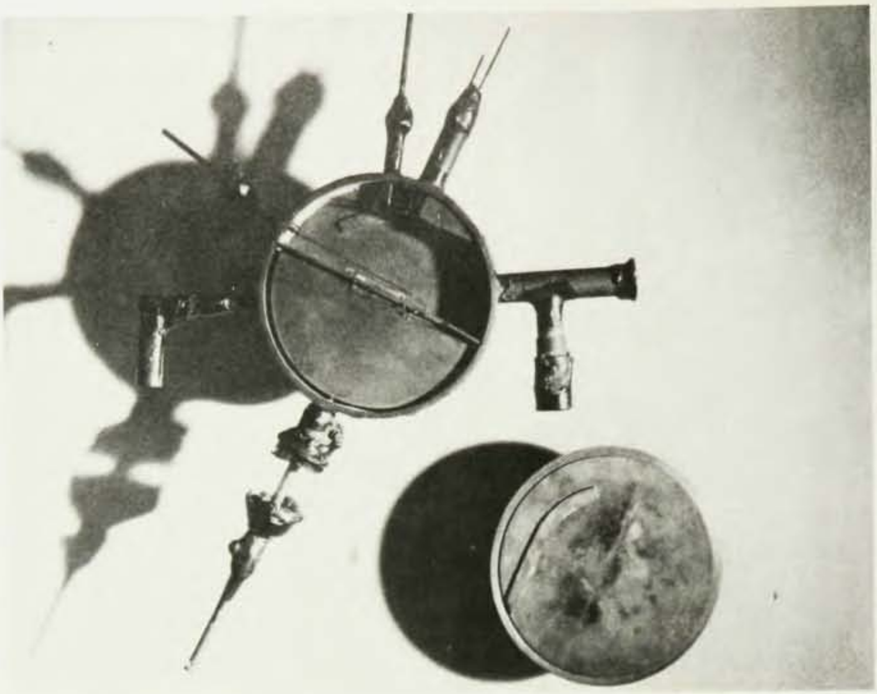


Some physics not in The Physical Review

Reminiscences from Berkeley in the 1930s, a period in which a small group of very young scientists stumbled into greatness, revolutionizing the way physics is done.

Robert N. Varney

Until the end of the nineteenth century—somewhere in the interval between 1880 and 1915—it was the style of published papers in physics, as in other disciplines, to offer extensive discussions of each stage of an experiment, whether successful or not. In this century the style changed: The discussions became curt, impersonal and matter-of-fact, with the result that many significant facts about how discoveries were made, such as mistakes, accidental discoveries, procedural complications, were not mentioned in the published record. This stylistic change may well have improved the clarity of many papers, but at the sacrifice of historically valuable information—at least some of which is also of scientific value. I hope this article can fill a few such lacunae in *The Physical Review*.

Robert Varney is retired from a position as senior scientist at the Lockheed Palo Alto Research Laboratory, where he performed research on molecular physics and gas discharges.

Berkeley

The decade 1929–1939 is surely one of the richer periods in the history of science both in the number of new findings as well as in their fundamental character. At the start of the decade, atomic nuclei were accepted to be composed of protons and electrons. By the end of the decade, the existence of neutrons, positrons, mesons and neutrinos was recognized, and the picture of the nucleus was drastically altered; the phenomena of artificial disintegration of nuclei and of artificial radioactivity had been discovered; the existence of deuterium was established and heavy water was being produced in significant amounts; fission of nuclei was discovered and the possibility of spontaneous release of nuclear energy in useful amounts came into sight; and perhaps most important of all for the future of physics and of physicists, the invention of high-energy particle accelerators occurred, and physics moved into the current era of large machines and large crews or teams of workers.

