ICARUS 48, 348–352 (1981)

OBITUARY

Harold Clayton Urey: 1893–1981

Harold Urey was one of the founders of modern planetary science, a major force in early American lunar exploration, an Associate Editor of *Icarus* from its inception in 1962 through 1979, and a member of the founding Advisory Board of The Planetary Society. His involvement with planetary science began in the late 1940s, and it is easy to forget that by then Urey had already completed several major scientific and public careers.

He was born in Walkerton, Indiana, on April 29, 1893, before the discovery of the electron or the invention of the airplane. His father, Samuel Clayton Urey, died as he was beginning elementary school, and his primary and secondary education owed much to the devotion of his mother, Cora Rebecca Reinoehl Urey. He graduated from high school in Montana, to which his family had moved, in 1911, and spent the next three years teaching in rural schools in that state and in Indiana. Urey then entered Montana State University majoring in zoology, his first research project being on Missoula River protozoa. His interest in biology remained with him all his life: its expression ranged from raising orchids to his trailblazing experiments with Stanley Miller on the first steps in the origin of life; his seminal 1960 reports for the Space Science Board of the National Academy of Sciences (of which he had been a founding member) urged that the understanding of the origin of the solar system and the search for life on other planets should be the principal scientific objectives of planetary exploration. "I don't like rocks," he once confessed. "I like life."

Urey received his baccalaureate degree in 1917, just as the United States entered World War I, and soon thereafter found himself working in Philadelphia as a research chemist in a munitions factory. He attributed his interest in an academic career to this experience. After the Armistice he returned as an instructor in chemistry at Montana State and in 1921 entered graduate school at Berkeley under the gifted chemist, G. N. Lewis. His developing interest in thermodynamics under Lewis evolved into his doctoral research on the entropies and heat capacities of gases, derived in part from spectroscopic data. This led naturally to an interest in early quantum theory, and he spent his first postdoctoral year, 1923–1924, with Neils Bohr at the Institute for Theoretical Physics in Copenhagen. From 1924 to 1929, Urey was at the Johns Hopkins University where he developed a graduate course on quantum mechanics which evolved into the textbook *Atoms, Molecules and Quanta*, written jointly with A. E. Ruark. This was one of the earliest books in the famous McGraw–Hill International Series in Physics, bound in two shades of green. At the time of its publication the only other books in the series were *Quantum Mechanics* by Condon and Morse and *The Structure of Line Spectra* by Pauling and Goudsmit. The book is a straightforward exposition concentrating on the transition from Bohr's quantum mechanics to the wave mechanics of Schrödinger and Heisenberg; it emphasized atomic and molecular spectroscopy and ran to almost 800 pages. The book is still fascinating five decades later. In it we can find a diagram on the abundances of the elements in stony meteorites (p. 41), and two pages later the speculation, stated to be "now quite generally believed," that nucleosynthesis occurs in stars and is the source of "highly penetrating" (presumably cosmic) radiation. This

0019-1035/81/120348-05$02.00/0

was not only before Bethe's discovery of the CN cycle in stars, it was before Chadwick's discovery of the neutron. As early as 1924 he was discussing stellar spectroscopy, from the viewpoint of fundamental quantum mechanics, in *The Astrophysical Journal*.

The next year, having moved to Columbia University, he announced that he, together with G. M. Murphy and F. G. Brickwedde, had discovered a heavy isotope of hydrogen, called deuterium (D). This was fundamentally a spectroscopic discovery, but it depended on his prediction of different vapor pressures of H_2 and HD, and, therefore, the possibility of separating the two by fractional distillation. For this work, Urey received the 1934 Nobel Prize in chemistry. Two decades later, his Illinois license plate bore the atomic weight of deuterium to several significant figures. Throughout the rest of the 1930s he occupied himself with the identification and isolation of isotopes of oxygen, nitrogen, carbon, and sulfur and their medical and biological applications; he would often point out the importance of these five atoms for living things. In the same period Urey became the first editor of the *Journal of Chemical Physics*.

By 1940 another war had intervened to change the course of Urey's life. He had championed the anti-Franco cause in Spain, and been active in helping refugees from Nazi-dominated Europe. He was Chairman of Columbia's chemistry department and quickly realized the military and political implications of the Hahn and Strassman discovery of nuclear fission. From 1940 to 1945 he led Columbia University's major contribution to the Manhattan Project (and also briefly welcomed such emigres as Enrico Fermi, Edward Teller, and Leo Szilard; all three eventually wound up with him at the University of Chicago). Urey's responsibility was the development of the most effective means for large-scale production of heavy water, intended as a neutron moderator, and for the separation of uranium isotopes. This was work which naturally followed his research of the previous decade. He pioneered the gaseous diffusion method for separating the isotopes of uranium hexafluoride, which led to the massive isotopic separation facilities at Oak Ridge, Tennessee, and elsewhere.

