hydrogen to be 1.0081. This value, if correct, would have brought the physical and chemical atomic weights of hydrogen into exact agreement and invalidated the basis of Birge and Menzel's prediction. Urey would not have undertaken the search for deuterium in 1931 and its discovery would have been delayed, perhaps for years. In 1932 Washburn and Urey discovered the electrolytic separation of deuterium from hydrogen. Dihydrogen gas generated by the electrolysis of water is depleted in deuterium. This fractionation explains the failure of Urey and Murphy to find any significant enrichment in deuterium in Brickwedde's first sample. Brickwedde took special precautions before he undertook preparation of the enriched samples. He took all of his equipment apart and cleaned it thoroughly to eliminate artifacts from impurities. Most significantly, the electrolyte in the cell used to generate the hydrogen to be liquefied was replaced by fresh alkaline solution. Brickwedde literally threw the baby out with the bath water. The dihydrogen produced from fresh alkaline solution is depleted in deuterium. The Raleigh distillation of this liquid hydrogen brought the deuterium con-

tent back to about natural abundance. As more and more water is added to the electrolytic cell to replace that electrolyzed, the deuterium abundances rise to the natural abundance level.

THERMODYNAMIC PROPERTIES OF ISOTOPIC SUBSTANCES

Urey's Nobel address was titled "Some Thermodynamic Properties of Hydrogen and Deuterium." The first part covered the discovery of deuterium. Two-thirds of the address dealt with the differences in the thermodynamic properties of isotopes and the feasibility of isotope separation based on these differences. By the time Urey initiated his work on deuterium, calculation of the thermodynamic properties of ideal gases from spectroscopic data had been placed on a firm foundation. Such calculations are particularly simple when one compares the differences in behavior of isotopic substances. Under the assumption of the Born-Oppenheimer approximation, the large enthalpy changes from the difference in the minima of the potential energies of products and reactants in a chemical reaction vanish for isotopic exchange reactions. Thus, Urey and Rittenberg calculated the differences in the degrees of dissociation of HCl(g) and DCl(g) and HI(g) and DI(g), respectively. They confirmed their calculations with experiments on HI(g) and DI(g). Gould, Bleakney, and H. S. Taylor confirmed the Urey-Rittenberg calculations on the disproportionation of HD into H_2 and D_2. The success of statistical mechanics to predict differences in the chemical properties of hydrogen and deuterium led Urey and Greiff to extend the method to isotopomers of polyatomic molecules of carbon, nitrogen, oxygen, and sulfur. For each of these elements, Urey and Greiff found exchange reactions with enrichment factors in the range from 1 to 4 percent at room temperature.

The predicted enrichment factors led Urey and Greiff to

suggest the chemical exchange method for the separation of isotopes of the light elements. The small elementary effect was to be multiplied by countercurrent flow of two-phase systems. Each phase is to consist principally of one chemical species. When one phase is a liquid or liquid solution and the other is vapor, the process is entirely analogous to distillation. In fact, distillation technology can be readily adapted to chemical isotope separation. To replace the boiler and condenser of a distillation tower, one requires chemical reactors that convert one chemical species to the other quantitatively. The exchange reaction must be rapid and reversible. These principles led Urey and co-workers to develop the $NH_3(g)$-NH_4^+(sol'n.), HCN(g)-CN^-(sol'n.), and $SO_2(g)$-HSO_3^-(sol'n.) reactions for the enrichment of ^{15}N, ^{13}C, and ^{34}S, respectively, during the 1930s. Each of the reactions developed by the Urey school involved acid-base exchange reactions in aqueous solution. These are the fastest chemical reactions. Reflux was achieved by cheap reagents—acids and alkali. Compared with other isotope separation processes, centrifuges, and diffusion, the chemical exchange process and the related liquid-vapor distillation have large throughput per unit volume of separating equipment. Urey and T. I. Taylor also achieved a small enrichment of the lithium isotopes on zeolites, the forerunner of the ion exchange version of chemical isotope separation.

Most of the people who worked with Urey on isotope separation in the 1930s were postdoctoral fellows. This was rather unusual for the time in American universities. These were talented people interested in academic careers, for which there were few openings. In addition, there were professionals, a chemical engineer with expertise in distillation, and a recent Ph.D. in physics who built a Bleakney-type mass spectrometer for Urey's program. Urey had no difficulty getting support from foundations after he discov-

ered deuterium and received the Nobel Prize. In fact, he chose to share an award from the Carnegie Institution of Washington with a member of the Physics Department, I. I. Rabi. Rabi never forgot Urey's generosity and the impact it had on his program on molecular beam research. Urey was a good judge of talent; his investment in Rabi paid off handsomely for science, the Carnegie Institution, and Columbia University. Today, bureaucratic restrictions would make it impossible for someone like Urey to give part of a grant to another investigator, no matter how qualified or promising.