I lived through this period, largely at the University of California at Berkeley, as an undergraduate physics major from 1927 to 1931, as a doctoral student from 1931 to 1935, as an instructor during 1935–36, and as a research fellow in the summers of 1937–40. Thanks to an early object lesson and to a natural interest and curiosity, I routinely visited my fellow graduate students in their laboratories, in physics and also in chemistry, and I became well acquainted with Ernest O. Lawrence, J. Robert Oppenheimer, G. N. Lewis, R. H. Fowler, Raymond T. Birge, Leonard B. Loeb and many others. These recollections of the period make

0031-9228 / 82 / 1000 24-06 / $01.00 © 1982 American Institute of Physics

The vacuum chamber of Lawrence's first cyclotron. It is about 4½ inches in diameter and was used to accelerate hydrogen ions to 80 000 eV. (All photos for this article are from the Lawrence Berkeley Laboratory, courtesy AIP Niels Bohr Library.)

no pretense of being a careful historical study with detailed verification of every statement; I have, however verified dates of publications.

Several of the following narratives and anecdotes have their origin in conversations over lunch: One almost unbelievable bonus of the decade was that graduate students were eligible for associate membership in the Faculty Club, located scarcely a stone's throw from LeConte Hall (the Physics Building). By good fortune, I was spared the worst pinches of the Great Depression to the extent that I could afford the monthly dues of $2. The club was visited at lunchtime by faculty from throughout the university; seating at lunch was unrestricted, and one could choose one's companions from literally the entire university faculty.

I believe there is far greater value in these reminiscences than mere entertainment or satisfying some natural curiosity about these now legendary figures. They show how a group of very young and very human scientists, working in an obscure university, pushed, drove, and stumbled in a scant ten years into eternal fame and greatness, revolutionizing both thoughts and methods, literally creating a new world. The purpose of this article is not to dwell on mistakes and difficulties but rather to demonstrate that science does not advance smoothly even in the hands of the greatest of scientists. How major scientists approach research is always a matter of interest. Lawrence's approach was inventive and intuitive. Oppenheimer's was mathematical, deductive, logical. The difference in approach is reflected in the nature of the discoveries each of these physicists made, and overlooked.

The faculty

Lawrence joined the physics faculty of the University of California as associate professor in the fall of 1928. He had just turned 27. His chief research activity as of 1928 was the measurement of the photoelectric effect in thin films. The concept of the cyclotron came to him early in 1929, and the first tiny device, 15 cm in diameter, was assembled early in 1930 and tested by Christmas of 1930. The first publications were in the spring of 1931.

Lawrence's driving methods rapidly expanded the program. Faced with construction of a larger cyclotron, he recruited a young radio engineer, David Sloan, as a graduate student in physics to design and build the required oscillators for the new machine. No sooner was the machine running than he recruited a young postdoctoral physicist, Franz N. D. Kurie, who had learned Wilson cloud-chamber technique as a graduate student. It soon became clear that a new device called a Geiger–Müller counter was going to be useful to search for radioactive products. Lawrence found C. Donald Cooksey to head this work. As cyclotrons grew in size, Lawrence recognized the need of expert electrical engineering, and he found William Brobeck to fill this need. A small stream of volunteer "postdocs" was rapidly growing into a river. Lawrence almost overnight changed the whole character of graduate-student research from small individual studies to large team efforts.

By the end of 1931, Lawrence and his students had a cyclotron operating that produced 1.25-Mev protons. It had a diameter of about 28 cm and an electromagnet that weighed slightly over a ton. The device was set up in one of the standard research rooms in LeConte Hall—in one of the larger research rooms, to be sure, but not yet in a separate building.

Oppenheimer became a part-time assistant professor at Berkeley in 1929, at age 25. The other portion of his time was occupied in a similar appointment at Caltech in Pasadena. The part-time nature of his appointment at Berkeley created an administrative problem whereby he was not eligible under the rules to direct doctoral research programs. He did so nonetheless; his students were listed as being under the direction of William Howell Williams. This situation persisted for 4 years and covered at least three successful PhD programs.

To say that Oppenheimer and Lawrence constituted a team would be misleading. One example of their scientific cooperation is described below. However, each of them ran his own programs completely independently of the other and with little day-to-day collaboration.