The ^{235}U produced in this way was the fissionable material that powered the Hiroshima nuclear weapon. In addition, the high-temperature nuclear reactions between deuterium atoms, or between deuterium and the still heavier hydrogen isotope, tritium, was later central to the development of thermonuclear weapons. While recognizing the apparent military necessity for the development of the first atomic bombs, Urey, like most of the principals in the early development of these weapons, was appalled by the consequences of their use on Japan. "Atomic bombs are evil," he wrote, in 1946. "They cannot be used to maintain peace." In the same year he foresaw the so-called *n*-nation problem ("Even small nations can make these bombs in numbers if they are such utter fools as to engage in the lethal business"); the possibility of diversion of fissionable material ("Atomic power plants must necessarily contain sufficient of these materials to make bombs. Undetected diversion of these materials for use in bombs might be comparatively easy"); and overkill ("Enough can be made to destroy completely all possible targets and kill the inhabitants of all major cities in any country. It is then impossible to destroy them twice or to kill people twice. Eventually, therefore, we cannot hope to keep ahead of other countries in an atomic war"). These remarks are contained in his contribution to the book *One World or None*. He believed that no nuclear power plants and no nuclear propelled naval vessels should be developed until "an atomic armament race" was prevented by international agreement. He said "There is no constructive solution to the world's problems except eventually a world government capable of establishing law over the entire surface of the Earth." But not many people listened.

He severely criticized the reckless and pathological activities of Senator Joseph R. McCarthy, and made an impassioned but unavailing appeal to save Julius and Ethel Rosenberg, convicted of betraying "atomic secrets," from execution. (When praised for his courage in these matters, he would smile and say that they were among the few benign consequences of the institution of academic tenure.)

In his late fifties, with a distinguished record of professional accomplishment and public concern, Urey might well have faded slowly into retirement; had he done so, no one would have accused him of an unproductive career. But instead he embarked on what may well be considered, if we manage to avoid nuclear self-annihilation, the most important of his contributions. After the war he moved to the University of Chicago which, under President Robert Maynard Hutchins, was actively recruiting the finest minds in the country. There, before moving into planetary science, he began measuring the oxygen-16 to oxygen-18 ratio in calcareous belemnites with a high-precision Nier mass spectrometer. These little beasts concentrated one oxygen isotope with respect to the other from the Mesozoic seas which they inhabited. But the isotopic fractionation was a function of temperature. Urey was able to measure the temperatures of the Cretaceous seas. They turned out to be quite warm. It was another dip into biology.

In the summer of 1950, Urey and Harrison Brown agreed to give a summer course on "Chemistry in Nature" at the University of Chicago. In preparing his lecture notes for this course, Urey found that on such questions as the heat balance of the Earth and the fractionation of the chemical elements during the early history of our planet he had something to say. At about the same time he was fascinated by Ralph Baldwin's 1949 book *The Face of the Moon*, published by the University of Chicago Press. Baldwin discussed basaltic lava flows, cratering statistics, and a general attempt to describe the evolution of the lunar surface. Indeed the story of lunar cratering mechanics was not entirely unlike the physics of the excavation of craters made by Urey's nuclear weapons. The twin themes of the origin of the Earth and the origin of the Moon converged into a full-scale rethinking of the nature and origin of the solar system, which found a systematic exposition in Urey's Silliman lectures at Yale, published in 1952 as *The Planets: Their Origin and Development*.

This was also the time when, at the University of Chicago, Gerald P. Kuiper was making great progress both in physical studies of the solar system and in his developing ideas on its origins. Although there was a considerable degree of mutual acrimony, there is no question that their ideas cross-fertilized. Urey wrote "Perhaps it will surprise readers of this volume that a physical chemist should undertake to prepare a book on the planets . . . indeed it astonished me. . . . However, as astronomers have had undisputed possession of the field since ancient times, except for some interference from religious leaders and ancient religious writings, some discussion from other sciences may prove useful." The mere fact that a scientist of Urey's eminence considered a full-scale treatment of planetary cosmogony possible was a major contribution to the field, quite apart from his specific conclusions. The book is filled with candid and illuminating comments on the scientific method such as "I early expressed certain tentative views with more confidence than was justified, was attacked for them, and found myself trying to justify them when perhaps they should have been abandoned."