During the 1930s, Urey and his co-workers measured the vapor pressures of compounds enriched in D, ^{15}N, and ^{18}O. The values obtained for ^{18}O were utilized in the partial enrichment of ^{18}O by the distillation of water.

ISOTOPES AS TRACERS

Enriched stable isotopes of H, C, N, O, and S have found wide application in agriculture, biology, chemistry, geology, and medicine. Urey used some of his enriched isotopes, particularly ^{18}O, to carry out tracer studies. He and Cohn measured the acid and base catalyzed exchange between water and acetaldehyde and acetone. They showed that acids and alcohols do not exchange oxygen with water. They provided the basis for Roberts and Urey to show unequivocally that it is the carbon-oxygen bond in the acid that is broken in esterification reactions. Their result has been of major importance in the elucidation of the mechanism of this important class of reactions. The use of ^{15}N as a tracer in biochemistry was initiated by Rittenberg and Schoenheimer with enriched samples supplied by Urey.

PALEOTEMPERATURES

When Urey moved from Columbia to the University of Chicago at the end of World War II, he decided not to

continue his interest in isotope separation or to undertake any research with direct military application. His first priority was to fill a prewar commitment to deliver the Liversidge lecture before the Chemical Society (London). For this purpose he decided to update and expand the earlier calculations of Urey and Greiff on isotope exchange equilibria using the advance method developed by Bigeleisen and Goeppert-Mayer at Columbia (SAM project) in 1943. The method afforded the possibility of calculating the temperature coefficient of an isotope exchange equilibrium constant in addition to the logarithm of the constant with confidence. In the course of these calculations Urey noticed that the fractionation factor for $^{18}O/^{16}O$ exchange between CO_3^{-2} and $H_2O(1)$ would decrease by 1.004 between O° and 25°C. Urey recognized the potential of utilizing this temperature coefficient to measure paleotemperatures.

The method depended on the development of isotopic assay methods with a precision of better than 0.1 percent in the $^{18}O/^{16}O$ ratio at the natural abundance level, which is 0.2 percent. Nier and Thode had each developed the dual collector method of measuring isotope ratios with a precision of 0.1 percent of the ratio. There were additional requirements to be met if the method were to be useful. Isotope exchange equilibrium would have to be established in the precipitation of $CaCO_3$ from H_2O. The record would have to be preserved over millions of years. It would be necessary to know the isotopic composition of the marine water in equilibrium with the $CaCO_3$. Urey assembled a research group that included graduate students, postdoctoral fellows, a paleogeologist, and an expert in electronics to attack these questions in a systematic way. They advanced the precision of measurements of isotope ratios by almost an order of magnitude and routinely obtained a precision of 0.02 percent in the $^{18}O/^{16}O$ ratio in CO_2. In the applica-

tion of their method, an unknown sample was intercompared with a standard using a dual inlet system. The results were expressed in δ o/oo = $[(R_{sample}/R_{standard}) - 1] \times 1{,}000$. The paleotemperature scale was calibrated by the isotopic analysis of $CaCO_3$ samples precipitated from water at known temperatures. The latter yielded a thermometric scale in terms of δ in good agreement with Urey's calculation and subsequent refined calculations by McCrea, a Ph.D. student. The final proof of the paleotemperature-scale concept came with the 1951 publication by Urey, Lowenstam, Epstein, and McKinney. They analyzed the $CaCO_3$ of a 100-million-year-old belemnite collected on the Isle of Skye by Cyril S. Smith. Samples of $CaCO_3$ at various distances from the axis of the belemnite core were analyzed for $^{18}O/^{16}O$. They found the fossil had a life history of four winters and three summers. The winter temperatures were 15°C; the summer temperatures were 21°C. The winters grew progressively colder during the lifetime of the belemnite.

Urey and his group founded a new branch of geology, which has flourished under the leadership of his associates and students and their students. For this achievement he received the Arthur L. Day Medal of the Geological Society of America and the Goldschmidt Medal of the Geochemical Society.

THE WAR YEARS, 1939-44: THE ATOMIC BOMB

Inasmuch as Harold Urey had studied with Bohr during his year in Copenhagen it was natural for him to attend the Fifth Washington Conference on Theoretical Physics in January 1939. It was at this conference that Bohr postulated that ^{235}U was the fissionable isotope. The possible need for separating the uranium isotopes was obvious. As the recognized world leader in isotope separation, Urey's main potential contribution to fission research was clearly in that

field. He thus became one of the members of the dedicated group of scientists, centered at Columbia University, who investigated nuclear fission before government contracts were available and who solicited and ultimately obtained government backing.

