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H. RICHARD CRANE
1907–2007

A Biographical Memoir by

JENS C. ZORN

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Biographical Memoir

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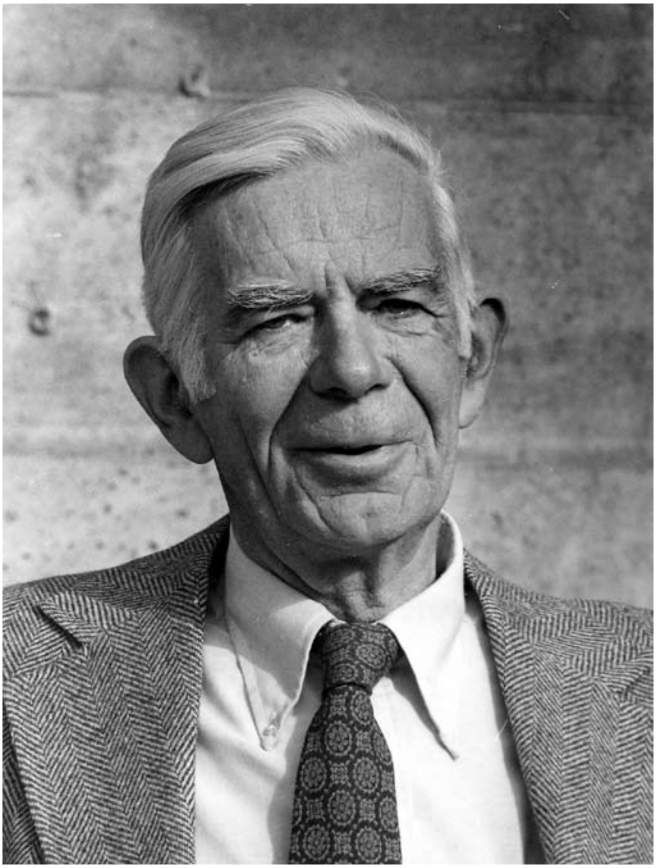

H. Richard Crane

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November 4, 1907–April 19, 2007

BY JENS C. ZORN

AN EXTRAORDINARY PHYSICIST with relentless curiosity and quiet intensity, H. Richard Crane contributed actively to science, first at Caltech for five years as a graduate student and postdoctoral fellow, and for the next 70 years at the University of Michigan. This is his story.

BEGINNINGS

Horace Richard Crane, known throughout his life as Dick, was born on November 4, 1907, in Turlock, California, then a small farming town on the Central Pacific Railroad in the San Joaquin Valley. His grandfather, Stephen Horace Crane, who came to Turlock from Connecticut in 1871, had prospered in agricultural and business ventures, and his father, Horace Stephen Crane (1866-1940) expanded the family's success. Horace managed farming and ranching enterprises; he served as Turlock's mayor from 1910 to 1914, and was owner of Turlock's First National Bank. The resources of the family became large enough to insulate it from California's subsequent economic oscillations. A century later the family's importance to Turlock is still evident. In what has become a city of more than seventy thousand inhabitants, Crane Avenue, Crane Park, and the Crane School are prominent landmarks.

Horace Crane and Mary Alice Roselle (1874-1961), who had come to Turlock from Nebraska, were married in 1903. Mary taught school before giving birth to Dick and his only sibling, John ("Jack") William Crane (1911-1947).

Neither Horace nor Mary had a scientific bent but both encouraged Dick to pursue his interests and supported his numerous projects. His curiosity in things technical had early beginnings. As a five-year old he spent hours in the blacksmith shop across the street watching the smith pound raw iron into horseshoes, fit them to horses, and nail them on. At six he worked with pliers and wire while recuperating from a tonsillectomy. When the family moved to a cattle ranch, Dick took up hunting, fishing, and trapping. He also became an ardent bird-egg collector, continuing that hobby when the family moved from the ranch to a farm on the edge of town.

In 1918 the family moved back into town, and Dick's penchant for science became clearer. He had a run at building model airplanes. He used the low-voltage transformer from an electric train set received at Christmas to do experiments with electricity. His mother had given him a subscription to *Scientific American* magazine, and he read every issue from cover to cover. After learning about telegraphy from visits to the local railroad station, he and some friends set up a small neighborhood telegraph system using dry cells and spark coil wire salvaged from automobile repair shops.

RADIO

In 1919 soon after commercial stations were opened in San Francisco and Los Angeles, Dick built a crystal set to listen to them. He learned about vacuum tubes and rapidly became Turlock's expert in the building and maintenance of radio receivers. He built his first transmitter with a Ford spark coil and by 1920 was sending messages in Morse code around the neighborhood. Soon afterward, he obtained an amateur radio license (with call 6FQA) and went on the air with a transmitter that used vacuum tubes to produce clean, continuous waveform signals.

In 1921 Dick passed the exam for a commercial operator's license and was hired for the summer to run a local radio broadcast station owned by a department store in Stockton, 40 miles north of Turlock. It was a one-man operation: Dick was technician, disk jockey, and announcer. He also signed on as second radio operator on a freighter for a voyage between San Francisco and San Pedro, California.

Dick's first boyhood explorations in physical science had been in the world of mechanical devices and water flows, where the relationships between cause and effect were readily seen. But he then began experiments where he could no longer directly observe the mechanisms of cause. His explorations with lights, electric motors, and the telegraph led to visualizations of unseen electrical currents and the long reach of magnetism. His experiments in chemistry gave him practice in the use of conceptual models to understand the interactions between invisible entities. His involvement with all aspects of radio made him familiar with waves and resonances. These experiences of his youth prepared him well for high school and, indeed, for the years beyond.

