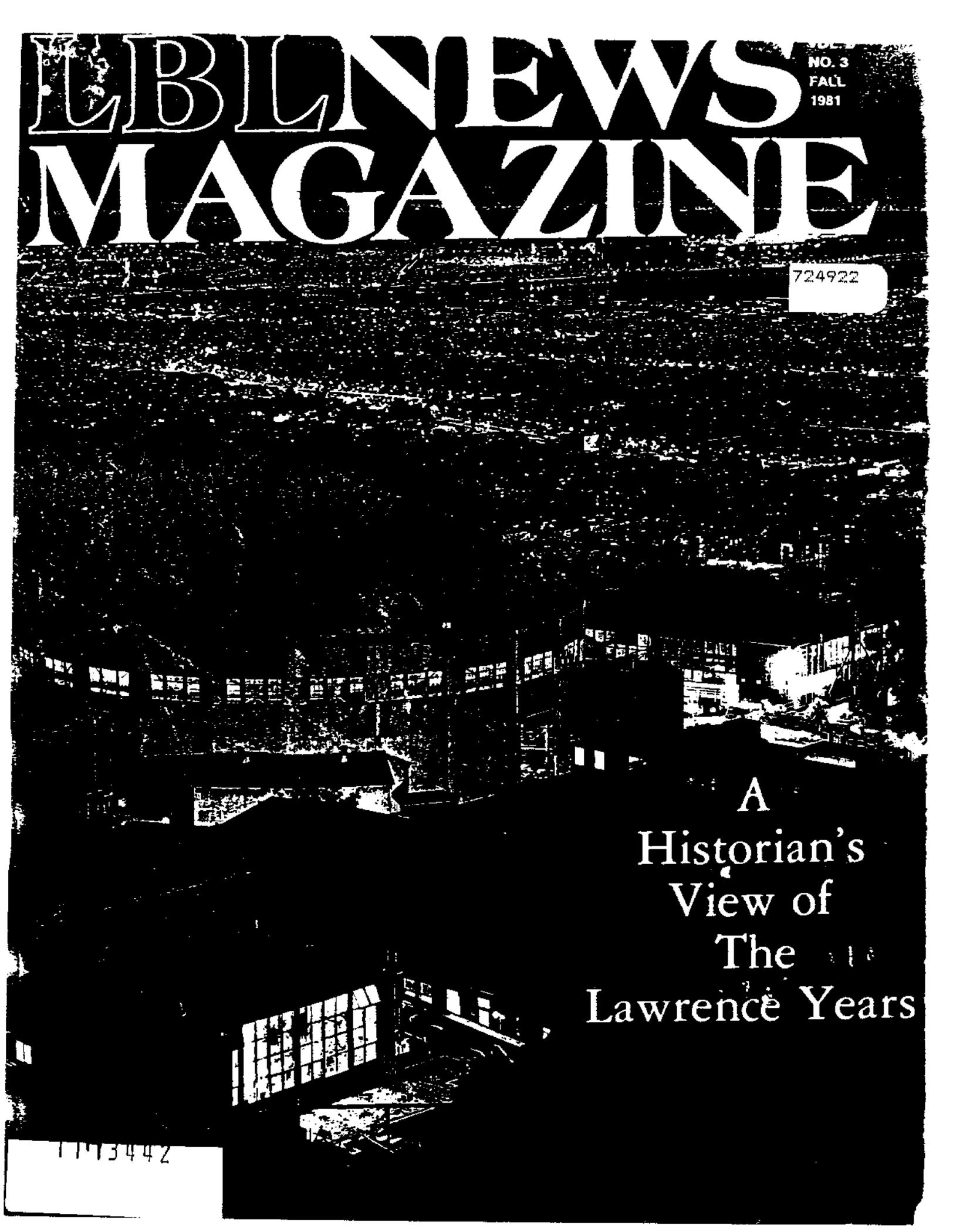


LIBRARY NEWS MAGAZINE

VOL. 3
NO. 3
FALL
1981

724922



A
Historian's
View of
The
Lawrence Years

1113442



1173443

Lawrence and his Laboratory: Nuclear Science at Berkeley



by
J. L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton

LAWRENCE BERKELEY LABORATORY
AND
OFFICE FOR HISTORY OF SCIENCE AND TECHNOLOGY
UNIVERSITY OF CALIFORNIA, BERKELEY

1981

Text © 1981 by The Regents of the University of California

1173444

Preface

THE LAWRENCE BERKELEY LABORATORY consists of ten divisions devoted to the study of as many sciences. The staff, which now numbers 3000, and their machines, from typewriter to Bevatron, have produced not only discoveries enlightening in science, ingenious in contrivance, and useful in practice, but also a vast quantity of records. On paper and on film, on perforated cards and magnetic tape, in archives, office files, personal papers, agency reports, and government storage centers, these records occupy about fifty thousand cubic feet.

All the manuscripts left by Galileo and his disciples pertaining to the institutions of Tuscan science in the 17th century comprise a few hundred volumes. This paltry literature, which in size does not exceed the run of log books for the Bevatron, has nourished the studies and disputes of historians for over two centuries. In contrast, we have the opportunities and disadvantages of endless material, no predecessors, and a distance in time from the object of our study insufficient (according to the rules of our trade) for historical perspective. We mention these points not to excuse errors, but to enlist help in a common cause. Like those whose labors and achievements we record, we seek a small signal against a vast background of noise.

The historical record of the last twenty years of the Laboratory is especially profuse and diverse. New opportunities arising from shifts in perceived national needs and a leveling off of support for high-energy physics then brought a multiplication and diversification of research programs. For thematic unity we concentrate here on the first stage of the Laboratory's development. We plan to bring coverage further forward in the much larger and more comprehensive history that we are preparing.

The present account runs on two levels. One is a general narrative of the main events and forces in the several periods into which we have divided the Laboratory's first thirty years. The other is an episode or two from each period that we take to typify it. In choosing these episodes we considered the leading part played by accelerators throughout most of the Laboratory's history. We shall have opportunity for broader coverage in the full history, and welcome suggestions about its scope and approach.

It is a pleasure to thank Judith Goldhaber and Ralph Dennis, both of the *LBL Newsmagazine*, for suggestions for illustrations and for the handsome book design; Jacqueline Craig of the Office for History of Science and Technology for formatting and producing the text; Edward J. Lofgren, chairman of the Laboratory's Fiftieth Anniversary Committee, for his generous support; Vicki Davis, LBL archivist, for her hospitality and cooperation; and many members of the Laboratory staff for documents, hints, comments, and criticisms.

J. L. Heilbron
Robert W. Seidel
Bruce R. Wheaton



Inspecting the steel shims in an alpha calutron tank to increase output of uranium-235.



Detail of the two ion source grids of the initial alpha calutron. Top beams exit downwards into the funnel-shaped electrode boxes.

1173478

started for Oak Ridge. Laboratory costs exceeded half a million dollars a month.

The first wave of Berkeley workers arrived had to see that the XAX magnet worked could begin on the first production "racetrack," a 24-fold magnification of the original. The racetrack could hold 96 calutron alpha tanks. To reduce magnetic losses and steel consumption, the racetrack was curved into an oval 122 feet long, 77 feet wide, and 15 feet high. Want of copper for the racetrack to produce the magnetic fields prompted a search for a possible only in wartime: Groves drafted 14 tons of pure silver from a government vault for the racetrack. Late in the summer of 1943 the racetrack was ready for testing. After a week of operation, it cleared the hurdle for full-scale production.

The first two of five projected racetracks failed in November and failed from contaminated oil; the second was limping in January, but produced 200 grams of uranium enriched to 12 percent by the end of February 1944, its fifth goal of one kilogram of enriched uranium a month. By April four racetracks were functioning, including the repaired number 1. They required constant attention. Many people from the Berkeley Laboratory helped to modify the units to reach production. Responsibility for operation passed to the Tennessee Eastman after the spring of 1944. Laboratory staff at Oak Ridge turned the



E. T. S. Walton John Cockcroft

offer projects of various content. In multiplying opportunities for donors he built not only particle accelerators but also a new hybrid science, nuclear science, a combination of physics, chemistry, biology, and medicine.

Nuclear science arose from attempts to open a field of physics. The study of nuclear transformations began in 1919 with Ernest Rutherford's discovery of the reaction $N^{14}(\alpha,p)O^{17}$, in which a nitrogen nucleus absorbs an alpha particle and ejects a proton to become an oxygen nucleus. The alpha particles came from the only source then available, naturally occurring radioactive elements. For a decade the few physicists who followed up the discovery had no other tool to penetrate the nucleus and made little progress. An extraordinary natural source, a gram of radium exclusive of its decay products, produces 37 billion alpha particles a second, of which perhaps one in one hundred thousand induces a transformation, too few by far to permit chemical separation and examination of the product. Furthermore, the energies of naturally occurring alpha particles, a few million electron volts (MeV), may only just suffice to bring them through the electrical repulsion of the nuclei on which they fall. Rutherford's group at the Cavendish laboratory in Cambridge discovered that naturally occurring alpha particles induce more transformations the faster they travel. A machine was needed to increase the number and speed of the particles, and the pace of nuclear physics.

The construction of an abundant source of energetic alpha particles would have appeared far-fetched if not impossible and useless before the first world war. In 1927, when Rutherford, as President of the Royal Society, expressed a wish for a supply of "atoms and electrons which have an individual energy far transcending that of the α and β particles from radioactive bodies," the prospect had come within the reach of technology. In the interim the rapidly growing demand for electrical power had caused industry to surmount technical challenges of high-voltage generation and transmission. The experience and apparatus so accumulated supported the work immediately undertaken to realize Rutherford's wish; and the disclosure in 1928 by George Gamow, that quantum mechanics allows easier penetration of nuclei than physicists had thought, further encouraged the quest for high energy. At Cambridge John Cockcroft and E. T. S. Walton used a voltage multiplier designed by Continental engineers around 1919. Merle Tuve at the Carnegie Institution of Washington used the air transformer invented by Nikola Tesla. Robert J. Van de Graaff, who worked briefly in a power plant in Alabama, devised his electrostatic generator as a source of direct-current power. After a year at Oxford as a Rhodes scholar, he adapted his system to the acceleration of particles. And Charles Lauritsen exploited the facilities of a high-tension



Ernest Rutherford at the Cavendish Laboratory.



Early particle accelerators, like MIT's Van de Graaff machine, had difficulty containing the high voltages required

to redesigning the calutron system for higher efficiency.

Many at the Laboratory, especially Lofgren and Kamen, thought that a second stage would be necessary to reach the required enrichment. Groves approved the idea. In the spring of 1943, during training at Berkeley for alpha operations, design began on the second or beta stage. Because beta would have only the enriched product of alpha as feed, it would process proportionately less material; its beam therefore did not need to be as broad, nor its dimensions as large, as alpha's. Beta design emphasized recovery, not only of the further enriched output but also of the already enriched feed. The first units were tried at Oak Ridge in late February 1944, but the sources had to be redesigned, and even by June difficulties persisted in recovering the precious beta feed strewn throughout the calutron. Process efficiencies stayed low: only 4 or 5 percent of the U-235 in the feed ended up in the output. A better source of enriched uranium feed

would have to be found to create the 10 kilograms or so of 90 percent U-235 that Oppenheimer thought necessary for a bomb.

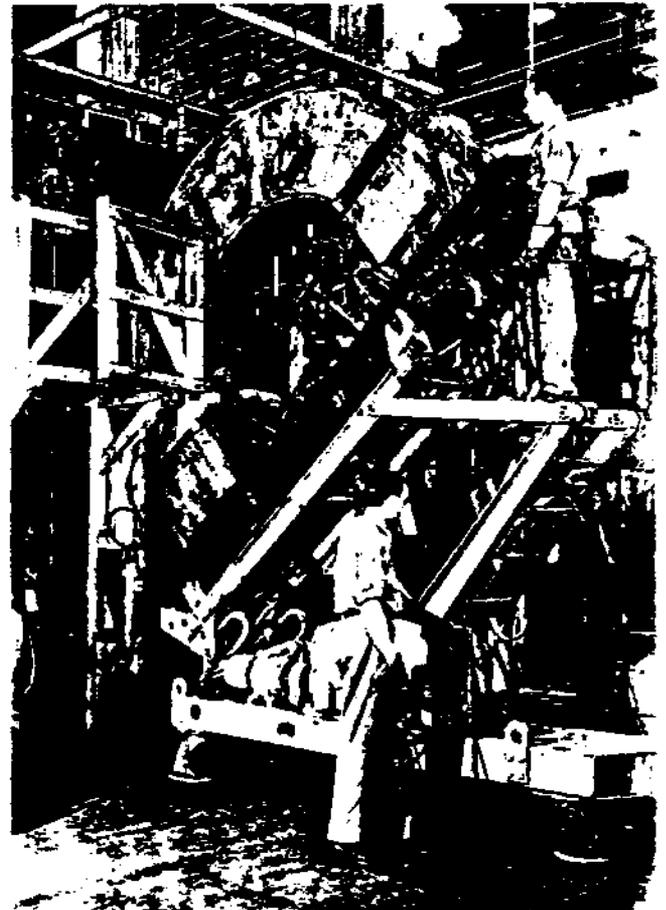
The gaseous diffusion procedure for separation of uranium isotopes, which had consumed more money even than the calutron, had not met its design goals by late 1944. Groves decided that it could not be counted on to produce high enrichment, and that whatever it did produce would have to be supplemented with other slightly enriched uranium and processed through beta calutrons. To augment the calutron feed, the MED constructed still another plant at Oak Ridge, this one working by thermal diffusion, a method developed by Abelson.

In the critical production period in the first months

Photos courtesy Oak Ridge National Laboratory



A vast bank of diffusion vacuum pumps operated underneath the alpha calutron racetrack to free the tanks of air.



The "C" shaped alpha calutron tank, together with emitters and collectors on the lower-edge door, was removed in a special "drydock" from the magnet for recovery of uranium-235.

1173400



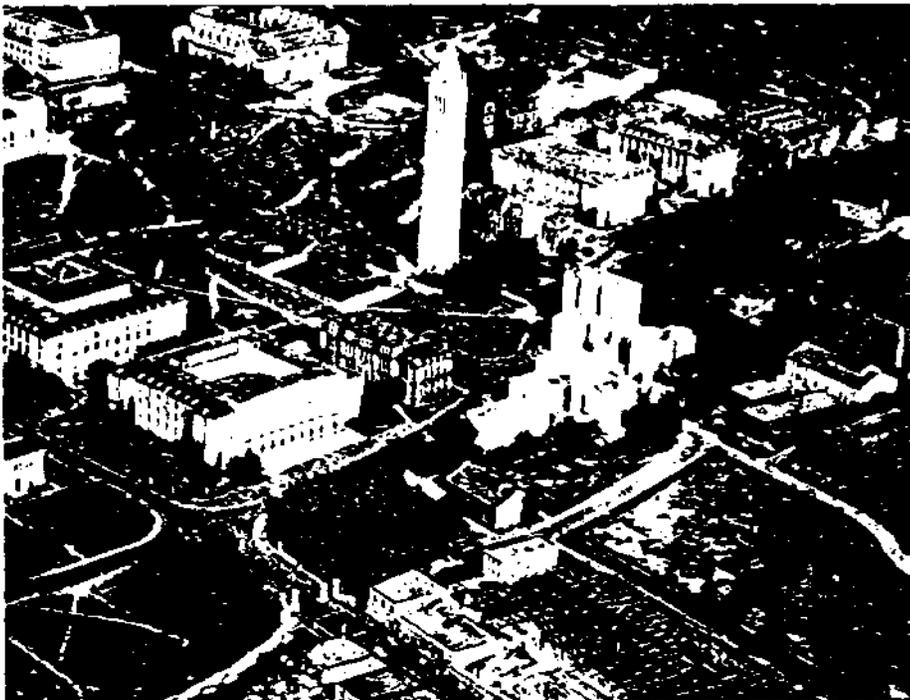
Carl D. Anderson (left) and Millikan (right) on Pike's Peak, where Anderson found evidence for the mesotron in 1936

Berkeley, may be illustrated by the main line of work of Millikan's research group around 1930. The line was study of "cosmic rays," the "birth cries of the universe" (both phrases coined by Millikan); its financial backing, local resources and the Carnegie Institution of Washington; its most elaborate method, the examination of tracks left by the rays as they crossed a big cloud chamber exposed to a strong magnetic field. In 1932 this installation gave evidence for the existence of a positive electron, which four years later brought its discoverer, Carl D. Anderson, America's fourth and California's first Nobel prize in physics.

The physicists at the University of California had also enjoyed more than an average share in the growth of American physics since the first world war. Their research facilities had been greatly improved in 1924 with the completion of LeConte Hall, the first physics building at a public American university built and furnished as lavishly as the best at the big private schools. Their departmental research fund rose from nothing to \$13,000 a year between 1920 and 1930; though less than what Millikan had from Caltech's endowment, it yet represented a substantial, and growing, commitment of the State to the support of physics. Their staff increased in numbers and improved in quality.



This cloud-chamber photograph, showing the track of a positively charged particle of electronic mass slowed down by passing upward through a lead plate, was among the earliest evidence of the existence of the positron adduced by C. D. Anderson (1932).



By permission of the University of California

*"Cosmic rays" and
"the birth cries of
the universe"*

U.C. Berkeley campus circa 1940. The Old Radiation Laboratory is the small, house-like structure to the right of the campanile at clock height; Le Conte hall is directly beneath the lab. Crocker Laboratory above it

1173449

*He boarded the
train for Chicago
with the world's
supply of plutonium
in his briefcase*

42

Narrative continued from page 35

Photo courtesy Oakland Tribune



McMillan recreating the search for neptunium at the time of the announcement of the discovery, June 8, 1940.



Glenn Seaborg adjusts a Geiger-Müller counter during search for plutonium at the Laboratory.

16-MeV deuteron beam of the 60-inch cyclotron to produce the 2.3-day activity. It still did not behave as a fission product, nor, as close inspection disclosed, as a typical rare earth. Philip Abelson, who had been searching for the same activity in uranium samples at the Carnegie Institution of Washington, where he had gone to set up a cyclotron, came on a visit to Berkeley and joined forces with McMillan. They showed that the activity grew from U-239 and that its chemistry resembled uranium's. The resemblance had protected it from detection by investigators who expected something similar to rhenium. No one had suspected, as McMillan and Abelson now did, that there existed a "second 'rare earth' group of similar elements." McMillan named the new element neptunium after the planet next beyond Uranus, and noticed (after Abelson's return to Washington) that it has a descendent that emits alpha particles. Before he could determine its chemistry, however, he went to MIT to help develop radar, the war technology then most pressing. With McMillan's consent, Seaborg picked up the work on the alpha emitter, element 94. They were to share the Nobel prize in chemistry in 1951 for their discoveries of the first transuranic elements.

The new element, called plutonium on McMillan's principle of nomenclature, proved elusive. The first isotope identified was not McMillan's alpha emitter but Pu-238, a shorter-lived decay product of neptunium made by irradiating uranium-238 with deuterium in the cyclotron. The discoverers, Seaborg, McMillan, J. W. Kennedy, and A. C. Wahl, learned enough about plutonium chemistry to know how to concentrate McMillan's alpha emitter (Pu-239). In May 1941 Kennedy, Seaborg, Segrè, and Wahl succeeded in doing so and also established the new isotope's fissionability. It appeared that in sufficient quantities plutonium-239 might sustain an explosive chain reaction. After Pearl Harbor, the OSRD authorized Lawrence to continue plutonium studies at Berkeley and Arthur Compton to supervise the work toward a controlled, self-sustaining, plutonium-producing chain reaction that had been started by Fermi at Columbia and moved to Chicago. In March 1942 Seaborg was asked to join Compton and Fermi to develop chemical processes to separate plutonium after production. On April 17 he boarded the train for Chicago with the world's supply of plutonium in his briefcase.

Seaborg's move did not put an end to work on plutonium in Berkeley. Wahl, for example, worked on the lanthanum-fluoride process that Seaborg used to isolate the first weighable amount of plutonium in the summer of 1942. The Dean of the College of Chemistry, Wendell Latimer, supervised the work and began investigations of the effects of heat upon materials to be used in the plutonium production piles. In work parallel to Latimer's, Hamilton's group at the 60-inch cyclotron examined the effects of fast neutrons on the gra-

1173482

Lawrence, a skilled amateur operator, knew their uses and designs. Like the high-potential machines of Cockcroft-Walton, Tuve, Van de Graaff, and Lauritsen, Lawrence's low-potential cyclotron would not have been possible without then recent industrial development. In his case the oscillator tubes and circuits associated with commercial radio, which allowed him to escape the pitfalls of high potential, were decisive.

The first successful cyclotron, built by Lawrence and his graduate student M. Stanley Livingston, accelerated a few hydrogen-molecule ions to an energy of 80,000 electron volts. Since each ion received an accelerating kick twice in a circuit as it entered and left the single flat semicircular electrode or "dee," those that managed to reach full energy and fall into the collecting cup 4.50 cm from the center of the instrument had made no fewer than forty turns. The result, reported to the American Physical Society meeting in January 1931, earned Livingston his Ph.D. and Lawrence \$500 from the National Research Council towards the construction of a machine that might be useful for nuclear physics.

Their main problem was to assure that the particles stay away from the walls of the dee during the many revolutions necessary for acceleration. Lawrence and Livingston bent the electric field lines and hence the paths of the particles toward the central horizontal plane of the cyclotron by placing a grid across the entrance to the dee. They achieved a small beam. An even larger one appeared, however, when Livingston tried a run without the shielding grid. The current reached a billionth of an ampere, about the number of alpha particles emitted each second from a gram of radium. The net electric forces at the mouth of the dee bow inward; their net effect on an accelerating particle is to push it back toward the desired plane.

A similar agreeable surprise occurred in 1931, after Livingston had taken great care to make the pole faces of a new magnet give a uniform field. Again it turned out that natural inhomogeneity in the field improved the strength of the beam. Lawrence and Livingston learned that thin, soft iron shims greatly encouraged the beam when placed between the wall of the vacuum tube and the magnet so as to make the total magnetic field decrease with distance from the cyclotron's center. The magnetic flux bulges outward and particles moving out of the central plane experience an increasingly large force pushing them back. The empirical discovery of the focusing exerted by unshielded electric and shimmed magnetic fields made possible cyclotron beams with useful intensities.

To obtain beams with useful energies, Lawrence required more powerful oscillators, a larger tank, and above all, a bigger magnet. The rapid pace of radio technology again helped. While Lawrence was studying the design of several large and expensive magnets, he

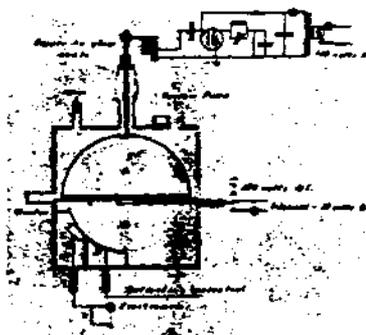


Diagram of the first successful cyclotron constructed by Lawrence and M. S. Livingston. The single dee is five inches in diameter.



M. Stanley Livingston (above) and the first successful cyclotron (below).



By permission of the Bancroft Library



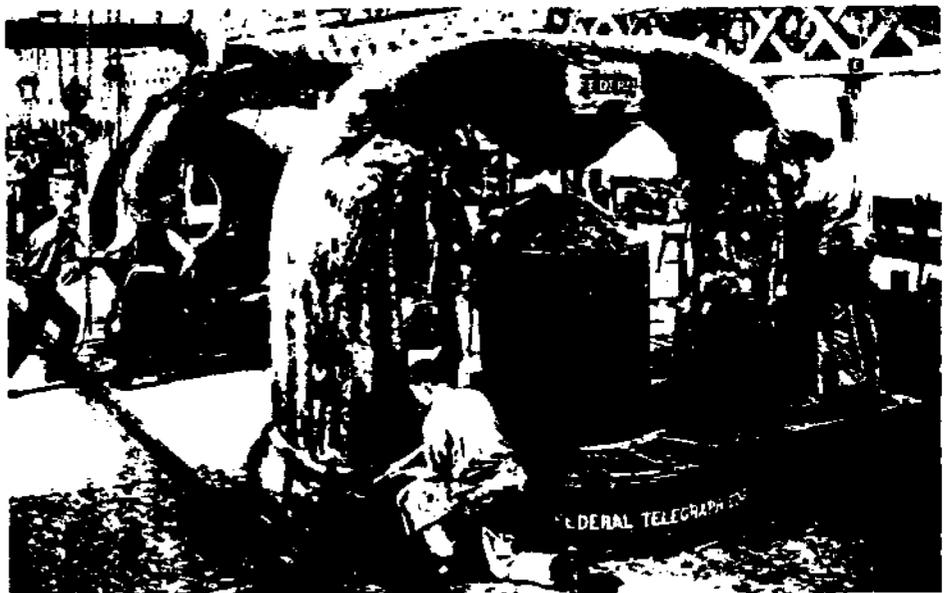
Sketch from Lawrence's notebook of an early shim for the 11-inch cyclotron.

The gift came as it was, eighty tons of metal fifty miles from Berkeley

learned that a huge magnet yoke stood idle at Palo Alto. The white elephant had been made by a local firm, the Federal Telegraph Company, for use in a method of radio transmission made obsolete by the vacuum tube. Lawrence was able to secure the yoke through Leonard T. Fuller, professor of electrical engineering at the University, who was also a vice president of Federal.

The gift came as it was, eighty tons of metal fifty miles from Berkeley. How would Lawrence get it home, and where would he put it? Who would pay to convert it for use in a particle accelerator? Lawrence appealed to Frederick Cottrell, a chemist formerly with the University, who had set up the philanthropic Research Corporation of New York on royalties from industrial use of a method of smoke precipitation he had invented. Lawrence hinted that the cyclotron might be useful for high voltage x-ray technology as well as for nuclear physics; the Research Corporation made available \$5000, and its president, Howard Poillon, secured \$2500 from the Chemical Foundation to move and equip the magnet yoke. During August 1931 Robert Gordon Sproul, president of the University, agreed to house the magnet and to pay for the power to run the cyclotron.

The Rad Lab, the forerunner of the present Lawrence Berkeley Laboratory, arose from the skillful mobilization of science, technology, philanthropy, and the University. To keep the Laboratory going and growing in the next decade, Lawrence would have to return again and again to these resources, and to expand them. The timing might not appear propitious. The Rad Lab came into existence as the coun-



Workmen at Federal Telegraph smoothing two castings for 80-ton magnets. The tall central pole had to be machined down for use in the cyclotron.

1173452



Economic Depression and calls for a "New Deal" dominated the national consciousness during the years Lawrence was building his Laboratory.

try was sliding into the depths of the Great Depression. Lawrence could have read in the business section of *Time* for the last week in September that the first quarter of 1931 bid fair to be the worst period ever in American financial history. By any measure—available electric power, tax receipts, balance of payments, housing starts, idle steel capacity—the country was scraping bottom. Hiring froze at many universities and at the big industrial research laboratories; foundations cut back commitments; the number of fellowships declined. Not until 1935 did the profession as a whole resume its growth. Lawrence set up a new laboratory at the nadir of the world's finances.

Lawrence's optimism engaged the willing belief of ordinary people sick of Depression

Against the dire circumstances of the early 1930s Lawrence's optimism, even boosterism, engaged the willing belief of ordinary people sick of Depression. He staffed his laboratory with graduate students and junior faculty of the physics department, with fresh Ph.D.s willing to work for anything, and with fellowship holders and wealthy guests able to serve for nothing. He and they created an environment of enthusiasm, congeniality, collegiality, and technical competence. There really was little money, despite Lawrence's success at grant gathering, while the machines consumed tens of thousands, and then hundreds of thousands of dollars, the staff made do with small salaries, if any, and none of the fringe benefits now common: medical insurance, secretaries, and paid travel to meetings.

Lawrence's machines and entrepreneurship succeeded first with physicists and chemists and their supporters. He wondered where his new science belonged. "Shall we call it nuclear physics or shall we call it nuclear chemistry?" He next found backers in the life sciences, whom he approached around 1935, as the Depression began to lift. The biggest of the Berkeley cyclotrons of the 1930s was built for "nuclear medicine." The hybrid, nuclear science, arrived to effect the advancement that none of its constituent disciplines could achieve alone.



Early cyclotroneers (left to right): J. J. Livingood, F. Exner, M. S. Livingston, D. Sloan, Lawrence, M. White, W. Coates, L. J. Laslett, T. Lucci.

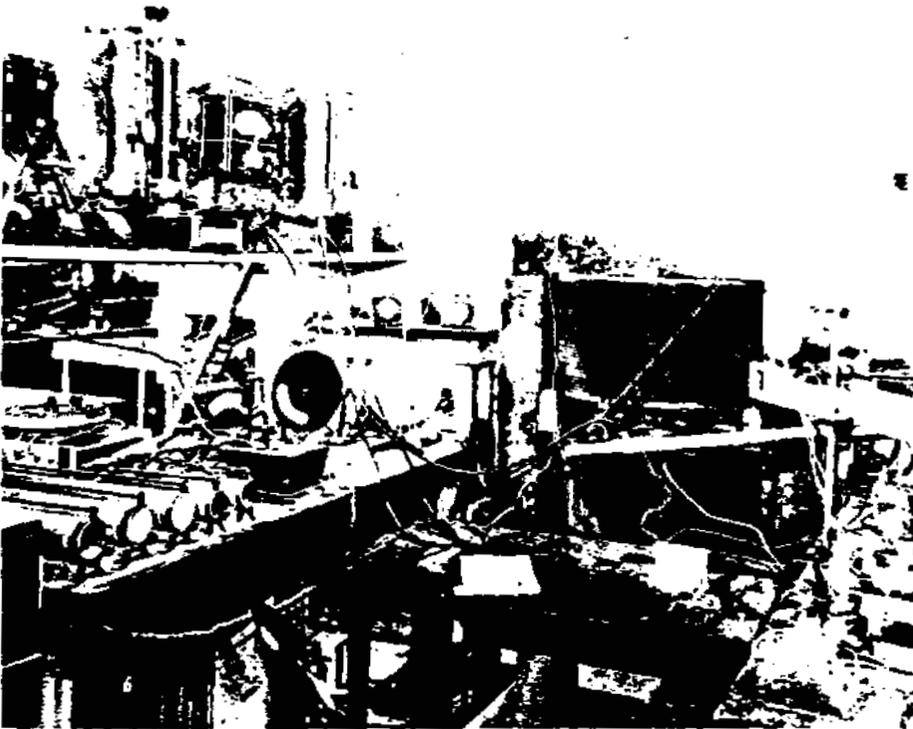
1173483

that yielded almost one millionth of an ampere of 1.2 MeV mercury ions. Lawrence's growing armament could shoot million volt particles from either end of the periodic table. With neither the linac nor the cyclotron, however, did Lawrence's associates do much nuclear physics. Sloan was reassigned to a project designed to keep alive philanthropic interest in the Rad Lab. Lawrence's backers, the Research Corporation and the Chemical Foundation, had just succeeded in breaking General Electric's patents on high-energy x-ray tubes. At their request Sloan developed a competitor, later installed at the University of California Hospital in San Francisco and the Crocker Institute for Cancer Research at Columbia. In hopes of supporting other scientific enquiries by its investments in accelerator technology, the Research Corporation patented not only the Sloan x-ray tube, but also the cyclotron and the Van de Graaff accelerator.

The making of million-volt protons in January 1932 appropriately opened a year of exceptional discoveries in nuclear science. The same month Harold Urey and his collaborators at Columbia declared the existence of a hydrogen isotope twice as heavy as the ordinary kind. In February James Chadwick announced his discovery of the neutron at the Cavendish Laboratory in Cambridge, England. In



David Sloan and J. J. Livingood work on the Sloan x-ray tube built at the University of California Hospital in San Francisco in 1932-3. With this machine, Lawrence's backers hoped to break the stranglehold of the large electrical manufacturers on the high-voltage x-ray tube market.



The 11-inch cyclotron, shown installed in Room 329 Le Conte Hall, was the first cyclotron to exceed 1 MeV

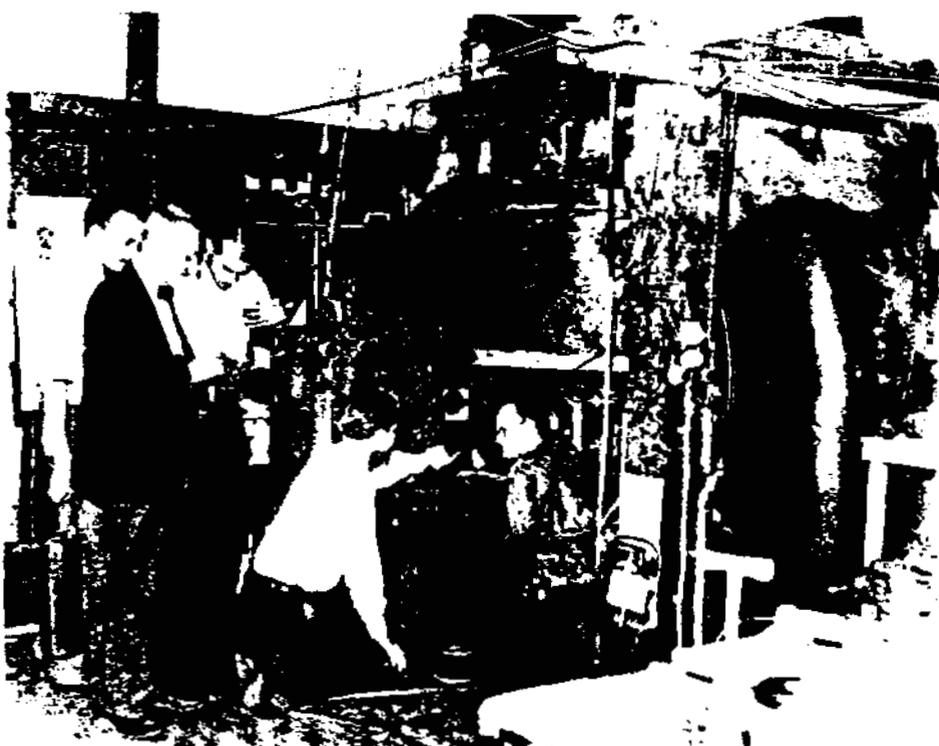
1173455



Donald Cooksey

April John Cockcroft and Ernest Walton, also at the Cavendish, succeeded in disintegrating lithium atoms with 125 kV protons from their voltage multiplier. In the fall Caltech's Anderson found the positron. And throughout the year Lawrence, Livingston and Sloan labored to produce a beam between the poles of their 75 ton magnet. The sheet metal tanks that held the cooling oil leaked. "We all wore paper hats," Livingston recalls, "to keep the oil out of our hair." Experimentation with shimming gradually brought the beam to larger radii and energies; two symmetric dees were installed; and in December the new 27-inch cyclotron produced 4.8 MeV hydrogen ions.

The artificial disintegration of nuclei was one of the purposes of the apparatus Lawrence had designed. The disintegration of lithium might have been achieved at Berkeley before its accomplishment at Cambridge, and perhaps more easily because of the eight-times greater energy of the California protons. But the planning of physics experiments had not paralleled the construction of the instruments to perform them. This negative consequence of Lawrence's concentration on accelerator improvement was to recur throughout the 1930s. Artificial radioactivity and nuclear fission, to mention only the



From left to right, F. Kurie, D. Cooksey, E. McMillan, Lawrence, and R. Thornton encouraging a beam in the 27-inch cyclotron

1173456

*Does the falling tree
make a noise if no
ear hears it?*

17

most dramatic cases, could well have been found at Berkeley; they were certainly produced there before being noticed elsewhere. In the case of artificial disintegration, the Laboratory lacked the proper detectors. Lawrence asked his old friend, Donald Cooksey of Yale, a masterly instrument maker, to provide what was needed. Cooksey and a student of his, Franz Kurie, built the detectors at Berkeley during the summer of 1932. They allowed the Rad Lab to confirm and extend the transformation first accomplished at the Cavendish.