Two papers written by Urey in 1938, "The Separation of Isotopes" (1939,1) and "Separation of Isotopes" (1939,5), throw light on the status of isotope separation at that time and on Urey's speculations about methods for separating isotopes of the heavy elements. He proposed a countercurrent flow centrifuge, designed to attain a number of stages of separation in a single machine, thus reducing the number of machines required in a cascade and the amount of material circulated between machines. Countercurrent flow in a machine was to be established by continuous distillation of a liquid (uranium hexafluoride in the case of the uranium isotopes) from the bottom cap of the rotor and condensation on the top cap. The liquid would then be thrown to the periphery and flow down the walls, countercurrent to the vapor flow.

In a third paper, "Separation of Isotopes by Chemical Means" (1940,2), Urey concluded that separation of the uranium isotopes would lead to most interesting progress in the study of the fission process and discussed the centrifugal fractionation column (countercurrent flow centrifuge) as affording the separation method most likely to succeed. In early 1940 it was definitely established that ^{235}U was the isotope fissionable by thermal neutrons. Urey, together with a group of Columbia University faculty members, began work on uranium isotope separation in May 1940, and a contract with President Roosevelt's Committee for Uranium for this work was executed in August. At approximately the same time, Urey was appointed chairman of an Advisory Committee on Nuclear Research to give tech-

nical advice to the Committee for Uranium. He coordinated experimental centrifuge studies at the University of Virginia; gaseous diffusion separation research at Harvard; and thermal diffusion, chemical separation, and centrifugal fractionation at Columbia.

Urey undertook personal direction of research on chemical separation of the uranium isotopes and on separation by the countercurrent centrifuge. The chemical separation involving uranium salts in immiscible solvents was not successful. The distilling centrifuge mentioned above was abandoned in favor of an all-gas countercurrent centrifuge, the theory for which was developed by Karl P. Cohen and the design was developed by Urey together with C. Skarstrom. Because some doubts had been raised by opponents of the centrifuge project about the stability of countercurrent gaseous flow, Cohen devised the theory and C. Skarstrom and Urey developed the design for a single-stage flow-through centrifuge. In early 1941 Westinghouse undertook to build a prototype of the flow-through design, a choice that had fatal consequences for the centrifuge project.

The Advisory Committee on Nuclear Research was soon reorganized under Vannevar Bush's National Defense Research Committee, and Urey and Dean George Pegram of Columbia University became members of a new parent Committee on Uranium. Urey had broad responsibilities for formulating the whole research program.

In view of some uncertainty in 1940 with respect to the feasibility of a divergent chain reaction using natural uranium with graphite moderator, Urey became interested in the use of heavy water as an alternative moderator because of its greater efficiency and its practically zero neutron absorption cross-section. Urey proposed using catalytic exchange between hydrogen and water to produce heavy water in

quantity. He invited professor H. S. Taylor of Princeton to study this process.

Centrifuge work was undertaken in early 1941 by Westinghouse in Pittsburgh and also was continuing at the University of Virginia. Urey turned his attention to the gaseous diffusion process. He reported in November 1940 an initial appraisal of separation by diffusion through porous barriers. K. P. Cohen measured the first actual separation by barriers using CO_2/H_2 mixtures. Estimates of plant size based on these observations showed that a diffusion separation plant would involve as large an undertaking as the centrifuge plant.

The last half of 1941 found the uranium program in a stage of constant ferment. Plutonium had been shown to be fissionable. The English gaseous diffusion process seemed likely to succeed, and the Columbia diffusion system was not far enough along to evaluate properly. Work in Britain indicated metallic uranium, and heavy water provided the best route to a chain reaction. The British were convinced that weapons could be made from reasonably small quantities of ^{235}U. The Uranium Committee was reorganized. A new Office of Scientific Research and Development was created in the Executive Office of the President as the center for the application of science to national defense, and Urey was a member of its Section on Uranium. He was given responsibility for uranium isotope separation by exchange methods and for heavy water production. V. Bush and J. B. Conant had overall responsibility for the uranium program. The chain reaction program was reoriented to plutonium production and weapons production. An electromagnetic separation project had been initiated.

By the end of 1941 and early 1942 the program moved from the research to the engineering and construction phases. The attempt to arouse the government to the military po-

tential of uranium fission had finally succeeded. The chain reaction group at Columbia, headed by Fermi and Szilard, was moved to the University of Chicago. The scope of Urey's direct responsibilities in 1942 included the English diffusion separation method, the American diffusion method, and the centrifugation method. A decision had been made to transfer all uranium work to the United States, and Urey took special pains to see that the British diffusion ideas were seriously considered.

In May 1942 the Section on Uranium's Executive Committee, of which Urey was a member, was asked by J. B. Conant to recommend a program to build atomic weapons. Their proposal to Conant showed strong input from Urey in that it placed great emphasis on centrifuges and heavy water production. This program included construction of a centrifuge plant, a gaseous diffusion pilot and production plant, an electromagnetic plant, pile production of element 94, and a plant for heavy water production. It was estimated that the proposed plans would result in the production of a few atomic bombs by July 1944.