HIGH SCHOOL

At Turlock High School an enthusiastic physics and chemistry teacher, George ("Pop") Senter (1897-1970), encouraged Dick's experimental bent by giving him afterhours access to the electrostatic machine and other demonstration equipment. Dick learned the basic glassblowing and vacuum techniques for making Geissler tubes, which he used to study electrical discharges in low-pressure gases. Indicative of his sophistication as a high-school student are the books he selected as prizes for winning a regional American Chemical Society competition: Arnold Sommerfeld's *Atomic Structure and Spectral Lines* and James Arnold Crowther's *Ions, Electrons, and Ionizing Radiations.*

In the spring of 1926 Professor Earnest Watson from Caltech's physics faculty came to Turlock to lecture and recruit. Fascinated by Watson's lecture—which featured spectacular demonstrations with liquid air—and with Senter's encouragement, Dick took the arduous admission exam and entered Caltech as a freshman in the fall of 1926.

CALTECH: 1926-1930

Crane's undergraduate academic program was thorough. In the first two years he took introductory mathematics, chemistry, physics, geology, a survey of biology, English, economics, history, a year of French, and a year of German. His junior and senior year courses included mechanics, electromagnetism, thermodynamics, optics, and atomic physics. While his undergraduate years coincided with the great discoveries of quantum mechanics, he recalled that there was little mention of this new knowledge in his courses. Beyond academics, he played violin in the orchestra and fulfilled the Caltech athletic requirement by taking up tennis.

In the summer of 1930 following graduation, Crane and his mother traveled to Europe, taking a long sea voyage through the Panama Canal. After they toured for some weeks, she returned to America. He stayed on, exploring possibilities for graduate work abroad before deciding to return to California.

On the way back he paused in New York to visit college friend Chester Carlson. At Carlson's urging Crane applied for jobs at Bell Laboratories and the Edison Laboratories, but he had no job offers in that time of economic depression.

Back in Turlock he weighed his options, applied to Caltech, and started graduate school in March 1931.

GRADUATE AND POSTGRADUATE WORK AT CALTECH: 1931-1935

Crane's recollections of graduate education dwell less on his formal courses than on the many informal evening seminars he attended at which students and professors went through recent books and papers, often page by page. Tolman led several of these seminars, helping students with Rutherford, Chadwick, and Ellis's book on radioactivity and with Gamow's book on nuclear physics. Jesse Dumond led a study of Siegbahn's book on X rays. Crane also audited a series of lectures in quantum mechanics by Oppenheimer.

As a graduate student Crane resided and ate many meals at the Caltech Athenaeum where he was a member of the Athenaeum Round Table dinner group. At dinnertime, residents and visitors customarily mixed and mingled, and Crane took advantage of opportunities to meet distinguished visitors from Europe, including Einstein, Jeans, and Sommerfeld. Caltech faculty and visitors frequently ate lunch there, enabling Crane to get acquainted informally with Hubble, Lawrence, and Tuve.

The Athenaeum was important in another way: It was there that Crane met Florence LeBaron, a young woman from Los Angeles who was an assistant manager of the Athenaeum. She bore the responsibility for assisting distinguished visitors, including Albert and Elsa Einstein, during their stays. As a graduate student Dick courted Florence. They were married in December 1934, a half year after he completed his Ph.D.

Upon entry to the Caltech graduate program Crane decided to sign on as a helper to Charles Lauritsen, an ambitious assistant professor on the cusp of promotion, who was then working on the development of high-voltage X-ray tubes. This decision was fortunate for Crane in that it would allow him to earn his Ph.D. in a bit more than three years, enabled by the talented and demanding Lauritsen who gave students free rein and was generous in sharing credit. The decision was fortunate for Lauritsen because he acquired a student who already had substantial skills as an electronic technician, machinist, drafter, and glass blower; who had the courage and agility to work in the upper reaches and inner recesses of the high-voltage systems; and whose talent for inspired improvisation fit well with the demands of laboratory research.

Crane began with the Lauritsen group expecting to do a thesis dealing in X rays, but this was a time of extraordinary change in physics. In December 1931 Urey at Columbia announced the discovery of deuterium. From the Cavendish Laboratory news came in February 1932 that Chadwick had discovered the neutron and in April 1932 that Cockroft and Walton had used proton bombardment to disintegrate nuclei. In November 1932 in a Caltech building adjacent to Lauritsen's lab Anderson discovered the positron.

By December 1932 Crane had completed building a 600 keV X-ray tube. Lauritsen realized that by adapting the tube to accelerate protons, deuterons, and alpha particles they could investigate some of these spectacular, recently discovered nuclear physics phenomena. Moreover, with high voltage coming directly from step-up transformers, the Caltech tube could deliver a hundredfold more ion current than Cockroft and Walton were obtaining from their cascade rectifier system.

Early in 1933 Joliot and Curie produced neutrons by using 1.3 MeV alpha particles from polonium to bombard beryllium: ${}^{4}He + {}^{9}Be \rightarrow {}^{12}C + {}^{1}n$. Crane and Lauritsen wanted to produce neutrons by the same reaction using alpha particles from accelerated helium ions. Although their alphas had less energy than those from polonium, their accelerator could put 10^{14} alpha particles/sec on target, vastly more than polonium sources could provide. They also had a very good neutron detector that Lauritsen had designed. The first artificial production of neutrons came when Crane and Lauritsen used alpha particle projectiles but the yields were meager, so they shifted to bombardment with deuterons and obtained orders-of-magnitude increases in neutron production.

The initial Caltech experiments with deuterons were done with a small sample of heavy water that Crane had obtained from G. N. Lewis at Berkeley; Crane then built an electrolysis separator so the Caltech would have its own source of heavy water.