The discovery of deuterium (as Urey called heavy hydrogen) also had strong consequences for Lawrence's program. In March 1933 his colleague in chemistry, G. N. Lewis, who had the largest reservoir of heavy water in the world, gave Lawrence enough to use as projectiles for the developing 27-inch cyclotron. For a time he had a quasi-monopoly of fast deuterons, which, he hoped, would help bring to Berkeley the lead in nuclear physics that the Cavendish then enjoyed. The performance of the deuteron exceeded his most extravagant expectations: it appeared capable of disintegrating every nucleus heavier than helium. But the higher Coulomb barrier presented to the deuteron by the heavier elements made this hypothesis unlikely, and Lawrence, Lewis, and Livingston claimed instead that on collision with just about anything the deuteron itself splits into its constituent proton and neutron. An argument with other nuclear laboratories ensued. It turned out that Lawrence's group had dirtied the cyclotron with deuterium, and that their fast protons arose from the interaction of the deuteron beam with the heavy-hydrogen contaminant. (See Episode beginning page 18.)

Early in 1934 Frédéric Joliot and Irène Joliot-Curie, working at the Institut du Radium in Paris, made the discovery that brought them the Nobel prize and redirected much of experimental nuclear physics. In investigating the emission of positrons from aluminum struck by alpha particles, they observed that the target stayed active after the bombardment stopped. It was a great surprise. Everyone had tacitly assumed that the explosion of a nucleus followed immediately on its swallowing an energetic particle, and had arranged his experimental practice to suit. At the Rad Lab belief that residual activity does not exist affected operations in at least two ways. First, no one thought about protection against radiation when the cyclotron was not running (and little enough when it was). Second, the detecting instruments and counters were not set to register electrons and gamma rays. Does the falling tree make a noise if no ear hears it? The cyclotron had been producing substances with much stronger artificial radioactivity than the little bit of radioactive phosphorus the Joliot's found, but no detector had listened. Lawrence and his students reproduced the French discovery within a half hour after reading about it in *Nature*. A weekend's work bombarding twelve elements with



Gilbert N. Lewis, the chemist who isolated heavy water and who was instrumental in bringing E. O. Lawrence to Berkeley.



Frédéric Joliot and Irène Joliot-Curie in their laboratory at the Institut du Radium, Paris, ca 1930.

A Productive Error

18

THE SIMPLEST OF the new isotopes recognized by physicists in the 1930s was the nucleus of heavy hydrogen, a compound of a proton and a neutron. This deuteron made a particularly interesting projectile for the cyclotron since it does not occur among the rays from radioactive substances. Therefore, Lawrence, G. N. Lewis, and M. S. Livingston had no definite expectation, but many high hopes, when they loosed the world's first high speed deuterons at a lithium target in March 1933. They found the deuteron to be ten times as effective a disintegrator as the proton. It offered a glimpse into the millenium; it appeared to release much more energy than it brought. When striking lithium it produced the fastest alpha particles ever observed. When striking all other substances, it coaxed out protons with a range of 18 cm in air. It began to seem too much of a good thing. Positively charged deuterons of a few million electron volts could not be expected to penetrate the Coulomb barrier of so heavy a nucleus as gold even by the tunneling discovered by George Gamow, E. U. Condon, and R. W. Gurney. Lawrence's group concluded that the fast protons they found came not from target nuclei, but from the disintegration of the deuteron. They accordingly announced that the nuclei of heavy hydrogen are unstable and decompose in the electric fields inside atoms.

This "disintegration hypothesis" was made public by Lawrence in May 1933 at a symposium at Caltech in honor of Niels Bohr. It had far-reaching consequences. First, the reaction Lawrence proposed implied that James Chadwick had grossly overestimated the mass of the neutron (m_n). Chadwick assumed that his neutrons had come into being when a boron nucleus absorbed an alpha particle according to the reaction $B^{11}(\alpha, n)N^{14}$. He computed the neutron mass by subtracting the kinetic energy of the neutron and the rest energy of nitrogen-14 from the total energies of the incident particle and the target atom. The result, according to the crude contemporary values of atomic masses: $m_n = 1.0067 m_p$, where m_p is the proton mass. Lawrence and his colleagues worked from the deuteron disintegration hypothesis, $d \rightarrow n + p$; and they fixed the neutron mass at $1.0006 m_p$ from the known rest masses and the hypothesis that the 18 cm proton carried away all of the deuteron's kinetic energy. (Neither value

agrees with the modern one, $m_n = 1.0014 m_p$.) Second, the implied instability menaced the theory Werner Heisenberg had devised on the assumption that the nucleus contains only neutrons and protons.

Since nuclear theory would disintegrate along with the deuteron should Lawrence's hypothesis hold, the two principals at the Caltech celebration welcomed it, although for different reasons. Niels Bohr, always hoping for conceptual revolution, extolled it as "a marvelous advancement" and the cyclotron as "the dream of yesterday... come true." Robert Millikan, nationalistic and opinionated, enjoyed the discomfiture of European theorists and praised the deuteron hypothesis as "altogether extraordinary [which it was] and most intelligently announced." Shortly after the meeting in Pasadena Lawrence appeared at the Century of Progress Exhibition in Chicago as the creator of a "new miracle of science, the most powerful cannon yet found for liberating relatively enormous stores of energy locked up in the inner core of the atom." The newspaper account of Lawrence's speech for Progress concluded: "The newest developments give only an inkling of what lies in store."

What lay in store was a tough time. In October Lawrence brought his results before the seventh Solvay Congress in Brussels. Attendance was a great honor; Lawrence was only the eighth American ever invited, and the sole one for 1933. He did not, however, have the burden and distinction of presenting a full report. He appended a few pages on the operation of cyclotrons and the disintegration of deuterons to a lengthy account of Cambridge work on accelerators. The author, John Cockcroft, declined to entertain Lawrence's hypothesis on the ground that too little data yet existed; but at the end of his report he allowed himself to hint that the cyclotron might be a wasteful and unnecessary machine.

The Cavendish physicists had come to Belgium in strength. After Cockcroft, Lawrence faced Ernest Rutherford, who declared that no neutrons come from lithium under deuteron bombardment, and Chadwick, who insisted that the mass of the neutron is exactly what he had said. Then came the theorists. Heisenberg observed that if disintegration occurred in the electric field of the nucleus, the yield should decline for heavy targets since the deuteron's penetration, and hence the rate of change of force on it, must decrease with increasing atomic number.

INSTITUT INTERNATIONAL DE PHYSIQUE SOLVAY

SEPTIÈME CONSEIL DE PHYSIQUE — BRUXELLES 22-29 OCTOBRE 1933



H. A. KRABBE E. F. BRY E. GABRI P. BLACKETT H. GUYON A. J. F. PIGANI
 E. STANLEY P. A. M. DIRAC J. ERDMAN C. D. ELLIS C. A. LAWRENCE
 C. HENRIOT F. JOLIOT H. WEISSBERGER E. T. S. WALTER P. DEBYE S. CARLBERG H. BRYNE H. L. BAYER L. E. VERHOEFFELT J. B. COCKROFT L. BRUNSWIG
 P. PERRIN E. FERMI H. S. HENNINGSEN W. PAULI E. HELLER E. FERDINAND
 E. SCHRODINGER H. J. B. L. J. BOHR A. JOFFE H. - EINSTEIN D. W. RICHARDSON L. H. RUTHERFORD H. DE BROGLIE H. L. MITCHELL J. CHADWICK
 P. LANGEVIN T. DE DONDER L. DE BROGLIE

Address: A. EINSTEIN at Dr. GUYON

Participants at seventh Solvay congress in Brussels, Belgium, in October, 1933. Lawrence is standing second from right; Rutherford is sitting sixth from right; Chadwick on far right; Bohr third from left; Heisenberg stands fourth from left, and Cockcroft fifth from right.

That being the case, added Bohr, we might suppose that the deuteron splits after entering a nucleus, but then the speed of the ejected proton should increase with atomic number like the nuclear Coulomb field, contrary to Lawrence's results.

The debate continued when Lawrence stuck his head in the Cavendish lion's den on the way back to

Berkeley. Cockcroft, Marcus Oliphant, and Rutherford all dismissed the deuteron hypothesis and advised Lawrence to look for contamination of his targets or his tank. Back in the friendly West, Lawrence hastily reviewed the possibility of systematic contamination with the help of chemical colleague Lewis. The resulting paper, sent to the *Physi-*

cal Review in December 1933, should have convinced "the most skeptical [according to Lawrence] that the deuteron is energetically unstable and disintegrates into a proton and a neutron." Some friends, for example Jesse Beams, thought Lawrence's answer decisive. Others, including Charles Lauritsen and Merle Tuve, repeated the experiments with immaculate apparatus and did not find Lawrence's protons. Then Rutherford found the fast protons, but only after deliberately contaminating his targets with deuterium. Lawrence's conviction waned. By April 1934, with artificial radioactivity claiming his attention, he had discarded the deuteron disintegration hypothesis.

The Cavendish had known about the deuteron hypothesis almost from its inception, through Lewis and through Cockcroft, who reported it after a visit to the Laboratory in June 1933. Cockcroft doubted the hypothesis and urged his colleagues to check it using the heavy water that Lewis had presented Rutherford in May, with generosity typical of Berkeley scientists. Not having on hand a machine capable of accelerating Lewis's gift to millions of electron volts, Rutherford sent alpha particles from polonium through heavy water in order to simulate a cyclotron's bombardment of helium by deuterons. He found no trace of the reactions expected on the disintegration hypothesis. Then, together with Oli-

phant and Paul Harteck, who prepared more heavy water, Rutherford neatly identified the agent of Lawrence's contamination. When protons fell on targets containing hydrogen, such as NH_4Cl , they gave precisely the same effect whether the hydrogen was ordinary or heavy; but when deuterons fell on the same targets, those containing deuterium gave a prodigious number of protons and only slightly fewer neutrons. The Cavendish group inferred that deuterons interact readily, and in two different ways, to make two isotopes previously unknown: the fusion of deuterons produces either hydrogen-3 (tritium) and a proton, or helium-3 and a neutron. According to their estimate of the masses, hydrogen-3 should be stable and helium-3 should not.

In explanation of his error, Lawrence told Beams that the cyclotron had been so prolific of results that it discouraged "methodical, quantitative measurements," or thorough investigation of one thing before running to another. Another cause for adherence to the disintegration hypothesis was the good press that accompanied its disclosure. Not only did the press's flattery blunt Lawrence's self-criticism, it also assisted his appeal for money. After his refutation of the objections of the Europeans, Lawrence wrote the Research Corporation that the explosion of the deuteron pointed both to a source of fuel and to the reform of nuclear theory: "This first definite case of an atom that itself explodes when properly struck is of great interest, not only as a possible source of atomic energy, but especially because it is not understandable on contemporaneous theories. The fact that the deuteron is energetically unstable promises to be a keystone for a new theoretical structure." What was required to place the stone, as Lawrence had told the Century of Progress Exhibition, was an "atomic gun which in comparison with the present [27-inch] one will be like a 16-inch [diameter] rifle alongside a mere one-pounder."

The conclusion of Rutherford's group, that tritium should be stable, set the proprietors of the world's best mass spectrographs to look for the heaviest hydrogen in pure samples of heavy water. Walker Bleakney and his associates electrolyzed 75 tons of ordinary water down to its heaviest cubic centimeter, which they tested in their sensitive spectrometer at Princeton. They found traces of mass-5 particles, which they declared to be molecules made of an



Photo courtesy American Institute of Physics

"The lions den" of the Cavendish: Rutherford, foreground, with colleagues; Chadwick is at right.

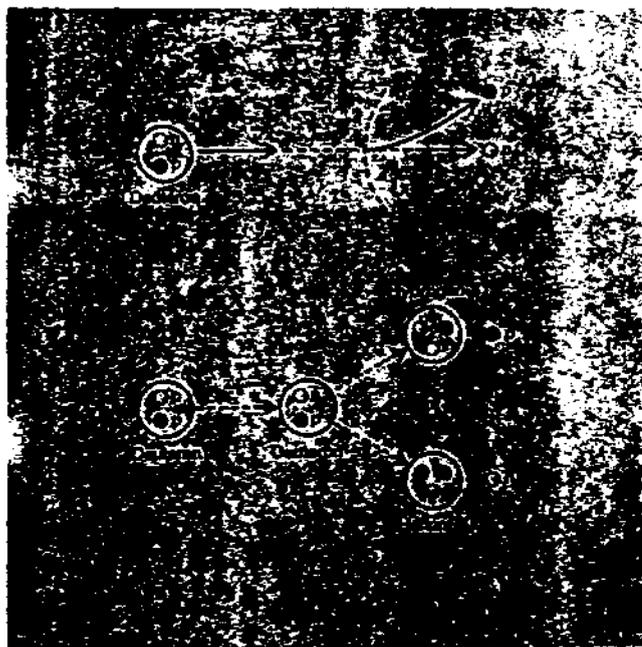
1173460

atom each of hydrogen-2 and hydrogen-3; and they guessed that hydrogen-3 constitutes about one part in a billion of ordinary water. Rutherford's group tried to confirm the finding with 11 cubic centimeters of the heaviest remainder of the electrolysis of 13,000 tons of water. The heavy part of the job was done by Norsk Hydro, and the lighter part by the dean of mass spectroscopists, Francis Aston. The 13,000 tons of Norwegian water contained not a drop of hydrogen-tritium oxide. Several groups in England and the United States then tried to make tritium by fusing deuterons. Again the Princeton physicists succeeded; again the British failed. Rutherford wondered whether Americans knew how to do experimental physics.

Since Rutherford thought that tritium is stable, he required a reason why he could not obtain it from the plentiful interactions of deuterons. His answer: tritium disappears quickly by combining with the bombarding deuterons. As for helium-3 the consensus, as represented by H. A. Bethe, held it to be unstable, decaying into the elusive tritium by

electron capture. It was precisely with this preconception—that tritium is elusive but stable and helium-3 is radioactive—that Luis Alvarez went to look for them in the summer of 1939.

The occasion of Alvarez's investigation was the temporary idleness of the just-completed 60-inch cyclotron, which had not yet acquired the shielding necessary for its high-energy work. He thought to use deuterons from the 37-inch machine to make helium-3 to feed the 60-inch, which would serve as a mass spectrometer. As a preliminary, to test whether the 60-inch would adventitiously deliver stray particles at the setting of the magnetic field for resonant acceleration of particles of mass 3, he had the operating crew reduce the field, which had been set and shimmed for alpha particles (helium-4), by one quarter. Nothing happened: apparently there would be no noise to complicate the experiment. To confirm the finding, Alvarez asked the crew to drop the field from full strength to zero and bring it back to the value of interest. They obliged, but, contrary to usual procedure, left on the radiofrequency oscil-



Top: Schematic diagram of Lawrence's hypothesis for disintegration of the deuteron to a proton and a neutron in the electric field of a nucleus. Bottom: Rutherford's proposal that two colliding deuterons decay either to hydrogen-3 or helium-3, yielding protons and neutrons.



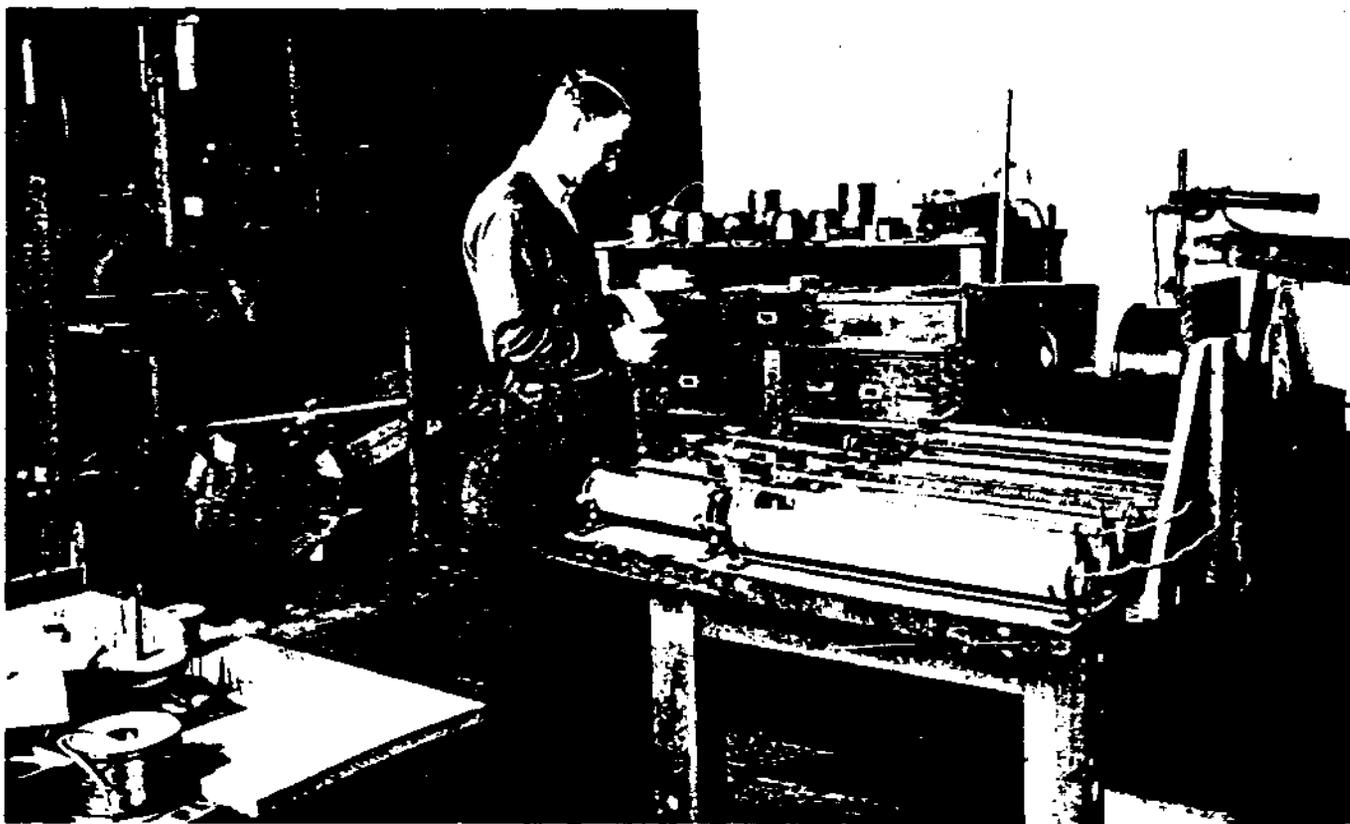
"Cornog's robot," a product of downtime on the cyclotron, ca. 1939. Model unknown.

lator when they switched off the field. As it dropped, Alvarez, still looking at the oscilloscope connected with the ionization chamber, saw a burst of particles apparently of mass 3. Eddy currents set up by the rapidly changing field had providentially shimmed the machine to accelerate ions of helium-3. All this happened before the intended experiment began, using the ion source installed for testing the 60-inch cyclotron. The source employed ordinary helium from a deep well in Texas, where it had lain for geologic ages. The radioactivity of helium-3 had been greatly exaggerated. In fact, as Alvarez declared, it is stable.

The stability of helium-3 implied the radioactivity of tritium. Alvarez tested this inference with the help of Robert Cornog, a graduate student who worked on the oil vapor vacuum pumps for the 60-inch machine. They routed the issue of heavy water irradiated with deuterons into an ionization chamber attached to an amplifier and found a long-term

activity whose carrier behaved like hydrogen. The number of active atoms agreed roughly with the number of neutrons produced in the bombardment, confirming the formation of tritium and a proton from two deuterons. The new isotope was long-lived. No appreciable decay could be detected in a sample imprisoned for five months.

It happened that an estimate of the half life of tritium already existed. It had been made at the Rad Lab in 1936 by McMillan, who had followed the decline of a beryllium-aluminum target that had been used to make neutrons by the reaction $\text{Be}^9(d,n)\text{B}^{10}$. He found an activity whose half life he put at over ten years. After the disclosure of the true nature of tritium, R. D. O'Neal and Maurice Goldhaber at the University of Illinois corrected McMillan's guess that the 10-year activity belonged to beryllium-10. They found no activity in beryllium from a target similar to McMillan's, but they acquired a nice radioactive gas on dissolving a bit of the target in acid. They



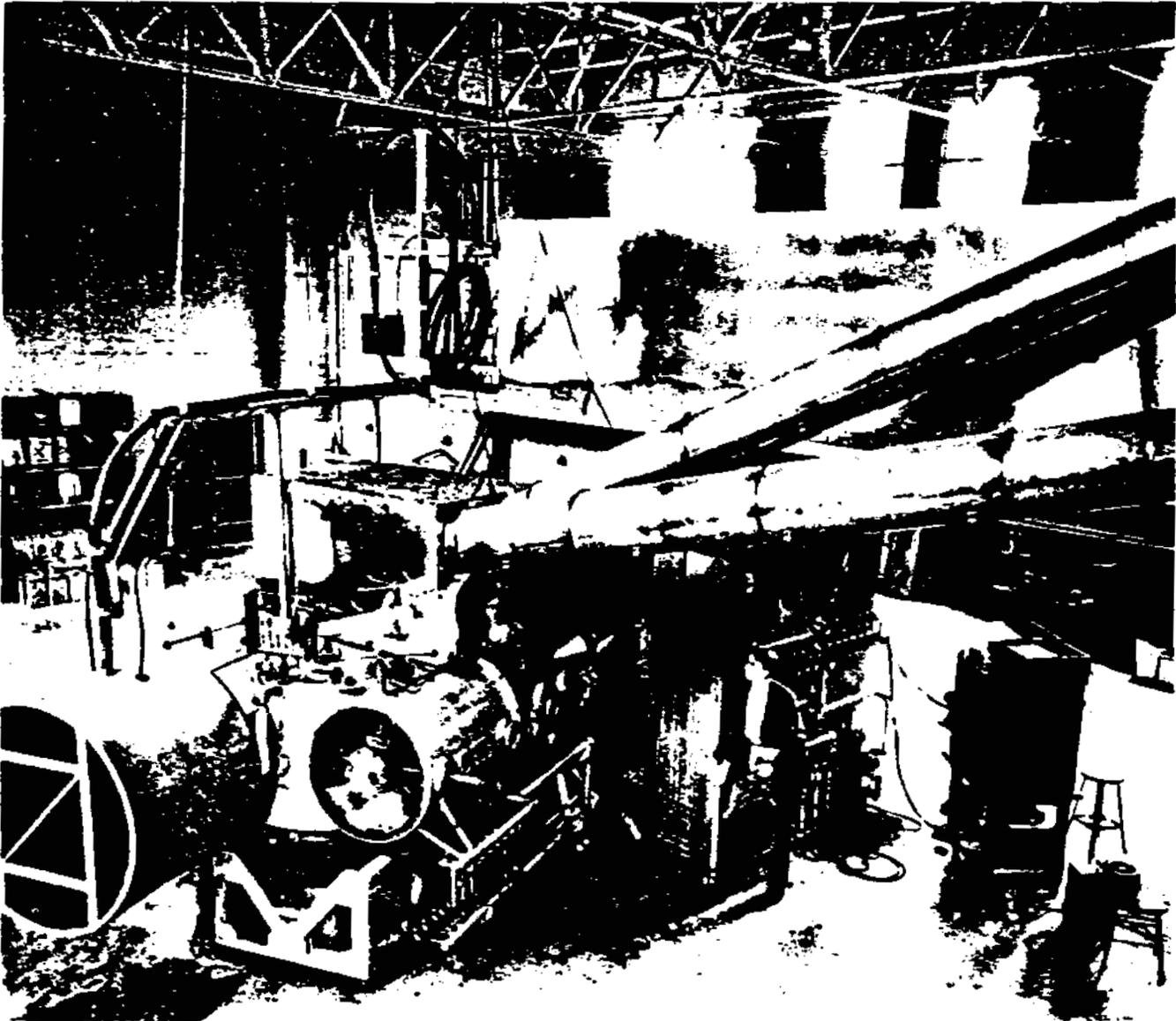
Luis Alvarez about 1938, just before his work leading to the identification of helium-3.

1173462

supposed that they dealt with a product of the reaction $\text{Be}^9(\text{d},\text{H}^3)\text{Be}^8$. Alvarez and Cornog accepted the suggestion and increased their estimate of half life accordingly.

Rutherford reported his negative results in his last scientific paper, published in August 1937. Had his Norwegian collaborators concentrated whatever tritium they might have gathered by repeated electrolysis of heavy water, his sample would have been

radioactive from trace amounts of tritium made in the atmosphere by cosmic rays and brought down in rain. The half life of tritium is 12.6 years. At the end of the second world war Willard Libby easily detected the residual activity of the sample with a Geiger counter. Apparently no one at the Cavendish laboratory in Rutherford's time had thought to make such a measurement.



Milt White beside the 60-inch cyclotron with which Alvarez showed the stability of helium-3.

1173463

Photo courtesy American Institute of Physics



Enrico Fermi in Rome, mid-1930s

Technetium, the first of the elements made by man

deuterons produced as many new activities. The subject was unexpectedly rich. "We are rather bewildered," Lawrence wrote his old friend Jesse Beams. "Already it is clear that nuclear physics offers a very extensive and complicat[ed] and interesting field of investigation."

Then Enrico Fermi's group in Rome showed that neutrons induced activity in practically all the elements. Lawrence, who had advertised possession of the world's most powerful neutron beam (formed by irradiating beryllium-9 with ten billionths of an ampere of accelerated deuterons) once again confirmed and extended European results, and expressed surprise at the richness of nuclear transactions. From March of 1934 until the Laboratory went to war, the investigation and production of artificial isotopes by neutron, proton, deuteron, and alpha-particle beams dominated its research program.

Lawrence committed the Laboratory to this program for several reasons. First, the detection and identification of new activities gave information about nuclear reactions and systematics and helped to determine conditions of stability. Nuclear scientists at Berkeley mapped the limits of the isotopic range of the known elements and, in 1940, pushed beyond uranium. Second, Lawrence knew from the work of Georg von Hevesy that radioactive tracers in the body could give unique information about metabolism and other physiological processes. The cyclotron could not only produce tracers in larger amounts than easily available in nature, it might also, and more importantly, create new radioisotopes with properties particularly adapted to biological research. No doubt the expressed interest of the Rockefeller Foundation and the Macy Foundation in the application of the techniques of the physical sciences to the life sciences encouraged Lawrence's attention to the creation of material for biomedical research. He received substantial sums from both philanthropies, and in turn supplied established workers like Hevesy with biologically active radioisotopes. The easy availability of these substances at Berkeley interested local faculty in tracer work, culminating in the discovery of carbon-14 by Martin Kamen and Samuel Ruben in 1940.

Soon after he began his search for useful radioisotopes, Lawrence had the good luck to make sodium-24 efficiently by bombarding rock salt with deuterons. The new substance runs through the body like ordinary sodium; its convenient half-life, fifteen hours, made it useful in diagnosis and therapy. "My medical friends tell me that the properties of radiosodium are almost ideal for many medical applications, such as the treatment of cancer." Lawrence predicted that sodium-24 would supersede radium, and to make sure he promoted it on a national lecture tour. A volunteer—the first two were Alvarez and Joseph Hamilton of the University's hospital in San Francisco—

*His audience
appreciated his
up-to-date natural
magic*

25

would down a solution of the isotope, and Lawrence would track its course through his body. Audiences appreciated this up-to-date natural magic with material less disagreeable, though no easier to procure, than skull moss or unicorn's horn. Lawrence received fresh supplies of sodium-24 by air mail just in time for these lectures, which increased the drama, and the value, of radioisotopes.

Radiosodium did not fulfill Lawrence's hopes. Other isotopes generated by his cyclotron, however, found important applications in medicine. Phosphorus-32 has been used successfully in the treatment of leukemia, polycythemia vera, other bone-marrow disorders, and Hodgkins disease; iodine-131 in the treatment of thyroid disease; and cobalt-60 in cancer chemotherapy. Perhaps the most interesting of these substances to the physicist and chemist is technetium-99, used in cancer diagnosis. Technetium, element 43, which occupies one of the four places in the periodic table still vacant in 1935, does not exist naturally. It was found in a molybdenum deflector strip from the 27-inch cyclotron, where quantities sufficient for radiochemical analysis had accumulated during months of exposure to fast deuterons. Lawrence presented this object to Emilio Segrè, who visited the Laboratory in the summer of 1936 and took the "invaluable gift" to Italy, to stimulate nuclear science at the University of Palermo, where he had recently become a professor. In June 1937 Segrè's group announced the first element made by man. Medical application of the new element began in 1947. Half of the seventy artificial radionuclides in common use in medicine today first made their appearances in cyclotrons, and half of these were discovered, or first synthesized, at the Radiation Laboratory.

The potential of radioisotopes for biological research and medicine gave a third reason for the search for new radionuclides: support of further cyclotron development by the sale of active material. Lawrence planned to reap the benefits indirectly, through grants from the Research Corporation, to whom he suggested patenting his method of making sodium-24. The Corporation did not succeed in obtaining a patent on radioisotope production by deuteron bombardment, but their patent on the cyclotron probably would have protected commercial production of radioisotopes until the invention of a different and more prolific source, the nuclear reactor, during the second world war. Although a radiopharmaceutical industry did not materialize in the 1930s, the hope that it might helped to sustain accelerator physics.

In 1936 the University of California officially established the Radiation Laboratory as an independent entity within the Physics Department. This reorganization brought a post of assistant director, to which Lawrence named the indispensable Cooksey, who had been living on his private income; provision for research students; and a



Joseph Hamilton drinking radiosodium, 1939. at right is R. Marshak.



Emilio Segrè in the early 1930s.

F173465

*The reorganized
laboratory was
dedicated to nuclear
science*

26



Lawrence appeared on the cover of *Time* for November 7, 1937, on the occasion of his winning the Comstock Prize of the National Academy of Sciences.



John Lawrence (left) became interested in the biological effects of neutrons during a 1935 visit to Berkeley, and soon joined his brother's team.

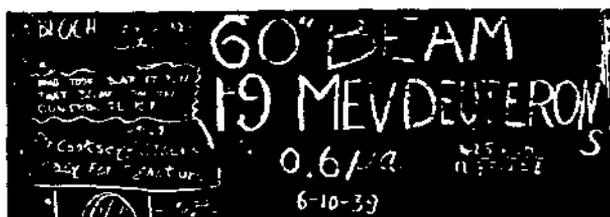
promise of help in raising money for the next cyclotron. The generosity of the University, mediated by Sproul, had been stimulated by an attempt by Harvard to acquire Lawrence as Dean of its School of Engineering.

The reorganized laboratory was dedicated to nuclear science rather than, as in its first incarnation, to accelerator physics. This transformation, as we know, resulted from opportunities opened by the discoveries of artificial radioactivity and the biological action of neutron rays, and also, perhaps, from concern about the effects of the increasingly intensive neutron background on the men who worked around the accelerator. A center for nuclear medicine already existed at the University of California Hospital in San Francisco, where Hamilton and Robert Stone operated the x-ray tube built by Sloan. They were joined by Lawrence's brother John, who had been interested in the biological effects of neutrons during a visit to Berkeley in the summer of 1935. Money for the machine promised Lawrence in 1936 was raised on the ground of its utility in medicine. The Chemical Foundation pledged \$68,000 for a "medical cyclotron," which was to be the special instrument of John Lawrence and his associates. University Regent W. H. Crocker, who had provided for the Sloan tube, gave what was needed to house the new machine; and by the time the 60-inch cyclotron first operated in June, 1939, spitting 16 MeV deuterons a meter and a half through the air, the Rockefeller Foundation and the National Advisory Cancer Council had also contributed.

Glenn Seaborg, who came from G. N. Lewis's College of Chemistry, and Kamen, from the University of Chicago, put the chemistry in nuclear chemistry in 1937. With J. J. Livingood, Philip Abelson, Segré, and McMillan, they determined the nuclear and chemical characteristics of a great many new substances. Kamen soon had responsibility for making the isotopes required in biological investigations. Everyone added to the list, which, by 1940, amounted to about 40 percent of the 335 artificial radioactive isotopes then known.

Accelerator design and improvement also changed in scope in 1936. Until then the staff, mostly physicists, had improvised as they went along, shimming here and there, servicing only at breakdowns (which were often enough), and struggling to maintain a vacuum in the cyclotron tank against the 30 ton forces on its iron lid. Because Lawrence had distrusted rubber gaskets, the vacuum was made tight with sealing wax, which tended to crack at places subject to large mechanical stresses. These recurrent faults, named after their discoverers—"Henry's hole," "Luie's leak," "Art's orifice"—would be inspected when the tank pressure became too high for operation. But often enough hours and sometimes days went into the search. The staff needed its well-known optimism and camaraderie to put up with

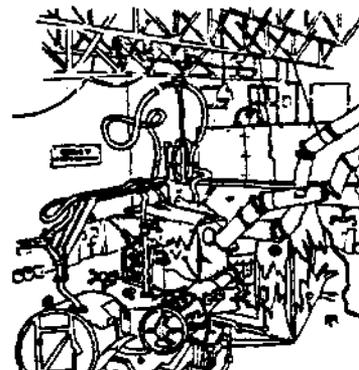
11-73466



The blackboard in the old Radiation Lab recorded many important moments, including the first beam from the 60-inch cyclotron.

the sulks and tantrums of the early machines.

The centerpiece of the Lab was the expanded 27-inch cyclotron. Late in 1936 Lawrence began to enlarge the pole pieces, as he had hoped to do since 1932. Even before the 27-inch was running he thought the yoke could support pole pieces almost 65 percent larger than those planned, but his budget could not stand the additional cost. Also, larger pole pieces and the consequent higher energy required expenditures for cooling systems for the vacuum structure and magnet. In the more comfortable circumstances of the Laboratory in 1936, Lawrence authorized the Pelton Company to increase the gap of the magnet another 2.5 inches and to bolt on peripheral iron rings to extend the pole faces. In September 1937 the new 37-



The California Pelican

*The sulks and
tantrums of the
early machines*

increased, organization of staff and regulation of experiments became necessary. Up through 1936 the machine ran when it could, tended by a two-man crew in two shifts a day. Every Monday a weekly schedule appeared on which the staff indicated the shifts they preferred and the targets they wanted irradiated; crews were composed primarily of the physicists who had built the machine and ran experiments on it. In 1937 the pace increased: an owl shift (11 in the evening to 3 in the morning) started up in May to make phosphorus-32 for John Lawrence's experiments; in July, primarily to meet demands of biological tracer research, the Laboratory began to work around the clock. Slavery to the machine began to irritate, and the good fellowship that characterized Lawrence's enterprise might have cracked like sealing wax had the war not brought other employment and motivation.