Larger cyclotrons

During the spring of 1932, Lawrence gave one of the regular Wednesday afternoon department-meeting lectures on his plans for a 5-Mev cyclotron. Leonard F. Fuller in the Department of Electrical Engineering had remembered that there was a 25-ton electromagnet lying in the back yard of the Federal Telegraph Corporation in Palo Alto, unused. It had been made for an enormous arc-type radio transmitter for the Chinese government, but before it could be delivered the government changed hands and the order was canceled. Federal Telegraph was willing to donate the magnet. New coils and new pole faces had to be made for the quite different use the magnet was to have, but it represented an enormous step for Lawrence.

At the end of Lawrence's lecture, Oppenheimer went to the blackboard and explained that a 16-Mev alpha particle could penetrate a nucleus (presumably basing his prediction on Rutherford's disintegration of nitrogen by alpha particles, in 1919 and 1922) and hence that a 4-Mev proton could be expected to do so. His conclusion was that Lawrence should hurry up with his 5-Mev device so that some nuclear physics could really be started. The little machine giving 1.25-Mev protons was of little interest except for scattering experiments.

During the summer, Lawrence was in England and saw the Cockcroft–Walton electrostatic accelerator in Cambridge that was producing ¾-MeV protons. John Cockcroft and Ernest Walton had also assumed that their energy was too low to be of interest, but under the repeated urging of George Gamow, they placed a lithium target in front of their proton beam and at once observed the first artificial disintegration in history. Lawrence cabled back to Berkeley, and the experiment was duplicated and extended to 1.25 MeV with the existing cyclotron.

Late in 1932, heavy water was discovered by Harold Urey (see PHYSICS TODAY, September, page 34), and very soon G. N. Lewis in the Chemistry Department at the University of California was producing it in high purity. The nucleus of the heavy isotope of hydrogen was at this time named the "deuton." Only Rutherford and his associates chose to call this new particle a "diplon," claiming that this was the correct Greek-based counterpart of the word "proton." Rutherford called the atom of heavy hydrogen "diplogen," and the term may be seen in the Proceedings of the Royal Society in articles of this period. In the summer or fall of 1933, Lawrence had visits with Rutherford, who was taken with Lawrence's charm as well as his competence, and they thrashed out the topic of the name for the isotope of hydrogen. According to the story, Rutherford agreed to give up the term diplon in favor of the term deuton provided the latter were modified to include his initial, *r*, in it, and so the term "deuteron" was coined.

Thanks to the supply of deuterium (in the form of heavy water) provided by Lewis, Lawrence very early began using deuterium in his newer cyclotron. (Normally, D_2^+ molecular ions were accelerated, reaching 10 Mev.) The massive new device was located in a wooden structure, the abandoned Civil Engineering Testing Lab that was on the verge of being torn down when Lawrence requested it as his first "Radiation Laboratory." It adjoined the main chemistry research laboratory, Gilman Hall, with a scant 10 meters of space separating the two buildings. In Gilman Hall, a graduate student named Willard Libby was running a search of all the chemical elements for traces of natural radioactivity, using a screen-walled Geiger–Müller counter for the exploration, placing the sample inside the counter but outside of the screen wall.

In time Libby discovered that he was encountering interference from some outside source that blocked his measurements, and before long he correlated the interference with the coincidental operation of the cyclotron. Some joking ensued as to whether the interference arose from the cyclotron magnet—obviously highly unlikely at a distance of some 100 meters from the magnet yoke. At about this time Chadwick discovered the neutron, and Libby's interference was identified as coming from stray neutrons generated by the cyclotron when deuterons were being used for the beam. Clearly neutrons in great abundance were present, and the group at Berkeley had missed making the discovery (as had Marie Curie and Pierre Joliot in France). In the face of being swamped by flying neutrons, the failure to discover neutrons at Berkeley was disappointing but scarcely blameworthy in view of the deep absorption in the enormous new technology of million-volt particles.