RESONANCES

Having bombarded targets with alpha particles and deuterons, Crane and Lauritsen decided to see what proton bombardment might do. Since mid-1933 they had been finding events in which the only visible product of protons incident on carbon appeared to be a single, energetic gamma ray: $^{1}H + ^{12}C \rightarrow ^{13}N + \gamma$. Moreover, these events had an energy dependence that suggested the existence of a resonance in the radiative capture process. These results were strongly challenged when Lauritsen presented them at the June 1934 American Physical Society meeting in Berkeley. But Tuve and Hafstad, who had an electrostatic accelerator with good resolution, borrowed a detector from Lauritsen to confirm and enlarge upon the Caltech results. Breit and Yost reexamined the theory in light of the new data and concluded that these resonances were indeed to be expected. Meanwhile, Fermi and his colleagues in Rome had published a series of reports, at first preliminary but then more definitive, on nuclear transformations resulting from neutron bombardment on 60 different elements. The probability for neutron capture could be altered dramatically by using a moderator to adjust the energy of the neutrons. These results from Rome, which spanned the periodic table, drew much attention. In 1936 Breit and Wigner published their theory for resonance reactions, the broad applicability of which was described by Bethe in his overview of nuclear dynamics published in the 1937 *Reviews of Modern Physics.*

Willy Fowler, interviewed later about his time as a graduate student in Lauritsen's group, made a reasoned argument that the first quantitative measurements of resonances in nuclear reactions were those obtained by Crane and Lauritsen in their 1933-1934 experiments with energetic protons on a few low-Z targets. By contrast, Fermi and his colleagues used tabletop experiments with neutrons to show that one could disintegrate elements throughout the periodic table. Thus it is not surprising that Bohr based his compound nucleus model on the work of Fermi and his colleagues, and that the findings of Crane and Lauritsen received less recognition.

A THESIS ON ARTIFICIAL RADIOACTIVITY

In mid-January of 1934 Joliot and Curie reported that exposing boron, aluminum, and magnesium to alpha particles from a polonium source created radioactivity. The Lauritsen laboratory with its accelerator capable of providing alpha particles, deuterons, and protons was well poised to pursue this finding. The first Caltech results were sent to the journals on February 27, and Crane seized the opportunity to make artificial radioactivity the basis for the doctoral dissertation he submitted on May 19, 1934. He wrote,

> The following thesis is a presentation and discussion of the work done up to date with the artificially produced radioactive substances in this and other laboratories. In many instances [positrons are] the product of disintegration …and this has made possible several experiments concerning the annihilation of positrons and the conversion of their rest mass into radiant energy…At

the time of this writing, the phenomenon of artificial radioactivity has been known for a little less than three months. It is therefore possible now, as it will undoubtedly not be possible within a short time, to include in a brief paper a reasonably detailed account of the whole of the field.

Brief indeed! Comprising 27 double-spaced sheets in all, the thesis has a title page, 19 pages of text, 6 pages of figures, and a page that lists eleven journal references, five of which have Crane and Lauritsen as authors. The thesis reports experiments in which a variety of targets were bombarded by deuterons and protons. Following a suggestion in the Joliot-Curie paper, Crane and Lauritsen used deuterons from their accelerator to produce radioactivity with the reactions they described, without neurinos, as

$$^{10}B + {}^{2}H \rightarrow {}^{11}C + {}^{1}n \rightarrow {}^{11}B + {}^{1}n + e^{+}$$

and also

$$^{12}C + {}^{2}H \rightarrow {}^{13}N + {}^{1}n \rightarrow {}^{13}C + {}^{1}n + e^{+}$$

A lower level of radioactivity was produced when similar targets were bombarded with protons, and evidence was found for a resonance capture at 400 keV. They measured decay times and performed chemical tests to confirm the identity of the active species.

In addition to analyzing individual events, Crane observed that gamma radiation from the entire sample decayed at the same rate as the ionization created by passage of positrons. This suggested that the gamma rays resulted from positron annihilation, an explanation supported by his finding that the energy of the gamma rays matched well with the mass-energy of the positrons. Crane concluded his thesis by pointing out that the positrons in these beta decays showed a continuous distribution of kinetic energy very similar to that exhibited by decay electrons, this suggesting that an extension of these experiments might resolve the apparent failure of the principle of conservation of energy in beta decay.

NEUTRINOS AND ELECTRONS

In late 1934, by then on a Caltech postdoctoral fellowship, Crane had been doing a cloud chamber experiment in which deuteron bombardment of a target produced lithium-8, a nucleus that undergoes beta decay: $^{8}Li \rightarrow {}^{8}Be + \nu + e$. The resulting beryllium-8 nucleus promptly decays into two alpha particles: $^{8}Be \rightarrow \alpha + \alpha$. Crane wanted to observe both steps in this decay sequence to determine the momenta of the electron and of the two alpha particles. If the momenta did not add to zero, the difference could be attributed to the escaping neutrino. He used a small pump to transfer gas containing freshly produced lithium-8 into the cloud chamber, but the transfer process created many condensation centers and the resultant fog obscured the observations he was trying to make. Crane's real success with neutrino work would not come until three years later, when he was no longer at Caltech.

Questions concerning the nature and behavior of the electron were swirling around during Crane's time at Caltech. Theories purported to give answers, but ambiguities remained and experimental tests were far from definitive. How do electrons behave when undergoing single and multiple scattering? Is there a distinction between ordinary electrons and the negatively charged particles emitted in beta decay? Are energy and momentum conserved in individual beta decays and Compton scattering events, or just when averaged over many events? Do free electrons exhibit the same magnetic moment as those that are bound within atoms? Can a beam of free electrons be polarized? The Caltech experience generated Crane's lasting interest in those questions, an interest that motivated much of his work over the next 30 years.

It is a tribute—both to Crane's contributions to the Lauritsen laboratory and Lauritsen's generosity in sharing credit—that over the years 1931-1935 Crane was coauthor and usually lead author of 22 out of the 23 *Physical Review* publications from that laboratory. (The 23rd paper, by Lauritsen and Oppenheimer, was on gamma-ray scattering.)

TRANSITION TO MICHIGAN

Harrison Randall, the Michigan department chair, had visited Caltech and Berkeley in 1934 and concluded that Michigan should build up an accelerator-based program in nuclear physics. In June of 1935 Lauritsen recommended Crane as a promising hire. Crane made a visit to Ann Arbor, liked what he saw, and accepted the offer of an instructorship at $3,000 per year.