In the summer of 1942 the reported experimental results on flow-through centrifuges were disappointing, showing only 36 percent of theoretical efficiency. Urey's protestations that countercurrent centrifuges would be easier to build and were more efficient were to no avail. Centrifuge work remained at a low level. It is an irony of history that subsequent experiments in 1943 and 1944 proved that countercurrent machines could operate close to theoretical efficiency. At least six nations have at the present time operated countercurrent centrifuges with UF_6, and it is the uranium isotope separation method of choice for five of them.

In November and December of 1942 there was a commitment to a full-scale diffusion plant, a smaller electromag-

netic plant convertible later to full size, and heavy water plants.

The research organization at Columbia University under Harold Urey's direct supervision had been growing rapidly. In 1942 and 1943 Urey attracted many eminent scientists from academia and industry to assist in the development of components of the diffusion plant and in his other activities. By the end of 1943 Urey had more than 700 people working on gaseous diffusion alone and several hundred more, including those at other universities and industrial laboratories, working on various other researches. He had little taste for administration, and the burden weighed heavily on him.

This effort produced some notable successes. A new process was devised for producing heavy water, based on dual-temperature exchange between hydrogen sulfide and water. A successful method for separating the boron isotopes was developed for production of the crystalline ^{10}B needed at Los Alamos. Low-leakage seals for rotating equipment and mass spectrometers for process analysis, and leak detectors, needed for both laboratory research and plant construction, were devised and produced. Progress was made on fundamental theory of separation by diffusion barriers.

However, the barrier remained recalcitrant. Copper barriers were abandoned, and efforts were concentrated on nickel barriers. Both electrodeposited (Norris-Adler) and compressed powder barriers were tried. As 1943 wore on, it was realized that, despite heroic efforts, barriers with the properties, uniformity and ruggedness necessary for manufacture were not available. Nevertheless, a pilot plant for manufacture of the electrodeposited barrier was being completed.

Difficulties were also becoming apparent in the other production projects. The electromagnetic separators that

had been installed in Oak Ridge, Tennessee, were experiencing severe operational problems. The laboratory in Chicago was openly critical of time schedules and of the graphite pile design that had been developed by the Dupont Company.

Urey saw his hopes for a contribution from the uranium program to the imminent 1944 war crisis fade, even as his fears of a German atom bomb remained lively. He renewed his efforts to realize a homogeneous heavy water uranium slurry reactor, proposed by Halban, for plutonium production. This led to the research piles in Chicago and later in the 1950s to the Savannah River plutonium production reactors. Urey championed P. Abelson's liquid thermal diffusion process, which seemed a last hope to achieve timely weapons production. A plant was hastily authorized in Oak Ridge in the second half of 1944 and was used to enrich the feed to the electromagnetic plant.

In the autumn of 1943 a new type of diffusion barrier, combining features of both the electrodeposited and compressed powder barriers that had been previously developed, was proposed by the Kellex Corporation. In the spring of 1944 a plant began producing acceptable barrier material of the previously developed electrodeposited type, whose production had been urged by Urey. Ten thousand workers had been building a huge diffusion plant at Oak Ridge. Early in 1944 the Army (General L. R. Groves commanding) made the decision to rely on the barrier developed at Kellex. Fortunately, both types of barrier eventually proved satisfactory. The first production from the gaseous diffusion plant occurred in March 1945. The plant operated with unprecedented reliability and economy during the postwar period, superseding all other methods, but most of the ^{235}U for the Hiroshima bomb was produced by the electromagnetic separation plant.

Early in 1944 when the decision was made to rely on the new barrier being developed by Kellex, it was clear to Urey that the diffusion plant would have little relevance to the war effort. He relinquished barrier development to his associate directors. Urey remained nominal head of the Columbia laboratories until 1945, but his heart was not in it. From that time forward his energies were directed to the control of atomic energy, not its application.

COSMOCHEMISTRY

Urey moved from Columbia to Chicago in 1945. Shortly thereafter he read a book by Ralph Baldwin, *The Face of the Moon*, which started him on a love affair with that object, which continued for the rest of his career. Colleagues at Chicago, or any available listeners, would be treated to monologues, sprinkled with the names of craters and other technical terms, which were impressive though bewildering. Urey came to regard study of the moon as a key to understanding the origin of the solar system. This led to a sustained, audacious attack on the broader problem.

His 1952 book, *The Planets*, is generally agreed to have begun the modern science of the solar system; it brought the term "cosmochemistry," as distinguished from geochemistry, into the language. The work systematized the state of our knowledge at that time and set forth a research agenda emphasizing physicochemical and chronological study of meteorites, the oldest and least altered materials in our possession.