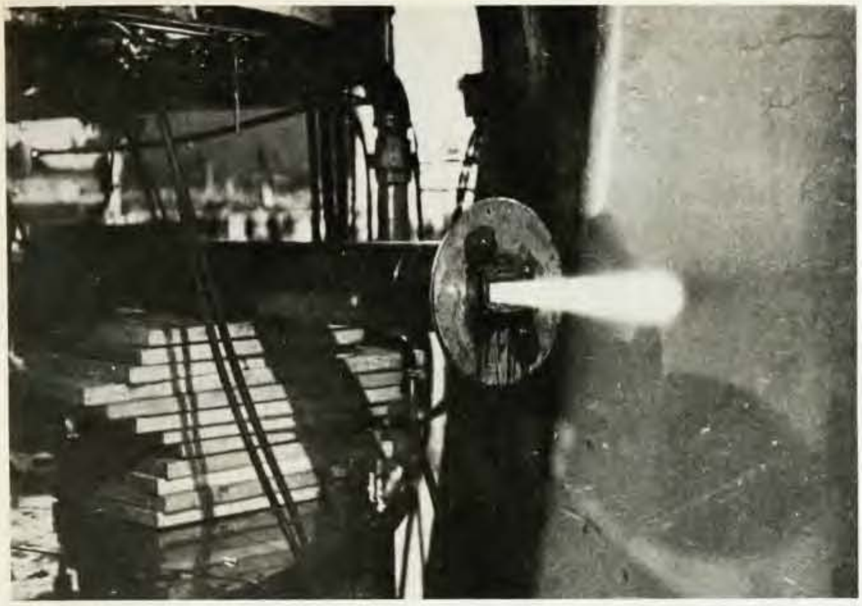
For a few years the crews working at the cyclotron took no precautions whatsoever against radiation injury. Kurie sat on the yoke of the cyclotron magnet for ten full hours of running time, operating his cloud chamber by hand. Dean Cowie aligned the cyclotron beam by eye, looking directly at the target lighted by the impact of the beam ions while the neutrons came on through the window into his eyes. As the cyclotron was perfected to produce 10-MeV ions, a window was devised to permit the ion beam to emerge into the air. It was a fascinating sight; the air glowed a beautiful purple for a distance of more than a meter beyond the window. While it is true that no one was so foolhardy as to put a finger into the beam, observers, including me, did stand right beside it. One trick that was repeated for all newcomers was to give them a transparent calcite crystal approximately 2 cm on an edge and a pair of crucible tongs to hold the calcite in the beam. After about a minute, the calcite glowed with a deep red phosphorescence that made it appear to be red hot, even when withdrawn from the beam. To add to the newcomer's amazement, one of the staff members then suddenly dropped the "red hot" crystal into the visitor's hand. The calcite was stone cold, the red radiation notwithstanding; whether it was radioactively hot, no one knew. No statistically meaningful data of long-term consequences of radiation exposure appear to exist, although Cowie suffered cataracts a few years later.

Artificial radioactivity

In 1933–34 it became the practice to keep a silver-walled Geiger–Müller counter in operation close to but outside of the cyclotron. As the cyclotron began running, the Geiger–Müller counter began to count, counting faster and faster as time went on until the counting rate rose so high that the counter paralyzed. It was assumed that the electromagnetic radiation that drove the cyclotron also excited the counter. However, when the cyclotron was turned off, the counter remained paralyzed for some minutes until it "cooled off" enough to resume counting at high speed. Thereafter the counting rate declined exponentially with a time constant of roughly five minutes.

During the ensuing days the news reached Berkeley of the work of Joliot and Curie in producing artificial radioactivity of boron by bombardment with fast alpha particles and of Enrico Fermi in producing radioactivity of silver by bombardment with neutrons. Upon learning the latter piece of news, Lawrence placed a silver dollar on a stand close to the cyclotron exit and after a few minutes of running time, found that the coin was beta-radioactive with a halflife of about five minutes. The supposedly spurious counting of the silver-walled Geiger–Müller counter was in fact the counter's response to the bombardment of its walls by the stray neutrons that emerged

Staff of the Radiation Laboratory at Berkeley sitting in the unfinished magnet of the 60-inch cyclotron in August 1938. From left to right, top to bottom: A. S. Langsdorf, S. J. Simmons, J. G. Hamilton, D. H. Sloan, J. R. Oppenheimer, W. M. Brobeck, R. Cornog, R. R. Wilson, E. Viez, J. J. Livingood, J. Backus, W. B. Mann, P. C. Aebersold, E. M. McMillan, E. M. Lyman, M. D. Kamen, D. C. Kalbfell, W. W. Salisbury, J. H. Lawrence, R. Serber, F. N. D. Kurie, R. T. Birge, E. O. Lawrence, D. Cooksey, A. H. Snell, L. W. Alvarez, P. H. Abelson.