Soon afterward, Dick and Florence packed their household goods and set off to Ann Arbor in their newly acquired Ford sedan, visiting many national parks along the way on a journey both regarded as a wonderful vacation. (Dick had previously owned a Cord soft-top roadster, a legendary machine now much prized by collectors, which he sold to buy the sedan because Florence found the roadster, with front-wheel drive and without power steering, difficult to handle.)

COMPTON EFFECT

Arriving at Michigan in the fall of 1935, Crane made a rapid start on his own nuclear physics research program. He immediately attracted three able graduate students and started building a high voltage accelerator and a cloud chamber. As those projects got underway Crane learned that a new experiment by Robert Shankland had challenged the usual interpretation of Compton scattering, a result that Dirac was using in a call for a fundamental revision of quantum electrodynamics. Crane and his students Gaerttner and Turin put the finishing touches on the cloud chamber and used it to observe the Compton scattering of gamma rays coming from a thorium source. They confirmed that both energy and momentum are conserved in individual events, thus showing Shankland's findings to be in error and so removing the basis of Dirac's conjecture.

THE MICHIGAN HIGH-VOLTAGE ACCELERATOR

In building a high-voltage alternating-current accelerator at Michigan, Crane was going against the advice "don't repeat your thesis" that he would later give to fresh Ph.D.s. Nevertheless he wanted to get his laboratory going, and his familiarity with accelerators enabled him to finish and test the accelerator by early 1937. It was able to deliver 250 microamperes of 1 MeV ions focused on target, a beam intensity that made it a workhorse for production of radioactive sources.

EARLY OBSERVATION OF THE MUON

In one of the first experiments with the accelerator Crane and his student Ruhlig bombarded lithium with protons to produce 17 MeV gamma rays that by Compton recoil yielded energetic electrons. These electrons were directed onto thin foils in a cloud chamber so their energy loss and absorption could be studied. While Ruhlig and Crane were observing these electrons, they noticed a few anomalous tracks in the cloud chamber. They paid little attention to these anomalies until March 1937 when Anderson and Neddermeyer, working with cosmic rays, reported firm evidence for a particle of unit charge having a mass intermediate between the electron and proton, the particle we now know as the muon. Crane then asked Ruhlig to reexamine the Michigan cloud chamber photographs in which the anomalies had been seen.

Ruhlig found a distinct track created by a positively charged particle that appeared to have 120 ± 30 × electron mass, and they reported this observation to the *Physical Review* in early January 1938. Ruhlig and Crane were thus in the first wave of experimentalists who reported muon mass values that eventually converged on 207 × electron mass.

ELECTRON SCATTERING

Over the years 1937-1941 Crane and his Michigan students published eight papers on the multiple scattering of electrons in carbon, aluminum, and lead. The results with carbon targets seemed to show a sense of agreement between theory and experiment, but results with lead targets were disappointing. In the hope that studies of single scattering might yield more satisfying results, Randels, Chao, and Crane did experiments to see how electrons scattered from argon, krypton, and xenon. The results were certainly as good as those obtained by others, but none had yet furnished clear support for Mott's scattering theory. There was clear motivation to do better experiments on electron scattering, a motivation that would lead later to the important g-2 measurements

In the early discussions of beta decay it was often asked whether beta particles of nuclear origin were different from the electrons that are involved when gamma rays undergo Compton scattering. Work from Michigan strongly supported the view that the beta particle was the same as the electron: Zahn and Spees had used a crossed field method to show that the change in mass of electrons, whether as beta particles or from an ordinary source, was completely consistent with what one would expect from the relativistic change of mass with velocity. Crane's group showed that the energy loss of beta particles in thin foils was independent of the energies at which the beta particles had been created.

EVIDENCE FOR THE NEUTRINO

There were many efforts to find the neutrino after Pauli proposed it in late 1930. Though it was hardly a quantitative experiment, what is generally regarded as the first observation of neutrino momentum was made in 1936 by Alexander Leipunski. He used a retarding potential method to measure the distribution of momentum in nuclei that were recoiling from the beta decay of carbon-11 and found more recoil momentum than could be attributed to electrons whose beta-ray spectrum had been previously measured.

Crane had maintained his enthusiasm for observing the neutrino. In 1937 he and his postdoctoral associate Jules Halpern used a cloud chamber to observe, in individual events, not only the recoiling nucleus but also the beta particle. They put a chlorine-38 beta source in the chamber and applied a magnetic field so that the beta particle momentum could be determined from the curvature of its track. Although the recoiling nucleus did not leave a track long enough to have a discernable curvature, its motion did generate ionization that they assumed to be proportional to the kinetic energy of recoil motion. To measure that energy Crane and Halpern shut off the clearing field in the cloud chamber for a fraction of a second before expanding the cloud chamber. This gave the ions time to diffuse several millimeters outward before droplets condensed around them. The well-separated droplets could be counted, providing a measure of the kinetic energy of the recoiling nucleus. With this experiment Crane and Halpern became the first to measure the recoil momentum of both charged particles in a given beta decay and show thereby that the neutrino must carry momentum if energy and momentum are conserved in the decay.

Going further, Crane recognized that it would be good to do an experiment in which a neutrino passing through a target material would produce an element not present in the target. Since sulfur-35 undergoes beta decay to chlorine-35 with a half-life of 80 days:

$$^{35}S \rightarrow {}^{35}Cl + e^{-} + \nu.$$

He set out to detect the inverse process by putting a source of neutrinos (1 millicurie of mesothorium) into a 3-pound bag of table salt, waiting 90 days, then testing for the presence of radioactive sulfur. This established an upper limit of 10^{-30} cm^{2} for neutrino capture by chlorine-35. Crane then described how a modest extension of his experiment could rule out the possibility that capture processes prevent neutrinos from escaping from the sun. That work was submitted for publication in January 1939.

Almost a decade later Crane was asked to contribute an article to the upcoming 1948 *Reviews of Modern Physics* issue that was to be a Festschrift for Millikan's 80th birthday. He chose to write on energy and momentum relations in beta decay and on the search for the neutrino. Comprehensive, broad ranging, and admiringly cited by many readers, this review was Crane's way of closing his involvement with the neutrino problem.