Preoccupation with the machine and isotope manufacture may have cost the Laboratory staff important discoveries in nuclear physics. But they did as well as or better than other accelerator laboratories. Their serfdom kept their apparatus in good order, while physicists who attempted to apply cyclotrons to research as soon and as often as possible found their machines frequently in need of repairs they could not readily make. At Berkeley, however, dependable beams



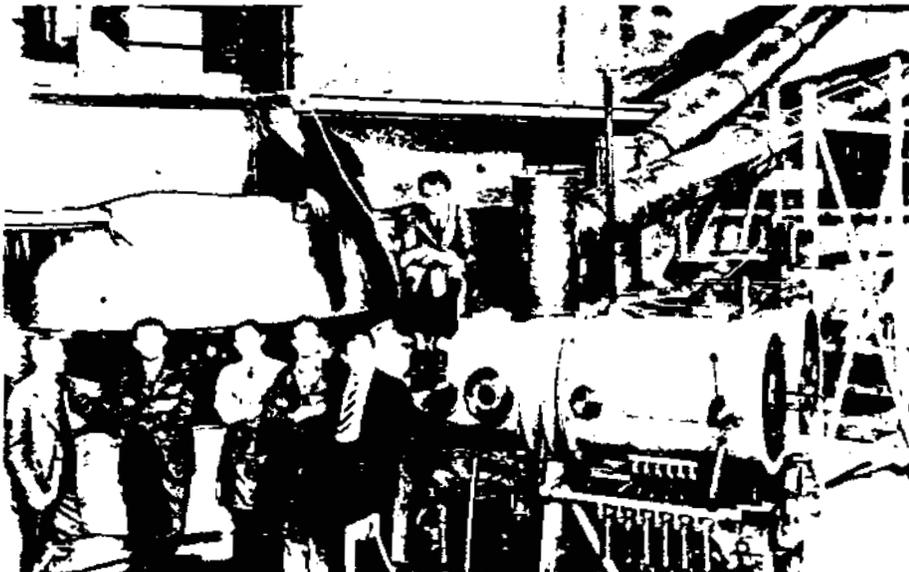
Rad Lab camaraderie found a social outlet at Di Biasi's restaurant in Albany. Back row, left to right, standing: Bob Cornog, Ernest Lawrence, Luis Alvarez, Molly Lawrence, Emilio Segrè; second row: Jerry Alvarez (seated), Betty Thornton, Paul Aebersold (standing), Iva Dee Hiatt, Edwin McMillan, Bill Farley; first row: Donald Cooksey, Robert Thornton, and one unidentified celebrant.

allowed, among much else, McMillan to discover long-lived radio-nuclides; Alvarez and Felix Bloch to measure the magnetic moment of the neutron; Segré to find technetium; and Alvarez and Robert Cornog to discover the stability of helium-3 and the radioactivity of tritium.

The expertise of Lawrence's "boys," as he liked to call them, drew prospective cyclotron designers to Berkeley from around the world. After a time in Lawrence's school they went forth to multiply machines in the Berkeley style. One of the first envoys was Livingstone, who built at Cornell and MIT. Already in 1936 cyclotrons financed by the Research Corporation and the Rockefeller Foundation and designed for physical, chemical, and biological work were under construction at Columbia, Cornell, Princeton, and Purdue, and the Universities of Chicago, Illinois, Michigan, Pennsylvania, and Rochester. British visitors returned from Berkeley to build in Cambridge, Liverpool, and Birmingham. Other machines came into existence at Harvard, Indiana, and Washington University of St. Louis. In Europe, cyclotrons appeared at Bohr's Institute in Copenhagen, at Joliot's in Paris, and at Siegbahn's in Stockholm. In the French style Joliot had tried to go it alone; but he mismanaged his magnet design and needed help from Berkeley to get his protons spinning. Japanese physicists recreated every detail they had seen in Berkeley, down to stop-gaps and jerry-rigging. Almost all the cyclotron laboratories in existence in 1939 had a direct tie to the first and biggest of them all.



Ernest Lawrence at the controls of the 37-inch cyclotron, about 1938.



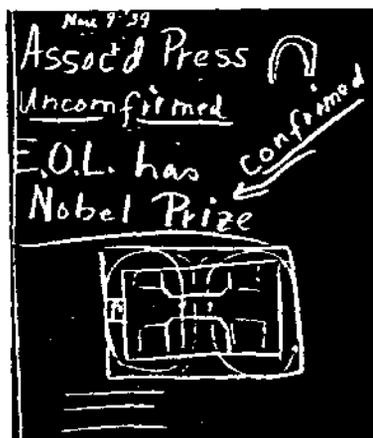
Posing with the newly completed 60-inch cyclotron in the Crocker laboratory are, left to right, D. Cooksey, D. Corson, Lawrence, R. Thornton, J. Backus, and W. Salisbury, and, on top, L. Alvarez and E. McMillan.

*At a time when
Lawrence's school
they went forth to
multiply*

1173469

Physics **3** Deflecting for War

30

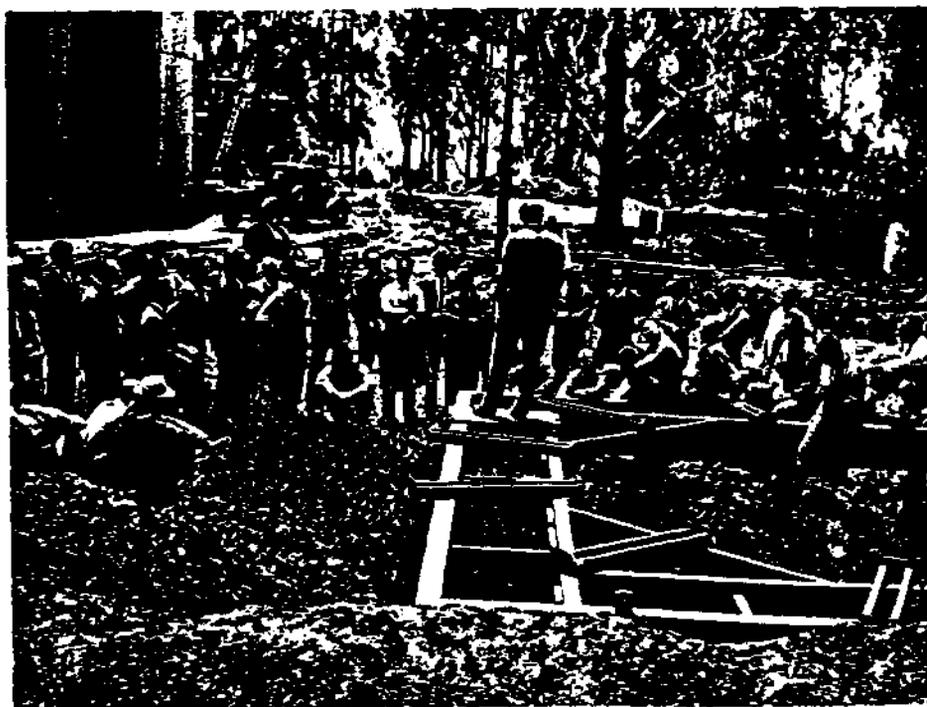


The Lab blackboard announced Lawrence's Nobel Prize.

The mobilization of the Laboratory brought irreversible changes in its size, scope, and corporate life

IN SEPTEMBER 1939, as the Nazis started World War II, Lawrence announced plans for a 100 MeV cyclotron. A tight bond developed between the two events. Fear that German scientists might contrive a bomb on the principle of nuclear fission introduced by Lise Meitner and Otto Frisch in January 1939 provoked a crash program to build one here, and the magnet for Lawrence's new accelerator, completed as a wartime priority, helped to develop the machinery for making the first nuclear explosives. The mobilization of the Laboratory brought irreversible changes in its size, scope, and corporate life. It became the embodiment of big science. Its prewar development had provided a base on which the temporary expansion demanded by the war could not only take place, but take hold.

The award of the Nobel prize in physics to Lawrence in 1939 helped his quest for money for the new machine among his usual sources. The Rockefeller Foundation pledged the principal amount, \$1.4 million, in April 1940. It was to buy a cyclotron with a magnet face 184 inches in diameter. The machine would open the frontier beyond 100 MeV, where there lurked "discoveries of a totally unexpected character and of tremendous importance." Perhaps, Lawrence guessed, the big accelerator might also induce artificial chain reactions and unlock the "vast storehouse of nuclear energy." A more sinister

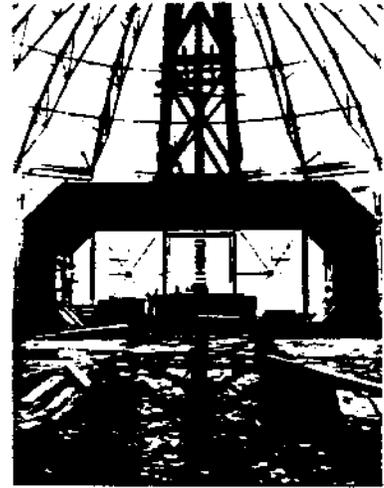


Lawrence encourages Lab workers during World War II.

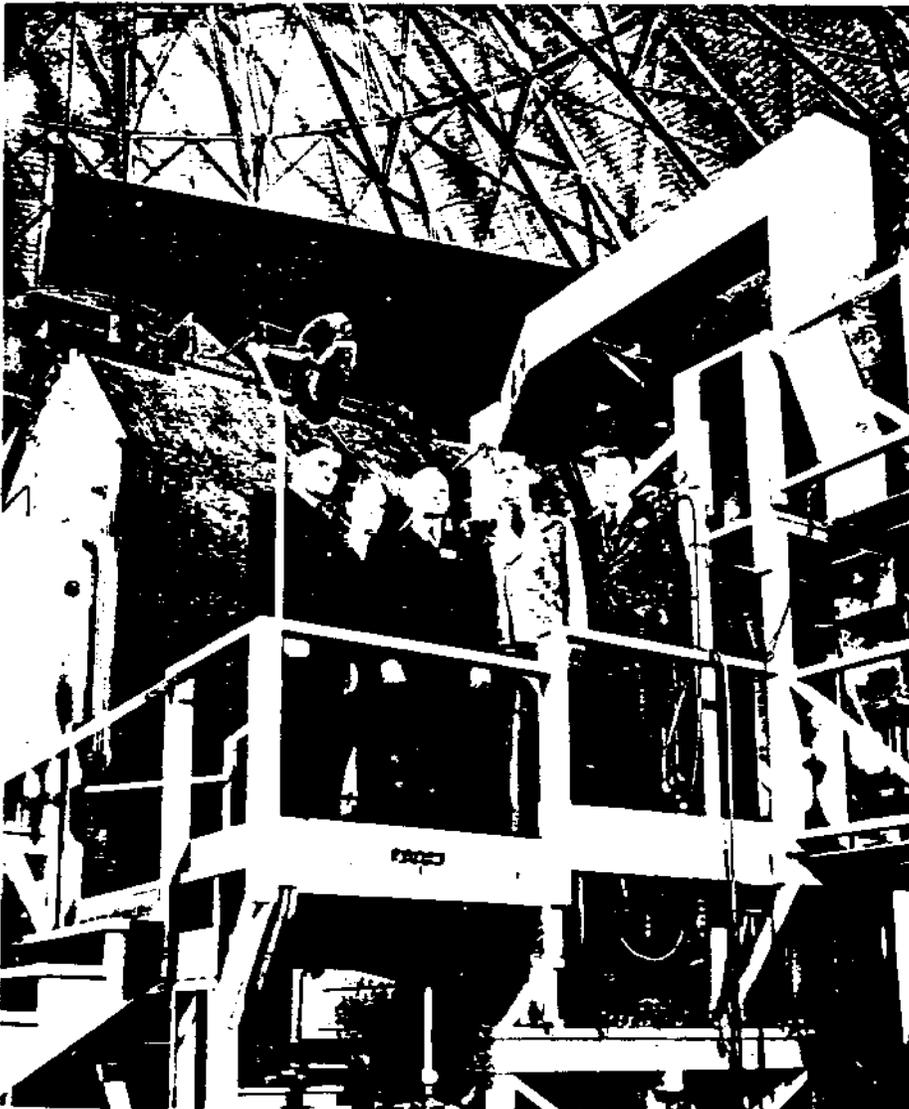
A more sinister connection...

connection or rather non sequitur appeared in *Newsweek's* coverage of the pouring in October 1940 of the thousand tons of concrete on which the accelerator would rest. "Japanese and German researchers are already studying the possibility of [using uranium] for military purposes, while nearly twenty American universities have or are now building cyclotrons."

The 184-inch cyclotron could not conveniently be housed on the campus. Its big concrete pad sat on a hill overlooking the University and the bay, a romantic site to which most of the Laboratory ascended after the war. At first, however, it seemed that little more



The magnet yoke for the 184-inch cyclotron during construction.



Tennessee Eastman officials and General Leslie R. Groves with Lawrence at the magnet for the 184-inch cyclotron in 1943.

S-1 Committee at Bohemian Grove, September 13, 1942. Left to right: Harold C. Urey, Lawrence, James B. Conant, Lyman J. Briggs, E. V. Murphree, and A. H. Compton.



32

The 184-inch magnet rated as a mechanism of warfare

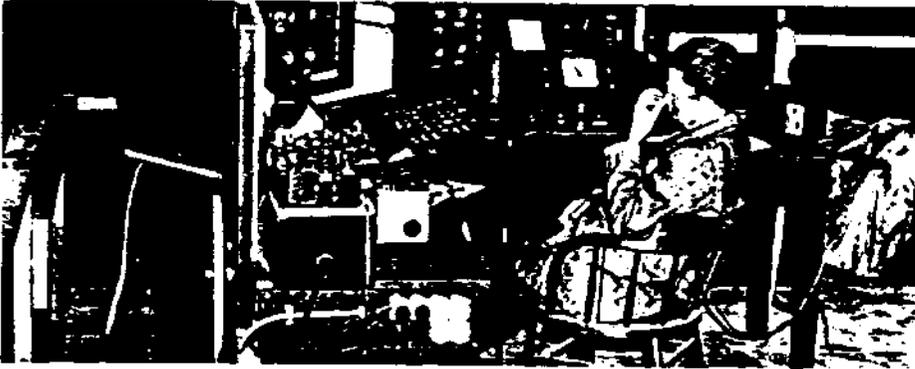
than concrete would be set there. Steel and copper were in short supply. Already in 1940 the nation was preparing for war, stockpiling and rationing strategic material, and surveying its scientific and technical manpower. In the spring of 1941 the Laboratory could not obtain the steel it needed. Lawrence appealed to the Office of Scientific Research and Development (OSRD), formed in June to guide research and development of "the mechanisms and devices of warfare." By January 1942 the Laboratory had an A-1-a priority for steel. The 184-inch magnet rated as a mechanism of warfare.

The magnet was adapted for use in a huge mass spectrograph to test the feasibility of Lawrence's plan to separate the fissile, or explosive, part of natural uranium, U-235, from its much more plentiful companion isotope, U-238. In 1939 A. O. C. Nier of the University of Minnesota had managed to separate a tiny amount of U-235 by mass spectroscopy, but few if any besides Lawrence thought the process would work on an industrial scale. As usual, he pushed his project hard, to the annoyance of Vannevar Bush, the head of the National Defense Research Committee, the forerunner of OSRD: "I made such a nuisance of myself," Lawrence recalled, "that Bush requested the president of the National Academy to appoint a committee to survey the entire uranium problem." It concluded, in November 1941, that Lawrence's method of separating isotopes should be pursued among others. The disclosure by the British that the calculations of Frisch and Rudolf Peierls indicated that "only" a few kilograms of pure U-235 would be needed for a bomb, convinced him that he had a practical method for making nuclear explosives. Here Lawrence left the merely large, like cyclotrons, for the gargantuan. Using conventional mass spectrographs, it would have taken about 25 million years to make the required kilograms. With Laboratory funds he converted the 37-inch cyclotron for a preliminary demonstration; a team under Segré devised a way to measure the enrichment by its radioactive properties; the OSRD contributed \$400,000; and in March 1942 Lawrence had enriched the fissionable isotope in a sample of uranium by a factor of five.

During 1942 the Laboratory rushed the design of ion sources, collecting cups and, above all, magnets, to multiply the separation. Many possibilities were tested with the help of the 184-inch magnet. Lawrence called the final product an "alpha calutron," the Greek letter signifying the first stage of the process and the neologism commemorating its origin in California. Ninety-six calutrons were to be combined into each of five production "racetracks." The magnet controlling each racetrack would consume 100 times the power required by the 184-inch cyclotron.

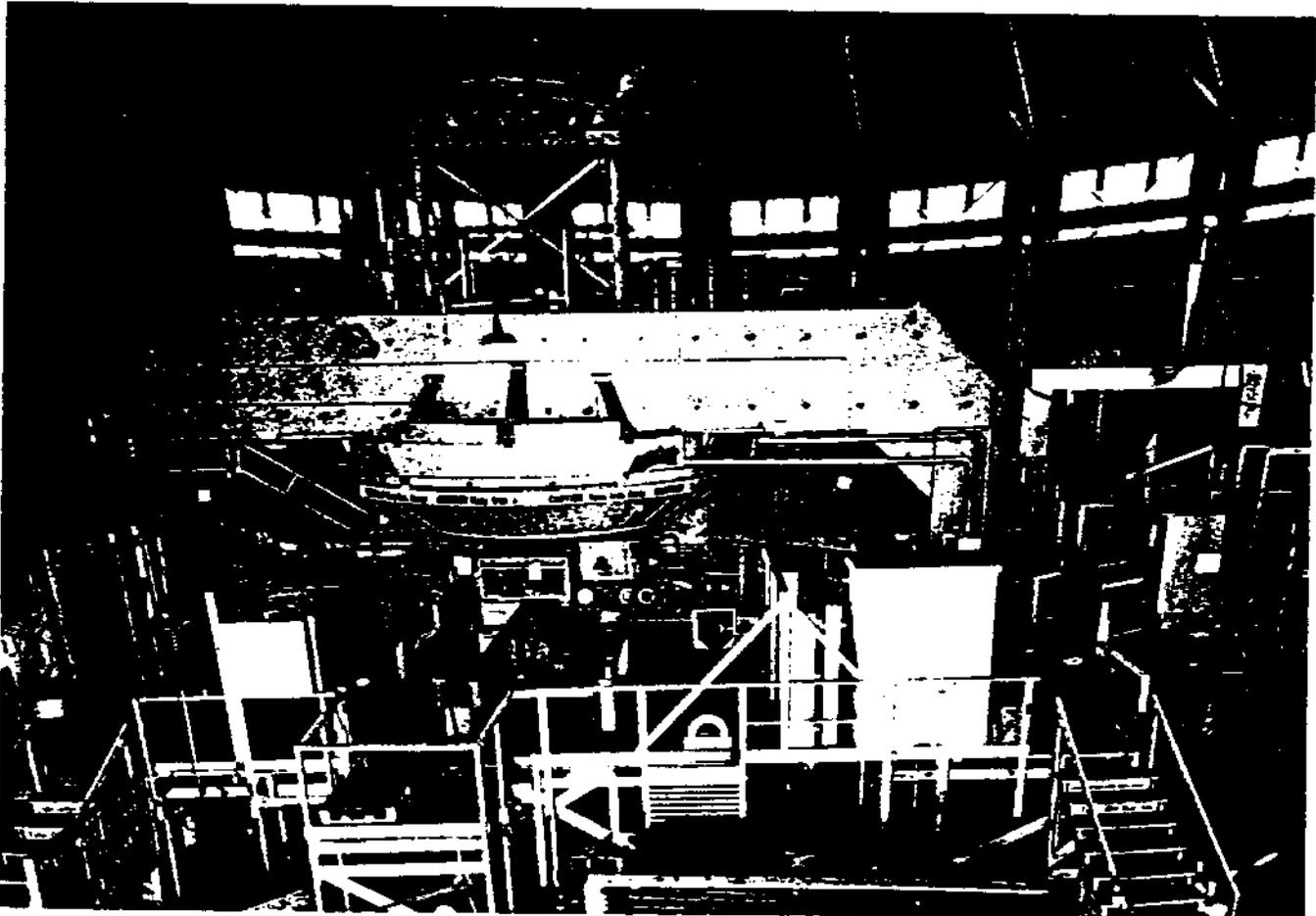
Construction of the huge electromagnetic complex began at Oak Ridge, Tennessee, under the direction of General Leslie R. Groves,

1173472



*As usual he pushed
his project hard*

Lawrence slumps in his chair from fatigue during calutron test.



The magnet of the 184-inch machine testing alpha calutron tanks. To the right is the vertical-pole XA prototype test magnet for isotope separation.

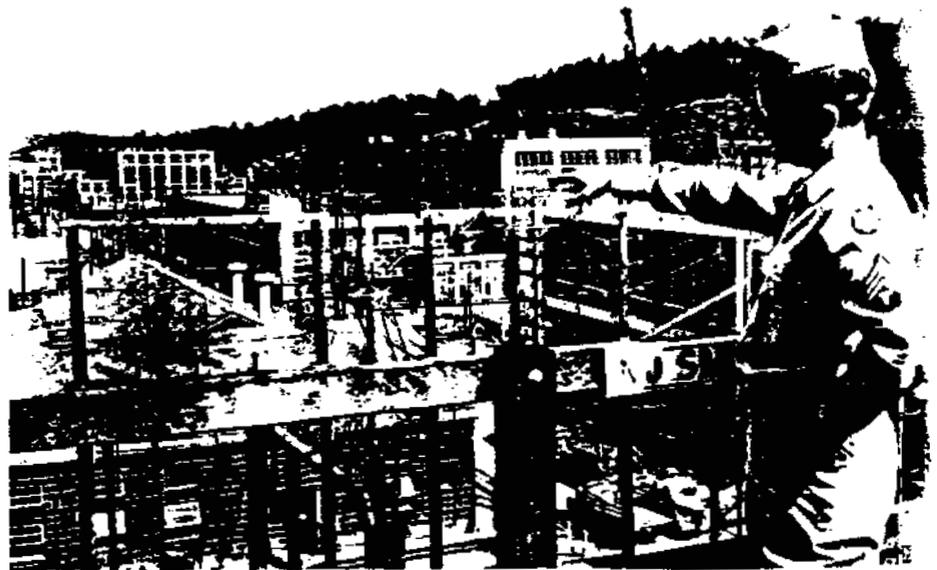
1173473

*No one stopped to
build a pilot plant*

commander of the "Manhattan Engineering District" (MED), set up in 1942 to implement the uranium project. Ground was broken on February 18, 1943. So urgent had the project become that no one stopped to build a pilot plant; the Laboratory had managed to make only a small test section of the great magnet proposed. Staff from Berkeley rushed to Oak Ridge to advise the contractor as construction proceeded. In August the first racetrack began to operate, successfully it was thought; but it soon collapsed, its vacuum leaky, its coils shorted, its tanks warped by its mighty magnet. Meanwhile Oppenheimer reported that a bomb would require three times as much U-235 as forecast. Lawrence and others flew in from Berkeley to diagnose the ailing racetrack, which was dismantled and returned to its manufacturers. The pressure overwhelmed even Lawrence. He spent the end of 1943 in a hospital in Chicago.

During 1944 the alpha calutrons improved and a second generation, called beta, were introduced. The beta calutrons refined further the yield from the alpha type. Nonetheless the total output stayed below expectation. To increase it to usable amounts, the alpha plants were adapted to accept enriched feed from other separation processes. Ultimately nine alpha tracks and six beta tracks operated at O.

Photos on this page: and next courtesy Oak Ridge National Laboratory



The huge electromagnetic separation complex at Oak Ridge, Tennessee, became the heart of a bustling, closely-guarded community.

1173474

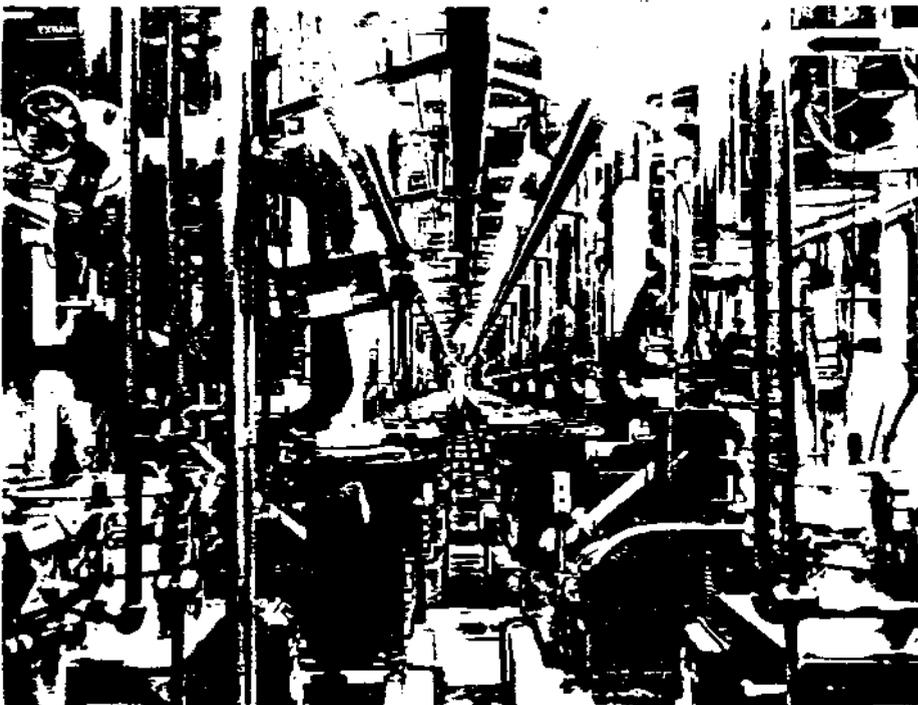
Ridge. Through the beta type passed the uranium for the bomb that destroyed Hiroshima. See page beginning page 20.

Electromagnetic separation of U-235 was not the only road to nuclear explosives that began in Berkeley. There, in 1939 and 1940, studies of fission products brought to light a new element, heavier than uranium, that promised to be as susceptible to a chain reaction as U-235. It happened this way. McMillan directed neutrons created by deuterons from the 37-inch cyclotron through a layer of uranium oxide spread on paper. He was interested to find that two radioactive substances, with half lives of 23 minutes and 2.3 days, remained embedded in the target; since fission fragments should have recoiled out of the paper, he inferred that the new activities came from elements about as heavy as uranium. The 23-minute activity belonged to U-239, which Otto Hahn, Lise Meitner, and Fritz Strassmann had synthesized in 1936. The longer activity was the product of the beta decay of U-239; it turned out to be an isotope of the first trans-uranium element, number 93. At first, however, it appeared to have the chemical properties of the rare earths, which are common fission fragments, and not those of the homologue of rhenium that 93 was expected to be.

McMillan returned to the problem early in 1940 when he used the



The alpha calutrons required constant attention to keep the ion beam current at a maximum.



Underneath each racetrack was a vast vacuum pumping system for the calutron tanks.

1173475

The Calutron

36

THE WORLD DID not lack methods for separating isotopes when it discovered the possible utility of a kilogram of uranium-235 (U-235). Known techniques, pursued simultaneously in Germany and the United States, included ultra-centrifugation, diffusion across thermal or osmotic pressure barriers, and deflection in electric and magnetic fields. The last method appealed to Lawrence, who had made his reputation on the precise control of beams of charged particles. In principle the technique is simple. When passing between the poles of a magnet, a monoenergetic beam of ions of naturally occurring uranium splits into several streams according to their momentum, one per isotope, each characterized by a particular radius of curvature. Collecting cups at the ends of the semicircular trajectories catch the homogeneous streams.

Most physicists in 1941 doubted that electromagnetic separation would succeed in practice because they expected that the mutual repulsion of the like-charged ions would prevent the formation of narrow beams. But Lawrence, who had seen a line of positively-charged ions pour from his cyclotron, guessed that negative particles formed in the air kept the beam from dispersing under its own electrical influence. He had the 37-inch cyclotron modified to demonstrate the feasibility of electromagnetic separation of uranium isotopes using the principle of the mass spectrograph. "It will not be a calamity," he wrote Compton, if uranium turned out to have no military applications; but if "fantastically positive and we fail to get them first, the results for our country may well be a tragic disaster." By December 1941 the uranium ion beam was passing 5 microamperes to the collector; a small amount to be sure, but enough to assure Lawrence that space charge would not be a formidable problem.

The fact that beams of uranium ions could be defined well enough to yield small quantities of isotopes suitable for laboratory research by no means assured that electromagnetic separation could be worked on the industrial scale necessary to make a kilogram of U-235. The process has little to work on, only the very small difference in mass—1.25 percent—between uraniums 235 and 238. Because the lighter ions respond slightly more readily to the magnetic field than the heavier, their trajectories bend in a tighter arc. At the end of their semicir-

cular travel, the ions of U-235 are relatively more plentiful on the inside than on the outside of the beam. But the maximum separation even in the ideal case is small, only one tenth of an inch for an arc with a diameter of 37 inches. Actual beams are far from ideal.

Many technical problems had to be solved before even a prototype could be tested in the field of the nearly-completed 184-inch magnet. The beams, though small, could melt the collectors during long hours of operation; the staff therefore installed water cooling for the collectors and tank liner. They contrived electric arcs to ionize the uranium chloride feed. They devised ways to extract the enriched uranium that collected at the receiver, and the still valuable feed material that condensed along with chloride "gunk" (to use their technical term) all over the inside of the tank. They made scrapers to clean the exit slits of the feed sources regularly lest the accumulated "crud" (another word of art) cut down beam strength. Lawrence's optimistic conclusion: by the fall of 1942 ten "calutrons" (as he called the electromagnetic separator), each with a 100 milliamper source and all operating within the 184-inch field, would produce four grams of enriched uranium a day. The "S-1" committee that oversaw the uranium project for OSRD recommended expending \$12 million to create a plant with 25 times that capacity before the fall of 1943. Lawrence did not doubt that



Schematic diagram of uranium isotope separation in the calutron.

1173476

other means, particularly reactor production of fissile plutonium, might ultimately be the most efficient way to a bomb. But in mid-1942 no reactor worked, and the calutron did.

The calutron design settled on in 1942, called "alpha," provided for enrichment of natural uranium to about 15 percent U-235. Extravagant effort went into designing powerful ion sources and aptly shaped, eventually parabolic collecting slots. The many modifications and security codes proliferated whimsical names: sources Plato, Cyclops, Bicyclops, and Goofy mated with receivers Gloria, Irene, Mona, or Zulu. Ions from Plato and his friends traversed an arc 48 inches in radius to reach collector slits placed 0.6 inch apart. The guiding magnetic field was shimmed not by the old black art but in obedience to calculations. Accurately machined and installed, the shims greatly increased the usable beam that reached the collectors.

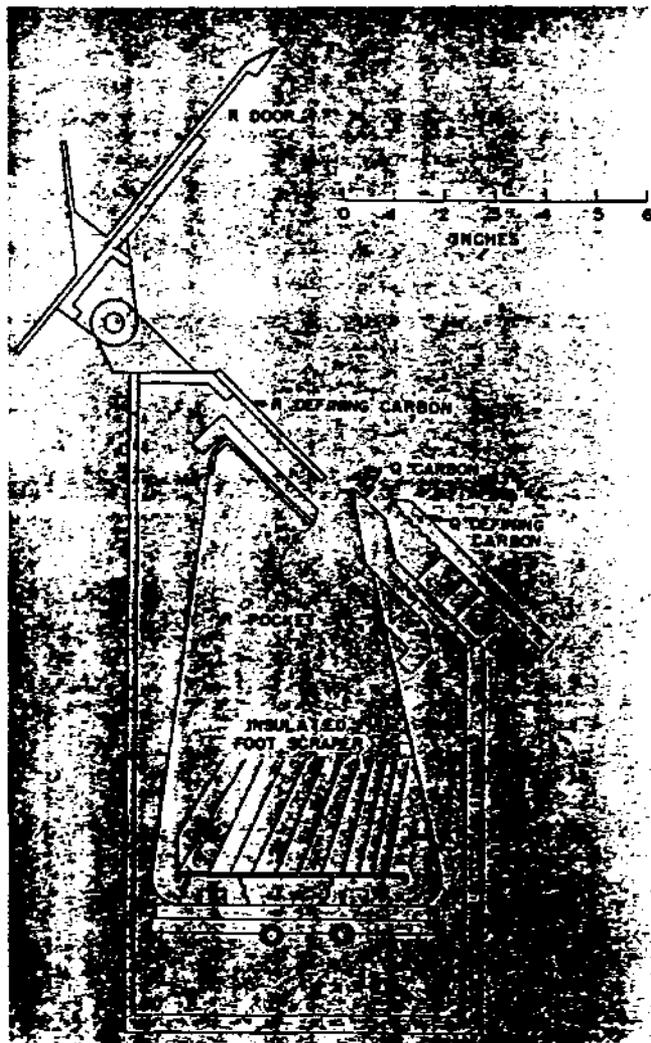
Among results obtained with the 184-inch magnet was a design superior to it for large-scale calutrons, the so-called "XA." The prototype of the magnets to be installed at Oak Ridge, XA was a rectangular, three-coil magnet giving a horizontal field in which the calutron tanks could stand side-by-side. It had room for four alpha tanks, each with a double source. By the spring of 1943, convinced that the Germans might be ahead, Groves decided to skip the scheduled pilot plant; from the XA and a scale model of the production magnet alone would come



Frank Oppenheimer (center right) and Robert Thornton (right) examine the 4-source emitter for the improved alpha calutron.

procedures for alpha operation at Oak Ridge. Tests of the first, full-scale system installed there, the XAX, were scheduled for July.

The spring and early summer of 1943 brought hundreds of trainees to Berkeley from Tennessee-Eastman Company, the operator for the Oak Ridge plant. The Laboratory labored to ensure that the test XA magnet system and alpha units were working by April in spite of delays in delivery of steel. Between April and July the training sessions ran continuously. In June a migration that by 1944 would reach 200

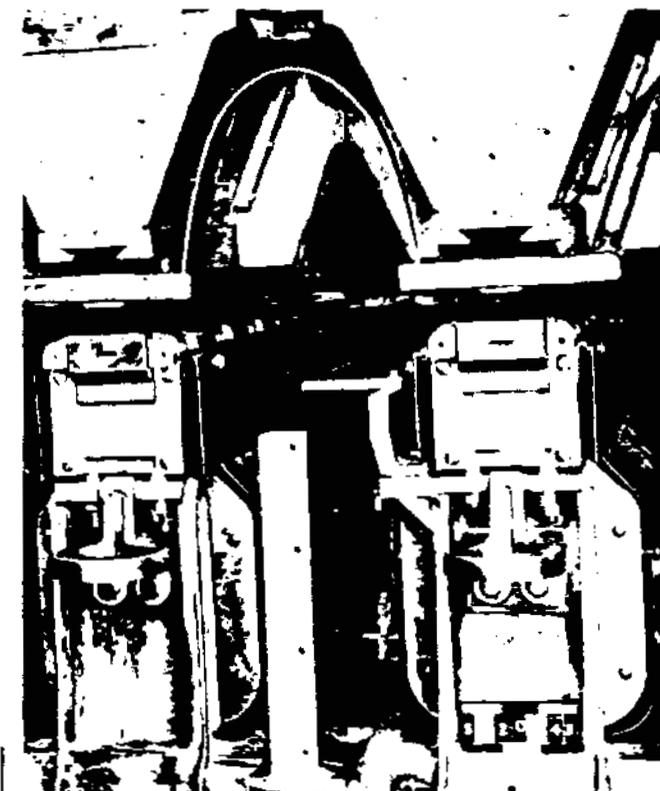


Design of receiver for alpha calutron. Uranium-235 collects in the small pocket between "Q carbon" and "Q-defining carbon."

1173477



Inserting metal shims in an alpha calutron tank to increase output of uranium-235.



Detail of the two ion source pipes of the initial alpha calutron. Top beams extend downward into the funnel-shaped electrode boxes.

started for Oak Ridge. Laboratory expenditures exceeded half a million dollars a month.

The first wave of Berkeley workers at Oak Ridge had to see that the XAX magnet worked. Then runs could begin on the first production system, or "race-track," a 24-fold magnification of the XA that could hold 96 calutron alpha tanks. To minimize magnetic losses and steel consumption, the assembly was curved into an oval 122 feet long, 77 feet wide and 15 feet high. Want of copper for the large coils to produce the magnetic fields prompted a solution possible only in wartime: Groves drafted 14,700 tons of pure silver from a government vault for the purpose. Late in the summer of 1943 the XAX was ready for testing. After a week of difficulty, it cleared the hurdle for full-scale race-track runs.