Cyclotron beam. The ion beam from the 60-inch cyclotron exits the vacuum chamber through a window, ionizing the air in its path and producing a beautiful purple glow.

from the cyclotron during running time when it was fed with deuterium. Sadly, the effect discovered at the Radiation Laboratory had failed to achieve recognition or identification at the University of California.

Lawrence and his colleagues soon discovered that a radioisotope of sodium of mass 24 units could be produced by neutron bombardment. Its halflife was about 15 hours. Lawrence had tablets of sodium chloride made up containing extremely small percentages of Na^{24}. During lecture demonstrations, he would swallow one of these tablets and then, holding his hand over a Geiger–Müller counter, demonstrate how the radiosodium reached his hand following absorption in the stomach and circulation via the blood stream. The counting rate exhibited by his hand also declined during the lecture, demonstrating both the radiologic and the biologic decay times.

It is interesting to note that at this time, Lawrence's mother was stricken with inoperable cancer. He and his brother John, who was a physician, used neutron-radiation therapy on their mother, the first test case. She recovered and in fact outlived Ernest.

Neutron bombardment of uranium

In 1936, Fermi's experimentation with neutron bombardment led him to irradiate uranium and other heavy elements. He discovered an array of radioactive halflives, which he attributed to "transuranic" elements that were formed by the absorption of neutrons. Following the practice introduced by Dmitri Mendeleyev in naming new elements, he attached the prefix "eka" to the chemical cogeners of the supposed new element, so that the literature began to contain words like eka-uranium, eka-gold, eka-thorium, and so forth. The fact that these substances were in fact fission products, lighter, not heavier, than their parents, was not recognized until the work of Otto Hahn and Lise Meitner in 1938.

In 1936, Philip Abelson, a graduate student of physics at the University of California, undertook the task of identifying the "transuranic" elements that emerged from the cyclotron when uranium was bombarded with neutrons. The technique was to be the detection of the $K\alpha$ x-ray spectrum of the element, because this spectrum had been known since the work of Moseley in 1914 to be a precise measure of the atomic number of the emitter. Abelson spend a month at Caltech with Jesse DuMond learning how to make a bent-crystal spectrometer that would be capable of five-figure precision in the reading of x-ray wavelengths. Back at Berkeley, Abelson used his device on freshly bombarded uranium. The wavelength he observed was precisely, to five significant figures, the same as the $K\alpha$ radiation of krypton. As of that time it was believed to be the characteristic L-radiation of eka-uranium. This was particularly marvelous since in the history of x-ray spectroscopy no two spectroscopic lines had ever been found to have identical wavelengths. Following the work of Hahn and Meitner it became clear that Abelson had indeed observed $K\alpha$ radiation from krypton and that he had been seeing fission of uranium into barium and krypton. While the discovery of fission was not made at Berkeley, once fission was recognized, Abelson's results served to identify at least one of the pairs of fission products.

Problems and their solutions

The decade included its share of breakdowns, failures and mistakes, most of which were overcome by the sheer power of hard work. More than once, the entire campus electrical supply was blown out by a cyclotron malfunction. For a period, while the cyclotron D's were still mounted on large Pyrex insulators with central conductors to carry in the radio frequency power, a startling beam-like glow discharge would occur that terminated on one of these insulators and melted a hole through it in a matter of seconds. The abrupt loss of vacuum, under full power operation, was devastating. Such a failure occurred many times, at about three-week intervals, until it was finally found that the entire radio frequency system had no dc ground at all. Collections of ions in an irregular and unpredictable way could lead to dc charging of the entire system of D's to very high voltages, leading ultimately to dc discharges in the low-pressure residual gas.