After three years as an instructor, Crane was promoted to assistant professor at Michigan in 1938. The Cranes then started their family with children Carol (1939-), Janet (1942-1960), and George (1945-). Advancement to tenured associate professorship came in 1942, and to full professor in 1945.

THE CLOUDS OF WORLD WAR II

With the September 1939 invasion of Poland by Germany, war preparations in the United States increased. Prominent university scientists involved in these preparations included Ernest Lawrence, Merle Tuve, Lawrence Hafstad, Richard

Tolman, Charles Lauritsen, Gregory Breit, and Robert Oppenheimer, all of whom knew Crane well. In the fall of 1940 Crane was among the first dozen Lawrence recruits to arrive at MIT for the start of the Radiation Laboratory. He stayed there for several months working on radar circuitry. Having earlier promised Tuve that he would help with proximity fuse development, Crane left the Radiation Laboratory in February 1941 for Washington, D.C.

Proximity fuses were considered so promising that the United States began a massive effort to develop a compact, practical fuse that could be fired from conventional artillery. Crane remained in Washington for most of 1941 to work on the fuse project and then returned to Ann Arbor where he and David Dennison designed and tested the pattern of radiation emitted by fuses. Late in 1944 Crane and his Ann Arbor colleagues were asked to adapt a proximity fuse for detonating an atomic bomb, but it was not used in that application.

After the war, Crane returned to university research to resume his work in biophysics, start a radiocarbon dating laboratory, and build a new accelerator

BIOPHYSICS

In Ann Arbor during the years prior to World War II, Crane maintained his interest in the biomedical aspects of radiation. He audited medical school courses in biology and physiology. Sensing a need, and having the precedent of the evening Caltech seminars, he established an ongoing seminar on the physics of neutrons and of ionizing radiation for a group of practicing radiologists in the medical school.

Immediately after the war, Crane was again drawn to biophysical work when the Biology Department at Michigan acquired an electron microscope but had it installed in Randall Lab where Crane could keep it running. It was at this time that he and Robley Williams developed the shadow-casting technique to show surface texture of the material observed with electron microscopes.

Crane was also involved in theoretical biophysics. In 1950 he published an influential paper "Principles and Problems of Biological Growth" in which he showed that following a single rule when joining identical objects inevitably leads to a spiral structure. This appeared well before the 1953 Watson-Crick papers on DNA. Crane returned to the problem in 1956 when he and Levinthal analyzed the energies involved in the unwinding of a DNA molecule.

RADIOCARBON DATING

In the summer of 1949 less than a year after Willard Libby's discovery that a biological sample could be dated by measuring its carbon-14 content, Crane in collaboration with anthropologist James Griffin and help from Patricia Dahlstrom established a radiocarbon dating laboratory at Michigan that continued to be productive for the next 20 years.

THE RACETRACK SYNCHROTRON

As the war came to an end in 1945 Crane learned about McMillan's invention of the synchrotron. He obtained funds to build an electron synchrotron on the Michigan campus, but he reasoned that injecting electrons into orbit would be easier if the orbit had some portions in a straight line, thus giving what he called a "racetrack" configuration. These straight-line portions would also be convenient locations for accelerating electrodes, targets, and detectors. At Crane's urging, David Dennison and Ted Berlin quickly worked out the theory for a 300 MeV racetrack accelerator, but implementing the design had its difficulties: The synchrotron project started in late 1946, however it was not until 1952 that the accelerator produced 60 MeV electrons. It did attain

100 MeV in 1954, but by that time it was no longer competitive with accelerators operating elsewhere.

That most synchrotrons, including the most energetic proton synchrotrons of today, utilize the racetrack concept confirms the significance of Crane's innovation. But Crane himself remarked, in retrospect, that the Michigan synchrotron should have been built either much smaller to provide more quickly a proof of principle that could guide the construction of other accelerators; or it should have been built on a much larger scale to provide electrons at energies high enough to provide truly interesting information.

In contrast to the postwar big-science approach used by the builders of competing accelerators at other universities, Crane had followed the pre-war pattern in which a senior faculty member recruited a few younger colleagues and graduate students along with a couple of technicians to design, engineer, build, test, and finally operate the accelerator. But the Michigan graduate students were also taking classes and the technicians were asked to be jacks-of-all-trades. Moreover, Crane, Robert Pidd, and the junior faculty members had teaching loads as well as distractions from other research and administrative tasks during the entire time of the synchrotron effort. Crane was writing his review on neutrinos, developing his theory of spiral structure in biological molecules, doing radiocarbon dating, and overseeing the upgrade of the Michigan cyclotron. And in 1953, midway in the synchrotron project, came the dramatic results from Crane's experiment on the gyromagnetic ratio of the free electron, an experiment that drew international attention with its success, this in stark contrast to the racetrack synchrotron that was being outpaced by electron accelerators located elsewhere.

MIDWEST UNIVERSITIES RESEARCH ASSOCIATION

By 1953 the United States had built two multi-GeV particle accelerators, the Cosmotron at Brookhaven National Laboratory on Long Island and the Bevatron at the University of California at Berkeley. Physicists in the Midwest felt that they were being overlooked. In response to this perceived neglect several dozen concerned physicists gathered in April 1953 at the University of Chicago. The group included Enrico Fermi, Ernest Courant, and Robert Wilson; from Michigan were Dick Crane and two of his junior faculty colleagues, Lawrence Jones and Kent Terwilliger. This meeting was the first in a series that led to the formation of the Midwest Universities Research Association (MURA) and to the design of an innovative multi-GeV accelerator. Some of these innovations were tested on a 400 keV cyclic electron accelerator built during 1956 in Ann Arbor by Jones, Terwilliger, and Crane.