The first two of five projected race-tracks started up in November and failed from contaminated cooling oil; the second was limping in January, but produced 200 grams of uranium enriched to 12 percent U-235 by the end of February 1944, its fifth of the total goal of one kilogram of enriched uranium per month. By April four race-tracks were functioning, including the repaired number 1. They required constant attention. Many people from the Laboratory helped to modify the units to reach production goals. Responsibility for operation passed entirely to Tennessee Eastman after the spring of 1944, and the Laboratory staff at Oak Ridge turned their attention

1173478



Photo courtesy Oak Ridge National Laboratory

Control panels and operators for calutrons at Oak Ridge. The operators, mostly women, worked in shifts, six to 12 hours a day.

1173479

to redesigning the calutron system for higher efficiency.

Many at the Laboratory, especially Lofgren and Kamen, thought that a second stage would be necessary to reach the required enrichment. Groves approved the idea. In the spring of 1943, during training at Berkeley for alpha operations, design began on the second or beta stage. Because beta would have only the enriched product of alpha as feed, it would process proportionately less material; its beam therefore did not need to be as broad, nor its dimensions as large, as alpha's. Beta design emphasized recovery, not only of the further enriched output but also of the already enriched feed. The first units were tried at Oak Ridge in late February 1944, but the sources had to be redesigned, and even by June difficulties persisted in recovering the precious beta feed strewn throughout the calutron. Process efficiencies stayed low: only 4 or 5 percent of the U-235 in the feed ended up in the output. A better source of enriched uranium feed

would have to be found to create the 10 kilograms or so of 90 percent U-235 that Oppenheimer thought necessary for a bomb.

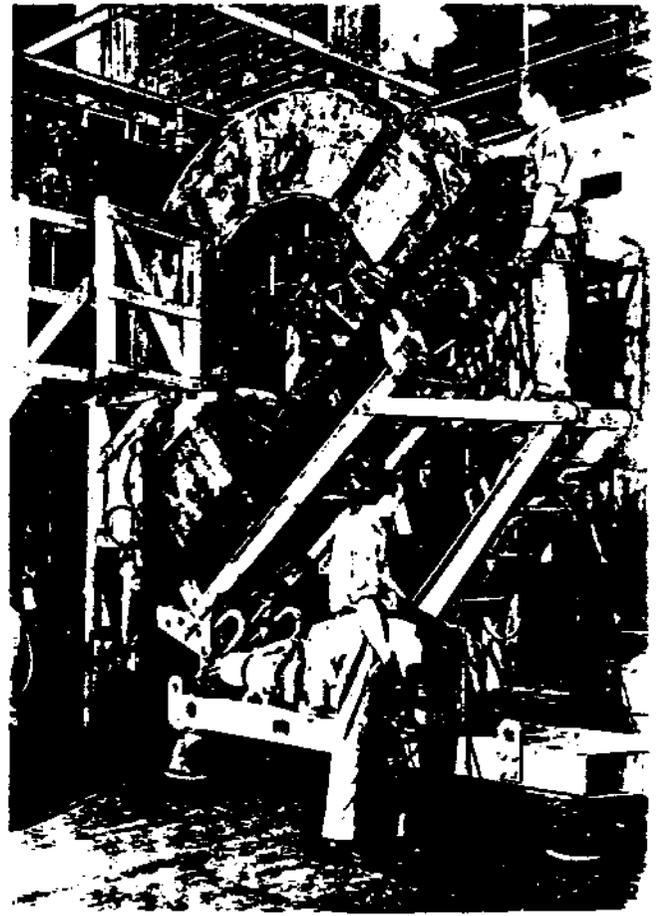
The gaseous diffusion procedure for separation of uranium isotopes, which had consumed more money even than the calutron, had not met its design goals by late 1944. Groves decided that it could not be counted on to produce high enrichment, and that whatever it did produce would have to be supplemented with other slightly enriched uranium and processed through beta calutrons. To augment the calutron feed, the MED constructed still another plant at Oak Ridge, this one working by thermal diffusion, a method developed by Abelson.

In the critical production period in the first months

Photos courtesy Oak Ridge National Laboratory

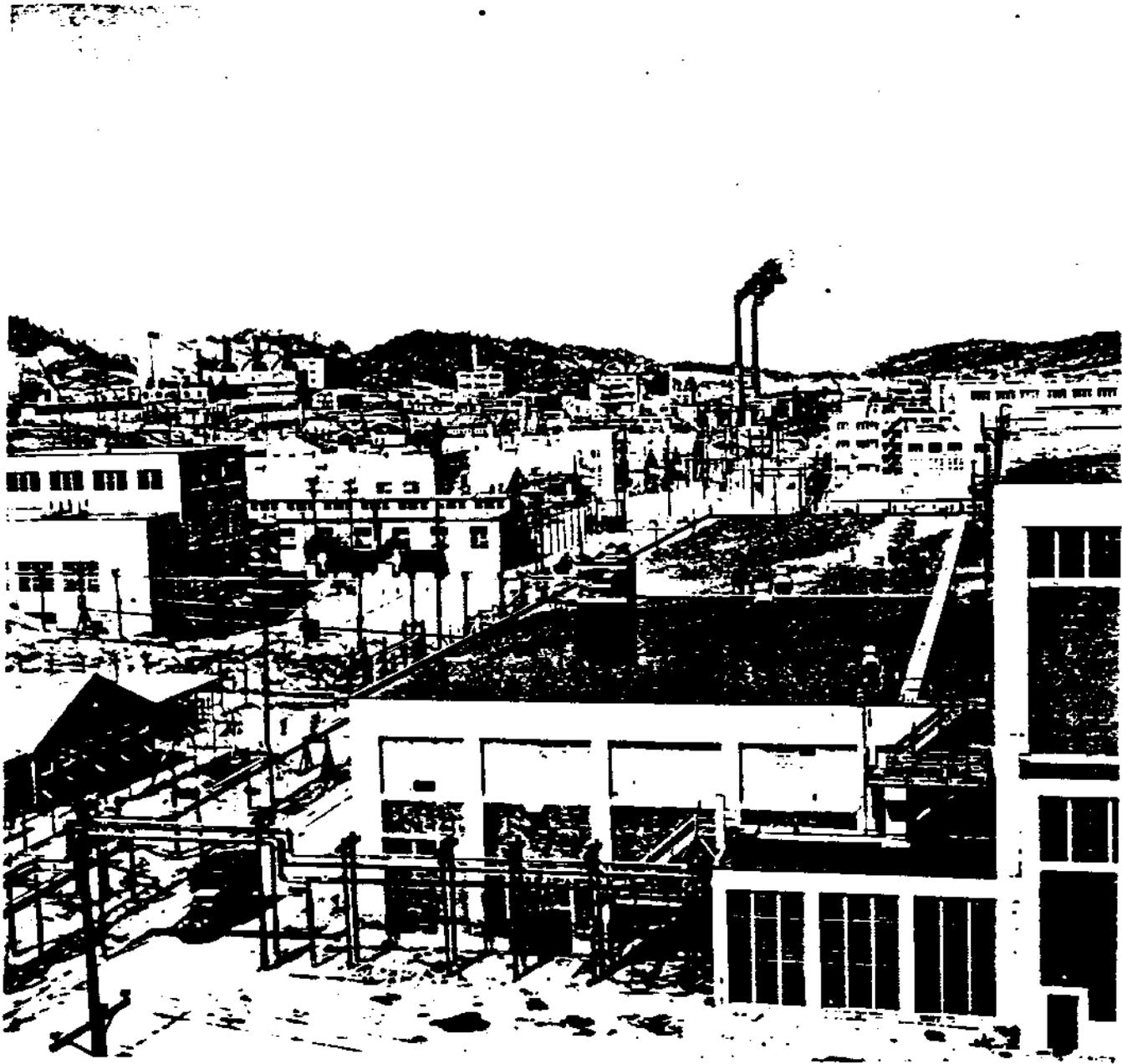


A vast bank of diffusion vacuum pumps operated underneath the alpha calutron racetrack to free the tanks of air.



The "C" shaped alpha calutron tank, together with emitters and collectors on the lower-edge door, was removed in a special "drydock" from the magnet for recovery of uranium-235.

1173400



An early view of the facilities at Oak Ridge; hundreds of workers were trained at Berkeley for the Tennessee-Eastman Company, operator of the Oak Ridge plant.

of 1945, the calutrons, particularly the six betas of 36 tanks each, produced weapons-grade U-235 using feed from the modified alpha calutrons, the small output from the gaseous diffusion plant, and whatever the new thermal process had to offer. Virtually

all the U-235 sent by courier on the train to Chicago and on to Los Alamos had passed through the beta calutrons. From these shipments Oppenheimer's physicists assembled the bomb that was to destroy Hiroshima.

He boarded the train for Chicago with the world's supply of plutonium in his briefcase

Photo courtesy Oakland Tribune



McMillan recreating the search for neptunium at the time of the announcement of the discovery, June 8, 1940.



Glenn Seaborg adjusts a Geiger-Müller counter during search for plutonium at the Laboratory.

16-MeV deuteron beam of the 60-inch cyclotron to produce the 2.3-day activity. It still did not behave as a fission product, nor, as close inspection disclosed, as a typical rare earth. Philip Abelson, who had been searching for the same activity in uranium samples at the Carnegie Institution of Washington, where he had gone to set up a cyclotron, came on a visit to Berkeley and joined forces with McMillan. They showed that the activity grew from U-239 and that its chemistry resembled uranium's. The resemblance had protected it from detection by investigators who expected something similar to rhenium. No one had suspected, as McMillan and Abelson now did, that there existed a "second 'rare earth' group of similar elements." McMillan named the new element neptunium after the planet next beyond Uranus, and noticed (after Abelson's return to Washington) that it has a descendent that emits alpha particles. Before he could determine its chemistry, however, he went to MIT to help develop radar, the war technology then most pressing. With McMillan's consent, Seaborg picked up the work on the alpha emitter, element 94. They were to share the Nobel prize in chemistry in 1951 for their discoveries of the first transuranic elements.

The new element, called plutonium on McMillan's principle of nomenclature, proved elusive. The first isotope identified was not McMillan's alpha emitter but Pu-238, a shorter-lived decay product of neptunium made by irradiating uranium-238 with deuterium in the cyclotron. The discoverers, Seaborg, McMillan, J. W. Kennedy, and A. C. Wahl, learned enough about plutonium chemistry to know how to concentrate McMillan's alpha emitter (Pu-239). In May 1941 Kennedy, Seaborg, Segrè, and Wahl succeeded in doing so and also established the new isotope's fissionability. It appeared that in sufficient quantities plutonium-239 might sustain an explosive chain reaction. After Pearl Harbor, the OSRD authorized Lawrence to continue plutonium studies at Berkeley and Arthur Compton to supervise the work toward a controlled, self-sustaining, plutonium-producing chain reaction that had been started by Fermi at Columbia and moved to Chicago. In March 1942 Seaborg was asked to join Compton and Fermi to develop chemical processes to separate plutonium after production. On April 17 he boarded the train for Chicago with the world's supply of plutonium in his briefcase.

Seaborg's move did not put an end to work on plutonium in Berkeley. Wahl, for example, worked on the lanthanum-fluoride process that Seaborg used to isolate the first weighable amount of plutonium in the summer of 1942. The Dean of the College of Chemistry, Wendell Latimer, supervised the work and began investigations of the effects of heat upon materials to be used in the plutonium production piles. In work parallel to Latimer's, Hamilton's group at the 60-inch cyclotron examined the effects of fast neutrons on the gra-

1173482



The Trinity test, first man-made nuclear explosion, Alamogordo, New Mexico, July 16, 1945.

phite moderator provided for the reactor. The 60-inch also prepared plutonium for the research in Chicago. In July 1944 it shut down after 20,000 continuous hours of operation for decontamination and overhaul. The machine designed to serve Asclepius had exhausted itself for Mars.

Groves decided to build a production plant for plutonium shortly after Fermi ignited and controlled a chain reaction in December 1942. Like the alpha racetracks and the diffusion plants at Oak Ridge, the manufacturing piles and chemical treatment facilities at Hanford, Washington, were built without benefit of a full-scale pilot plant. And, again like the Oak Ridge complex, Hanford delivered enough fissile product to fill a nuclear bomb by June 1945. Two practical designs for a weapon then existed, one of which—using U-235 as ingredient—seemed secure enough not to require expenditure of the precious material in a test. The other, using plutonium, had a complex and problematic explosive trigger. No fault could be found with it when the first nuclear explosion released by man lit up the sky above New Mexico on July 16, 1945. A similar performance demolished Nagasaki a few weeks later.

Lawrence attended the desert test, code named Trinity, at which he felt no sin, remorse, or dread, as others since have thought they did, but rather relief that the thing worked. A few weeks before Trinity he, Fermi, Oppenheimer, and Compton had advised the Secretary of War, Henry Stimson, on the use of the new weapon. Lawrence preferred a demonstration before Japanese representatives to immediate use against a populated center. After further consideration, he changed his mind to agree with his fellow advisers that only application without advertisement would guarantee prompt surrender and a great saving in American lives. When the Emperor of Japan surrendered unconditionally on August 15, 1945, the Laboratory could rejoice that it had helped to end the war in the Pacific.

The mobilization had extended throughout the Laboratory, to nuclear medicine as well as to nuclear physics and chemistry. Hamilton and his colleagues had studied the physiological effects of fission products for the OSRD. John Lawrence and his associates in the new Donner Laboratory had examined biological consequences of high-altitude flying. Using radioactive isotopes of inert gases, they had penetrated the secrets of decompression sickness and other maladies, and tested 1500 persons in a low-pressure chamber simulating high altitudes. Tracer studies at the Laboratory had brought fundamental contributions to the understanding of the circulation and diffusion of gases and practical devices like oxygen equipment, a parachute-opener, and methods to measure the rate of circulation and perfusion of the blood by capillaries.

Before the war Lawrence's associates had learned to work together



First plutonium sample used to determine its fission properties in March, 1941.



Twenty micrograms of pure plutonium hydroxide in capillary tube, September 1942.

*At Trinity,
Lawrence felt no
sin, remorse, or
dread, but rather
relief that the thing
worked*

*During the war
formality and
hierarchy entered
the Laboratory*

44



Lawrence challenged by Laboratory security guard at wartime Laboratory.



J. Robert Oppenheimer

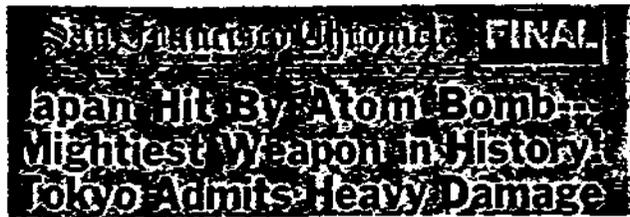
to keep their machines producing radioisotopes, therapeutic rays, and beams for biological, physical, and chemical research. During the war formality and hierarchy entered, apparently for the first time on paper in an organization chart composed in June 1942. Three committees advised Lawrence on planning, production, and operation of calutrons. Wallace Reynolds managed the Laboratory, a job now spun off from Donald Cooksey's office. Brobeck ran an engineering department. Laboratory shops became the production section. Thornton headed research and development. Purchasing, auditing, and personnel divisions appeared. To coordinate the newly specialized functions of the Laboratory, Lawrence set up a process engineering committee in November 1942 and charged its members with the "responsibility for seeing that the ball is carried in the right direction and with all speed." They divided responsibility for design of calutron components from the central magnet (Brobeck) to peripheral "gunk catching, cleaning and recovery" assigned to E. J. Lofgren, who interrupted his graduate studies to come to the Laboratory during the war.

The charts grew larger and the committees proliferated as the Laboratory and the war effort expanded. By May 1, 1943 the staff numbered 826 plus 65 guards. The total rose to nearly 1200 in June 1944, including a small British force led by M. L. E. Oliphant. Before the war the staff of the Laboratory could be photographed within the yoke of the 60-inch magnet. Now their individual photos filled books



Machine shop crew at the Laboratory during World War II.

1173484



Newspaper headlines on August 7, 1945, revealed to the Bay Area public for the first time that the laboratory had played a crucial role in the war effort.

of pass records maintained by an increasingly obtrusive security service. Organization of another kind also came to the Laboratory during the war. Oppenheimer tried to set up a local of the Association of Scientific Workers; Lawrence counseled physicists not to join it. A more telling recruitment, conducted by the Federation of Architects, Engineers, and Technicians, prospered when forced transfers to Oak Ridge began in 1943. Known communists led the Federation. Security officers arranged to draft some organizers and to fire others.

Bomb design demanded a higher order of security. A remote and isolated laboratory was set up at Los Alamos under Oppenheimer. Groves hoped to compartmentalize the study of the weapon as he had its production facilities. As if physics had been declared a crime and its leading perpetrators imprisoned, Los Alamos grew until in 1945 it held more than 2000 of the nation's physicists. They included many from the Laboratory, among others Alvarez, Lofgren, McMillan, Segré, Serber, and Wilson.

The surrender of Japan ended the emergency that had created Los Alamos but not the large organization and tight security that had come to characterize nuclear science. Elaborate classification schemes, supervision by government bureaucracies, and depersonalized administrative hierarchies remained at the Laboratory as at Los Alamos, Argonne, Oak Ridge, and Hanford. Otherwise the terms of parole were most generous. The methods and resources of big science, enlarged by the war, were to dominate the study of nuclear physics in the peace.

The terms of parole were most generous

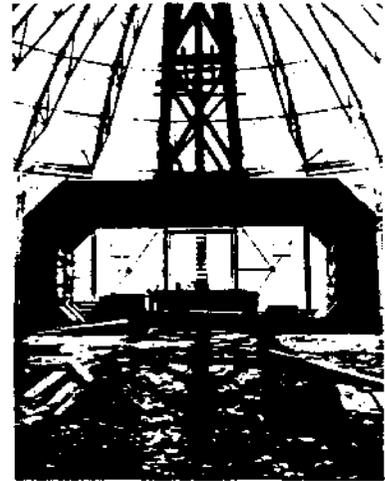


General Groves and UC President Sproul admire the Medal for Merit awarded Lawrence in March, 1946 for wartime achievements of the Laboratory.

A more sinister connection . . .

connection or rather non sequitur appeared in *Newsweek's* coverage of the pouring in October 1940 of the thousand tons of concrete on which the accelerator would rest. "Japanese and German researchers are already studying the possibility of [using uranium] for military purposes, while nearly twenty American universities have or are now building cyclotrons."

The 184-inch cyclotron could not conveniently be housed on the campus. Its big concrete pad sat on a hill overlooking the University and the bay, a romantic site to which most of the Laboratory ascended after the war. At first, however, it seemed that little more



The magnet yoke for the 184-inch cyclotron during construction.

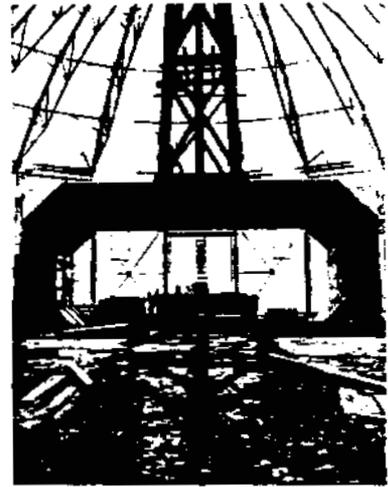


Tennessee Eastman officials and General Leslie R. Groves with Lawrence at the magnet for the 184-inch cyclotron in 1943.

A more sinister connection...

connection or rather non sequitur appeared in *Newsweek's* coverage of the pouring in October 1940 of the thousand tons of concrete on which the accelerator would rest. "Japanese and German researchers are already studying the possibility of [using uranium] for military purposes, while nearly twenty American universities have or are now building cyclotrons."

The 184-inch cyclotron could not conveniently be housed on the campus. Its big concrete pad sat on a hill overlooking the University and the bay, a romantic site to which most of the Laboratory ascended after the war. At first, however, it seemed that little more



The magnet yoke for the 184-inch cyclotron during construction.



Tennessee Eastman officials and General Leslie R. Groves with Lawrence at the magnet for the 184-inch cyclotron in 1943.

1173487

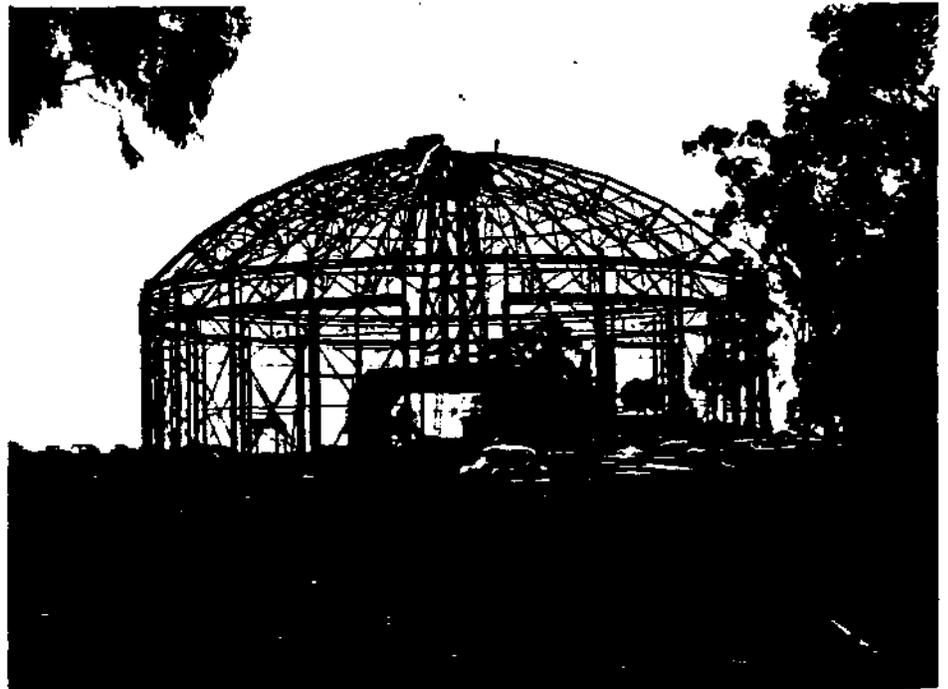
Demobilized **4** Physics

46

*The new compact
between science and
government*

THE MILITARY SERVICE of the Laboratory and the reputation and resourcefulness of its director insured that Lawrence would be a leader in the new compact between science and government struck during the war. We have a measure of the effect of the success of the Manhattan Engineering District on these arrangements in the form of plans for the future that Lawrence drew up in 1944 and revised in 1945. In the earlier plan he assumed that the Laboratory would continue as a division of the University's physics department and proposed the establishment of a second division for medical physics to accommodate John Lawrence and his colleagues in the Crocker and Donner laboratories. He expected to have a small permanent staff of scientists and technicians, and to reimplement the frugal policy of allowing visitors and students to do much of the work. As for expenses, he thought to make do with \$85,000 a year and some war surplus: "a considerable proportion of the [wartime] laboratory equipment, particularly supplies and machine tools, [might] be kept available either through gift from the Federal Government or purchase by the University."

By the time of the plan of 1945, however, Lawrence knew that science would be both honorably discharged and held in ready reserve

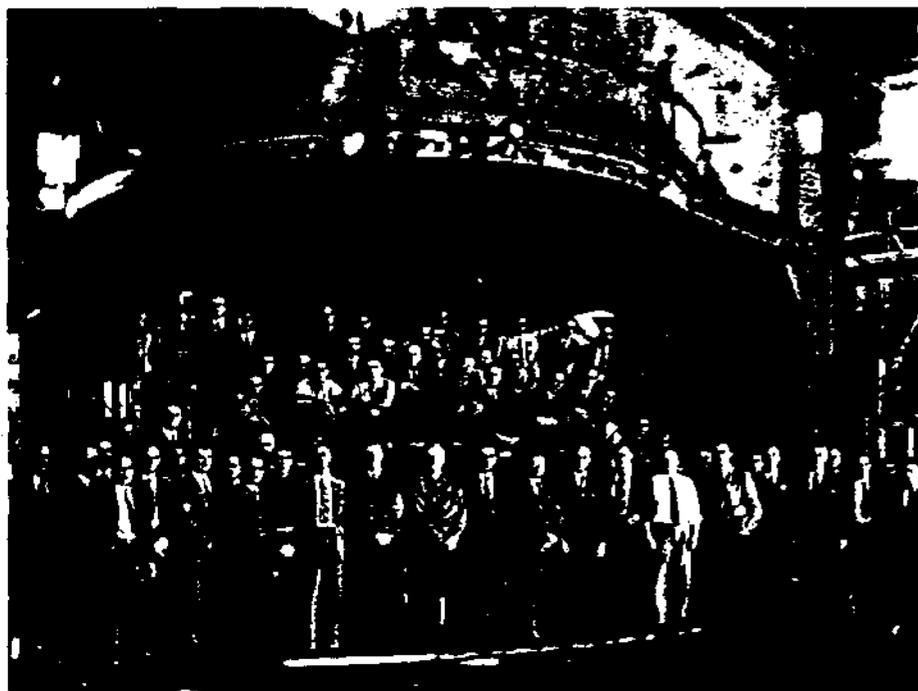


The magnet yoke for the 184-inch cyclotron was set in place and the building erected around it.

for the national defense and welfare. The increasing production of uranium-235 and plutonium meant that high status in national councils rather than unending Congressional investigations would reward the Manhattan Project. Four months before Trinity, Lawrence wrote the MED offering to accept \$7 to \$10 million for the Laboratory's first year of postwar operation, a hundred-fold increase over the budget he had estimated the year before. After Trinity, which confirmed his confidence in dealing with Groves as it did Truman's in negotiating with Stalin, he set the postwar laboratory staff at 239, including 66 scientists. Alvarez and McMillan, returning from Los Alamos, had plans for new accelerators that would exploit wartime technology and surplus materiel worth millions. The entrepreneurs who had attracted the nation's philanthropic wealth to nuclear science before the war turned to the new public provider.

Groves admired Lawrence's drive and confidence, and the MED generously supported the Rad Lab's conversion to peace-time research. "[It is] in the best interest of the government," Groves said, and authorized the completion of the 184-inch synchrocyclotron and the construction of an electron synchrotron, both of which used a concept that McMillan had developed towards the war's end. The

*Honorably
discharged, but held
in ready reserve*



In 1945-46, the 184-inch was converted from a calutron to a synchrocyclotron; Lawrence and staff posed with the magnet.

1173480

*AEC research policy
was shaped to
assure the future of
fundamental nuclear
science*

completion of the 184-inch synchrocyclotron cost the District \$170,000, the construction of the electron synchrotron \$230,000 in cash plus \$203,000 in surplus capacitors from Oak Ridge. Alvarez got support for preliminary work on a linear accelerator designed to produce 2,000 MeV protons and estimated to cost \$5.5 million in all, including 750 surplus radar generators valued at \$1.5 million. Seaborg, returning from Chicago to direct nuclear chemistry, had \$75,000 for a "Hot Lab" for research on radioactive isotopes. The staff exceeded Lawrence's highest estimates. In February 1946, numbered 479, with a monthly payroll of \$194,000, the semiannual budget from the MED amounted to \$1,370,000. When University Regent John Francis Neylan complained that local contributions to the atomic bomb had not been adequately appreciated, Lawrence showed him Groves' gift of radar apparatus as an indication of the kind and quantity of credit the Laboratory was getting.

Groves could not guarantee support of the Laboratory after January 1, 1947, when the Atomic Energy Commission (AEC) took charge of the nuclear energy program. Lawrence had supported the May Johnson bill, which would have created a Commission dominated by the friendly military. He withdrew from the debate when man



Nuclear chemistry prospered in the postwar era, with the discovery of several new elements by the team including Seaborg (left) and Albert Ghiorso

*Lawrence's
establishment
prospered under the
new regime*

nuclear scientists lined up behind the McMahon bill, which provided instead for a civilian commission under Presidential control. Once the McMahon bill was approved and the Commission appointed, Lawrence took steps to insure that the agency would favor the kind of research he thought important.

The AEC formulated its research policy in 1947 in a series of meetings, the most important of which Lawrence arranged at Bohemian Grove in August. There Oppenheimer, chairman of the AEC's General Advisory Committee, called for broad and strong support for basic scientific research. Lawrence, better at personal negotiation than at speech-making, took AEC Chairman David Lilienthal on a four-day trip in the coastal mountains before the meeting. Congressional rejection of Vannevar Bush's plan for a National Science Foundation had already inclined the commissioners to support fundamental research under the AEC; and after the meeting and eating at the Grove, the one strong opponent, the Commission's director of research James Fisk, who had opposed the financing of accelerators by the agency, conceded the necessity. In October 1947 the AEC appropriated \$15 million for atom smashers. Prohibited by the Atomic Energy Act of 1947 from giving grants for research, the agency developed a system of contracts with universities and set up an independent Division of Research to administer them. To complete the loop, Kenneth Pitzer, professor of chemistry at Berkeley, succeeded Fisk. By the end of 1948 AEC research policy had been shaped to assure the future of fundamental nuclear science.

All the main components of Lawrence's interdisciplinary establishment prospered under the new regime of peacetime financial support for scientific research. In the "Hot Lab," the most prominent locus of nuclear chemistry at the Laboratory, Seaborg, Albert Ghiorso, James Kennedy, B. B. Cunningham, and others elaborated the rich and varied chemical properties of the actinide elements. They continued work begun during the war at Chicago where their identification of americium (element 95) and curium (96) among the products of plutonium bombarded in the Berkeley and St. Louis cyclotrons confirmed the actinide concept, the existence of a series of heavy homologues of the rare earth elements. After their return to Berkeley Seaborg and his associates synthesized additional members of the series, berkelium (97), californium (98), and mendelevium (101), in the 60-inch cyclotron.

The Donner Laboratory, at first of interest to AEC for its studies of the physiological effects of fissile materials and their products, soon won federal support for continuation of its prewar work in medical diagnosis, instrumentation, and therapy. An example is the treatment of acromegaly and Cushing's disease with beams of charged particles, initiated by John Lawrence and Cornelius Tobias. Other



The triangle in the glass tube contains the world's first sample of americium, produced in the 60-inch cyclotron in 1944.



Newspaper headlines announced the discovery of another new element, berkelium.



John Gofman initiated studies that led to the understanding of the effects of lipoproteins on cardiovascular disease.

One of the new areas was the study of organic compounds labeled with carbon-14

work, like that leading to the discovery of the lipoproteins and their effects on cardiovascular disease by John Gofman, Frank Lindgren, and their collaborators, brought the Laboratory into entirely new areas.

One of the new areas, cultivated both in Donner and the Old Radiation Laboratory, was the study of organic compounds labeled with carbon-14. Melvin Calvin took charge of this work at the end of the war in order to provide raw materials for John Lawrence's researches and for his own study of photosynthesis. Using carbon-14, available in plenty from Hanford reactors, and the new techniques of ion exchange, paper chromatography, and radioautography, Calvin and his many associates mapped the complete path of carbon in photosynthesis. The accomplishment brought him the Nobel prize in chemistry in 1961. About that time his interdisciplinary bio-organic chemistry group became the Division of Chemical Biodynamics and obtained a new building on campus, recently renamed the Melvin Calvin Laboratory, in which to pursue their work.

Similar institutional growth began from Latimer's wartime investigations of the behavior of reactor materials at high temperature. His successor as Dean of the College of Chemistry and head of the Laboratory's general chemistry program, Pitzer, proposed in 1959 to expand these studies to the investigation of novel materials for



Melvin Calvin shown with some of the apparatus he used to study the role of carbon in photosynthesis.

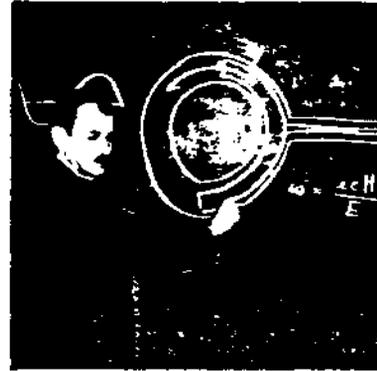
*Experience and
opportunity in
physics*

51

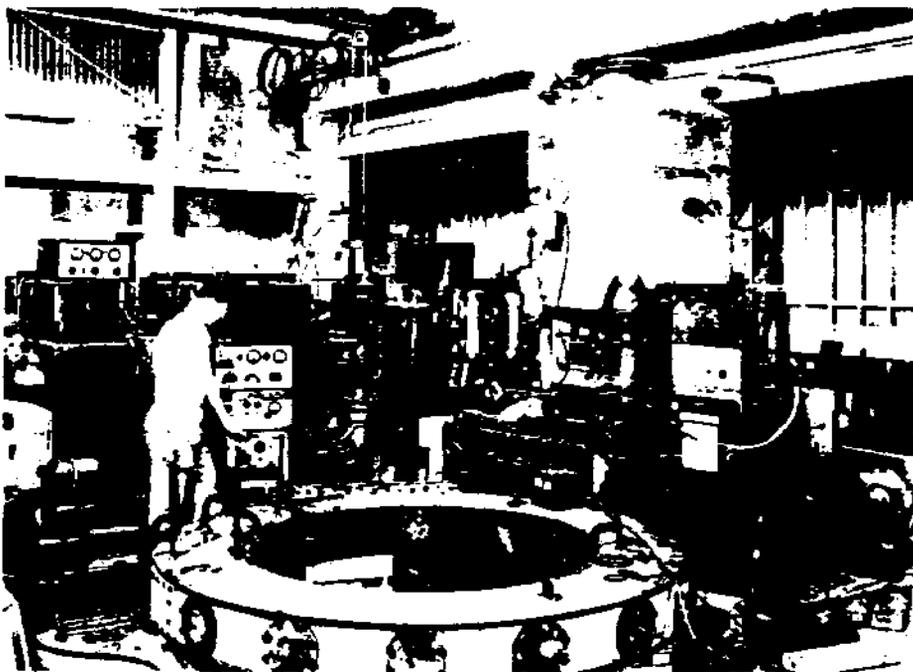
applications to space exploration and other new technologies. The AEC agreed, and Latimer's old chemistry group became the nucleus around which several other campus programs collected to form the Inorganic Materials Research Division.

Although the bases for major programs in materials research and chemical biodynamics were set soon after the war as nuclear medicine and chemistry resumed their growth, experience and opportunity in physics determined the directions of most Laboratory effort from 1945 to the mid-1960s. Alvarez remarked in 1947 that history had apparently repeated itself. Then, as in the early thirties, the Laboratory was simultaneously investigating several machines: the 184-inch cyclotron, the electron synchrotron, the linear accelerator, and a 10-BeV proton synchrotron. Only the new Brookhaven National Laboratory, organized in the Northeast by fourteen universities to provide a counterweight to Berkeley in nuclear research, had an accelerator program nearly as ambitious.

The research most characteristic of the Laboratory exploited the then unrivaled beam of the synchrotron, as McMillan named machines built on his principle of phase stability. In a conventional cyclotron the relativistic mass increase ultimately shuts off acceleration: the particles fall progressively out of phase with the radiofrequency field until they reach the gap between the dees as the field



The principle of phase stability—basic to the 184-inch cyclotron and the electron synchrotron and their successors—is explained by its author Edwin McMillan.



The completed ring-shaped non-metallic vacuum chamber for McMillan's electron synchrotron, ca. 1947.



Vannevar Bush (left) and Arthur H. Compton at Del Monte Lodge, 1948.



The 184-inch cyclotron operated for the first time on November 1, 1946. In the foreground, left to right, are Thornton, Lawrence, McMillan, and James Vale.

*Just before midnight
on November 1,
1946*

there drops to zero. Thereafter they will be decelerated. As McMillan (and, independently, the Soviet physicist V. I. Veksler) showed, a net acceleration might be achieved by decreasing the oscillator frequency without changing the magnetic field (the principle of the synchrocyclotron) or by changing both frequency and field so that the accelerated particles describe a path of constant radius (the proton synchrotron). In the case of relativistic electrons, only the magnetic field need be altered (the electron synchrotron).

and study of mesons, rather than the manufacture of exotic isotopes or the multiplication of information about scattering, that became the prime achievement and justification of the new machine. Once again, however, the key discovery came not from Berkeley or an accelerator laboratory but from study of cosmic rays.