The book's principal conclusions—then thought quite radical—were that the terrestrial planets were formed at low temperatures, and that the core of the Earth differentiated from the mantle "at least partly" during geological time. Urey's advocacy of the view, shared with Kuiper, that terrestrial planets arose from massive protoplanets of cosmic composition has not stood the test of time as well (although his later view that they accumulated from objects of roughly lunar mass may fare better). But *The Planets* is still enormously provocative. For example, in a one-paragraph discussion of Titan we read "The mean temperature of Titan should be about 90°K, and the vapor pressure of methane at this temperature is 0.1 atm. . . . The atmosphere is apparently not saturated at the surface. However, the presence of noncondensible gases, which might be nitrogen and argon, would provide an inert atmosphere, and hence just as in the case of water on Earth less than a saturated amount of methane should be present . . . it may be that methane forms glaciers on Titan, since the melting point is 90.7°K." Or, "The calculation on the heat balance of the Moon . . . shows that the interiors of objects of similar mass regardless of their original temperatures must have risen above the melting point of ice in their interiors, and hence the water of the jovian moons must all be at or near their surfaces. In fact, water flows instead of terrestrial lava flows may occur from time to time." He would not have excluded Enceladus. Or, in a discussion of the evolution of the terrestrial planets: "As time progressed, hydrogen would be lost from all these planets and photochemical dissociation would produce hydrogen from water in the high atmosphere which would escape while the oxygen remained. Gradually, ammonia would be oxidized to nitrogen and methane to carbon dioxide. However, intermediate oxidation states would include many organic compounds such as aldehydes, acids, amines, amino acids, and so forth, and the oceans should have been more or less dilute or concentrated solutions of organic compounds . . . it can be postulated that photochemical processes arising from ultraviolet light from the Sun or atmospheric electrical processes caused the formation of such thermodynamically unstable compounds. They are soluble in water and in the absence of organic life would remain for long periods of time in the primitive oceans . . . this would provide a very favorable situation for the origin of life."

It was on this question of the origin of life that I first went to see Urey in 1952, before *The Planets* had been published. He was very accessible and generous to an enthusiastic but very unsophisticated undergraduate and, among other things, he urged me to look up a graduate student of his who was carrying out an experimental program to check his suggestion about amino acids and other organic molecules. The student was named Stanley Miller, and the results were first hinted at later that year. Urey's starting point had been the realization that cosmic abundances require the early composition of the Earth's atmosphere to be reducing. When asked more specifically what organic compounds he expected to be made, he replied "Beilstein," referring to the massive German language compendium on all organic compounds known to humans. He was not far wrong. When Miller presented their results in a colloquium at the chemistry department at Chicago, there were many in the audience who voiced their concern that some terrible mistake had been made—that, for example, Miller had exercised insufficient care in sterilizing his reaction vessel and that the ninhydrin-positive compounds he was detecting were not prebiological but biological. Urey rose vigorously to Miller's defense, arguing both that the control experiments had been performed and that Miller's amino and alpha-hydroxy acids were precisely the sorts of compounds that *should* be produced in such experiments. He was, of course, right. The Miller–Urey experiment is now recognized as the single most significant step in convincing many scientists that life is likely to be abundant in the cosmos—a triumphant contribution to Urey's old love, biology.

When I remember Urey the man I see a melange of images: He once called his secretary from Pittsburgh and asked, a little petulantly, "I'm in Pittsburgh. Why am I here?" (He had a certain tendency toward absentmindedness). I recall one geologist cautioning, in the acknowledgments to an important paper, that not all those he had thanked were in agreement with his conclusions, "as one of them has been at some pains to point out." That was Urey. I see him lecturing gently before the fledgling "University of Chicago Astronomical Society" that Toby Owen and I founded, and I see him in furious debate at scientific meetings—where he would often rise to correct the pronounciation of the world kilometer. When it's a *unit* of measurement, he would say, you accent the first syllable: cénti-meter, not centímeter. When it's an *instrument* of measurement, you accent the second syllable: thermómeter, not thér-mometer. Thus, kílometer, not kilómeter. I remember his willingness to change his mind in a case where he had blocked the advancement to tenure of a young scientist at another institution and then later asked to be forgiven. He would tell his graduate students that he would be happy to have his name on their papers or not, according to what they thought would best aid the advancement of their careers. I recall a luncheon I had with him at a COSPAR meeting in Warsaw in the early 1960s, after he had moved to the University of California at San Diego, in which he complained about being treated as "the fastest gun in the West." To make their reputations, some younger scientists had come gunning for him, he said. Then he brightened, and concluded that the youngsters had their merits: they forced him to reconsider his ideas. He was 70 years old.

I look at my ancient copy of *The Planets* and at the signature he wrote for me on the title page and think about his role in guiding NASA into serious scientific exploration of the Moon, of his delight at the results from the early Ranger and Surveyor missions, how he helped make experimental solar system cosmogony and the search for extraterrestrial life respectable, about how much the planetary community owes to him. My last letter from him, dated May 20, 1980, accepts our invitation to serve on the Board of The Planetary Society. He died seven months later, in his 88th year, a scientist who transcended disciplinary boundaries, who confounded the traditional wisdom about significant research being the province of the young, and who helped carry us to the Moon and the planets.

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