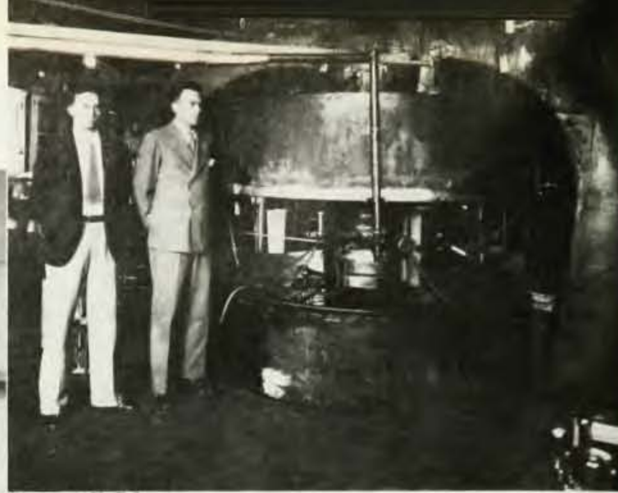
On another occasion, after some new oscillator tubes had been built and connected (the so-called "grounded grid oscillators"), no beam could be formed at all. Apparently it was Lawrence's intuition that resolved this problem. On his suggestion, tests were made that disclosed that the two D's were in phase instead of being 180° out of phase. Once identified, the error was quickly corrected.

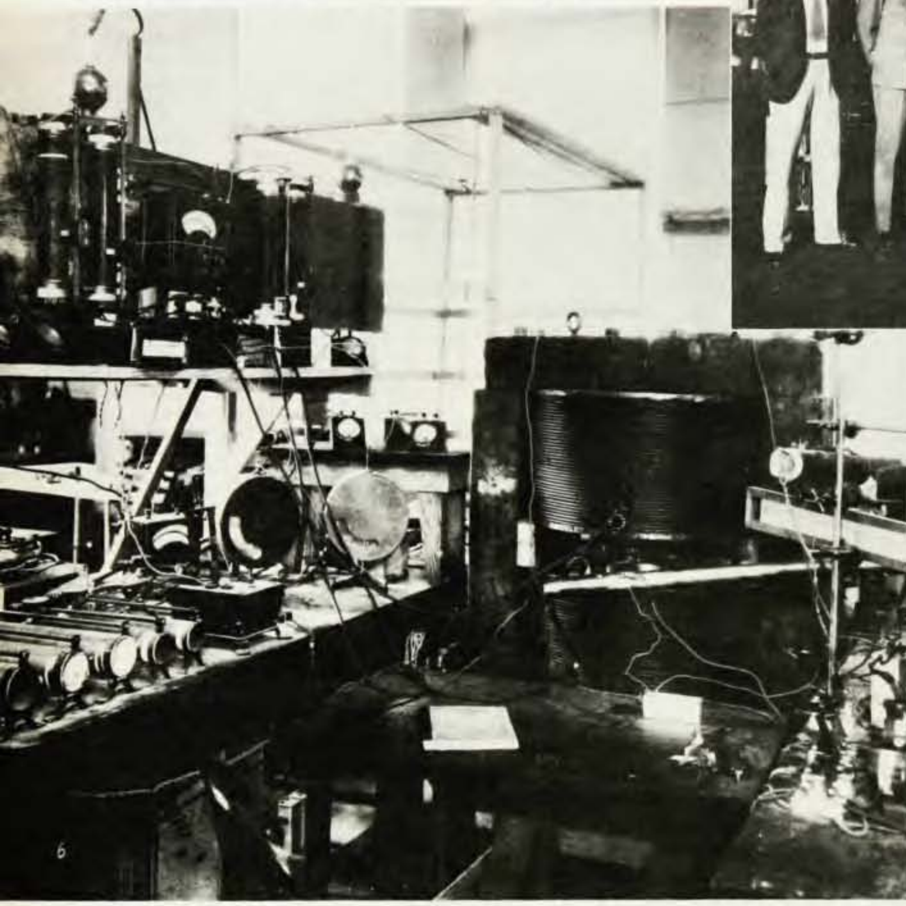
Early cyclotron results were disappointing because the beam of protons was so weak. Lawrence thereupon directed the operators to retune the cyclotron for mass 3, that is, for the H_3^+ ion, and a beam some 60 times stronger was at once found. This again must have been a hunch on Lawrence's part. It is true that H_3^+ molecular ions had been reported in canal-ray experiments in the 1920s. However it was 1936 before the paper by Joseph Hirschfelder, Dean Eyring and Hugh S. Taylor appeared that established that the H_3^+ molecular ion was indeed a stable one, and it was late in the 1950s before the stability of these ions became a matter of working knowledge to experimental "ion" physicists.