In 1937 Seth Neddermeyer and Anderson found a track that they identified as the trace of a particle with the charge of the electron but a greater mass. The new particle immediately seemed to find its place in theory. Like Dirac's theory of the electron, Yukawa's account of the strong force between nucleons required the existence of a novel particle. Anderson's "mesotron" had about the mass expected of Yukawa's field quantum or "Yukon;" and nuclear physicists, then still parsimonious with theoretical entities, thought the two particles one. After the war a major difficulty in the identification obtruded: as Marcello Conversi, Ettore Pancini, and Oreste Piccioni were the first to show, the negative mesotron escaped capture by matter far longer than theory allowed the Yukon, which, by hypothesis, interacts strongly with nucleons. Physicists began to entertain the idea that two mesons (to use the modern term) exist. Confirmation of this hypothesis came early in 1947 in pictures obtained by C. F. Powell's group in Bristol, England. (See Episode beginning page 54.)

The pictures were tracks of charged particles in a special photographic emulsion designed just after the war on the initiative of G. P. S. Occhialini by C. Waller of the Ilford film company. The tracks in question, found after inspecting a few plates exposed by Occhialini, apparently showed one charged meson (which Powell and his associates called π) giving birth to another (μ). The π turned out to be the Yukon or nuclear-force meson, and the μ to be Neddermeyer and Anderson's cosmic-ray particle. The mass of the pions as determined by Powell's group put them within the manufacturing capabilities of the new Berkeley machine. The Laboratory already had a film group, headed by Eugene Gardner, struggling to adapt Powell's technique to the great flux of particles from the synchrocyclotron. After months of experiment with exposure and development times, and with the position and orientation of the film in secondary beams from targets struck by deuterons and alpha particles, Gardner's group had not found a trace of a pion. The first detection of artificially created pions came in February 1948, shortly after the arrival at the Laboratory of G. C. M. Lattes, who had worked with Occhialini and Powell.

The first pions found were negatively charged. Soon Gardner's group detected positive pions and daughter muons caught where their parents ended their careers within the films. Accurate values for the masses of both sorts of meson were obtained. At the end of 1948



Photo courtesy Millikan Library, California Institute of Technology

Cal Tech's Carl Anderson (above) set off the search for the mesons with his discovery of a particle with the charge of the electron but a greater mass.

The new particle immediately seemed to find its place in theory

Machine Made Mesons

54

THE COMPETENCE OF the original 184-inch cyclotron to make mesons had been disputed. When it was designed in 1939 theorists knew too little about the process of creation to determine the threshold of energy; and even had they been wiser, experimental uncertainty about the mass of the cosmic-ray meson would have left the matter doubtful.

In arguing the need for the 184-inch cyclotron to the Rockefeller Foundation in 1940, Lawrence gave high priority to the study of nuclear forces, in which mesons had been implicated. Taking the meson mass to be $150 m_e$ (electron masses), he assured the Foundation that he would make the elusive particle in the proposed machine, which would certainly give alpha particles of 150 MeV. And that, he said, was the likely production threshold. He reasoned that the energy supplied to a heavy nucleus by an incoming alpha particle or deuteron would be quickly shared among the target nucleons; to have much chance of creating a meson in such a process, the incident particle would need an energy very much larger than his estimate of the rest mass, 75 MeV, equivalent to $150 m_e$. A light target nucleus, with fewer opportunities for sharing, appeared to be more promising, although the exigencies of momentum conservation then doubled the ante. The threshold Lawrence proposed to cross in 1940 was therefore $2mc^2$ (m the meson mass). He wrote the Rockefeller Foundation: "We are planning everything with the idea that we shall certainly be able to produce alpha particles at at least 150 million volts energy."

Lawrence's sanguine estimate of 1940 referred to a machine in potentia. After the war, with the thing in hand, he hedged his bet. Writing Groves about the \$170,000 that the MED had given to complete the synchrocyclotron, he declined to say what might happen when it went on. "The fundamental interactions of heavy particles are not well enough known to predict what may happen (for example, the important question of the production of mesotrons is unpredictable at present)." Apparently he then thought prospects sufficiently remote that no special hunt was made for mesons until the end of 1947, a year after the great machine first delivered alpha particles at just short of 400 MeV. By then theorists had made it appear likely that the "4000 ton atom smasher"—as journalists liked to call the synchro-

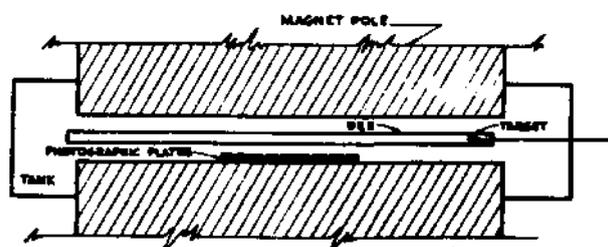
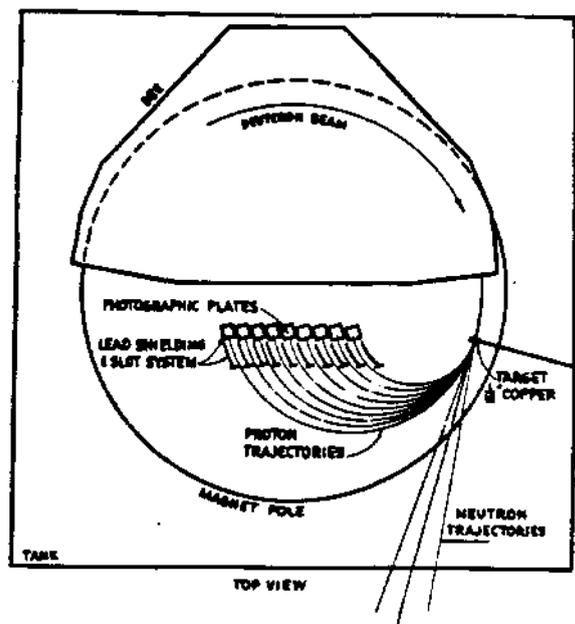


Artist's conception of the 184-inch cyclotron with a faint beam emerging toward the observer.

cyclotron, creating the impression that it split nuclei by falling on them—could indeed make mesons, and with something to spare.

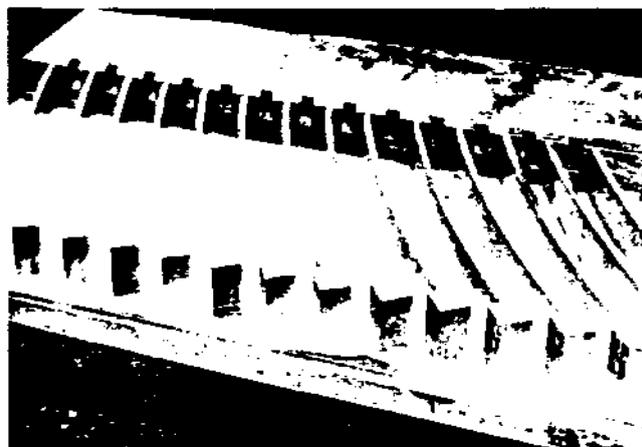
At the University of Chicago, Edward Teller and W. G. McMillan observed that at the very high energies in question bombarded nuclei become almost transparent. When a collision does occur, it involves only one nucleon from the target, whose kinetic energy might contribute to the stock necessary for meson production. For the most favorable case, McMillan and Teller found that the threshold energy might be as little as 95 MeV. As they said, "this result is radically different" from the 206 MeV that would be required by using the latest value of the cosmic-ray meson's mass (as determined by W. B. Fretter of Berkeley's physics department) and estimating in Lawrence's manner. Two of Oppenheimer's students improved the calculation of the collision cross-section and concluded that neutrons stripped from 195 MeV deuterons might do the trick. If the kinetic energy of the incident nucleon within the deuteron or alpha particle were also taken into account, the threshold might sink below the admittedly crude estimate from Chicago.

On the strength of these considerations, the group responsible for the detection of nuclear particles by photographic emulsions undertook to look for meson tracks on plates bombarded with deuterons and 380 MeV alphas (95 MeV/nucleon). The group had some idea of the likely appearance of the tracks from



LOCATION OF PHOTOGRAPHIC PLATES FOR STUDY OF PROTONS FROM THE TARGET

Schematic of the arrangement within the tank of the 184-inch synchrocyclotron devised by Gardner et al. to detect negative mesons, the paths of which are bent around the shielding and into the plate by the accelerator's field. 1948.



Emulsion holder used in the pi-meson search. The curved channels act to prevent particles with the wrong radii of curvature from reaching the plates placed in back.

the beautiful photomicrographs made by Powell and Occhialini. But their results also raised the threshold for meson production. The Berkeley materialists could not make the cosmic-ray particle of $202 m_e$ (the μ or muon) without first making its parent π particle (pion), the mass of which they estimated at above $250 m_e$, perhaps more than $350 m_e$. They required a minimum incident energy possibly as high as the 150 MeV per nucleon originally estimated by Lawrence and possibly beyond the reach of the synchrocyclotron.

The leader of the Laboratory's film group was Eugene Gardner, who had taken his Ph.D. under Lawrence in 1943 with a thesis on calutron ion sources and had then worked at improving the alpha process of electromagnetic separation at Oak Ridge. Gardner's job was to adapt Powell's technique to the abundant flux from the synchrocyclotron. He and his group worked closely with Eastman Kodak, to which Lawrence had become consultant late in 1945, on the development of a domestic film to compete with the nuclear emulsion created by Ilford, Ltd. for Powell's group in Bristol. For 18 months Kodak supplied new test plates and new methods of development while Gardner's group tried to find the best position and exposure time for the plates within the cyclotron tank. Their notebooks record constant disappointment and, occasionally, exhilaration. A note of November 30, 1946 by one of the group reads: "Please don't take this plate out of the micro-



The Ilford company produced special photographic plates containing extra chemical elements for the first time.

scope without calling me. This is a *beautiful* plate.... Can't go home—searching this plate is too exciting." The beautiful plate had been made by Ilford.

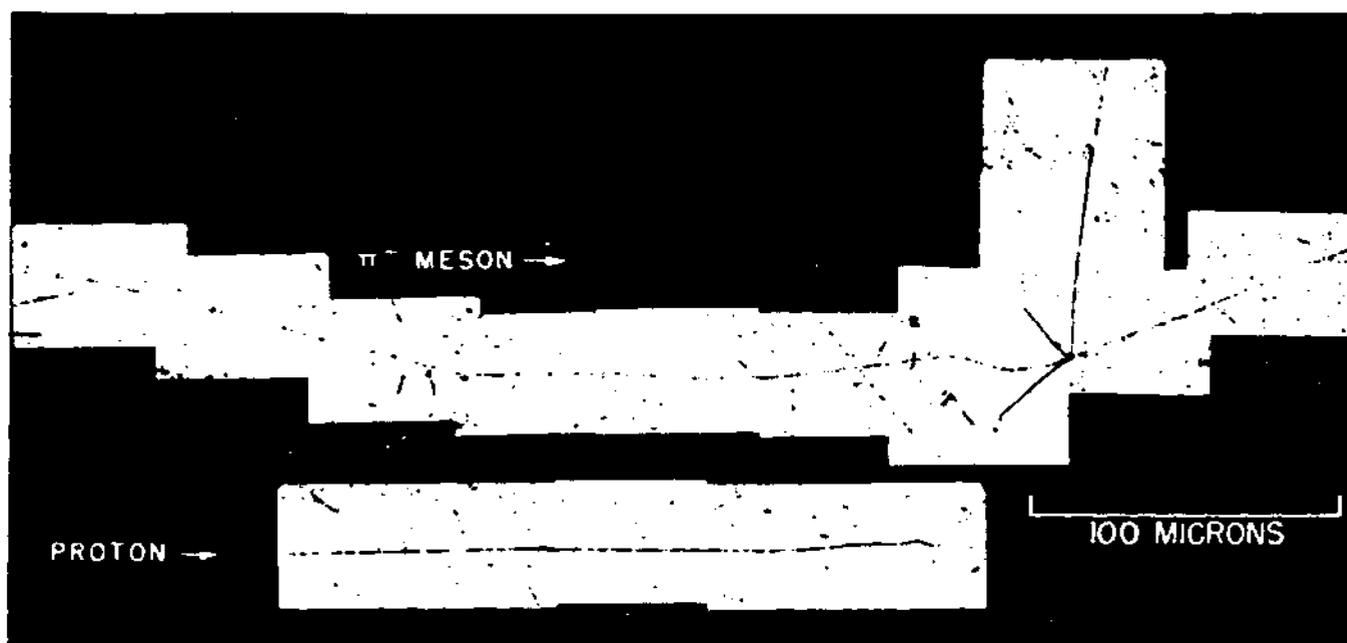
Identification of a track rests on its length and the number and distribution of its grains. These parameters, which characterize the charge, mass, and velocity of the responsible particle, may also be affected by irregularities in the plate or in its development. Interpretation of a unique event may plausibly be disputed. During the first six months of operation of the big synchrocyclotron, the film group found a few odd tracks unlike those for protons, alpha particles, and deuterons, and unlike one another, which they took to be the work of mesons. On careful examination they appeared to be artifacts of Kodak film. "Every time we run a set of plates in the cyclotron we look for meson tracks," Gardner wrote James Chadwick. "But so far we haven't found any."

In the autumn of 1947 the group began to search for mesons directly. A thin target of carbon was placed within the cyclotron tank to intercept the beam of alpha particles; adjacent to the target stood a stack of photographic plates so positioned that *negatively charged* π mesons created in the carbon and

projected in the direction of the beam would be directed into them by the field of the big magnet. In December, after three months of development and exposures, the group had found no mesons. There was nothing wrong with their experimental setup. They lacked some relevant experience.

Immediately after celebrating the new year, G. C. M. (Giulio) Lattes wrote Lawrence for permission to work at the Laboratory. He would come on a Rockefeller Fellowship and with the approval of the AEC to teach the film group what he had learned during two years' collaboration with Powell. He arrived in February 1948, preceded by a package of Ilford plates. They were exposed in Gardner's apparatus and developed according to Lattes's recipe, which differed from Berkeley practice. Then Lattes, who knew what to look for, discovered what the Berkeley group had not been able to find. As Gardner reported the result: almost immediately after his arrival, Lattes had "made the Bristol technique successful in detecting for the first time man-made mesons."

The plates became prolific, and the team practiced: a good scanner might find as many as fifty mesons on a plate in an hour. They found muons as well as



Photomicrograph of the track of one of the first π mesons found by Gardner and Lattes, 1948

pions. They had more raw data than they could handle. While developing the emulsion technique Gardner had prepared plates for analysis by physicists outside Berkeley, and he continued the practice even as it became likely that the emulsions contained something interesting. Powell, a frequent recipient, later called attention to this uncommon liberality: "Many laboratories all over the world are greatly indebted to our American colleagues of Berkeley, California, for the very generous way in which, promptly and without conditions, they have exposed photographic plates to the particles provided by their machines."

Among the results of Gardner's group was the answer to the question that had perplexed the meson experiment from the start. They found no pions when they sent 165 MeV protons against their carbon target; the yield at 200 MeV was but one percent, and that at 300 MeV less than half of the yield at 345 MeV. For alpha particles, the number of pions observed fell by two thirds as the energy of projection declined from 380 to 342 MeV, and to fewer than seven events (less than one percent of the maximum) at 260 MeV. The 184-inch machine as originally designed probably could not have made

mesons.

The University and the AEC officially announced the artificial production of mesons on March 9, 1948. Typically, Lawrence used the opportunity to argue the case for the next generation of accelerators. "To exploit fully the knowledge which the meson may provide, it will be necessary to construct super-giant cyclotrons." *Time* reported the discovery and hinted that the study of mesons might "lead in the direction of a vastly better source of atomic energy than the fission of uranium." The University of California explained that the work of Gardner and Lattes opened the newest new age since the discovery of fission. "It might roughly be compared with the discovery of America."

Gardner mastered the photographic technique and oversaw its adaptation to the cyclotron despite a progressive enervation that contrasted sadly with the robust machine he served. He died in November 1950 of berylliosis contracted from dust that he had inhaled while drilling beryllium for calutron test fittings at Oak Ridge.

The detection of the charged pion encouraged the search for evidence for a neutral one, which, according to a theory published by Oppenheimer and his

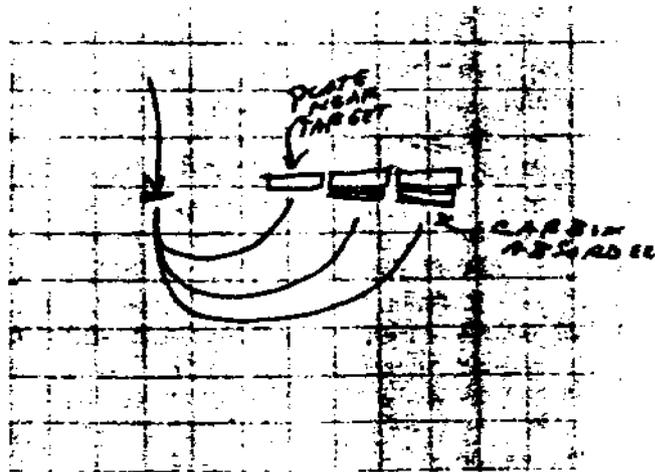
JULY 25, 1948

22 - Plate of 11000...
 23 - Plate of 11000...
 24 - Plate of 11000...
 25 - Plate of 11000...
 26 - Plate of 11000...
 27 - Plate of 11000...
 28 - Plate of 11000...

PLATE NUMBER	EXPOSURE	RESULTS	PLATE NUMBER	EXPOSURE	RESULTS
22	+		22	+	
23	-		23	-	
24	-		24	-	
25	-		25	-	
26	-		26	-	
27	-		27	-	
28	-		28	-	

BAD

Troubles plagued the exposure of emulsions and entire series of results were marked "bad."



Mesons produced by the beam hitting the small target at left follow curved paths in the magnetic field of the cyclotron. This sketch is from F. Gardner's notebooks.



Robert Serber, Laboratory theorist, writing for a photographer shortly after the announcement of the discovery of machine-made mesons by Gardner and Lattes in February, 1948.

students in 1948, might be the source of the electromagnetic radiation associated with the soft component of cosmic rays. Neither the hypothetical meson nor the two photons into which theory required it to decay would leave tracks in emulsions or cloud chambers. Experimental detection of neutral mesons accordingly would depend on subtle analysis and circumstantial evidence. Two different groups at the Laboratory, proceeding in two different ways, nonetheless managed to clinch the case.

In September, 1949 R. Bjorkland, W. E. Crandell, B. J. Moyer, and H. York observed pairs of electrons made by photons from a target struck by protons from the 184-inch synchrocyclotron. Their main experimental findings were a rapid increase of pairs beyond proton energies of 175 MeV and a telling relationship between two curves. The curves gave the yield of pairs in the detector as a function of the energy of the photons producing them for two different cases: photons leaving the target in the direction of the proton beam and (after reversal of the synchrocyclotron fields) photons leaving against the direction of the beam. The two plots could be transformed into one another on the assumption that all the pho-

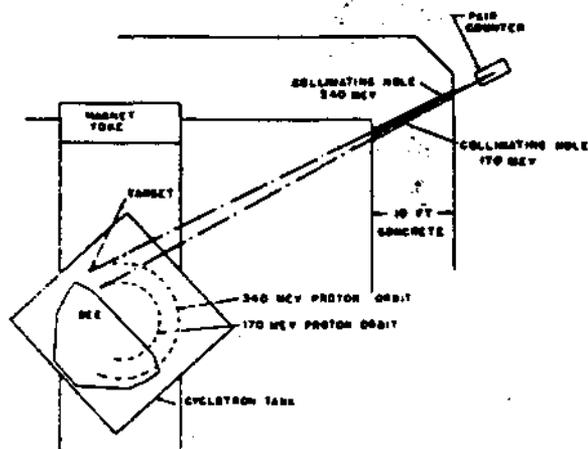


Fig. 1. Plan view of experimental arrangement.

Schematic of the experiment of Bjorkland et al., showing observation through a port in the concrete shielding of disintegration photons proceeding against the proton beam. Photons moving in the direction of the protons are observed through the same port with the beam reversed. 1950.

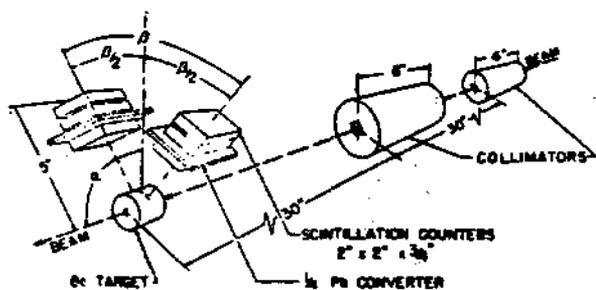


Fig. 1. Experimental arrangement.

Schematic of the experiment of Steinberger et al. Coincidences registered as a function of the angles α and β agreed with the hypothesis that the photons, which created electron pairs that activated the counters, were the only decay products of a neutral relativistic pion. 1950.



Wolfgang "Pief" Panofsky collaborated with Alvarez on the proton linac and built SLAC, still the world's most powerful electron accelerator and home of PEP.

Photons came from particles traveling along the beam at one third the velocity of light and emitting equal numbers of photons in opposite directions. The possibility of this Doppler superposition and the threshold energy of around 175 MeV, as well as behavior incompatible with other possible sources of the photons, made a strong circumstantial case that the radiator was a decaying neutral pion.

Further evidence—coincidences between photons liberated in opposite directions—could not then be obtained at the synchrocyclotron because its concrete shielding blocked the necessary observations. In experiments concluded in April 1950, J. Steinberger, W. Panofsky, and J. Steller avoided this difficulty by procuring their photons from a target struck by a collimated beam of x rays from McMillan's electron synchrotron. The photons from the pion decay were examined at right angles to the x-ray beam by coincidence counters. The energy distribution of the electron pairs made by the photons and the yield of coincidences at various angular separations of the counters agreed with the assumption that the photons arose from the decay in flight of a relativistic neutral pion. Thus was confirmed the existence of the first particle detected at an accelerator, and the analysis of the unmarked graves where nonionizing neutral particles disintegrate into nonionizing radiation.



C. M. G. Lattes (left) and E. Gardner with the nuclear emulsion positioning apparatus for the 184-inch cyclotron.

*The output of
research papers
matched that of the
machine*

McMillan's electron synchrotron came to life and made mesons by photoproduction via 335 MeV gamma rays. Perhaps the most elegant experiments in this series concerned the neutral pion, the first elementary particle discovered through the use of a high-energy accelerator.

The output of research papers, over 100 in all through 1949, matched that of the machine, a phenomenal performance in the judgement of Stanley Livingston, then established in the rival fortress at Brookhaven. The only declared rival was the synchrocyclotron at Birmingham, scheduled for completion late in 1949. Its designers hoped to reach 1.3 BeV. It made Lawrence "anxious," or so he wrote in February 1948, responding to Sproul's worry that Columbia's new machine, to go on line by the year's end at 400 MeV, would eclipse the 184-inch. Lawrence expected to outdistance Columbia with modifications to the synchrocyclotron already planned. To beat Birmingham he would need a new accelerator.

On March 8, 1948, representatives of Brookhaven met with Lawrence and his senior staff to fix the design energies for the proton synchrotrons that the AEC had promised to fund at each laboratory. Evidently the bidding would begin above 1.3 BeV. It began at 2 BeV,



"Old Town"—the city of the 184-inch cyclotron—during the peaceful days after the war.

the amount calculated by Brookhaven for abundant production of mesons by proton-proton collisions. Lawrence then revealed that he was about to announce that the 184-inch had made pions plentifully with 380 MeV alpha particles. The Eastern ambassadors quickly covered: Berkeley's discovery made the need for a 2 or 3 BeV machine even more evident, they said, since only a complicated reaction could produce mesons so near the theoretical threshold. The minutes of the meeting record its consensus that the performance of the synchrocyclotron "raise[d] many questions which cannot be answered without a 2.5 to 3 BeV proton accelerator." That satisfied Lawrence. He preferred to strike into the unknown, into the land where antiprotons might dwell. It was decided that Brookhaven would build to 3.0 BeV and Berkeley to 6 or a little more, just above the threshold for making negative protons as calculated by McMillan and Wolfgang Panofsky.

In the first postwar years the Laboratory took the lead in experimental nuclear physics as well as in accelerator design. The period of productive relaxation ended in 1950. The explosion of the first Soviet atomic bomb in August 1949 called the Rad Lab to the colors once again. This time the particle accelerator would play a direct role in the nation's defense.

1173503

*He preferred to
strike into the
unknown, into the
land where
antiprotons might
dwell*

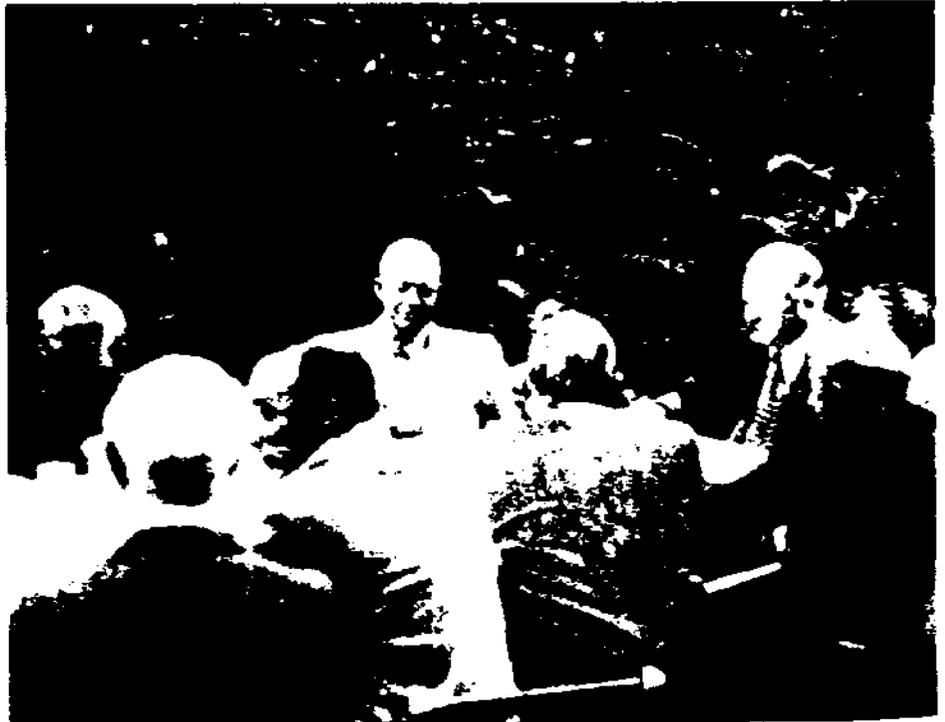
Cold War in 5 Science

62

*Lawrence did not
demobilize as fully
as his laboratory*

LIKE THE COUNTRY as a whole, the Rad Lab demobilized rapidly and enthusiastically after 1945. Between 1946 and 1949 less than thirty percent of its contracted services related directly to military problems. The residual work centered in the Crocker Laboratory. There a group under Joseph Hamilton that had participated in the Bikini tests of 1946 advised the Navy on the decontamination of ships exposed to nuclear explosions. The tests also interested the Army Chemical Corps, which chose Hamilton as chairman of its Advisory Committee on Radioactive Warfare. The Crocker Laboratory studied the biological effects of radioactive aerosols and of fission products. The rest of the Laboratory's defense work in the early postwar years concerned the separation of fissionable elements. Thus was fulfilled its contractual obligation to assist the AEC on "problems for which the laboratory personnel or facilities are particularly well adapted."

Lawrence did not demobilize as fully as his laboratory. He continued to push the calutron process, the efficiency of which he promised to increase tenfold. He stayed more bullish than the AEC, which stopped calutron development in 1948. That was a mistake, Lawrence said, making an argument since become familiar: only ongoing improvement could guarantee "national leadership in this



Lawrence lunching with future president Eisenhower and past president Hoover at Bohemian Grove, July 23, 1950.

1173504

field." He also disagreed with the AEC's policy of exclusive custody of nuclear weapons, and with its General Advisory Committee's (GAC) depreciation of radiological warfare. Against the one he held that nuclear weapons should be domesticated by the military for strategic bombing; against the other, and with the Department of Defense, he proposed expansion of the sort of work that Hamilton was doing for the Navy. The old boosterism backed Hyman Rickover's plan for a nuclear power plant for submarines. "To be credible, the project would have to be big," he is reported to have told the Admiral, suggesting a three-year, hundred-million dollar, cash-and-crash program.

This zeal did not match the mood of the country or Truman's policy to seek international control of nuclear weapons through the Baruch plan. Lawrence declined to join Baruch's advisory panel. And the AEC turned down the proposal of its GAC that Lawrence direct a project on reactors that would speed up development of nuclear propulsion systems. The Commission decided that a crash program of the Lawrence type was not necessary and instead set up a Reactor Development Group under Walter Zinn at the Argonne National Laboratory.

Then came the first Soviet nuclear bomb, detonated on August 29, 1949, to the astonishment of Western politicians and the vindication of their scientific advisors. It strengthened Lawrence's conviction that Stalin sought world domination. What could the Laboratory do to help the nation meet the latest Soviet threat? Lawrence, Alvarez, and others decided to put the Laboratory behind Edward Teller's program for a thermonuclear weapon, or superbomb, which had withered in the shadow of fission development and international control. Consultation with Teller at Los Alamos convinced Lawrence that he could advance the program significantly by finding the neutrons to make the tritium necessary for the new weapon. He thought first to construct a heavy-water reactor like the Canadian plant at Chalk River and went to Washington to advocate his reactor and Teller's bomb. AEC commissioner Lewis Strauss liked the plan, as did members of the Armed Forces Special Weapons Project and the Joint Committee on Atomic Energy (JCAE). "A very great change has taken place in the climate of opinion," wrote GAC chairman Oppenheimer; "two experienced promoters have been at work, i.e., Ernest Lawrence and Edward Teller." GAC member I. I. Rabi of Columbia applauded the return of the champions. "It is certainly good to see the first team back in. You fellows have been playing with your cyclotrons and nuclei for four years and it is certainly time you got back to work!"

Confident that the project would be approved, Lawrence appointed Alvarez to direct it. While plans went forward to construct the reac-



Edward Teller

*Truman announced
that the program
would proceed*

tor north of Berkeley at Benicia, Lawrence enlisted more congressional and AEC support and cleared the project with the University's regents. Late in October, however, Teller discovered that members of the GAC did not share his and Lawrence's sense of urgency. They even opposed a crash program for the superbomb. Lawrence sent Robert Serber, Oppenheimer's long-time friend and collaborator, to Princeton to persuade the GAC's chairman of the virtues of thermonuclear weapons. Alvarez planned to appear before the GAC itself to discuss the heavy-water reactor. To the great and pained surprise of Berkeley's lobbyists, the GAC voted to advise the AEC not to "pursue with high priority the development of a super bomb." Further, it extinguished Lawrence's reactor on the bay and recommended a more modest program under Zinn.

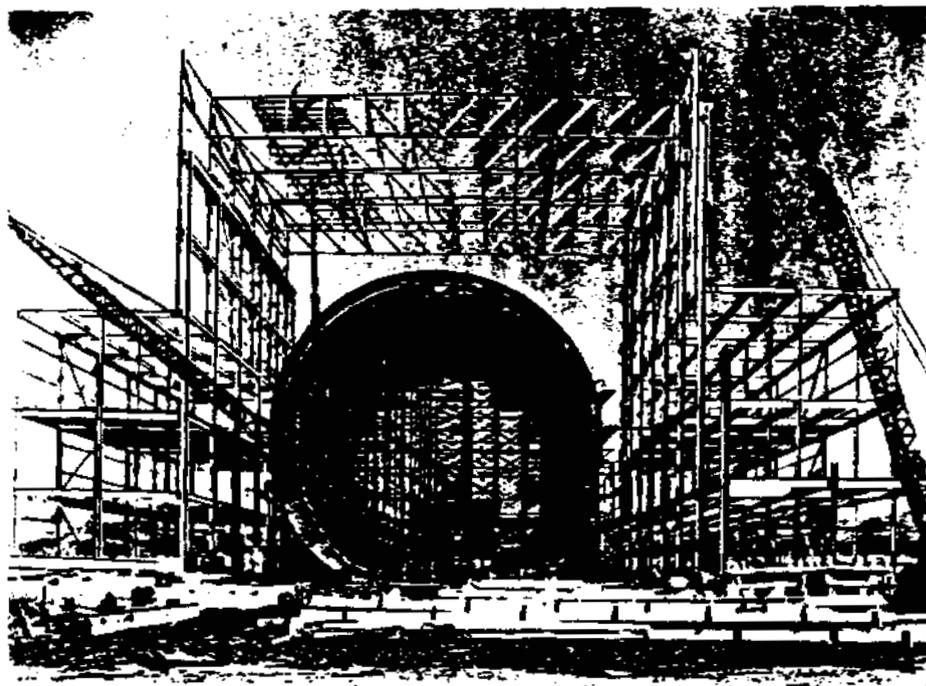
Although the GAC's technical and moral objections convinced three of the five members of the Commission, they did not move Strauss. He recommended to Truman that he order the AEC to place "the highest priority [on the development of the Super] subject to the judgment of the Department of Defense as to its value as a weapon." Gordon Dean, the fifth commissioner, Senator Brian McMahon of the JCAE, and representatives of the Department of Defense and the National Security Council campaigned for the big bomb. On January 31, 1950, three months after the GAC had rendered its decision and two weeks after Klaus Fuchs had revealed his treason, Truman followed the counsel of his secretaries of state and defense and announced that the program would proceed.

While the debate flourished, Lawrence considered how to make grams of neutrons without a reactor. He believed that the nation would have to draw on a reservoir to synthesize tritium for the Super and substitutes for diminishing stocks of fissionable material. Lawrence's alternative neutron fountain was of course an accelerator, a huge linac that could make, say, plutonium by causing neutrons to irradiate tailings of depleted uranium-238 from Oak Ridge. To release the neutrons, an intense beam of deuterons with energy of several hundred MeV would be required. Lawrence proposed a prototype 25 MeV, high-intensity, linear accelerator to the AEC on New Year's day, 1950. Four days later he asked that, once the prototype worked, a machine delivering 350 MeV deuterons be built to make a gram of neutrons a day. It would have a beam intensity of about one ampere, a million times that of the synchrocyclotron. It would cost, he said, between \$100 and \$150 million. (See Episode beginning page 66.)

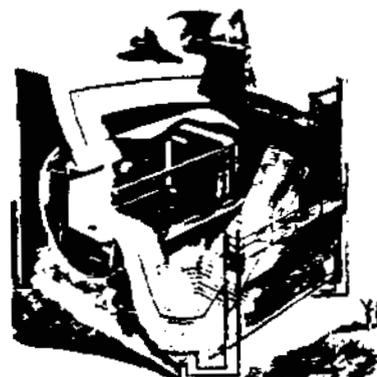
The plan offered much. The Commission was worrying about procuring enough uranium to fuel its reactors and fill its bombs. A machine that could make plutonium from useless and practically inexhaustible uranium tailings without consuming fissionable material,

and without presenting safety hazards that would require sequestering in a remote company town like Hanford, would be a most welcome way to crack what Lawrence called the "bottleneck of this raw material problem." To this weighty, practical rationale, he added a characteristic consideration: the new plant might prove even more important for uses unknown and necessarily unspecifiable than for the purposes for which it would be built. "Several men have stated it roughly as follows [Lawrence wrote the AEC]: 'I can't see how the AEC can afford to do without a high-performance accelerator. There are so many important things which can't be done with a pile, but need some sort of accelerator. One never knows when a new process of military importance will demand the existence of an accelerator. In fact, the mere existence of such a machine may easily influence the thinking of scientific and technical men along lines which they would otherwise dismiss at the outset as absurd.'"