Two papers out of many in the following years were especially influential. Craig and Urey (1953,1) reclassified the meteorites using chemical criteria and set the stage for detailed comparison between meteorite (chrondite) chemical abundances and those of nonvolatile elements in the sun and other stars. Suess and Urey (1956,2) used meteoritic

and solar abundances to make an improved table of abundances of the elements, showing clearly the influence of nuclear shell closure and other specific nuclear effects on elemental and isotopic abundances. This paper was the basis for the first successful account of the origin of the chemical elements in stars by Burbidge, Burbidge, Fowler, and Hoyle (1957). The intimate interplay between chemical and astrophysical problems became widely understood for the first time.

It is sobering to realize that when Urey wrote *The Planets* even so basic a fact as the age of the earth was not yet settled. He began to look for experimental areas beyond the $^{18}O/^{16}O$ system where his mass spectrometric skills could be applied. A young graduate student named Jerry Wasserburg turned up at Chicago, and Urey put him to work on the $^{40}K/^{40}Ar$ isotopic dating system. His thesis, which involved collaboration with R. J. Hayden of the Argonne National Laboratory and Professor Mark Inghram, was a first step on the path by which Wasserburg made a number of fundamental contributions to geochronology in subsequent decades.

Urey took great interest in the existence of diamonds in two classes of meteorites: stony objects called ureilites (not named for him) and big metallic meteorites like the one that made Meteor Crater in Arizona. He hoped that the diamonds were formed in thermodynamic equilibrium at high pressures. We know now that in the iron objects they were formed by shock; the situation in the ureilites is not so clear.

It was a natural step for a chemist thinking about the origin of the planets to think about the origin of life on this particular one and perhaps on Mars or elsewhere. Starting from the abundance of hydrogen in the sun and other stars, and the abundance of methane in the outer planets, Urey

concluded that it was likely that the earth's atmosphere was originally reducing, rich in CH_4 and NH_3 rather than CO_2 and N_2. He suggested that thermodynamics favored the formation of organic compounds in such an atmosphere.

Not long after this a graduate student named Stanley Miller presented himself to Urey and proposed to do an experimental thesis testing this hypothesis in the laboratory. Urey told him it was too difficult for a thesis problem, but Miller won a grudging permission to try. Within a month he was exhibiting organic muck in a flask containing methane, ammonia, and water, excited by an electric discharge as a model for lightning discharges in such an atmosphere. Miller showed that the solution products contained amino acids and other possible precursor compounds for life.

Some of us were present at a crowded seminar in which Miller presented his results, with Urey in the front row. By the end of the presentation it was obvious to all that this was an important milestone. In the question period Enrico Fermi turned to Urey and said, "I understand that you and Miller have demonstrated that this is one path by which life might have originated. Harold, do you think it was *the* way?" Urey replied, "Let me put it this way, Enrico. If God didn't do it this way, he overlooked a good bet!" Today the assumed early reducing atmosphere is no longer widely accepted, but the impetus given by Urey still remains.

THE LA JOLLA YEARS

In 1958 Urey passed a milestone—his sixty-fifth birthday. When it became clear that he would then become emeritus at the University of Chicago, his friends at the newly forming University of California, San Diego, led by Roger Revelle, offered him an appointment there, and he accepted. That was the year after the Soviet launch of Sputnik I, and national attention was focused on space. The National Aero-

nautics and Space Administration also was new, and the first U.S. satellites were reaching orbit after some embarrassing failures.

At UCSD Urey joined a small number of younger faculty members who were planning a major university with a strong science and engineering side. He immediately started up a vigorous research program, involving both carbon and oxygen isotopic measurements (for paleotemperatures and other purposes) and comparable data for heavier solid and gaseous elements for dating. At the same time, his presence on campus gave a mark of quality to the place that other new foundations could not match. While professing himself unsuitable for any administrative functions, Urey was flying to Washington frequently to press advice on the new space agency. According to Robert Jastrow, it was Urey who persuaded NASA to make unmanned missions to the moon an early focus of its space efforts.

In 1960 UCSD formed its Department of Chemistry, with Urey as one of its founding members. Others were old friends and disciples—Joe Mayer, Jim Arnold, Hans Suess, and Stanley Miller. Urey was particularly emphatic about the importance of biochemistry, which became a major component of the developing department. In the following years he influenced the department and university mainly by example.

Urey was very active at the time of Apollo 11, when the first lunar samples were returned and the first data were appearing. Though he (unfortunately) almost never talked about the past, he told his colleagues one story that time. He told us that it was only in 1910, when he was seventeen years old, that he saw his first automobile in rural Montana. Less than sixty years later his friends showed him the first rock returned from the moon, an achievement in which he had played a significant role.

His powers of concentration, even into his eighties, were

remarkable. He could think intensively about one problem for long periods; his well-known absentmindedness was the inverse of his sharp focus on one important problem at a time. He loved science.