Crane served as vice president of MURA in1956-1957 and as president from 1957 until 1960, a term during which he had the difficult task of overseeing a change in the laboratory directorship. MURA had been a vigorous and creative group whose members made many fundamental contributions to accelerator science, but its major proposals did not receive funding. In 1967 the University of Wisconsin took over the MURA laboratory and converted it to a successful synchrotron radiation facility. Eventually the Midwest did get its accelerators in the Argonne Zero Gradient Synchrotron and later the Fermilab Tevatron. It is arguable that the MURA effort was an essential precursor to the success of those machines

G-2 OF THE FREE ELECTRON

From atomic spectra it was clear that bound electrons have magnetic moments associated with their spin angular momentum. In the early days, however, there was some suggestion that free electrons might behave differently. In 1926 Bohr argued, on the basis of the uncertainty principle, that a Stern-Gerlach experiment could not usefully discriminate between electron spin states, and some had interpreted his argument to mean that the spin of the free electron was unobservable.

On the other hand, it was well known that electromagnetic radiation could be polarized by scattering and that the polarization could be detected with a second scattering. In 1929 Mott predicted that a similar polarization dependence would be seen when electrons scattered from heavy nuclei. Over the years 1929-1939 several investigators undertook double scattering experiments to look for electron polarization. They found qualitative support for Mott's predictions, but the results were not crisp.

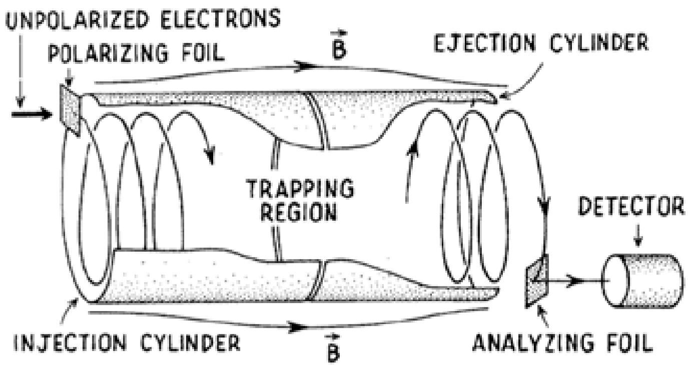
Meanwhile evidence was accumulating for an anomaly in the g-factor of bound electrons. Dirac theory predicts $g = 2$ exactly, but high-resolution optical spectroscopy done during 1937-1940 suggested that this was not quite right. Then the atomic beam spectroscopy of 1947-1950 showed that bound electrons had a magnetic moment about 0.1 percent higher than predicted by Dirac theory. Some thought that free electrons might not possess the same anomalous magnetic moment.

THE MICHIGAN G-2 EXPERIMENT

In 1951 the Michigan synchrotron was being assembled. Before the rest of the accelerator was ready, its 600 KeV electron injector was operational. Crane suggested to William Louisell, an advanced graduate student in need of a thesis problem, that he and Pidd use the injector for a double scattering experiment to test the Mott theory. Electrons were to be conveyed from the first to the second scatterer through a long tube around which a coil was wrapped to provide a guiding magnetic field. The electrons would undergo a spiraling motion as they went through the tube; the spins would also precess in that field. Crane reasoned that if g were exactly 2 then the spin precession rate would match the orbit frequency and the detected polarization would be independent of the number of orbits made during the electron's trip between polarizer and analyzer. But he also recognized that the spin direction would undergo about one rotation for every thousand orbits if g-2 for the free electron were different from zero in the amount suggested by the atomic beam experiments. He discussed this design with George Uhlenbeck and Ken Case, both of whom expressed their doubts about the feasibility of the experiment and quoted the Bohr objection. But Crane, who realized that such doubts were equivalent to doubts that free and bound electrons were the same, was convinced that his scheme would work and went ahead, explaining later that "there was no reason to be flagged off by the theorists."

Louisell, Crane, and Pidd were able to make the spins precess in a manner consistent with $g = 2.00 \pm 0.01$. Although this proof-of-principle experiment did not yield a value for the anomaly, it drew much attention when the results were presented at the 1953 Washington meeting of the American Physical Society.

Since the difference between the spin precession rate and the orbit frequency of an electron in a magnetic field is proportional to g-2, the value of g-2 can be precisely measured by observing the difference over a long enough time. To this purpose Schupp, Pidd, and Crane modified the magnetic field to function as a magnetic bottle that could store the electrons for a controlled time between their first and second scattering.



In January 1957 while the g-2 experiment was still in progress, the news broke that parity was not conserved in the weak interactions, and it became of interest to have a limit on the electric dipole moment of the electron. Nelson, Schupp, Pidd, and Crane used the g-factor apparatus to set a limit of 3×10^{-15} e-cm on the dipole moment, improving the then-existing limit by a factor of 100.

By 1961 Schupp, Pidd, and Crane reported a g-2 value with an uncertainty of 0.2 percent, not quite precise enough to make challenging comparisons with values obtained by bound state electron measurements. The Michigan researchers had already seen the way to build an apparatus that would yield much more accurate results. This third-generation g-2 experiment was started in 1957 by David Wilkinson (1935-2002) who had just enrolled in the Michigan Ph.D. program after having worked as an undergraduate research assistant to Crane for two years in the building of an electron accelerator for MURA. Wilkinson's modus operandi resembled Crane's at Caltech: that of an unusually capable, solo graduate student doing both the physics and the engineering of a large, complicated experiment under the guidance of a supportive adviser. Wilkinson achieved a thousandfold reduction in the uncertainty for g-2. His appeared to be the definitive precession experiment on the electron. In 1963 he went to Princeton where he had an extraordinary career in experimental astrophysics and cosmology. The Wilkinson Microwave Anisotropy Probe satellite is named after him.