The AEC approved construction of the prototype accelerator, Mark I, after Truman's decision to pursue the Super in January 1950, and design of the production machine, Mark II, after the Korean war started that summer. Mark I was designed to produce polonium for use in the weapons program and in radiological warfare. Mark II's target arrangement would permit manufacture of tritium and plutonium as well. Then there was Mark III, a high-intensity strong



The vacuum vessel for Mark I went up before its enclosure, 1952.



Proposed target structure for the 1500-foot Mark II accelerator. Scale is given by the two figures at lower right.



Schematic of Mark II, 1500 feet long. The injector is at near end, target at far.

A Neutron Foundry

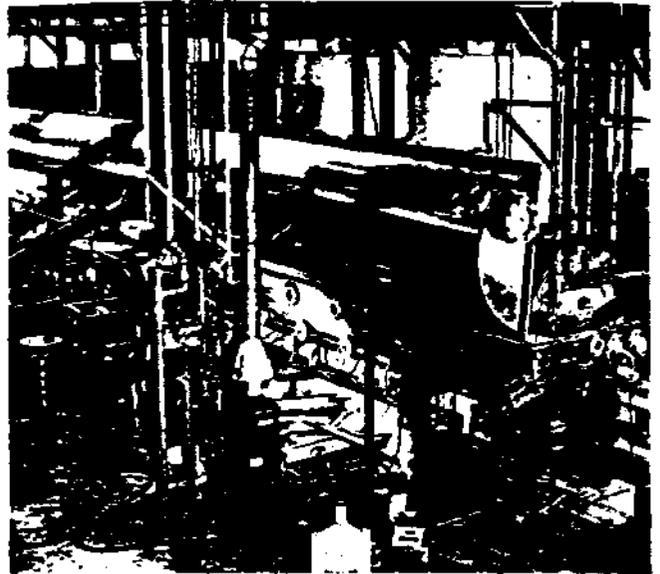
66

THE MATERIALS TESTING ACCELERATOR program had much in common with the wartime MED: a sense of urgency, an imposing price (about \$5 billion if fully implemented), and a leap into the technological unknown. Its springboard rested on two supports, both of recent construction when the program began in 1950: Luis Alvarez's linac and Herbert York's demonstration that high-energy deuterons knock streams of neutrons from almost anything. The exploitation of these neutrons for radiological warfare seemed to Lawrence "a very nice example of the unpredictable value of fundamental science. When the 184-inch cyclotron was built... we didn't honestly think that anything of a practical character would come out of it in a few years."

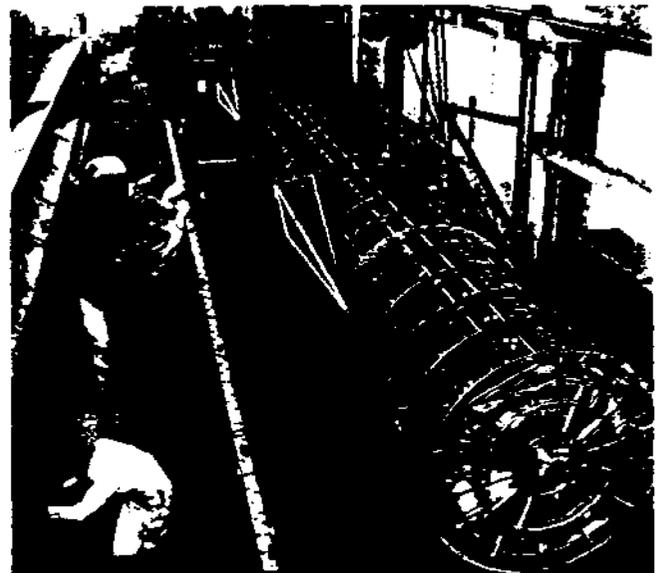
The principle of the Alvarez linac differs from that of the original Wideröe version (the same that inspired the invention of the cyclotron). Both use drift tubes to protect the particles from decelerating phases of the radiofrequency field. The Wideröe tubes, however, also function as electrodes, whereas the Alvarez tubes are passive, the accelerating field arising from electromagnetic radiation flooding the apparatus. The stems supporting the tubes bring electricity in the first case and only cooling water in the second.

The Alvarez linac exploited sources of electromagnetic radiation at the 1.5 meter wavelengths developed for radar during the second world war. In acknowledgement of the contribution of the Laboratory to the MED, General Groves made it a gift of 750 surplus radar oscillators. They were to form the core of the equipment that Alvarez, Panofsky, and a team of specialists used to create the oscillating field within the linac that would direct and accelerate protons down its 40-foot length. The machine may be pictured as a cylindrical wave guide or resonant cavity containing a time-varying accelerating field everywhere the same. The field automatically bunches particles that enter the gaps between the drift tubes as the field is increasing there. Particles arriving slightly early (or late) receive a smaller (or larger) push than the mean, and come in better time to the next gap.

Distortions in the field introduced by the drift tubes make a sideways force in the gaps between them. As a result, some of the beam runs into the walls of the drift tubes. By placing a conducting grid



The steel vacuum chamber for the proton linac built in 1947 by a group under Luis Alvarez.



The 40-foot long radio-frequency cavity of the proton linac lifted out of its vacuum vessel.

1173508



View inside the partially opened linac chamber showing the drift tubes between which protons are successively accelerated.

across the opening of the tubes, Alvarez's group distorted the transverse field so that it always focused the beam. Another important accomplishment was the redesign of the circuits for the oscillator units Groves had provided, which did not work efficiently for their new purpose. Alvarez explained that wartime experience made the group's task unexpectedly easy: "It has been our experience all through the development of the linear accelerator that all problems involving the phasing of oscillators and cavities are an order of magnitude easier than people without direct experience in the field predict. Every time we tried something new in this line we were surprised at how easy it was to do what had first looked like a very tough job."

The linac, completed in 1948, gave a beam of unprecedented intensity and collimation: 0.4 milliamp, 85 percent of which could be directed on a target 3 mm in diameter. The emergent energy, 32 MeV, the highest available for protons before the 184-inch synchrocyclotron accelerated them, was acquired partly from a conventional Van de Graaff accelerator used as an injector and mainly (28 MeV) from the linac. The construction, evacuation, and cooling of the copper cavity and its steel vacuum

enclosure brought new experience in engineering. The copper liner, made of long sheets specially milled, so closely resembled the fuselage of an airplane turned inside-out that its fabrication was subcontracted to the Douglas Aircraft Company. A system of integral water pipes prevented the 2.5 million watts of radiofrequency power developed within the cavity by the 200 MHz oscillator from melting the machine.

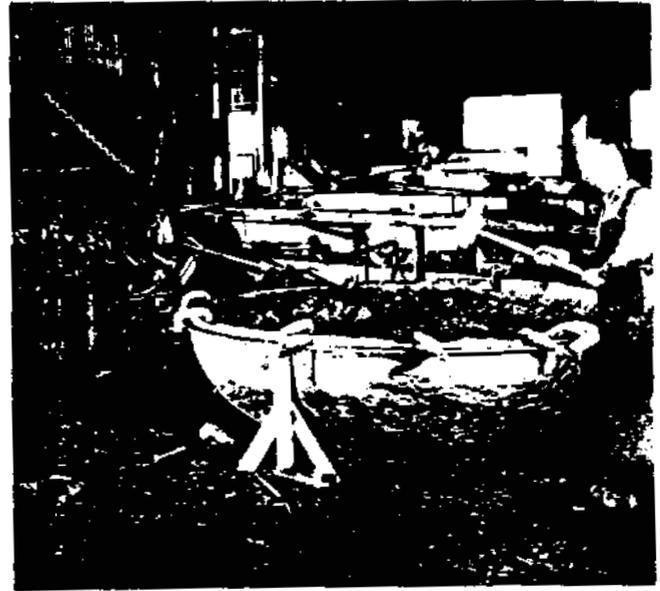
This impressive accelerator, with innards the size of a small plane and power requirements in megawatts, was but a "toy," as Lawrence said, compared with the "great particle reactor" he proposed to a subcommittee of the JCAE visiting Berkeley in October 1949. A huge apparatus of the Alvarez type would make free neutrons by bombarding heavy metals like thorium, and these neutrons, when absorbed by lithium, thorium, or depleted uranium, would create the useful explosive materials tritium, uranium-233, or plutonium, as the AEC might desire.

For a year and a half, from January 1950 through the summer of 1951, the Laboratory refined the plan. According to the last design, the MTA (as the project was named) would give 500 milliamp of 350 MeV deuterons that would make a gram of free neutrons a day. To guard against arcing in the gaps, which had proved troublesome in pilot studies, the plan reduced the potential gradient in them and extended the cavity to 1500 feet, thereby assuming the maintenance of a vacuum of one billionth of an atmosphere in a volume of over four million cubic feet. The price would be more than \$300 million (up \$235 million over the first estimate) per machine, exclusive of target. The whole would be surrounded by a concrete wall 80 feet high and between 7 and 20 feet thick. Power alone would run \$14 million a year, about two-thirds of the operating budget. Thus the hypothetical bottleneck of raw materials would be hypothetically broken. "If we can do that for a few hundred million dollars, why, let's get going and do it." The members of the JCAE to whom Lawrence addressed this remark agreed; they had no trouble with the money after receiving assurance, as one of them put it, that "neutrons theoretically are actually apparently there."

The primary target was to be thorium plates in stainless steel tubes cooled by liquid sodium. A

magnetic field would sweep the tip of the beam across the tubes. Neutrons emerging from the thorium would fall on a secondary target of uranium, perhaps jacketed in aluminum as at the Hanford reactors and at least twelve feet thick; or into an absorbing lattice similar to the Hanford slugs imbedded in graphite, the slugs to consist of uranium tailings, lithium, or thorium, or any two, or all. The projected cost of the target: \$55 to \$70 million depending on the constitution of the lattice.

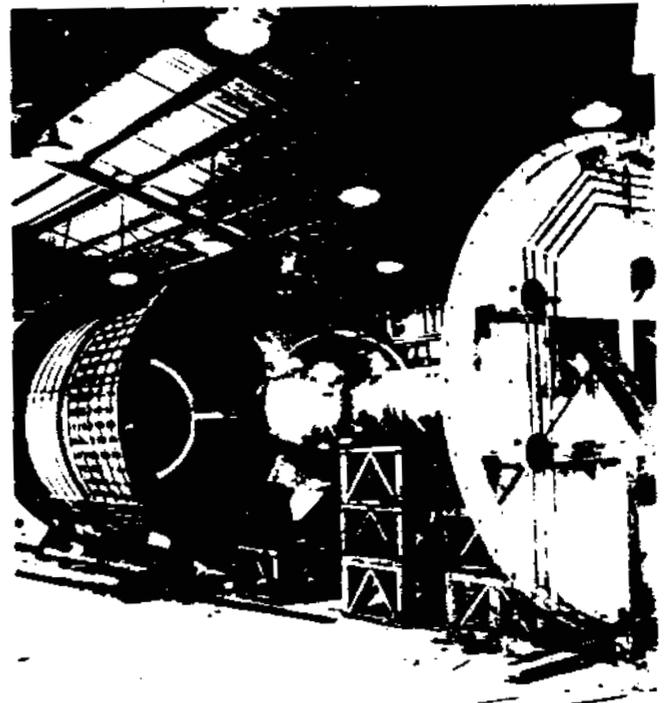
Only the front end of the gigantic machine, the so-called Mark I, was ever built. Authorized late in March 1950 for \$7 million (it had consumed \$21 million by the end of 1951), it was to test the possibility of magnifying the proton linac to the size of the production accelerator (Mark II), make 50 milliamp of 30 MeV deuterons, and use them to create polonium-208, an agent of radiological warfare, from bismuth. Mark I contained the largest nothing in creation, an evacuated space 60 feet long



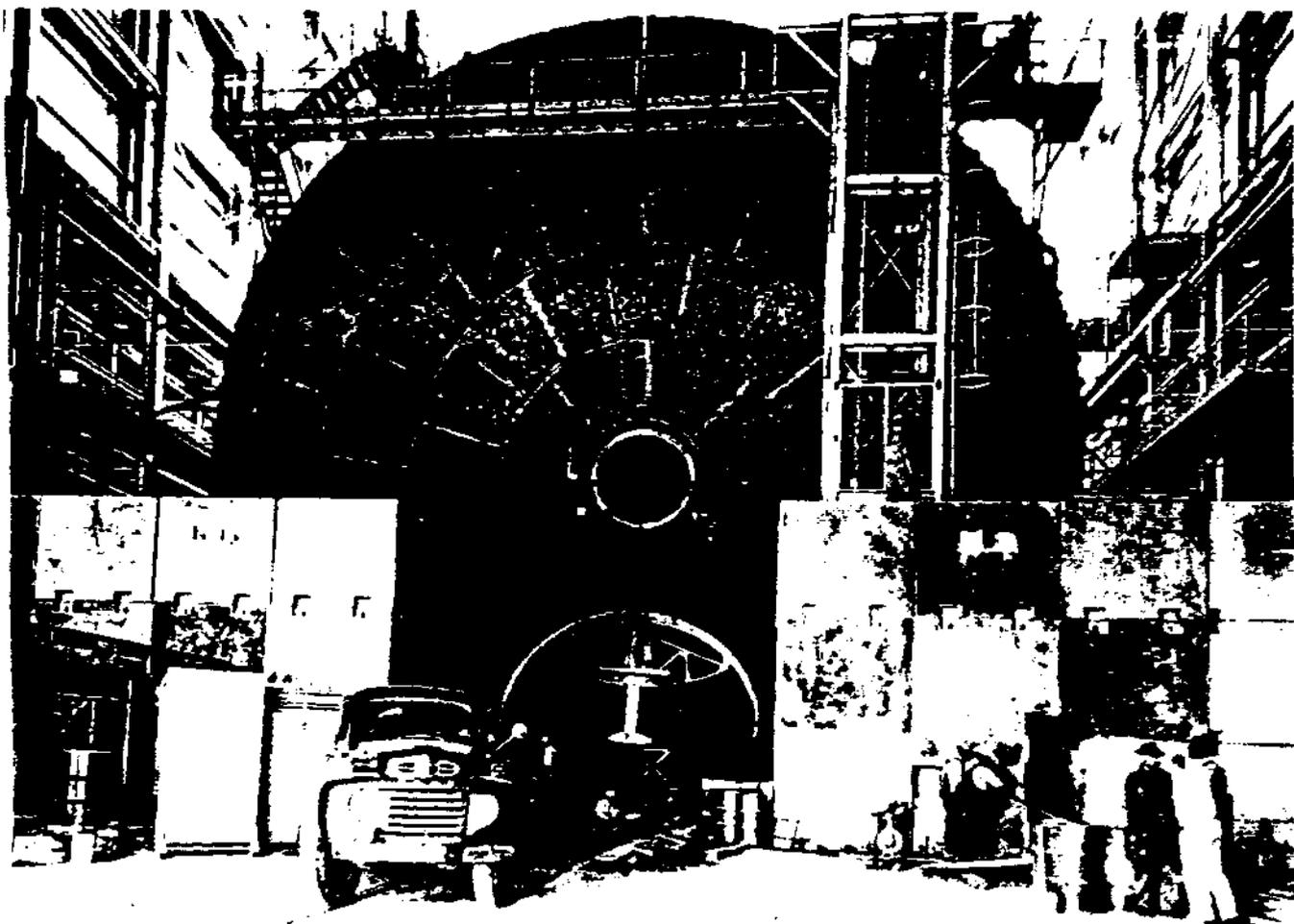
Shaping the copper ends of the large drift tubes for Mark I.



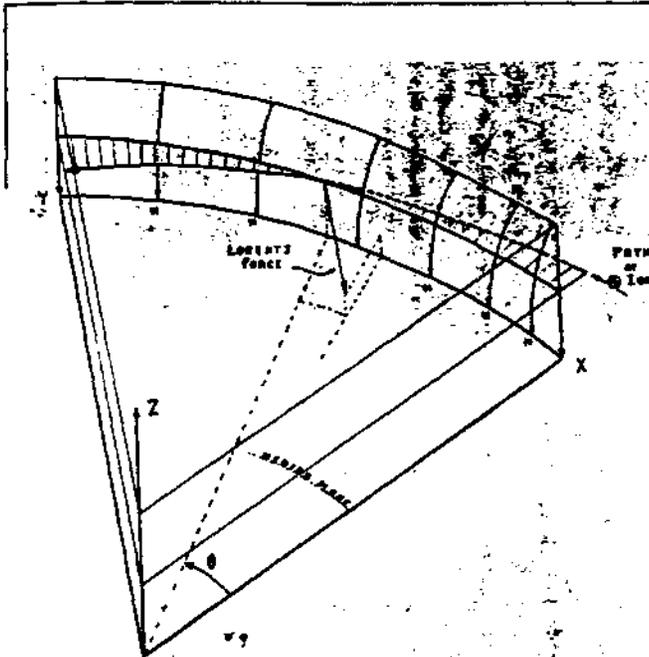
Aligning the central support for one of Mark I's drift tubes.



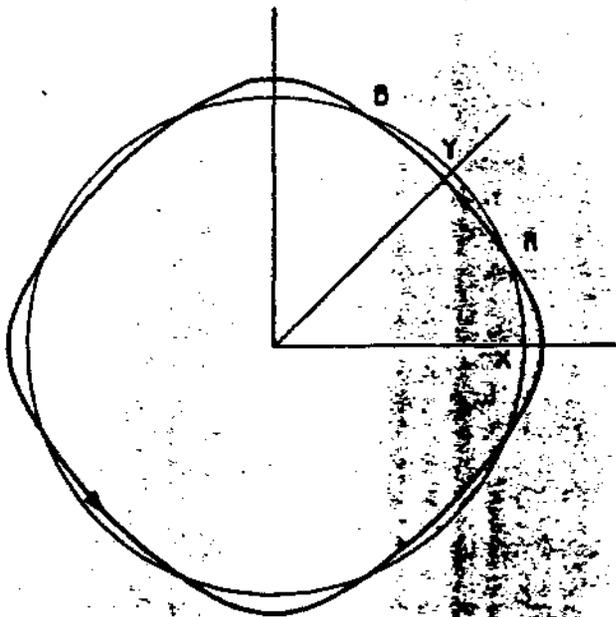
Assembly of the powerful radio oscillator for Mark I.



Looking down the vacuum tank of Mark I, railroad tracks were used to move the massive drift tubes within the vessel.



The principle of sector focusing: a particle crossing the bulging field at the edge of a sector is pushed back into the horizontal plane.



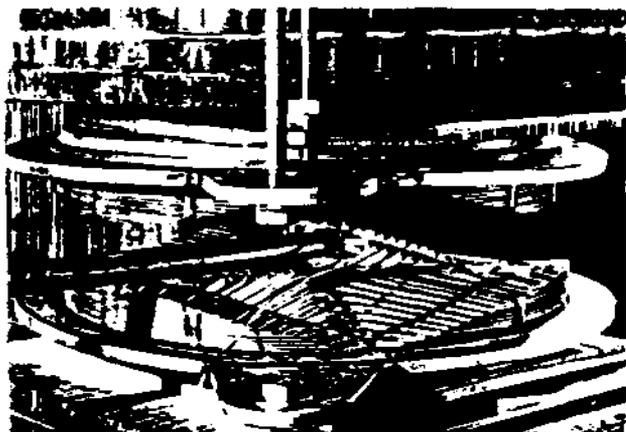
Sector focusing produces particle orbits with lobes, one per sector-pair.

projectors summed up: "Mark I is believed to have provided by far the largest heavy particle beam of any machine ever built, and it provided information and developed technology which made it possible to build a substantially better high current accelerator at considerably reduced cost."

Other dividends came with Mark III, the production cyclotron Lawrence first brought to the attention of the AEC in the spring of 1950. Their rebuff as usual did not stop him. In November, having had tests made on a small model suitable for accelerating electrons, he returned to the Commission confident of making his gram of neutrons by the cyclotron as well as by the linac. The agency, concerned about the extraction of the beam, continued to prefer the straight path. Lawrence convinced himself that circular was better: "it is like the electromagnetic process in the Manhattan District," he told the JCAE. "Everyone felt that, thank God, this electromagnetic process will give us something; if we keep it going long enough, we will get a bomb." In September 1951 Lawrence formally proposed to make a prototype production cyclotron giving 15 milliamp of 300 MeV deuterons at a cost of \$20 million exclusive of the target. He now explicitly rated it more promising than the Mark II linac, not only for its primary purpose, but also for "truly new possibilities that might well turn out to be of decisive importance which would give us a tremendous advantage militarily and peacewise." The Commission did not bite.

One reason for Lawrence's eagerness to proceed with the Mark III cyclotron was to test the principle of sector focusing, which had been suggested in 1938 by L. H. Thomas as a contribution to the debate about the practical limitation on accelerating particles imposed by relativity. Many physicists, including Lawrence, had recognized that if the magnetic field of a cyclotron *grows* radially it can speed up a particle in outer orbits to compensate for the slowing down arising from relativistically increasing mass. They also recognized that by so distorting the field they would lose the focusing into the plane of the median orbit that they customarily secured by a slight radial *decrease* in the field. Thomas thought of a way to keep the spiraling orbits planar while allowing the average magnetic field to increase.

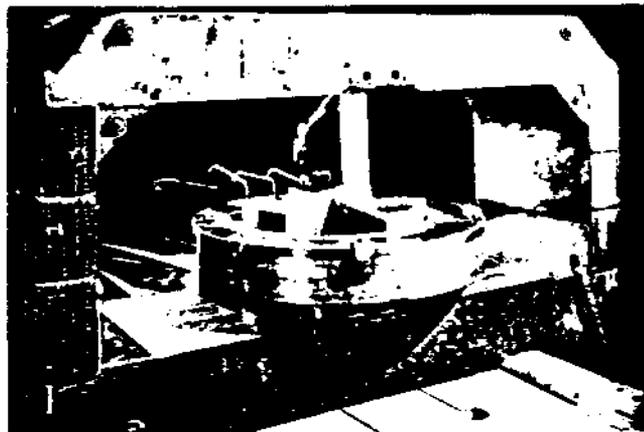
His trick divides the plane of the orbit into several pie-wedge sections and requires particles in alternate



shaped pole faces for the 1/10 scale electron model of the Mark III sector-focused cyclotron, 1951.

sections to be exposed to a relatively strong (and in intermediate sections to a relatively weak) magnetic field. The stronger sections bend the particles' paths more sharply toward the center than the weaker sections do. Orbiting particles cut obliquely across the magnetic field lines that bulge outward at the boundary from strong to weak field. This cutting induces a force that pushes a particle more strongly back toward the median plane the farther from the plane it is. Since the angle of the path with respect to the mean circular orbit changes sign at the other side of a wedge, the reverse bulge in magnetic field there also tends to drive errant particles back.

A sector-focused cyclotron accordingly keeps accelerated particles approximately in phase even if their masses increase. It therefore can use a fixed frequency oscillator for the accelerating field. It runs



The magnet yoke of the model for Mark III showing the three strong/weak field regions.

continuously, and so can give a beam more intense than those of pulsed machines like the Alvarez linac and synchrocyclotron. Thomas' principle therefore seemed the obvious and perhaps the only way to Robert Thornton and his colleagues when Lawrence charged them with the design of a cyclotron for the MTA project. Their conception, the J-16 cyclotron, did not materialize. It would have produced deuterons of 220 MeV after acceleration between magnetic poles shaped to give three strong and three weak fields extending out to its full 312-inch diameter. They demonstrated its principle, however, on an electron accelerator made to one-tenth scale. It was the first machine to show the merit of sector focusing, which subsequently regulated the design of many research cyclotrons throughout the world, including the Laboratory's 88-inch machine.

The engineering study concluded that the thing might work

Narrative continued from page 65



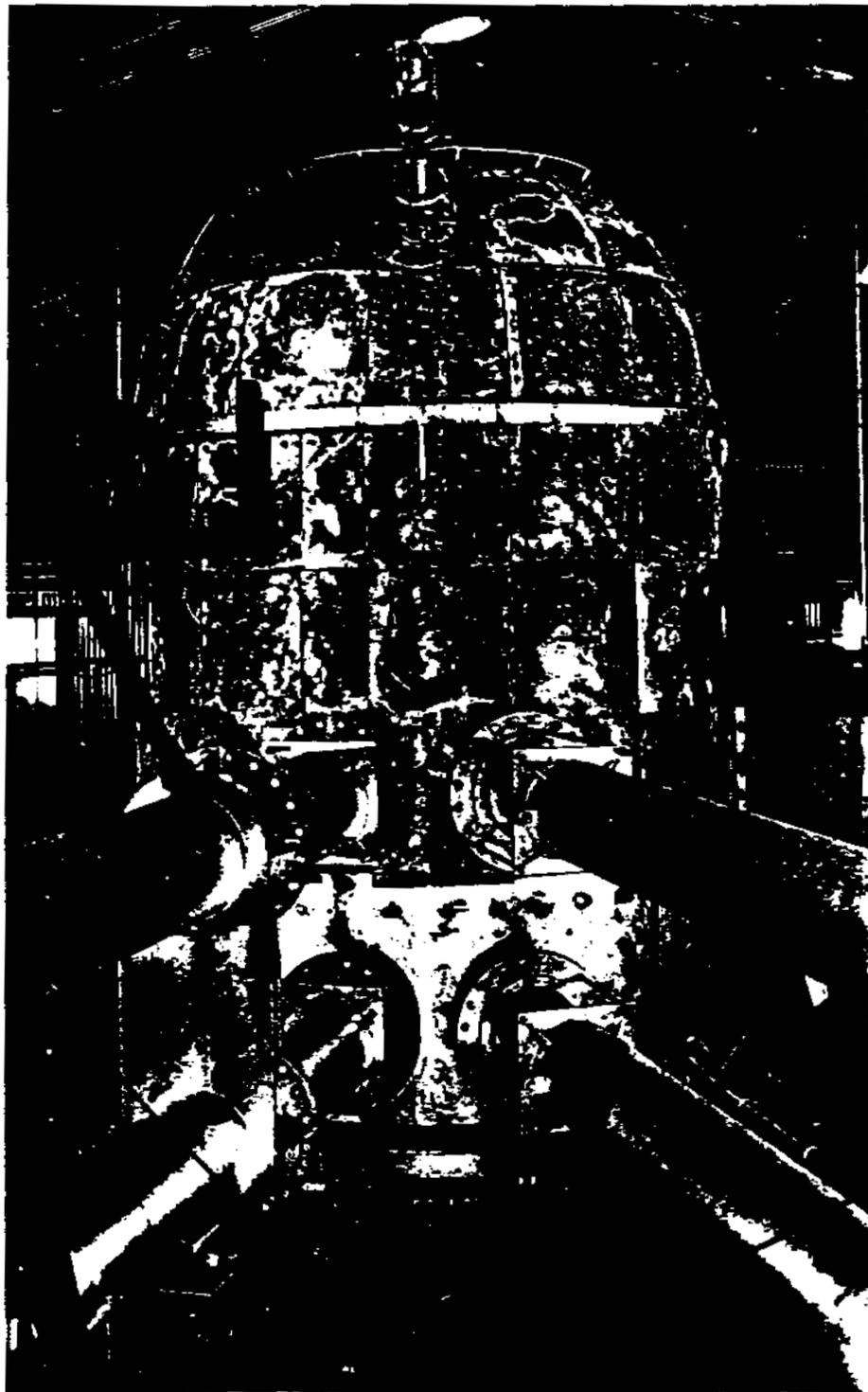
Livermore Naval Air Station at about the time it became the site for Mark I.

focusing cyclotron, which Lawrence began to urge in 1951. The vast undertaking received a new laboratory and a new administrative arrangement. The Livermore Auxiliary Naval Air Station about 45 miles from Berkeley became the site of Mark I, code named "Materials Testing Accelerator" or MTA. The California Research and Development Corporation (CR&D), a subsidiary of Standard Oil of California, assumed overall responsibility when the University declined to do so.



Artist's concept of a possible target configuration for Mark III.

In contrast to the cyclotrons on which the Laboratory had chiefly built its reputation, the MTAs were intended to give extremely intense beams. New techniques for injection and focusing had to be devised, very high vacuum attained, spurious discharges suppressed, and a thousand other technical obstacles overcome. The staff was confident that it could solve them all. An engineering study completed in August 1950 concluded that the thing might work. Lawrence declared that "the Mark II program should go full speed ahead," somehow estimated its chances of success at ten to one, and urged that ten similar 1500 foot machines be built across the country. But the AEC, recalled to prudence by its GAC, postponed construc-



The completed oscillator for Mark I.

1113515

The AEC had found another and cheaper way

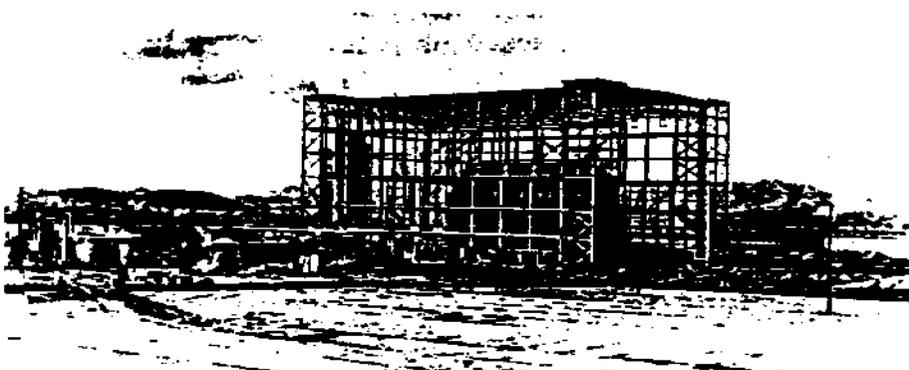
tion of Mark II until Mark I had proved itself.

While the AEC debated Mark II, the Laboratory was finishing a preliminary study for Mark III, a 350 MeV, high-intensity cyclotron. In November Lawrence asked that it be constructed immediately. His former colleague Pitzer, director of the AEC's Division of Research, warned that although the Commission attached "the greatest importance to the prompt development of an efficient neutron production accelerator," it had no interest in "small production...by 'quick and dirty' or inefficient methods." Among causes for doubt was the problem of extracting a beam from a cyclotron, which had not yet been solved satisfactorily for conventional machines. Mark III called for the simultaneous emergence of three beams directed at three different targets.

In March 1951 the AEC deemed Mark I sufficiently promising to justify siting studies for Mark II. And that was enough to return Lawrence to lobbying for Mark III. In testimony before the JCAE early in April, he pointed out the comforts of a family of high-intensity accelerators. "If these processes work...they can be used in peacetime for the production of fissionable material, but when war comes, they could overnight be producing radioactive materials...on a scale so that we could do all our fighting with radioactive materials and not use atomic bombs. That would be a great thing." The JCAE admired the argument. It put pressure on the AEC, which authorized another feasibility study, which found favorably for Mark III. The Laboratory proposed a 300 MeV model, to be called the J-16.

Even Lawrence's associates at California Research and Development doubted the wisdom of proceeding simultaneously with three untried machines. Research on targets for Mark II had fallen three months behind schedule, and CR&D feared more delays should the J-16 be authorized. In any case neither design had matured to the point that the Laboratory was willing to set the principal engineering parameters. During the war the MED had ordered a freeze on cyclotron designs. No one in the AEC wished to do as much for Mark II. Instead, the agency deferred its construction pending a thorough review of its economic potential.

One of the chief justifications of MTA was need for fissionable material should foreign sources of uranium fail. By the summer of 1952 the argument no longer weighed. The AEC had found another and cheaper way. By offering bonuses for new domestic sources of uranium and by increasing the price of ore, the Commission stimulated prospecting that uncovered rich deposits on the Colorado plateau. It appeared that the country did not require an emergency operation to make plutonium from scrap uranium. Attempts to convince the AEC to favor accelerator production of uranium-233, whose relatively small critical mass fit it for use in gun-type bombs



Vacuum vessel of the MTA Mark I under construction at Livermore.

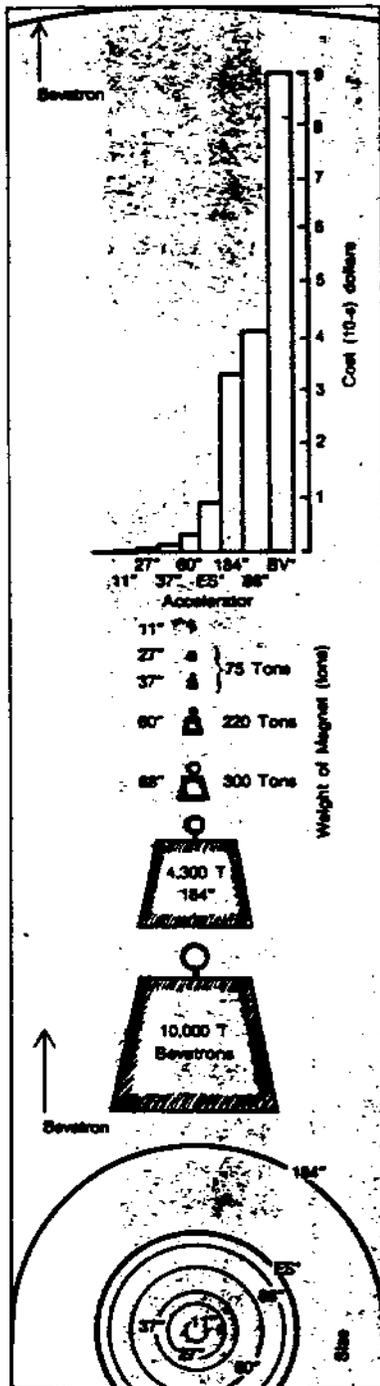
and tactical warheads, did not avail. Nor did calculations to demonstrate that the fissionable materials yielded by MTA would release more energy than consumed in producing them. On August 7 the AEC terminated Mark II and reduced Mark I to a small program in its Division of Research.

Meanwhile two major changes had come over the Livermore site. First, Mark I showed that it could hold a high vacuum and a large potential gradient and enabled its builders to judge it a success. Second, in June 1952 the AEC had established at Livermore the locus for what Teller called "healthy competition" for Los Alamos. The Livermore Weapons Laboratory became a branch of the University of California Radiation Laboratory under the direction of Herbert York. The administrative arrangement lasted until 1971. Until then Livermore took most of the Laboratory's work in applied science, including weapons development and projects Pluto (nuclear rockets), Plowshare (peaceful applications of nuclear explosives), and Sherwood (controlled thermonuclear reactions). The division of labor not only encouraged the Berkeley branch to reconcentrate on basic nuclear science, it also reduced and eventually eliminated classified research there. The Livermore branch was a consequential legacy of MTA. Or, as Lawrence put it from the reverse perspective, "the MTA project made it possible for us to save at least a year or perhaps two years on the development of the Livermore Weapons Laboratory."

The parent Laboratory gained \$375,000 severance pay to increase the magnetic field of the 184-inch cyclotron to 23,000 gauss, which made possible acceleration of protons to 750 MeV. The rationale was that the AEC desired to know the neutron yield at such an energy. Perhaps most important, experience gained in MTA and a few pieces of its furniture assisted the completion of the long-awaited Bevatron at an energy almost double that planned before the Mark brothers came on the scene.