One scene may give the flavor of the man at the end of his career. On any given morning he might burst into the office of a colleague eager to talk. Seeing the colleague perhaps discussing current research with students, he would apologize and begin to back out. Of course, he was invited in. He would then rush to the blackboard and begin "I've finally figured out. . . ." He would soon be pouring out words faster than even close associates could assimilate them. "Does that seem right?" he would say at the end. Maybe one question or comment would emerge. He'd thank the group warmly, again apologize, and rush out. The effect of this display on young graduate students was remarkable.

Urey's last two scientific papers were written and published in 1977, when he was eighty-four years old. Years earlier the largest research building on campus, housing chemists and engineers, had been christened the Harold and Frieda Urey Hall, to recognize the role they both played in the founding and early development of UCSD.

UREY'S PERSONAL LIFE AND HIS POLITICAL AND EDUCATIONAL ACTIVITIES

Thus far we have been concerned with the scientific achievements of Harold Urey. He is also well remembered by all who knew him as a person intensely interested in the well-being of his fellow man. This concern was displayed not only for his students, research associates, and faculty colleagues but also with respect to social and political problems of national and international importance. He had a great interest in such problems, some of them closely related to the wartime work with which he had been involved.

He devoted the same concentrated effort and careful thought to their possible solutions as he did to the solving of the problems of his scientific researches. Having concluded that certain actions were required, he then, with the same vigor and determination that were characteristic of his scientific work, would bend every effort toward furthering these actions.

While at Columbia University he had been chairman of the University Federation for Democracy and Intellectual Freedom and a champion of loyalist Spain. As early as 1932 he espoused Clarence Streit's Atlantic Union plan for a world governmental federation. He became greatly disturbed by the rise of Hitler and the progress of Nazism. He was active in securing posts for refugee scientists and in extending his hospitality to them when they arrived in this country. In her book *Atoms in the Family,* Laura Fermi recounts how Harold and his wife Frieda helped her and her husband Enrico become their neighbors in Leonia, New Jersey, when the Fermis arrived at Columbia University from Italy.

As World War II ended, Urey, then at the University of Chicago, became concerned and worried about the potential of atomic bombs, in whose creation he had played such an important role. His interest in world government, begun at Columbia University, returned with renewed vigor. He worked diligently on the public speaker's platform and, through his writings presented in the press, toward the creation of a world free of the dangers and dread of war.

During this period of lecturing and writing on the problems created by nuclear energy developments, Urey actively opposed congressional passage of the May-Johnson bill, which he feared would permit military control of peacetime activities in the field of nuclear energy. He strongly supported the eventual McMahon bill in its final form and was a leader in the fight for its passage. His doubts concerning the jus-

tice of the executions of Ethel and Julius Rosenberg for atomic energy secrecy violations received national attention. His views on this matter were not ones that were popular with large sections of the American public. He was called before the House Un-American Activities Committee. He wrote, "I doubted seriously if justice had been done. I was only interested in one question. Had they indeed violated the laws of the United States and had justice been done? It is my firm conviction that justice was not done in that case."

Harold Urey was an educator, in the undergraduate and graduate classroom, in the research laboratory, and on the public platform. At the end of World War II, he returned to teaching at a time when many, including himself, felt that this country had, in the course of intensive war research, temporarily abandoned both basic research and the training of a new generation of scientists. At this time, when many were worried about how to keep the "secret" of the atomic bomb and how to prevent dominance by the Soviet Union, he wrote, "The real problem that faces this country is a long-term one. It is a problem of the proper education and inspiration of our youth." He approached with great zest the teaching not only of graduate students but also of first-year undergraduate chemistry courses. His interest in the "inspiration of our youth" even extended to public grade schools. His wife Frieda wrote, "He enjoyed nothing so much as taking his moon-globe to the fifth grade class in the La Jolla schools and telling the students about the moon and the planets."

Harold was fond of and proud of his family. While at Johns Hopkins University, he visited his mother, then living in Seattle. While on that visit he renewed his acquaintance with a friend from his University of Montana days, and she introduced him to her younger sister Frieda Daum, who was working as a bacteriologist. As Roger Revelle of La Jolla

described it, "Harold Urey was never thought of as an outdoor man but he spent the next two weeks hiking in the Cascade Mountains with Frieda. Within a year they were married and their careers as mountaineers were ended. After that Harold's outdoor activity was confined to his garden." Frieda and Harold had three daughters (Elizabeth, Frieda, and Mary Alice) and one son (John). At the time Harold was notified of the award of the Nobel Prize, Frieda was expecting their third child. In order to be with Frieda he did not attend the December 10 ceremonies in Stockholm. Mary Alice was born on December 2, and Frieda and Harold sailed for Stockholm the following February and attended a special award ceremony.

The friendliness and hospitality of Harold and Frieda and their family brought people together socially in a way that created a most pleasant academic atmosphere and added greatly to the enjoyment of life on the part of the families of Harold's fellow faculty members and of the research associates and students in the universities of which he was a member.