In 1961 Crane once again attracted a bright, ambitious graduate student: Arthur Rich (1937-1990) had come to Michigan with a master's degree from Columbia University. After completing his Ph.D. thesis with a g-2 measurement for the positron, Rich was appointed as an assistant professor, this occurring just as Crane was assuming the chair of the physics department. Rich became the de facto leader of the g-factor group and soon started two ambitious projects: one with John Gilleland (a second-generation g-2 measurement on the positron) and another with John Wesley (a fourth-generation high-field g-2 experiment on the electron motivated by Rich's reinterpretation of Wilkinson's data). Rich and his students, in collaboration with G. W. Ford and Valentine Telegdi, also showed how the basic g-2 method could be enhanced by adding a radiofrequency field to manipulate the spin while the electron was in the trap.

In the early 1970s it had become clear that the Michigan precession experiments had reached their practical limit and that resonance experiments had better prospects, so Rich and Wesley wrapped up the Michigan g-2 effort by writing a comprehensive general review of lepton g-factor studies.

Throughout Crane's career his primary goal had been to explore fundamental physical phenomena by conducting experiments; untested theoretical objections never deterred him. Many of his successes stemmed from his unusual ability to combine intuition and visual reasoning with formal calculation. Some of the intuitions that served him so well for so long in earlier times seemed less useful when, in the dozen years following its initial success, the g-2 experiment required for its interpretation ever more of the subtle, less intuitive aspects of quantum electrodynamics. Already in the late 1950s Crane began to change the emphasis of his work to include more administration and more effort in science education, changes made easier at first because David Wilkinson, and then Arthur Rich, were so capable in g-2 research.

g-minus 2

To celebrate the pioneering studies of the free electron's magnetism by H. R. Crane and his students at the University of Michigan 1950-1960

Bronze and Stainless steel 84"x42"x42"

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TRANSITION TO LEADERSHIP

Crane, widely recognized for his research, began to accept formal appointments to leadership positions in the late 1950s. His activities at Midwest Universities Research Association have already been mentioned. In 1962 he was recruited by the Commission on College Physics, and he served as its president from 1967 until 1970. The American Association of Physics Teachers chose him as vice president in 1964 and as president for 1965-1966. He was the chair of the Michigan Physics Department from 1965 until 1972. Having been a member of the American Institute of Physics (AIP) Board of Governors since 1964, Crane was persuaded in 1971 to serve as chair, a post that he held for four years.

These were not simple times. From World War II through the early 1960s, science, especially physics, had been ascendant; the main tasks of leadership were to manage opportunity and to organize growth. Not so in the late 1960s. Political and social stresses, many attributable to the Vietnam War, had altered the climate. The scope of projects supported by the National Aeronautics and Space Administration had narrowed. In 1969 Congress mandated that the Department of Defense should support only mission-related research. The drop in basic research funding substantially shocked the physics research community. Employment for physicists, particularly newly graduated Ph.D.s, became a serious problem as university investments in physics abated and industrial and government laboratories became more cautious about supporting exploratory research. Crane, speaking from his AIP leadership position, urged physicists to explore opportunities beyond their traditional boundaries. He helped bring the American Association of Physicists in Medicine into the AIP's fold of member societies.

LEADERSHIP IN TEACHING

In 1957 the Physical Science Study Committee (PSSC) began its effort to improve physics education in high schools, and its success led to the formation of the Commission on College Physics (CCP) under the leadership of Walter Michels of Bryn Mawr College. Michels, who had been Crane's instructor in an undergrad lab at Caltech, asked Crane to organize two conferences on the teaching for undergraduates majoring in physics and engineering. Those conferences, held in Ann Arbor in May and November 1962, were quite successful. The resultant expansion of the CCP led to its move in 1964 from the confines of a few rooms at Bryn Mawr to almost an entire floor of a new physics building at Michigan. This move was negotiated just as Crane was elected vice president of the American Association of Physics Teachers and just before he became chair of the Michigan physics department.

Initially the focus had been on teaching for science majors, so Crane had addressed himself to improving the range and depth of problems given for homework and in examinations. As time went on, the CCP paid more attention to the needs of the many students who take physics as a prerequisite to professional careers. Crane called these students "the captives" and he devoted much effort to developing course materials, programmed learning modules, and laboratory equipment for them. He knew that scientists were not going to run the world, but he was firm in believing that those who run the world should understand science.

LATER RESEARCH ON GEOMAGNETISM AND ON PENDULUMS

In the early 1970s, even though awash in administrative responsibility and in teaching-related efforts, Crane found time to return to his long-standing interest in the sources and variations in the earth's magnetic field. He did several bench-top experiments in which the orientation of a magnetic dipole was varied with respect to the axis of rotation of a conducting, nonmagnetic enclosure that could be rotated. He measured the transverse and axial components of the field outside the enclosure and found that rotations of the enclosure tended to suppress the transverse components of the observed field. Moreover, he found that the axial component of the observed field would show sudden reversals whenever the internal dipole's orientation passed the equator. He showed that a reasonable scaling of this model could apply to the polarity reversals of the earth's magnetic field, a phenomenon that occurs randomly on a timescale of 10,000 years or so, and a phenomenon that we still do not understand.

Several years thereafter he published three widely cited papers on the design and construction of Foucault pendulums, some as short as 70 cm.

BEYOND RETIREMENT: PUBLIC EDUCATION

Having reached the statutory age, Crane retired in 1977 and decided to devote his energy to a broader range of science education. A significant factor in this choice was his wife, Florence, who had long been involved with public affairs at city and state levels. (Her concerns about women in Michigan's criminal justice system led to important reforms, and one of the state's correctional facilities is named in her honor.) Dick directed his efforts toward several groups: K-12 schoolchildren, community college students, and the general public. Remarkably, he sustained those efforts on both local and national levels for another 25 years.

In 1977 Crane's friend Cynthia Yao began the work that established the Ann Arbor Hands-On Museum of Science.

Crane was a leading figure in the team of volunteers who designed and built the exhibits and provided continuing support over many years.

In 1983 Crane began writing a regular column for the American Association of Physics Teacher's journal *The Physics Teacher* in which he explained the physics and engineering of everyday objects and devices. He wrote regularly until 1996 when 78 of his columns were collected in *How Things Work*, a book notable for its friendly yet sophisticated descriptions of pre-digital technology.