*The division of
labor reduced and
eventually
eliminated classified
research at Berkeley*

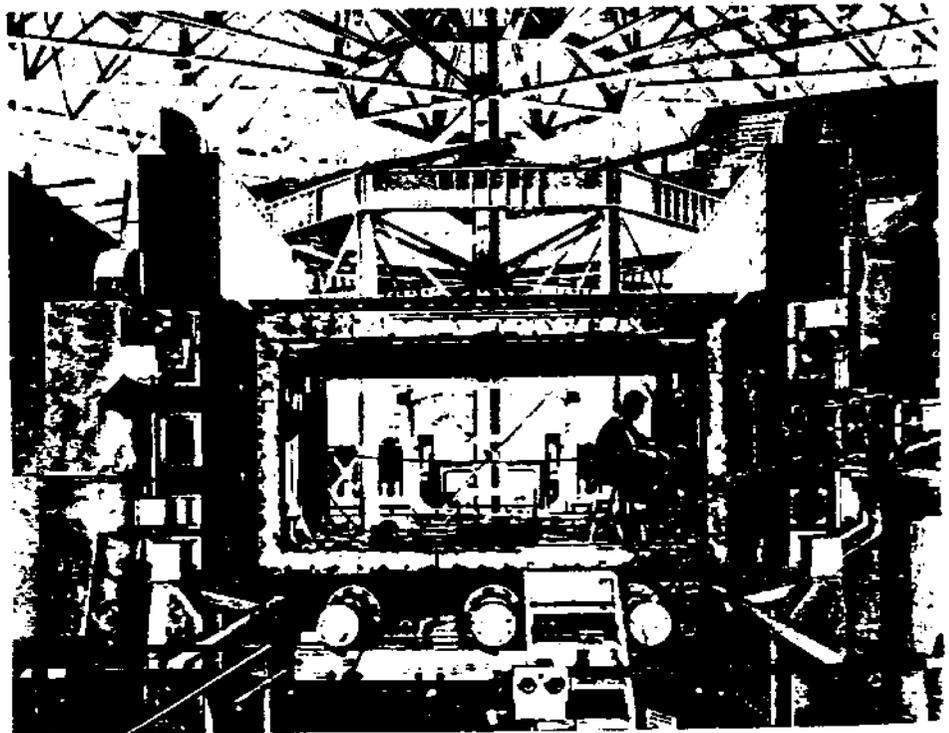
Bumper 6 Crop



Relative sizes, magnet weight, and cost of earliest particle accelerators at Berkeley. ES is the electron synchrotron, BV the Bevatron.

B ROBECK SET THE top energy of his Bevatron at 10 BeV because, he wrote in 1947, it seemed "the largest machine that could be made in the near future without departing from the techniques used on machines at present in operation." A more pertinent statement would have replaced "techniques" with "level of funding": at an estimated price of one to two million dollars for each billion electron volts, his accelerator would cost ten times more than the 184-inch cyclotron. The design presented many technical uncertainties. To help resolve them, the Laboratory built a model to quarter scale, which itself belonged in the same class as Birmingham's synchrotron (1.3 BeV, inaugurated 1953) and Brookhaven's Cosmotron (3 BeV, 1952).

Among the problematic features of the Bevatron's design was the size of the gap or aperture in which the magnets constituting the machine's backbone would confine the beam. Forty-four of these vertebrae made up a half doughnut with a mean radius of 18.2 meters; and two such halves, together with the straight sections that joined them, defined the raceway for the particles. (The Bevatron actually built has four curved and four straight sections.) One straight section served to admit the proton beam from a small linear accelera-

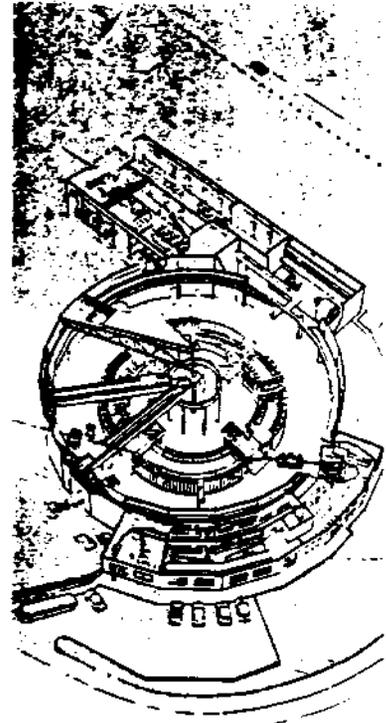


Fabricating one of the four straight connecting segments of the Bevatron.

*One or two million
dollars a BeV*

tor, which took its feed from a Cockcroft-Walton machine; in the final design, the particles gained 500 keV in the first stage of acceleration and another 9.5 MeV in the second. Brobeck explained that initiation in the Cockcroft-Walton machine would cut loss of the embryonic beam from internal scattering, and that acceleration by the linac would reduce the range of frequency and field strength through which the synchrotron's oscillator and magnets would have to function. The other straight section, which, like the first, would have no focusing magnets, was to contain targets and extractors for the accelerated beam.

Brobeck thought that the mechanisms for injection and extraction and the straight sections without magnetic guidance might cause the beam to oscillate widely around the median orbit through the doughnut halves. Accordingly, he provided for a large aperture between the magnet poles, some 4 feet high and 14 feet wide (in the radial direction); should the beam behave better than expected, the gap could be reduced by changing the pole tips. The plan sacrificed energy for intensity: the bigger the gap, the more particles would survive the many turns necessary to accelerate them; the smaller the gap, the greater the maximum field available to restrain them and the



Artist's conception of the Bevatron. The beam injector is at 4 o'clock, the experimental area and emergent beam at 8 o'clock.



Lawrence, Brobeck, Harold Fidler, and Donald Cooksey in the aperture of a Bevatron magnet section, 1950.

Lawrence set his sights on 6 BeV, the threshold for antiproton production

78

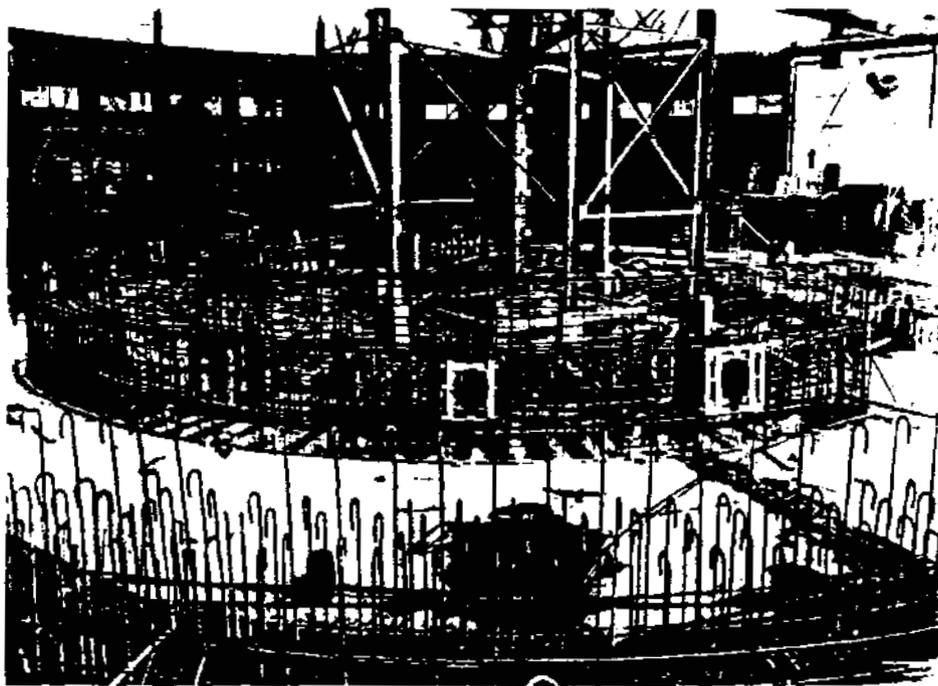


Edward Lofgren, physicist in charge of the Bevatron, examines it with its designer and builder, William Brobeck.

higher the attainable energy.

Despite its advertisement, Brobeck's machine with a 4 ft \times 14 ft aperture would have given protons of only 1.5 BeV. Already in the fall of 1947 a British visitor to the Laboratory reported that the plan provided far too large a hole; and in March 1948, at the fixing of the maximum energies of the Bevatron and Cosmotron, Lawrence set his sights on 6 BeV, the threshold for antiproton production. To reach it, however, the aperture that Brobeck planned would have to be reduced by a factor of 14, to 1 ft \times 4 ft. Lawrence preferred not to gamble against such odds, and, in ordering the steel for the magnets, bet conservatively on a gap 4 ft \times 10 ft. Experience with the quarter-scale model in 1949 inspired another reduction, to 2 ft \times 6 ft, for an energy of 3.67 BeV. That was the machine that the Bevatron construction group expected to build in 1950. Subsequent modification might have brought it to 6 BeV, a little above the expected threshold for the creation of the antiproton.

In December 1951 the plan changed again: the gap narrowed to 1 ft \times 4 ft; the Laboratory would reach directly for 6 BeV. The decision did not represent a return to recklessness. Experience with the quarter-scale model and with big beams and cyclotron design in the MTA project suggested that the Bevatron would steer protons more



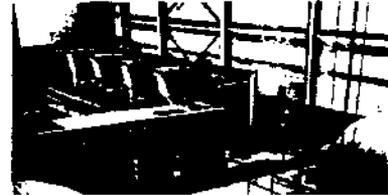
Preparation of the foundation for the giant Bevatron magnet.

1173520

*That stammerer,
history, repeated
itself*

79

accurately than anticipated. Calculations and operating results from Brookhaven's Cosmotron confirmed the inference. The upshot of the two-year interruption of work on the Bevatron by cold-war service was a handsome reward. When completed in 1954, the 10,000 ton synchrotron could accelerate well-behaved protons through 4,000,000 turns in 1.85 seconds without their deviating from the



Strange and Contrary Particles

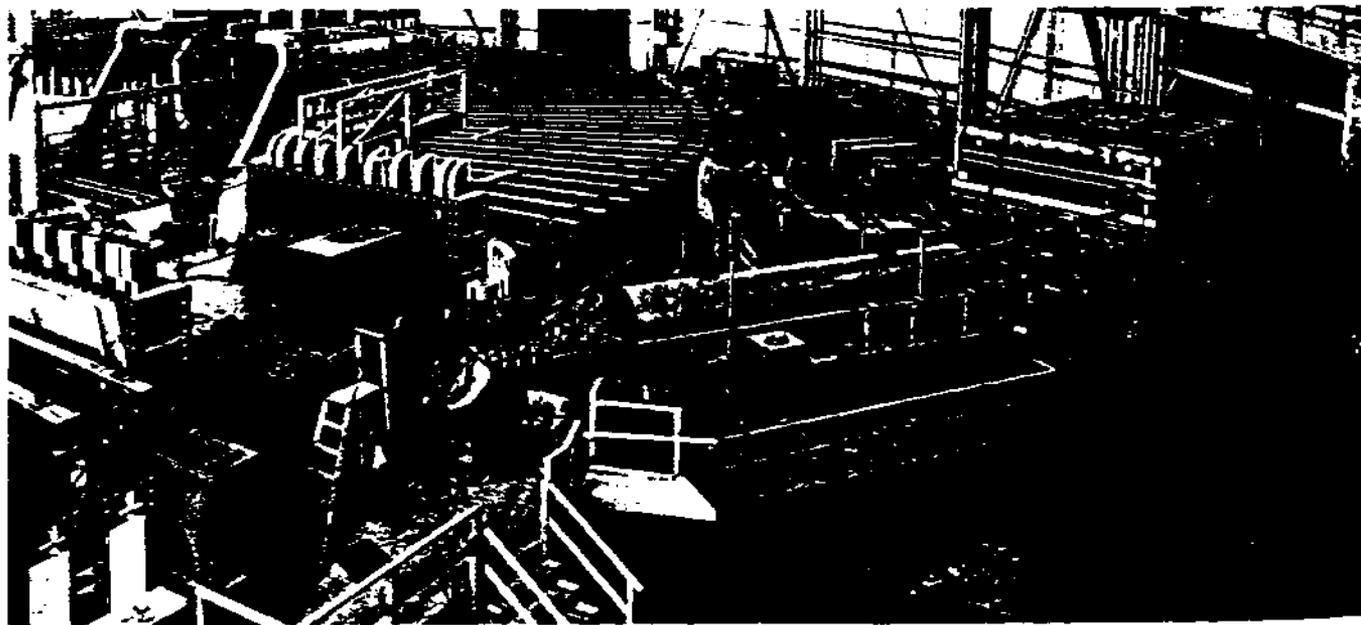
80

ON JANUARY 21, 1954 the operating crew began the first pumpdown of the Bevatron tank. It took a day to bring the huge system to 10^{-1} mm Hg, four days to reach $5 \cdot 10^{-5}$, another four to hit $3 \cdot 10^{-5}$. Many tiny holes were found, mostly in the welds and some too small to deflate a soap bubble. The second pumpdown, lasting 16 days, reached $1.5 \cdot 10^{-5}$ mm Hg. Four days into the evacuation, on February 2, the crew injected a pulse from the linac and led it around the tank. Two weeks later the first acceleration was achieved, for a few milliseconds; on April 1 the beam reached 6.1 BeV; a week later a generator shorted, and the great machine went down for two months.

When the beam came back, Lofgren's crew worked to increase its strength and reliability. By October 1954 intensity had reached 10^{10} protons/pulse, a hundred thousand times what it had been in June. The machine worked for a little over a third of its scheduled 55 hours/week, and of that third about a third went to physics research. Beginning in November, running time rose to two-thirds of scheduled time except for protracted shutdowns, which averaged about a week per quarter. The beam could deliver 11 pulses a minute at the redesign

energy of 6.2 BeV. The Bevatron, though still temperamental, had become available for research.

Most of the experiments performed between November 1954 and September 1955 concerned proton-proton scattering, pion production and scattering, and the life history of the K meson. A mixed pion and kaon beam became available by the end of the year. With fast electronic detectors, experiments could be done on the relatively few kaons present, about one for every 10^{10} protons incident on a polyethylene target in the Bevatron tank. That was enough to aggravate, and to help resolve, one of the oddest puzzles of high-energy physics. The puzzle had been set by cosmic-ray physicists. Since 1947 they had collected evidence for the existence of heavy mesons, each having about 1000 times the electron mass, and all quite different in manner of decay. When the Bevatron began to operate, six different sorts of K particles, as the mesons were called collectively, had been distinguished according to their descendents. The question arose whether physicists had to accept a half-dozen kaons as elementary, or whether the several sets of descendents represented not independent ancestors, but the several ways in which a single



A Cockroft-Walton accelerator (right) fed protons to an Alvarez linac (center) for injection into the Bevatron at 10 MeV.

1173522

indecisive one might divide its estate of energy. The physicists who discussed the matter at the fifth Rochester Conference on High Energy Physics in 1955 decided that "the situation was neither clear nor were all the answers easily forthcoming through cosmic radiation investigations." Hence the great utility of the Bevatron's kaon beam.

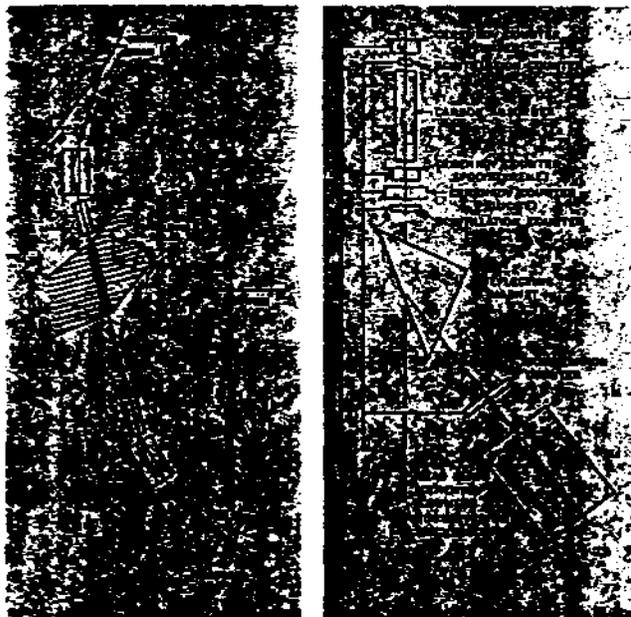
To help resolve the problem, several groups at the Laboratory sought to determine the masses and half lives of the several K particles or modes. Gerson Goldhaber of Segré's group and his associates exposed an emulsion stack to a stream of particles from a target in the Bevatron beam and examined the tracks of kaons that came to rest in the emulsion. They and a group led by R. W. Birge confirmed that all kaons had about the same mass, including the so-called tau meson, which decayed into three pions. Furthermore, they found that the lifetimes of the kaons, from creation in the target to

decay in the emulsion, were about the same for all sorts of K^+ mesons, except for tau, which they did not determine. Alvarez and his students confirmed the last finding another way, by detecting kaons and their products electronically; and, together with Sulamith Goldhaber, he made it plausible that the τ^+ lived about as long as the other K^+ mesons.

The confluence of these vital statistics strengthened the view that there is one sort of kaon with several ways of disappearing. But a weighty objection confronted this conclusion. One of the K^+ forms, called theta, disintegrates into two pions. As Richard Dalitz demonstrated, if θ and τ are the same, its two different decay modes, to two or to three pions, cannot both conserve parity. That nature makes no distinction in the parity, or mirror symmetry, in such decays was then firmly believed. Another puzzle confirmed at Berkeley: the behavior of K^+ mesons differs strikingly from that of K^- mesons. The positive form never, and the negative form often, provoke nuclear reactions giving off particles heavier than protons (hyperons). Yet both sorts of mesons have the same mass and lifetime.

Both these puzzles—the amazing similarity between τ and θ , and the equally odd dissimilarity between K^+ and K^- —helped prompt profound contributions to theory. Murray Gell-Mann and others introduced a new quantum number, hypercharge or strangeness, the selection rules of which prevented K^+ from participating in the sort of nuclear debauch the K^- enjoyed. Berkeley emulsion groups played a part in confirming Gell-Mann's systematics. As for the likeness of τ and θ , T. D. Lee and C. N. Yang declared it an identity and sacrificed parity in certain "weak interactions" involving mesons and hyperons.

The pursuit of kaons, though exciting and rewarding, had an air of déjà vu: once again a great accelerator made possible the detailed study of particles first found in cosmic rays. Another quest beckoned, the detection of a particle of fundamental importance then not yet found among nature's products. Several groups began to look for the antiproton early in 1955. Two hoped for a quick victory using detectors that had worked well for mesons. One, under Chaim Richman, stuck emulsions in a beam of negative particles from a metal target. Another, under Wilson Powell, used a cloud chamber. They both hoped to find evidence of the end of the career



Two detectors of anti-protons. At left is the arrangement Segré's group used successfully in 1955. M indicates bending magnets, Q focusing quadrupole magnets, S scintillation counters and C Cerenkov counters to eliminate false counts. At right is the Lofgren group's detector that analyzed the beam from Segré's magnets. The small Cerenkov counters distinguished the antiproton from a meson, the large one registered the annihilation of an antiproton with a proton.

of antiprotons in annihilation explosions. They found nothing.

Another alternative was to detect the negative proton directly by evaluating its momentum and velocity and thence deducing its mass. Lofgren's group had placed pop-up targets in the magnet aperture that could be thrust abruptly into the beam. Negatively charged shrapnel from the collision, primarily pions, came through a window in one of the straight sections of the Bevatron and thence through a bending magnet, which operated on the derived beam as a prism does on light. Segrè's group built a system of three magnetic quadrupole lenses to focus whatever antiprotons they might catch onto a suitable set of detectors. The other group aiming at direct detection, Lofgren's, also used the Segrè group's magnetic analyzer, which selected for a momentum of 1.19 BeV/c to within two percent. Concurrently, a team in Segrè's group led by Goldhaber sought evidence of the annihilation of antiprotons in an emulsion stack inserted in the analyzed beam.

Two methods of finding the velocity of particles in the analyzed beam presented themselves. One was to time their flight between encounters with two scintillation devices. The other used a series of Cerenkov counters that could determine v/c (v the particle velocity) to within a few percent. The Laboratory had pioneered the development of these agile counters. In unusually fruitful thesis work, completed in 1951, R. L. Mather had designed the

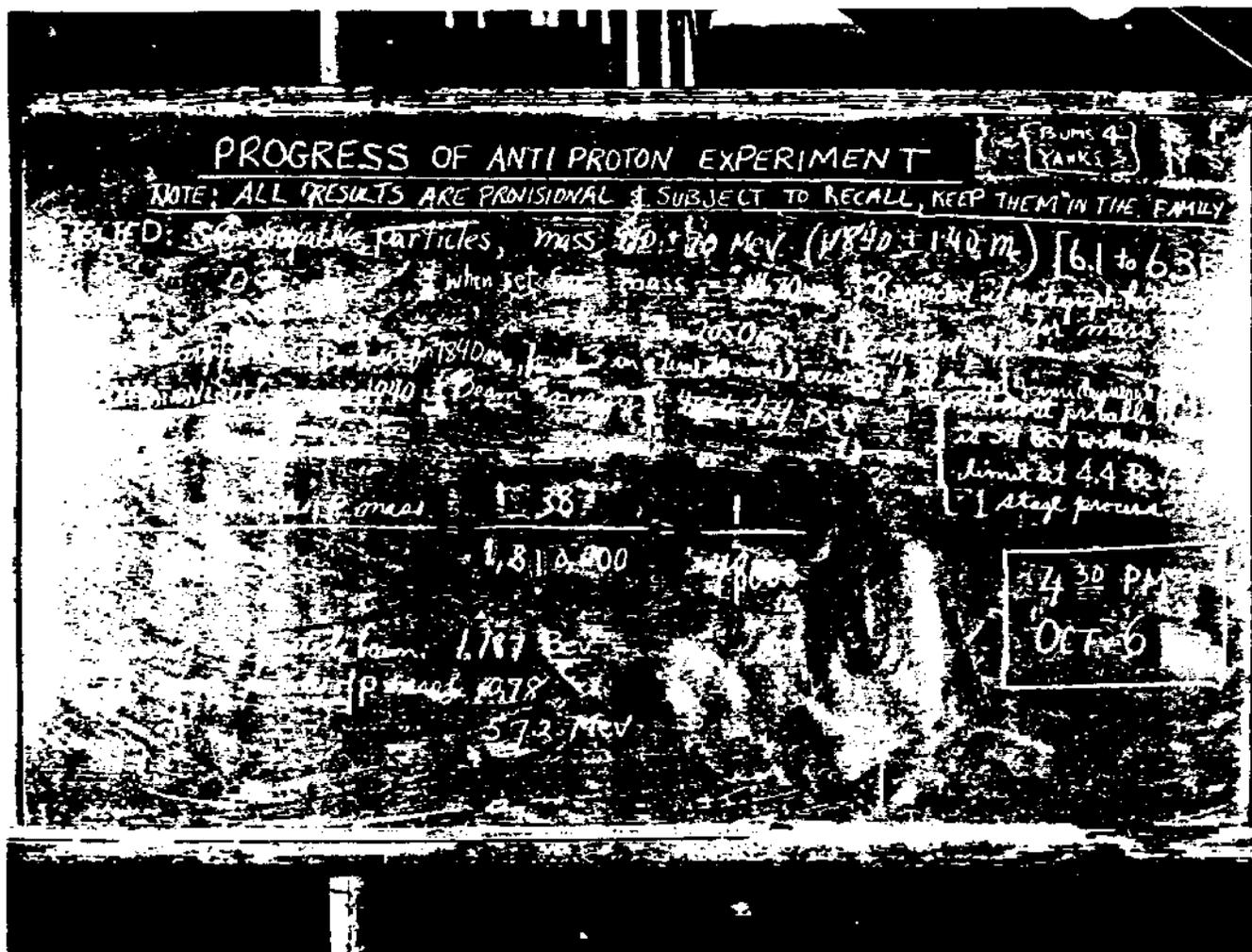
world's first Cerenkov detector for protons. The accuracy with which he could determine velocity recommended his system to Segrè, who was one of his advisors; together they used it to improve the range-energy estimates for the evaluation of the speeds of charged particles in emulsions and cloud chambers. The Cerenkov counter came into common use at the Laboratory to discriminate between particles with like charges and equal momenta but different velocities.

Lofgren's group used scintillation counters in coincidence merely to define the trajectory of a particle through their counter telescope. Three Cerenkov devices did the measuring work. Since the Segrè group's magnetic analyzers would select antiprotons with velocities just above $3c/4$, the telescope contained one counter, of polystyrene, with a threshold at $0.76c$. In order to discount the faster π^- and K^- mesons that pour through the analyzer, the telescope had Cerenkov devices of water and lucite with thresholds above $0.78c$. With the scintillation and polystyrene counters in coincidence and the water and lucite counters in anticoincidence, the telescope would register negative protons of appropriate velocity and reject the accompanying noise. After passing the third scintillation counter the now identified antiproton would enter a block of lead glass, where, if all went badly for it, it would annihilate itself and an ordinary proton, producing a shower of secondary fast particles that would light up the lead glass counter. After testing their telescope on ordinary protons, Lofgren's group set it looking for negative ones at the end of July 1955.

By then Segrè's group had constructed a more elaborate detecting system. Two scintillation counters fed a circuit that timed the passage of a charged particle between them, about 50 billionths of a second for an antiproton with $v = 3c/4$. Any one of the 50,000 negative pions that accompanied each antiproton traversed the same distance in even less time and could easily be distinguished from the heavier particle by the advanced coincidence circuitry designed by Clyde Wiegand. The arrangement also registered accidental coincidences when one meson tripped the second counter 50 billionths of a second after another had tripped the first. To rule out these spurious events a Cerenkov counter, with threshold above the velocity of the antiproton and below that



Antiproton detecting setup at the Bevatron, 1955



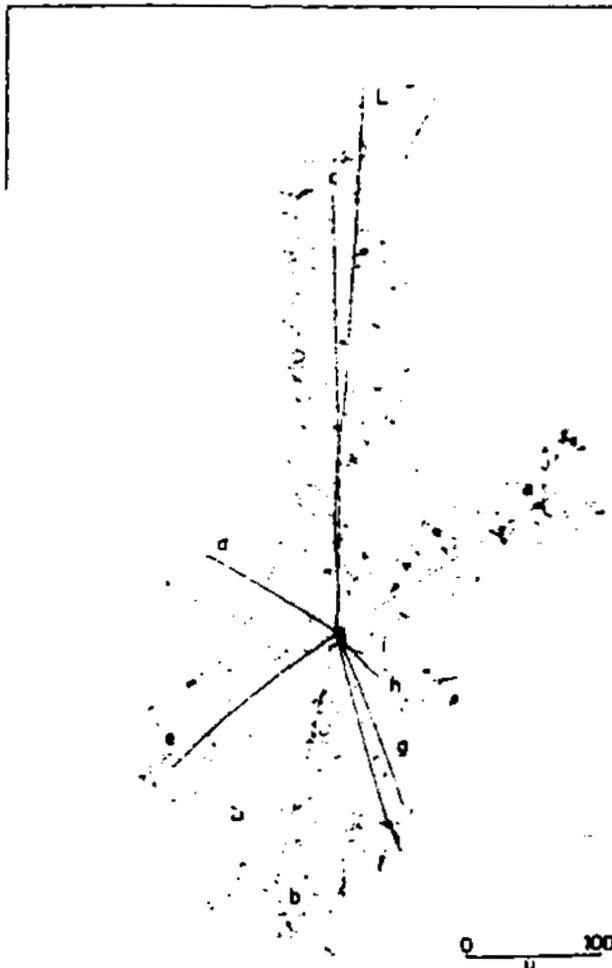
Progress of anti-proton experiment with telescope at the Bevatron

of the mesons, was placed in anticoincidence with the scintillation counters. (It played the part of the water and lucite counters in the Lofgren group's telescope.) A second Cerenkov counter, designed by Wiegand and Owen Chamberlain, had a mirror system that confined its counts to particles moving at between $0.75c$ and $0.78c$. By the first of August the detecting system of the Segrè group was ready for testing at the Bevatron.

Planning for that moment had started toward the end of 1954. Segrè's group acknowledged "very useful suggestions" concerning focusing that were then contributed by Oreste Piccioni of Brookhaven, an expert on quadrupole magnets and beam extraction,

who visited the Laboratory in December and the following January. In April 1955, on Alvarez's initiative, Piccioni was invited to join Lofgren's group, which he did in September, hoping to try "a pet experiment based on time-of-flight with counters... discussed at length with Segrè, Chamberlain, and the others [of Segrè's group]." Meanwhile Segrè and his colleagues had proceeded. When Piccioni arrived at Berkeley it was only a question whether the antiproton would be sighted first through the telescope of Lofgren's team, or through the more sensitive combination of counters of Segrè's group, or in its death throes in their emulsion stack.

Time on the Bevatron did not come for the asking

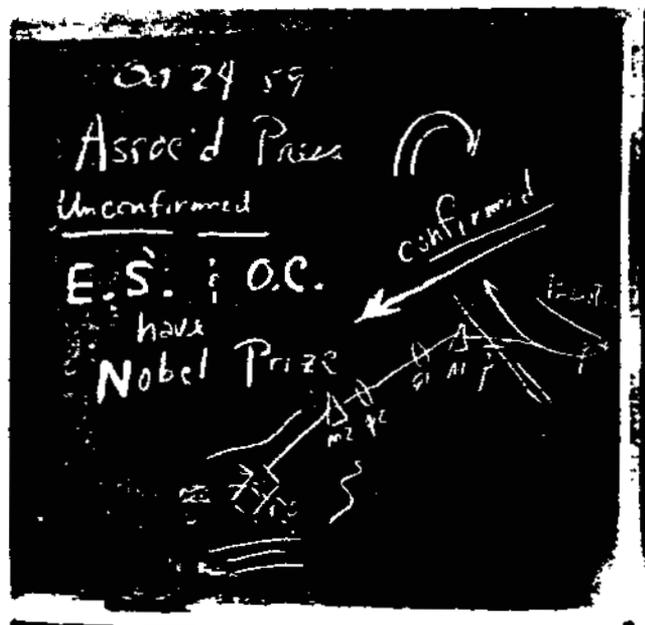


The first annihilation star ("Faustina") of an antiproton found in film exposed by the Segrè group, 1955.

The Laboratory physics division set priorities for the big machine to which its users conformed in negotiating schedules under Lofgren's diplomatic management. Various contingencies affected the implementation of the proposed schedule; the machine might not work, the appointed group might not be prepared, the preceding experiment might be prolonged. Log sheets from the earliest days of physics research on the Bevatron show both the ideal and the real worlds, the scheduled experiments and those performed. During the first week of August Segrè's group was scheduled for three of the six days of Bevatron operation, and ran for five; during the second and third weeks it had no time, while Lofgren's and Powell's groups sought antiprotons in

their own ways; on August 29 Segrè's group returned and ran as scheduled until the Bevatron broke down on September 5. On the 21st, a week after operating crews had revived the machine, Lofgren's group was to begin a four-day hunt for the antiproton. Instead it ceded its time to Segrè's group, which immediately got its first antiproton counts. For the next month the entire research effort at the Bevatron went to confirming and extending the counts. The physics division decreed that Segrè's equipment would remain in place indefinitely; and money was found to increase nominal operating hours from 65.5 to 81 a week.

The experiments following up the Segrè group's discovery of the antiproton centered on a search for the cataclysmic explosion that should mark the end of the career of an antiproton captured by an unfortunate nucleus. Lofgren's and Moyer's groups joined forces to find the sort of evidence of annihilation in a Cerenkov counter that Lofgren's group had sought earlier. Segrè's group pressed forward with the scanning of emulsion stacks in collaboration with a group under Edoardo Amaldi in Rome. The Rome team found the first annihilation star, whose visible energy (the combined energy of all ionizing frag-



An antiproton Nobel Prize announcement on a blackboard.



Surrounding Edward Lofgren (center) are discoverers of the antiproton (left to right) Emilio Segrè, Willard Libby, Robert Chamberlain, and Thomas Ypsilantis.

ments) amounted to above 826 MeV, an amount deemed appropriate for an explosion initiated by an antiproton. Other persuasive terminal events did not come to light before the Segrè group's equipment was removed just before Christmas to make way for scheduled experiments on K mesons. A subsequent exposure to a beam of slower antiprotons, some of which ended their lives in the emulsion, created stars more generously, including one with a visible energy greater than the rest energy of the proton. The large group by then engaged in the experiment recommended their observations as a "conclusive proof that we are dealing with the antiparticle of the proton." The discovery brought Segrè and Chamberlain

the Nobel prize in physics for 1959.

The Segrè group had announced their discovery of the antiproton by electronic counters in a letter sent to the *Physical Review* on October 19, 1955. The previous day Lawrence and Willard Libby, acting chairman of the AEC, had informed a wider audience. They intimated that the discovery, by fulfilling "one of the important purposes" for which the Bevatron was built, justified the expenditure. Nine months later the machine's steward, Lofgren, announced that the Bevatron was "obsolete in design and in a few years will not even be in the class of high energy physics." It was time for the next turn in the spiral of high energy.



The Lab as it appeared about 1955. The Bevatron occupies the central round building, the 184-inch sits under the dome above that.

Laboratory contributed significantly to the improvement of the new detectors and their ancillary electronics.

For some purposes, however, the *via nova* could not replace the slow old way, the cloud chamber and the emulsion. In particle physics as elsewhere a picture may be worth a thousand words or clicks. Students of the tracks of ionized particles had discovered the positron and the mesotron, and cleared up the mess about the mesotron and the Yukon. In 1949 one of Powell's emulsions disclosed still another particle, about half as heavy as the proton, the K meson or kaon. Its strange behavior invited study, its relatives, if any, detection. One of the first achievements of Lofgren and his operating crews was to cause a mixed beam of kaons and pions to issue from the Bevatron. But to realize the full promise of kaon beams and other projectiles from the Bevatron a track detector faster than the cloud chamber and more discriminating than the emulsion was needed.

One day in 1952 Donald Glaser and some colleagues at the University of Michigan were doing physics in a saloon. Someone observed that a stream of beer bubbles made a nice track. Glaser took the suggestion seriously and sought a process in a liquid that could register the path of a charged particle and then quickly expunge the marks. He thought that bubbles might be formed in a superheated liquid much as condensation droplets arise in a cloud chamber. He was right, and in April 1953 he showed a meeting of the American Physical Society pictures of tracks made by cosmic-ray muons crossing a small vessel filled with hot ether.

Alvarez learned about Glaser's device at the meeting and set about developing it in much the same way that Lawrence had transformed Wideröe's accelerator. To make a suitable particle detector, Alvarez had to transcend the intrinsic limitations of Glaser's technique, create several new technologies, walk confidently through unknown terrain,

*One day in 1952,
Donald Glaser and
some colleagues
were doing physics
in a saloon*

Bubbles in a sea of protons

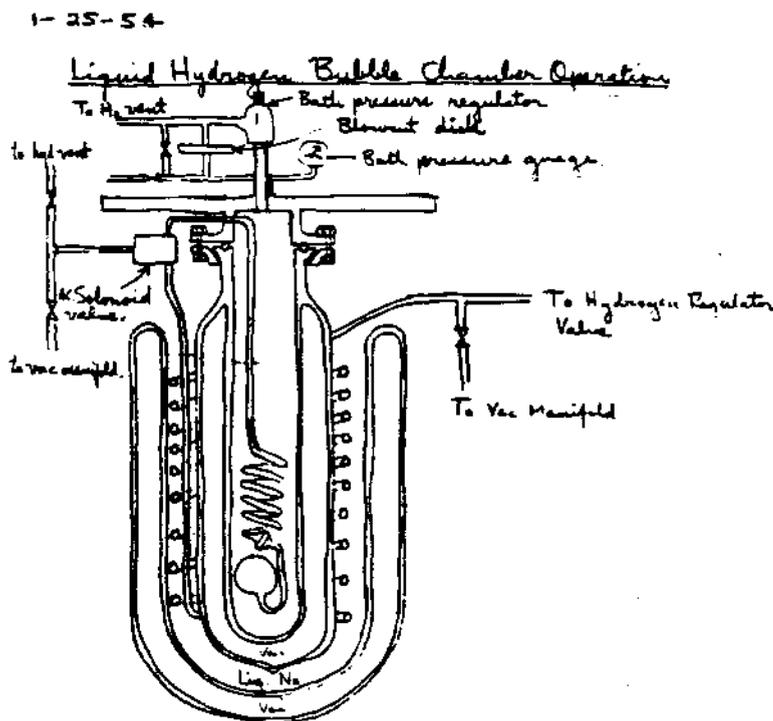
and raise a lot of money.