Lawrence once tried to construct cyclotron D's of aluminum instead of copper. When I asked him how the aluminum had worked, he made an angry remark and did not answer the question. The unsuitability of aluminum for electrodes seems to be one of those bits of physics laboratory lore that is widely known but has rarely if ever been reported or explained in print.

An early scientific experiment per-

Growth of the cyclotron. The 11-inch machine (below) was able to accelerate protons to 1.2 MeV in 1932. The 27-inch machine (right) incorporated a surplus magnet from the Federal Telegraph Company.





formed with the cyclotron was the analysis of the breakup of the deuteron into a proton and a neutron when it struck a target with sufficient energy. This study led to the first evaluation of the binding energy of the proton and neutron in the normal deuterium nucleus, a figure of 2.19 Mev. A highly successful union between experimentalists and theorists occurred, the analysis of the data being remembered in history under the name of the "the Oppenheimer–Philips process." It might have occurred to Lawrence following this incident to add a theorist to his staff who would devote full time to the analysis and interpretation of experimental observations, but such theorists are rare indeed (one thinks of Hans Bethe), and Lawrence may never have found one.

Hansen's rhumbatron

In approximately 1935, William W. Hansen at Stanford hit upon a new accelerating device. Starting with a hollow metal cavity shaped like a prolate spheroid about 1 meter long, he fed a radio-frequency voltage, whose frequency matched that of the 1-m length of the cavity, to a point inside the cavity fairly close to the polar end of the spheroid. A standing electromagnetic wave then formed inside the cavity, whose maximum amplitude at the mid-diameter was considerably larger than the applied signal. Hansen named this device a "rhumbatron" claiming that the name was derived from the Greek term for waves breaking on a beach. Hansen envisaged that an electron or ion beam might be introduced at one end of a short diameter of the spheroid, and the particles would then be accelerated by the voltage to the opposite end of the diameter. He further envisaged reversing the ion or electron path outside the spheroid in time to be accelerated again on the opposite phase of the wave. (Actually, the rhumbatron was to be the first step in the invention of the klystron, which generates microwaves.)

Hansen in due course proposed to Lawrence that he, Hansen, should describe his new rhumbatron at one of Lawrence's Monday evening journal club sessions, and Lawrence extended the invitation. The following day, after the meeting, Lawrence told me that he had been firmly convinced that the "rhumbatron" was nothing but a great spoof on the then new word "cyclotron," and that the one thing Lawrence must do was to control his temper and take the spoof with good humor. He sat through an hour of the meeting repeating over and over to himself, "Now don't lose your temper." Then just as the talk was nearing its end, Lawrence realized that Hansen was serious and that this was not a spoof at all. And then Lawrence was upset because he hadn't really listened to Hansen and had no idea what the rhumbatron was all about.

A digression to chemistry

A high point for physicists and chemists at Berkeley in the 1930s was the chemistry seminar at which Lewis presided with his pungent cigar in hand. Each session consisted of two parts, a 30-minute prepared lecture by one of the staff on some current publication, and an unexpected and extemporaneous progress report by a graduate student invited by Lewis on the spot to speak. The terror in the hearts of all the graduate students at the prospect that this might be *their* day can well be imagined. No fatalities appear to be on record.

During 1930, Otto Stern was a visitor at the Department of Chemistry where he presented a series of lectures on the third law of thermodynamics. William F. Giauque at this time was measuring the entropy of solid hydrogen down to temperatures very near to absolute zero, and his findings suggested that the entropy of condensed hydrogen failed to approach zero as the third law demanded that it should. Stern's lectures at each session degenerated into arguments as to whether the third law was indeed true. To Stern's amusement, as he recounted some twenty years later, each session further degenerated into arguments as to whether the *second* law of thermodynamics was true or not. Where but at the feet of perhaps the world's greatest scholars of thermodynamics could one have sat and heard the very fundamentals of the subject challenged to their roots? Lewis, classified in some circles as a mystic, seemed to seek a violation of the second law in biological processes. Whether he actually wished for such a violation or was simply in pursuit of new science—as he always was—is open to question.