Dick and Florence had a special admiration for community colleges and the special role they play in providing hands-on, practical training. They recognized that donations to those colleges have unusual leverage in helping a large, often underserved portion of the local population. Accordingly, the Cranes generously underwrote scholarships and endowed a physical sciences activity fund at the Washtenaw Community College. The college recognized these contributions by naming a new arts and sciences building in their honor.

HONORS AND RECOGNITIONS

The University of Michigan recognized Crane's achievements by naming him to the George P. Williams University Professorship in 1966, the same year that he was elected to membership in the National Academy of Sciences. The American Physical society selected him for the Davisson-Germer Prize in 1967, and Caltech awarded him their Distinguished Alumni Medal in 1968. He became a fellow in the American Academy of Arts and Sciences in 1971. The American Association of Physics Teachers recognized his contributions to teaching by honoring him in 1977 with its Oersted Medal, and a decade later with the Melba Newell Phillips Award for creative leadership of the AAPT.

In 1986 President Ronald Reagan awarded Crane the National Medal of Science.

THE LAST YEARS

His university celebrated Crane's 90th birthday with a symposium at which Abraham Pais, Norman Ramsey, David Wilkinson, Andrew Sessler, and Samuel Krimm spoke about Crane's wide range of contributions to physics. A less formal gathering marked his 95th birthday, an occasion for the first display of a sculpture celebrating the g-2 experiment that is now installed on the grounds of the Randall Physics Laboratory.

After his retirement, Dick and Florence continued to live in the house at 830 Avon Road that the architect Robert Metcalf built for them in 1953. Dick stayed on there for a short time after Florence's death in 1993, but as his need for care increased, he moved to the home of his daughter, Carol, and her husband, Fred Kitchens, on Cavanaugh Lake, about 20 miles from Ann Arbor. There he spent the remaining three years of his life, receiving visitors to whom he often expressed his sense of privilege in having lived with so much engagement in an exciting century of physical discovery.

Horace Richard Crane died on April 19, 2007, in Chelsea, Michigan, just months short of his 100th birthday.

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SELECTED BIBLIOGRAPHY

1933

With C. C. Lauritsen and A. Soltan. Artificial production of neutrons. *Phys. Rev.* 44:514.

1934

With C. C. Lauritsen and W. W. Harper. Artificial production of radioactive substances. *Science* 79:234.

With C. C. Lauritsen. Radioactivity from carbon and barium oxide bombarded by deuterons, and the conversion of positrons into radiation. *Phys. Rev.* 45:430-432.

With C. C. Lauritsen. Evidence for an excited state in the alpha particle. *Phys. Rev.* 46:537-538.

1936

With E. R. Gaerttner and J. J. Turin. A cloud chamber study of the Compton effect. *Phys. Rev.* 50:302-308.

1937

High potential apparatus for nuclear disintegration experiments. *Phys. Rev.* 52:11-17.

1938

With A. J. Ruhlig. Evidence for a particle of intermediate mass. *Phys. Rev.* 53:266.

With J. Halpern. New experimental evidence for the existence of a neutrino. *Phys. Rev.* 53:789-794.

1939

With J. Halpern. Further experiments on the recoil of the nucleus in beta-decay. *Phys. Rev.* 56:232-237.

1946

The racetrack: A proposed modification of the synchrotron. *Phys. Rev.* 69:542.

1948

The energy and momentum relations in beta decay and the search for the neutrino. *Rev. Mod. Phys.* 20:278-295.

1950

Principles and problems of biological growth. *Sci. Mon.* 52:376-389.

1954

With W. H. Louisell and R. W. Pidd. An experimental measurement of the gyromagnetic ratio of the free electron. *Phys. Rev.* 94:7-16.

1956

University of Michigan radiocarbon dates. *Science* 124:664.

With C. Levinthal. On the unwinding of DNA. *Proc. Natl. Acad. Sci. U. S. A.* 42:436-438.

With D. W. Kerst, F. T. Cole, L. W. Jones, L. J. Laslett, T. Ohkawa, A. M. Sessler, K. R. Symon, K. M. Terwilliger, and Nils Vogt Nilsena. The attainment of very high energy by means of intersecting beams of particles. *Phys. Rev.* 102:590-591.

1957

With E. W. McDaniel. Measurement of mobilities of negative ions in gases. *Rev. Sci. Instrum.* 28:684-689.

1963

With D. T. Wilkinson. Precision measure of the g-factor of the free electron. *Phys. Rev.* 130:852-863.

1964

Remedial programs. *Am. J. Phys.* 32:465-473.

1966

With A. Rich. Direct measurement of the g-factor of the free positron. *Phys. Rev. Lett.* 17:271-275.

Experiments in teaching captives. *Am. J. Phys.* 34:799-807.

1968

Students do not think physics is relevant. What can we do about it? *Am. J. Phys.* 36:1137-1143.

1969

Problems for introductory physics. Part 1. *Phys. Teach.* 7:371-378.

1970

Problems for introductory physics. Part 2. *Phys. Teach.* 8:182-187.

1974

Alignment of the earth's magnetic field with the axis of rotation and reversals of polarity: Laboratory experiments on a mechanism. *Proc. Natl. Acad. Sci. U. S. A.* 71:4400-4403.

1977

Transient magnetic effects in a scale model of the earth's core. *Proc. Natl. Acad. Sci. U. S. A.* 74:4744-4748.

Response of the Oersted medalist. *Am. J. Phys.* 45:599-601.

1981

Short Foucault pendulum: A way to eliminate the precession due to ellipticity. *Am. J. Phys.* 49:1004-1006.

1990

The Foucault pendulum as a murder weapon. *Phys. Teach.* 28:264-269.

1996

How Things Work. College Park, Md.: American Association of Physics Teachers.

2000

How we happened to measure g-2: A tale of serendipity. *Phys. Perspect.* 2:135-140.