Harold was greatly concerned with the welfare of his scientific colleagues, students, and postdoctoral research associates. He was always interested in his students' development of their own independent scientific careers. He was concerned that they be established in suitable posts upon completion of their researches in his laboratory and was active in locating suitable academic and other positions for them. He was diligent in seeing that they received appropriate credit for their work under his supervision. The first paper on the establishment of the Urey paleotemperature scale was published under the sole authorship of his student John McCrea. Likewise at Urey's insistence, the sole author of the first article on the Urey-Miller theory and

experiment on the origin of terrestrial life was his student Stanley Miller.

Abolition of the boundaries between scientific disciplines was a basic tenet of Urey's philosophy. His own academic career, described above, was exemplary in this respect. Concerning his stay at Bohr's Institute for Theoretical Physics he said, "Bohr didn't know I was a chemist. He thought I was a physicist." He claimed he had learned most of his physics in Copenhagen restaurants while dining with Professor H. A. Kramers. In 1933 Harold founded the interdisciplinary *Journal of Chemical Physics*, which provided an appropriate medium for publication of the already-large body of work that bridged the traditional fields of chemistry and physics. He became the first editor of that journal, a position he held until 1940, by which time it had become a leading scientific journal.

Urey may be considered to have established at least four fields of scientific research: stable isotope chemistry, including isotope geochemistry, geochronology, and isotope separation; paleotemperature measurement; cosmochemistry; and the origin of terrestrial life. The scope of his interests and influence are reflected in the thirty-two chapters of the monograph *Isotopic and Cosmic Chemistry* contributed by former students, postdoctoral associates, and colleagues on the occasion of his seventieth birthday. Karl Cohen, in an obituary in the *Bulletin of the Atomic Scientists*, said, "Urey's pioneering work underlies every method of isotope separation successfully employed on a large scale, for every element from hydrogen to uranium." Craig, Miller, and Wasserburg, in their introduction to *Isotopic and Cosmic Chemistry*, wrote, "The measurement of the paleotemperatures of the ancient oceans stands as one of the great developments of the earth sciences; a truly remarkable scientific and intellectual achievement." Cohen, Runcorn, Suess, and Thode in the *Biographi-*

cal Memoirs of the Royal Society of London, speaking of Harold as "the founder of the field of cosmochemistry" wrote, "Urey, undoubtedly, was the first who rigorously defined this field by its problems and by asking precise questions." His ideas concerning the primordial atmosphere and the beginning of life on earth opened up a completely new approach to the study of the origin of life on this planet.

Urey's vigorous and concentrated pursuit of these researches and his enthusiastic interactions with those around him concerning his latest ideas continued to the end of his life. After his retirement at age sixty-five from the University of Chicago and his arrival at the University of California, his work continued with unabated intensity, and he published 105 scientific papers, 47 of them concerned with his study of the moon, in the remaining twenty-three years of his life. Some ten years after his retirement from Chicago he was asked by Professor James Arnold in La Jolla, "Harold, why do you put in so many hours at work?" Urey replied, "Well, you know I'm not on tenure anymore."

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HAROLD CLAYTON UREY

Born: Walkerton, Indiana / April 29, 1893
Married: June 12, 1926 to Frieda Daum
Children: Gertrude Elizabeth Baranger
Frieda Rebecca Brown
Mary Alice Lorey
John Clayton Urey
Hobbies: Gardening and raising orchids (cattleya, cymbidium, and others)

EDUCATION

University of Montana, Missoula, 1914-17, B.S. in biology with a minor in chemistry
University of California, Berkeley, 1921-23, Ph.D. in chemistry with a minor in physics
American-Scandinavian Foundation Fellow, Niels Bohr Institute for Theoretical Physics, Copenhagen, 1923-24

PROFESSIONAL EMPLOYMENT

1911-14	Teacher in rural schools in Indiana, 1911-12; Montana, 1912-14
1918-19	Barrett Chemical Co., Baltimore, research chemist
1919-21	University of Montana, instructor in chemistry
1924-29	Johns Hopkins University, associate in chemistry
1929-36	Columbia University, associate professor of chemistry, 1929-34; Ernest Kempton Adams Fellow, 1933-36; professor of chemistry, 1934-45; executive officer, Department of Chemistry, 1939-42; director of war research, SAM Laboratories, 1940-45
1933-40	*Journal of Chemical Physics*, editor
1945-58	University of Chicago, Institute for Nuclear Studies: Distinguished Service Professor of Chemistry, 1945-52; Martin A. Ryerson Distinguished Service Professor of Chemistry, 1952-58
1956-57	Oxford University, George Eastman Visiting Professor
1958-70	University of California, San Diego, professor of chemistry-at-large