There is a sequel to this story, according to Stern: He succeeded in convincing Giauque that the illusion of violation of the third law occurred when Giauque cooled the hydrogen so rapidly that, at the very low temperatures involved at which the hydrogen was solid, diffusional equilibrium did not have time to be reached and the "equilibrium" requirement of the third law was not met.

In about 1937, Lewis reported in his seminar that he was observing refraction of beams of neutrons by paraffin. Both Oppenheimer and Lawrence, who were present, were merciless in their denunciation of the purported findings. In time Lewis showed that the observed effect arose from the reflection of neutrons from the concrete floors of the laboratory, by redoing the experiment out-of-doors. This was the last experiment that Lewis—by then 62—performed, although it was not, of course, the end of his enormously imaginative and creative theoretical career.

Rarely has a decade in history been so studded with fantastic new findings, nor has one ended so completely and abruptly. By the end of 1939, thoughts were turning to the war in Europe. By 1940 secret defense projects were growing by leaps and bounds, and by 1941 military reserve personnel were being drawn from universities to the services and defense projects. In 1943, Prof. Raymond T. Birge, chairman of the physics department, remarked with wry humor that the University of California had achieved a new distinction: It was now the largest high school in the world, with thousands of military trainees pouring through its classrooms for basic, elementary training.

But Lawrence's impact had been made. The world of science was turned to "the new nuclear physics." In 1939, Wayne Nottingham of MIT sent out a general invitation to all physicists still interested in atomic, molecular, and electronic physics to attend a conference on the subject. A grand total of 35 appeared: Everyone was interested in nuclear physics. (The other fields, however, experienced a resurgence after the end of WWII.) The place of the University of California and of Lawrence had been irrevocably established in a scant ten years. And at the same time, the place of enormous and expensive equipment as "standard" in physics was firmly implanted.

It may not be remembered that Lawrence was awarded the Nobel prize in Wheeler Auditorium on the Berkeley campus in 1940, the war rendering travel to Stockholm for the award out of the question. When the check (at that time amounting to \$40 000) was presented to Lawrence, he turned to Robert Gordon Sproul, president of the university, who of course was on the platform, and passed the check to him with the remark that this was the start of the million dollars Lawrence needed for his next project, and it was now up to Sproul to find the balance. Sproul accepted the check with noticeable dismay. The million, and many more, were indeed raised, as history has long since recorded.

* * *

An early draft of this paper was delivered as an invited lecture at the International Symposium on Atomic and Surface Physics held at Maria Alm, Austria, February 10–16, 1980. □

Construction of a building for the 184-inch cyclotron in 1941, on the Berkeley campus of the University of California. The building is still a part of the Lawrence Berkeley Laboratory.

High Pressure

Tem-Pres Reactor Vessels

to 60,000 psi 900°C

Tem-Pres designs and constructs reactor vessels that accept simultaneous temperatures and pressures far beyond the limits of standard commercial units. Constructed of Rene 41, 316 stainless steel and Unitemp L-605, these vessels meet the needs of both academic and industrial laboratories for elevated pressure and temperature studies.

- P-V-T studies
- Accelerated Corrosion Testing
- Special Environmental Testing
- Solubility Determinations
- Electrical Conductivity Measurements
- Syntheses at Elevated Temperatures and Pressures
- Stability and Phase Compatibility Determinations
- High Pressure Differential Thermal Analyses
- Crystal Growth in Neutral or Pressure Media

specialists in high pressure/high temperature research systems

contact R. M. Shoff
Leco Corporation
Tem-Pres Division
Blanchard Street Extension
P.O. Box 390
Bellefonte, PA 16823
Phone: (814) 355-7903

LECO®

Circle number 17 on Reader Service Card