HONORS, PRIZES, AND AWARDS

1934 Nobel Prize, Chemistry
Willard Gibbs Medal, American Chemical Society
1935 Silver Medal, Research Institute of America
1940 Davy Medal, Royal Society, London
1943 Franklin Medal, Franklin Institute
1945 Manhattan Project Certificate of Award for Service, U.S. War Department
1946 Medal of Merit, President Harry S. Truman
1950 Distinguished Service Award, Phi Beta Kappa
Centennial Award, Northwestern University
1957 Jesuit Centennial Citation, Chicago
1960 Silver Medal, Research Institute of America
Cordoza Award, Tau Epsilon Rho Law Fraternity
1961 Alexander Hamilton Award, Columbia University
1962 J. Lawrence Smith Medal, National Academy of Sciences
1963 Remsen Memorial Award, American Chemical Society, Baltimore Section
1964 University of Paris Medal
National Medal of Science
1966 Gold Medal, Royal Astronomy Society, London
American Academy of Achievement Award, Golden Plate Award
1967 Man of Distinction Award, Women's Guild of Temple Emanu-El, San Diego
1969 Chemical Pioneer Award, American Institute of Chemists
Arthur L. Day Medal, Geological Society of America
Leonard Medal, Meteoritic Society
1970 Linus Pauling Award, Oregon State University
1971 400th Anniversary of Johann Kepler Medal and Citation, American Academy of Arts and Sciences
1972 Gold Medal Award, American Institute of Chemists
1973 Honorary Council and Medal, Higher Council of Scientific Research, Barcelona
Silver Medal, 50th Anniversary of International Fair of Barcelona
Knights of Malta Award
Priestley Medal, American Chemical Society

NASA Medal for Exceptional Scientific Achievement
1974 Headliner Award, San Diego Press Club
Medallion, Honorary Member of Indiana Academy, Indianapolis
Dedication of the Harold C. Urey Laboratory for Isotopic Paleotemperature Research, University of Miami, Coral Gables, Florida.
1975 V. M. Goldschmidt Medal, Geochemical Society
1976 NASA Group Achievement Award, U.S. Members of Joint Editorial Board for Foundations of Space Biology and Medicine, for joint US/USSR treatise
1978 Honorary member UCSD chapter of Phi Beta Kappa

HONORARY DEGREES

1935 Princeton University, D.Sc.
University of Montana, D.Sc.
1939 Rutgers University, D.Sc.
1946 Columbia University, D.Sc.
Oxford University, D.Sc.
1948 Washington & Lee University, D.Sc.
1951 Yale University, D.Sc.
University of Athens
McMaster University, D.Sc.
1953 Indiana University, D.Sc.
1955 University of California, LL.D.
1957 University of Birmingham, D.Sc.
University of Durham, D.Sc.
1958 Wayne State University, LL.D.
1959 Hebrew Union College, Jewish Institute of Religion, D.H.L.
1960 University of Saskatchewan, D.Sc.
1962 Israel Institute of Technology, D.Sc.
1963 Gustavus Adolphus College, D.Sc.
University of Pittsburgh, D.Sc.
University of Chicago, D.Sc.
1965 University of Notre Dame, LL.D.
1966 University of Manchester, D.Sc.
1967 University of Michigan, D.Sc.
1969 Franklin and Marshall College, D.Sc.
1970 McGill University, D.Sc.

PROFESSIONAL ASSOCIATIONS

Academia Scientiarum Olisiponensis, Lisbon (Lisbon Academy of Sciences)
Academie Royale des Sciences, des Lettres et des Beaux Arts de Belgique (Honorary)
American Academy of Arts and Sciences
American Association for the Advancement of Science
American Association of University Professors
American Astronomical Society
American Astronautical Society (fellow)
American Chemical Society
American Geophysical Union (honorary fellow)
American Institute of Chemists (honorary)
American Philosophical Society
American Physical Society
Associacion Venezolana para el Avance de la Ciencia (honorary)
Chemical Society, London (honorary fellow)
Federation of American Scientists (life member)
Franklin Institute (honorary)
French Chemical Society (honorary)
German Society of Aeronautics and Astronautics (honorary)
Geological Society of America
Illinois State Academy of Science
International Association of Geochimica and Cosmochimica
International Astronautical Academy
International Platform Association
Mellon Institute (honorary)
Meteoritical Society (fellow)
National Academy of Sciences
National Institute of Sciences of India (honorary)
Phi Sigma Biological Society (honorary)
Royal Astronomical Society, London (associate)
Royal Institution, London (honorary)
Royal Irish Academy (honorary)
Royal Society, London (foreign member)
Royal Society of Arts and Sciences, Göteborg
Royal Swedish Academy (honorary)
Smithsonian National Association
Société Royale des Science de Liège (foreign member)

Weizmann Institute of Science (honorary fellow)
World Academy of Arts and Sciences, American Division (corresponding member)

CLUBS

Chemists' Club, New York (honorary)
Cosmos
Quadrangle and Tavery (Chicago)

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