The first step, the substitution of liquid hydrogen for superheated ether, reduced the detecting medium to one both homogenous and simple, a sea of protons. It brought with it the technical problem of handling large amounts of liquefied gas under high pressure at 20° above absolute zero. By the end of 1953, John Wood of Alvarez's group had made a chamber an inch and a half in diameter and found tracks in liquid hydrogen. He also found that accidental boiling did not impair formation and photographing of the tracks. The point was of great importance: Glaser and others had supposed that useful records can occur only in vessels with smooth glass walls, which give no purchase for formation of unwanted bubbles.

Freed from the size constraints imposed by all-glass vessels, Alvarez's group built a second chamber, 2.5 inches in diameter, with a metal body and glass windows, which worked so quickly that bubbles made at the walls did not reach the active volume before the tracks were photographed. The third model, 4 inches in diameter, began operation at the Bevatron on November 19, 1954. It played a part in chamber development similar to that of the 37-inch machine in cyclotron design. The chamber analogue to the 60-inch cyclotron



Bubble-chamber inventor Donald Glaser examines a xenon chamber built at LBL in the early 1960s.



Sketch of the first liquid hydrogen bubble chamber (1.5-inch diameter), built by John Wood and A. J. Schwemin in 1954.

1173529

*"I don't believe in
your big machine,
but I do believe in
you"*

88



First tracks observed in liquid hydrogen by John Wood, 1954.



Luis Alvarez with Berkeley-built bubble chambers.

measured 10 inches, and, like it, was planned by engineers—in fact by eleven members of the Laboratory's engineering department—as well as by physicists. The treatment of the large amounts of liquid hydrogen, about six times as much as for the 4-inch chamber, became a special study. It was pursued with the help of the National Bureau of Standards' Cryogenics Engineering Laboratory at Boulder, Colorado, which had been established to help prepare liquid deuterium and tritium for the Eniwetok test of the proto-hydrogen bomb in 1952.

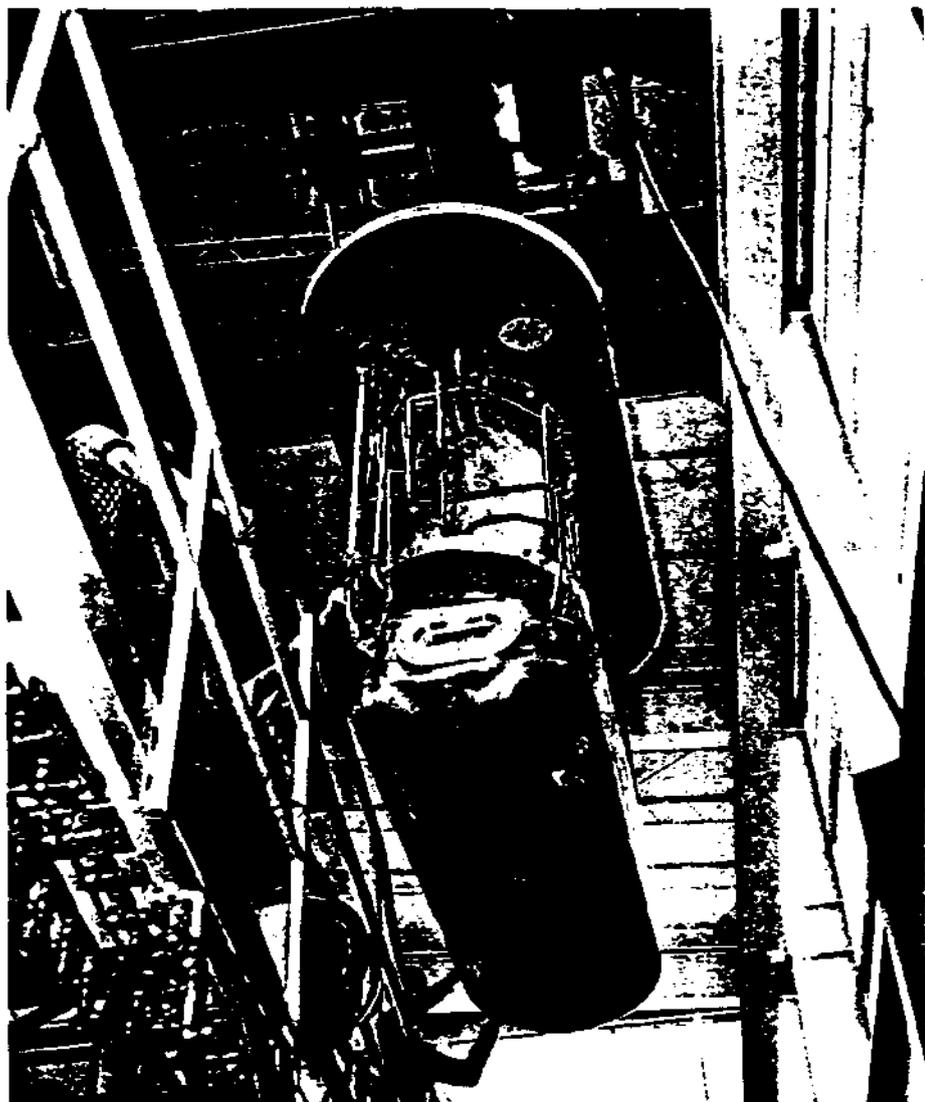
While the 10-inch instrument neared completion, Alvarez planned its successor. On January 10, 1955 he proposed a 30-inch rectangular chamber to Lawrence and his senior associates. They agreed, provided development took place "with a minimum of interference" with accelerator work. On reconsideration, Alvarez decided to extend the chamber to 50 inches, long enough to observe the behavior of decay products of new and problematic hyperons. Further consideration altered the dimensions to 72 in. × 20 in. × 15 in.

With this design Alvarez's group jumped from the equivalent of the 60-inch cyclotron to the Bevatron without the benefit of an analogue to the 184-inch synchrocyclotron. The window of the proposed chamber, 8280 cubic inches of optical glass, would have to withstand 100 tons of pressure. The plan required large and dangerous volumes of liquid hydrogen, a huge refrigeration system, metals capable of bearing enormous stresses at temperatures below -253°C and, because only one window was provided, an optical system to illuminate and photograph the interior of the chamber from one side. The plan astonished Lawrence. Even he, accustomed to building on or just over the edge of technology, doubted the bubble chamber could be scaled up from 10 inches to 72 in a single step. "I don't believe in your big machine," Alvarez recalls his saying, "but I do believe in you, and I'll help you to obtain the money."

Alvarez accompanied Lawrence on his next visit to Washington, where they lobbied AEC commissioners Lewis Strauss, Willard Libby, and John von Neumann. In addition to the chamber, the supplicants asked for money to develop a machine capable of reading photographs of tracks and feeding the information into a computer. If the big chamber worked, it would generate data far faster than the unmechanized physicist could digest it. The AEC granted the \$750,000 originally estimated. By the end of the year, increases in costs of special equipment, the analyzing magnet, and safety measures had driven the price to \$1,250,000, not including \$200,000 for an IBM computer to help with data reduction.

A team of engineers and physicists led by James Gow and Paul Hernandez took four years to create the chamber. The optical window, the largest piece of clear optical glass then in existence, was pol-

1113530



The 72-inch chamber removed from its instrumentation.

*Damn the
torpedos — full
speed ahead*

ished by a manufacturer of telescopes. The Rad Lab built the magnet, 115 tons of steel and 20 tons of copper giving 18 kilogauss. The chamber itself, 3.25 tons of austenitic stainless steel, held 12.5 cubic feet of liquid hydrogen. After many trials, Alvarez and Duane Norgren devised the critically important method of one-sided photography, the "coathangers" retrodirective illumination system. A 15-inch prototype was built to test the optical and refrigerating systems. "It is obvious from whom I learned the 'damn the torpedoes—full speed ahead' attitude that my colleagues and I took in the development of large bubble chambers," Alvarez recalled, tracing his lineage to

1173531

From left to right, H. P. Hernandez, McMillan, L. W. Alvarez, and J. D. Gow, standing before the 72-inch bubble chamber.

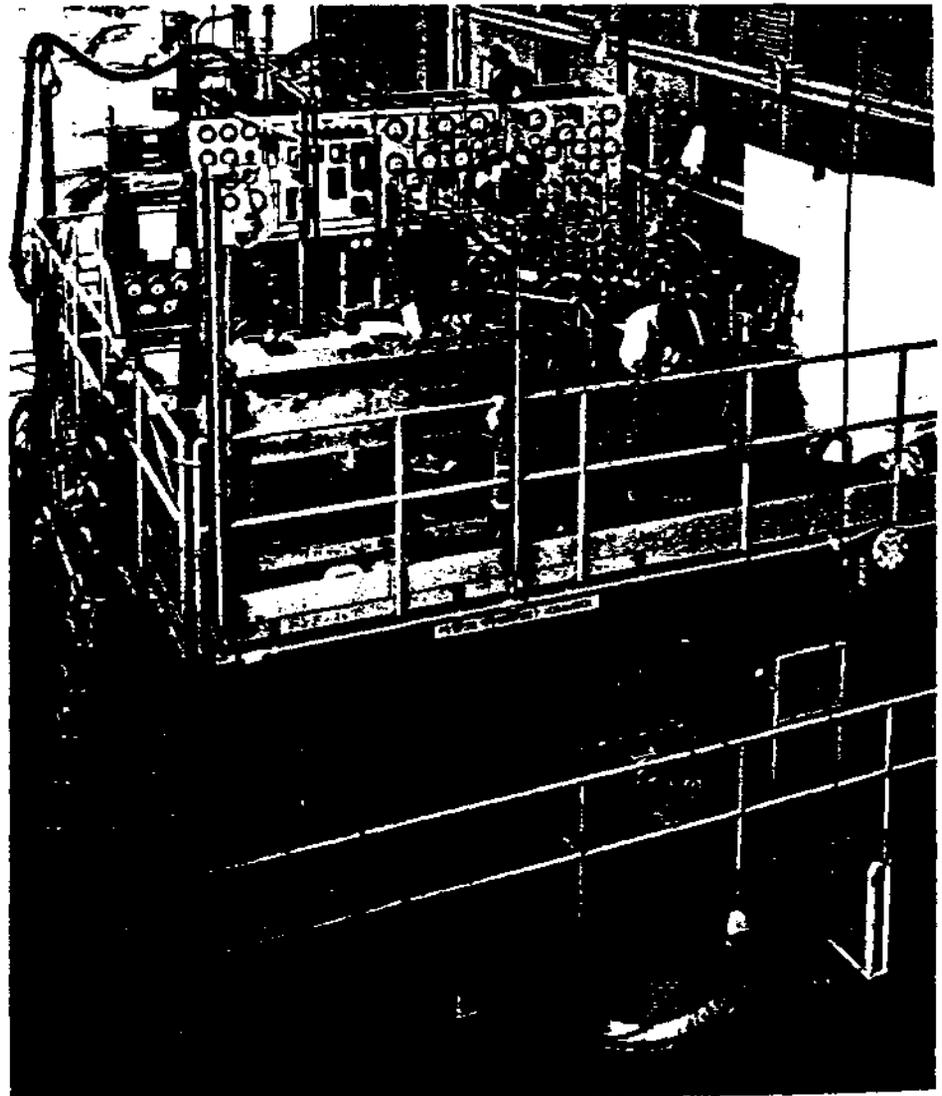


90

The bubble chamber walked to its home near the Bevatron

Lawrence. "It resulted in [our] having an operating 72-inch bubble chamber before Brookhaven—our most serious competitor—even had their 20-inch bubble chamber."

The 72-inch detector was finished in March 1959. Weighing 240 tons without its refrigeration system, it walked from its place of assembly to its home near the Bevatron on elephant-like hydraulic feet. Its new building had 7500 square feet of space, shop facilities, a crane, two compressors, and safety facilities including a big sphere to catch deuterium released from the chamber in an emergency. A three-megawatt motor-generator set supplied the magnet. The final



The 72-inch liquid hydrogen bubble chamber in its home, building 59.

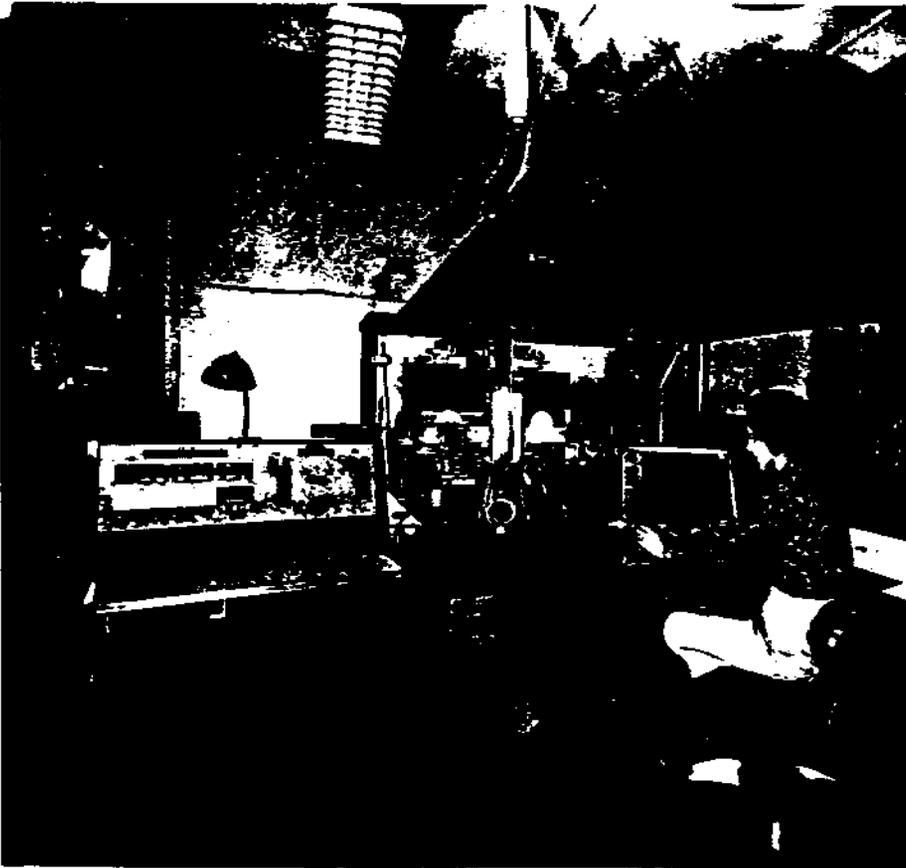
1173532

cost of the project: \$2,100,000.

The data reduction program kept pace with the construction of the big chamber. The first successful device, the "Franckenstein" created by a team directed by Jack Franck, worked with stereo pictures from the smaller chambers. The monster projected the tracks, measured them, and punched the results on IBM cards. Its operator first aligned an optical index with the projected track. A scanner, consisting of a photomultiplier and an electronic time discriminator, locked onto the track and directed its own motion along it; the operator controlled the speed of motion and periodically registered track coordinates by pressing a button. The machine could measure five to ten events an hour; the standard method for reducing cloud-chamber data could manage one a day. Franckenstein made many friends when introduced at the Atoms for Peace Exhibition in Geneva in 1958.

An IBM 650 computer, acquired by the Laboratory in 1957, and an

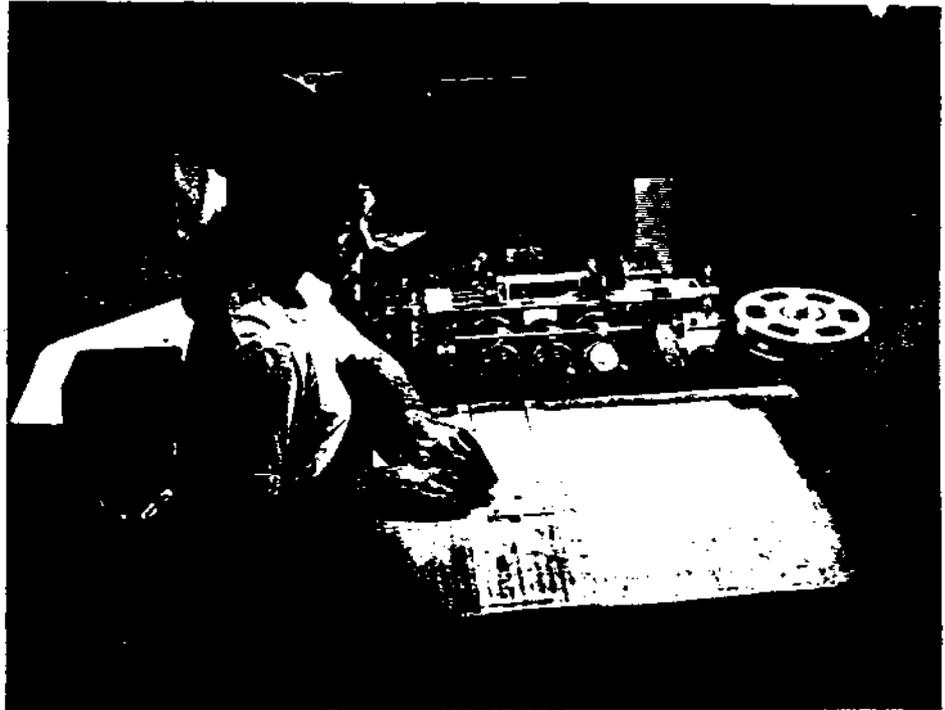
*The Franckenstein
and the McCormick
Reaper*



Jack Franck's "Franckenstein" reduces bubble-chamber film to machine-readable data.

1173533

*In 1968 the team
could measure and
analyze one and a
half million events a
year.*



Operator maps particle tracks with Alvarez Scanning and Measuring Projector.

IBM 704 on the Berkeley campus completed the data analysis and the first bubble-chamber system. A team led by Frank Solmitz and Arthur Rosenfeld created the programs that reconstructed the tracks supplied by Franckenstein and compared them with those of hypothetical interactions. It took seven physicists two years and more to train IBM's electronic brains to interpret impulses sent through Franckenstein's optic nerve. It was none too soon. In an experiment lasting several months, pions passing through the chamber produced some 80,000 hyperons and 4,000,000 other interactions of possible interest. Still, two machines and 30 persons could analyze only 200 events a day, a very small fraction of the Bevatron's bounty.

The "McCormick Reaper," devised by Bruce McCormick soon after Franckenstein came to life, had the potential to enlarge the harvest. The photomultiplier of its traveling sensor was to send signals directly to a computer. The scheme was so much in advance of the computer art of the day that the project had to be abandoned. A second attempt to realize the potential of the spiral-scan method also foundered on technical difficulties. But in 1963, on the third attempt, with Jack Lloyd as chief engineer, the Spiral Reader began its work. The number of measured events jumped from 80,000 in 1962 to



The semiautomatic Spiral Reader could analyze up to 150 bubble chamber events an hour.

300,000 in 1965. By 1968 the Alvarez team could measure and analyze 1.5 million events a year. To keep pace, the Laboratory updated and expanded its complement of computers: an IBM 709 in 1960, a 7090 in 1961, a CDC 6600 in 1966, another in 1967, and so on. The 1,500,000 events measured in 1968 were about a thousand times as many as the Laboratory could have handled twenty years earlier.

The bubble-chamber films contain many items of the first importance. Probably the most exciting event in theoretical physics in 1956 was the discovery by T. D. Lee and C. N. Yang that parity does not hold in certain cases. Confirmation came first from examination of beta decay by C. S. Wu and others at the National Bureau of Standards. At the Laboratory, F. S. Crawford, M. L. Stevenson, and others in Alvarez's group passed pions from the Bevatron into their 10-inch chamber and observed that the decay of the resultant Λ

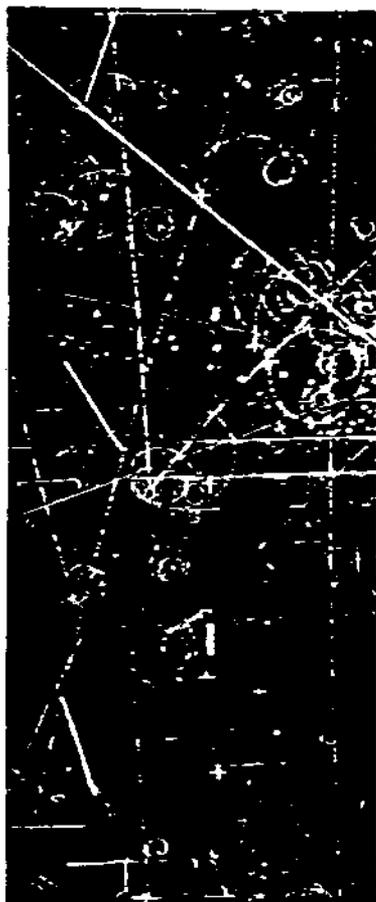


A view of tracks in the window of the 72-inch bubble chamber

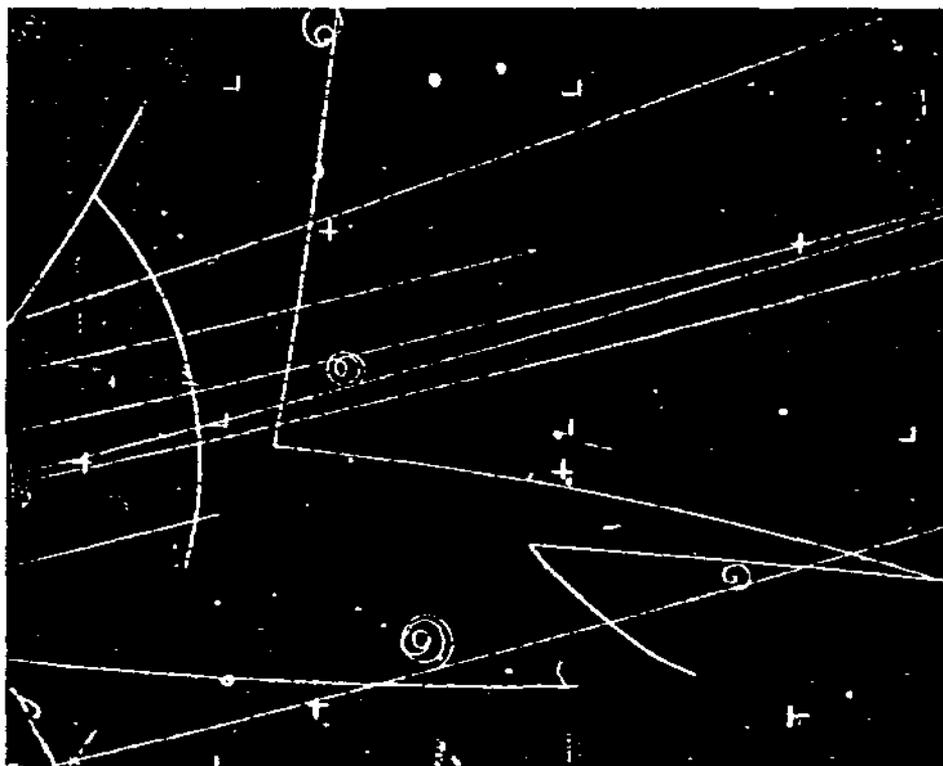
1173535

*The bubble chamber
films contain many
items of the first
importance*

94



A negative kaon entering from below produces an uncharged kaon and an uncharged Ξ^0 that, in turn, decays into two uncharged particles ($\Lambda + \pi^0$). The dotted lines in the inset follow the trackless participants.



A pion entering from the left and striking a proton produces two uncharged particles (K^0 and Λ) that leave no tracks until they too decay.

hyperons (created by negative pions striking protons) also violated parity. They found in the bargain that Λ decay does not respect charge conjugation, which requires a reaction involving a set of particles also to hold if each member of the set is replaced by its antiparticle. The experiment had the inconvenience that both the Λ and the kaon produced with it have no charge, and so can be detected only by their decay products. The relative ease and frequency with which these ghost-like occurrences could be found on bubble chamber records were a striking demonstration of the value of the new detector. A similar performance occurred in one of the first experiments run with the 15-inch chamber. Alvarez and his associates admitted negative kaons into the vessel and uncovered a new particle, the Ξ^0 , although neither the Ξ^0 nor its decay products ($\Lambda + \pi^0$) nor the particle created with it (K^0) leaves a track in the chamber.

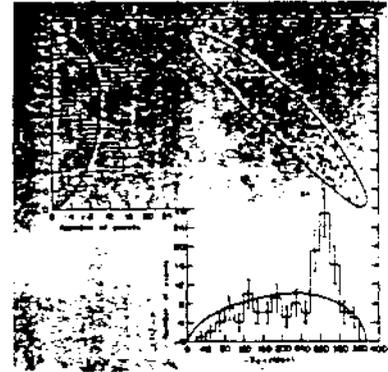
Perhaps the farthest reaching of the discoveries made with the Bevatron were the so-called "resonances" or energies at which fleeting combinations of particles occur. The first case found at Berkeley (Fermi had noticed one earlier) concerned the Λ hyperon and two pions. Bogdan Maglich's plot of the numbers of the two pions

against their kinetic energies showed a strong peak, where, it was supposed, the total energy of the Λ and one of the mesons allowed them to stay together for the time it takes light to travel a few nuclear diameters. They called this brief encounter (or the compound constituted by it) the $Y^*(1385)$, the number signifying its resonant energy. It aroused great interest when reported at the Rochester Conference on High Energy Physics in 1960, for it implied the possibility of creating a spectroscopy for the heavier elementary particles.

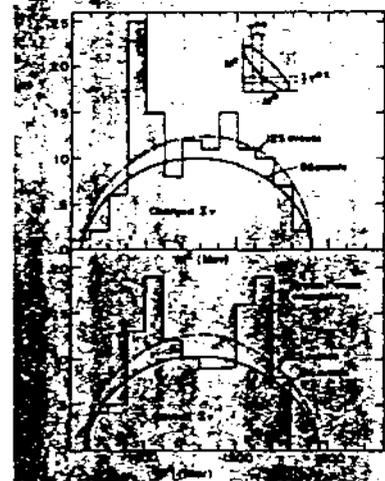
In the ensuing rush, the Berkeley group, working with the 15-inch chamber, found the first kaon resonance, $K^*(890)$, and another hyperon one, $Y^*(1405)$, and still others; and some were detected elsewhere using film from the 72-inch chamber, for example, the $\Xi^*(1530)$, discovered by Harold Ticho of UCLA. It has a special interest, since it, like the K^* and Y^* , perfectly fit the predictions of the "eight-fold way," a scheme of particle spectroscopy developed independently by Murray Gell-Mann and Yuval Ne'eman. From the masses of the $Y^*(1385)$ and the $\Xi^*(1530)$ and the rules of the way, the mass or energy of another particle, the $\Omega^-(1676)$, could be inferred. Since this particle, named in the belief that it would be the last of its kind, would confirm a central point in the eight-fold way, it was eagerly sought. Unfortunately its creation lay beyond the capabilities of the Bevatron; it was found at Brookhaven in 1964.

The great success of the liquid hydrogen bubble chamber overshadowed advances in detectors made elsewhere in the Laboratory around 1960. Wilson Powell's group, for example, made a 30-inch propane bubble chamber, the output of which they analyzed with their own computer programs. Clyde Wiegand of the Segrè-Chamberlain group and others continued to shorten the resolution times of counting systems. W. H. Barkas' group automated measurement and analysis of nuclear emulsions. A team under W. A. Wenzel of the Lofgren group introduced spark chambers to the Laboratory. This technique, first used successfully in 1959, exploits the sparks that mark the passage of a charged particle between closely spaced parallel electrodes. An automatic scanner for the spark chamber was devised by Denis Keefe and Leroy Kerth.

The rise of detecting equipment to something like the status of the accelerators they served may be traced in the organization of the Laboratory. New groups with special missions appeared: one for bubble chamber operation and development under Gow, another for data analysis development under Hugh Bradner, a third, under H. H. Heckman, to advise on photographic problems. An electronic computer group developed under David Judd. The scanners too were organized into teams, their outputs compared and their operations



Evidence for the first Berkeley "resonance," a brief combination of a Λ particle and two pions called $Y^*(1385)$.



Correlation evidence for the $Y^*(1405)$ hyperon resonance.



Wilson Powell (on ladder) with his propane bubble chamber



Laboratory physicist Denis Keefe with spark chambers developed in the 1960s for particle identification



Lunch during the International Conference on High Energy Physics, 1966. Left to right: McMillan, Val Fitch, Murray Gell-Mann, Victor Weisskopf, Geoffrey Chew, and Sidney Drell.

97



None gambled in the Berkeley style

Physicist Angelina Galtieri consults log with operator in control room of the Bevatron.

analyzed. Particle detection in the age of the bubble chamber came to resemble factory production.

Other institutions followed the Rad Lab's lead. In April 1958, at an international meeting at Imperial College, London, the British declared plans for a 60-inch chamber and the Russians and the CERN physicists contemplated going to 80 inches. They had not yet accomplished much by Berkeley standards. CERN had a working chamber of 4 inches, Italy one of 6 inches, and the Soviet Union one of 15 inches that did not function. The Alvarez group helped by distributing Bubble Chamber Engineering Notes all over the world. As Gow found out at the meeting at Imperial College, however, the Europeans tended to design very conservatively. None gambled in the Berkeley style.

1173539

The **7** End of **7** the Beginning

98



Edwin McMillan became director of the Laboratory when Lawrence died in 1958.



Andrew Sessler, director from 1973 to 1980, widened the Laboratory's research interests to include energy and environment studies.



David Shirley, formerly head of the Materials and Molecular Research Division, became Laboratory director in 1980.

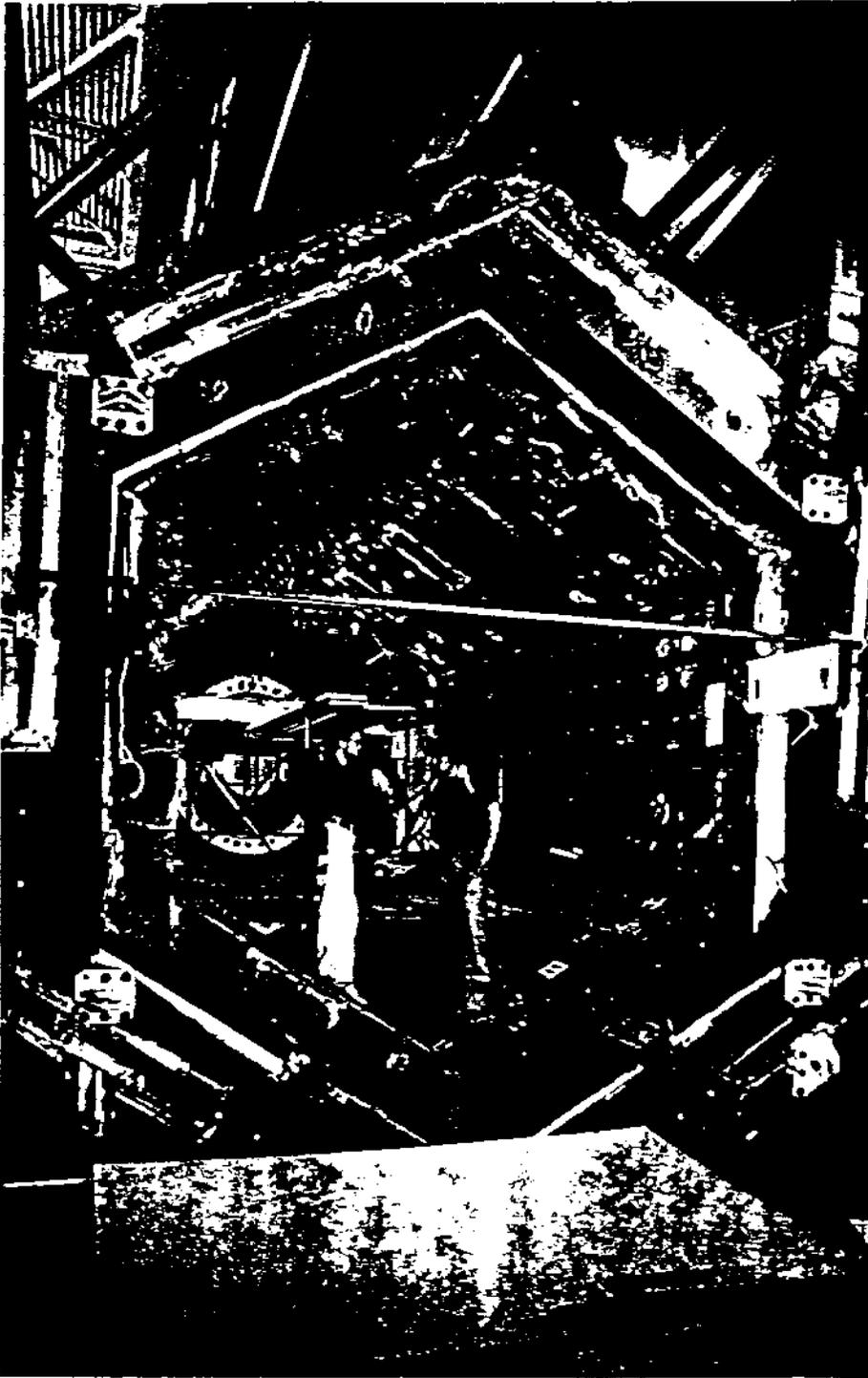
IN THE SPRING of 1958 Lawrence suffered a serious recurrence of his chronic colitis. Although already failing, he accepted against advice and as his duty President Eisenhower's request that he serve as a technical expert in talks with the Soviet Union toward a ban on testing nuclear weapons. Briefings in Washington and the journey to Geneva proved too much. He was rushed back to a hospital at Stanford, where he died on August 27. His passing ended an era in the Laboratory's history. His confidence and extravagance, enthusiasm and ingenuity, energy and entrepreneurship, could not be duplicated. Nor perhaps would his way have fit the future as well as it had the past. Events in the 1960s would call into question the values he held dear—science, technology, industry, growth, and patriotism; and changes in national priorities would shift the center of gravity of the Laboratory's work.

As Lawrence's successor the University chose Nobel laureate Edwin McMillan, a leader in high-energy physics and accelerator design and associate director for the Physics Division. McMillan served until 1973, when the Lawrence Berkeley Laboratory had become a collection of interdisciplinary groups working in fields as diverse as metallurgy, catalysis and surface science, electron microscopy, theoretical chemistry, photoelectron spectroscopy, earth sciences, hydrology, physical chemistry, cellular biology, oncology, and laser chemistry and biology. His successors, Andrew M. Sessler (1973-80) and David A. Shirley, have presided over further diversification as the conservation and development of sources of energy became a concern of the Laboratory's patron, the Department of Energy.

These new opportunities opened while funding of high-energy physics leveled off, and the most recent big accelerator built in the United States, which the Laboratory had designed and expected would come to Northern California, was under construction in Weston, Illinois. Although in consequence high-energy physics declined in relative importance at the Laboratory, substantial contributions to the field continue to come from Berkeley. For example, the Laboratory has collaborated closely with the Stanford Linear Accelerator Center (SLAC), where a 20 BeV electron linac began to operate in 1967. Together they have designed and built a positron-electron colliding beam ring (PEP) that will provide collision energies exceeding those of the Weston accelerator. Laboratory staff have also constructed several advanced particle detectors at SLAC, the latest of which, now just operational, is the novel Time Projection Chamber. By exporting its talent and technique, the Laboratory has maintained a place at the frontiers of high-energy physics.

Accelerators now operating at the Laboratory have been designed or adapted to give beams of heavy ions. In the early 1950s, following

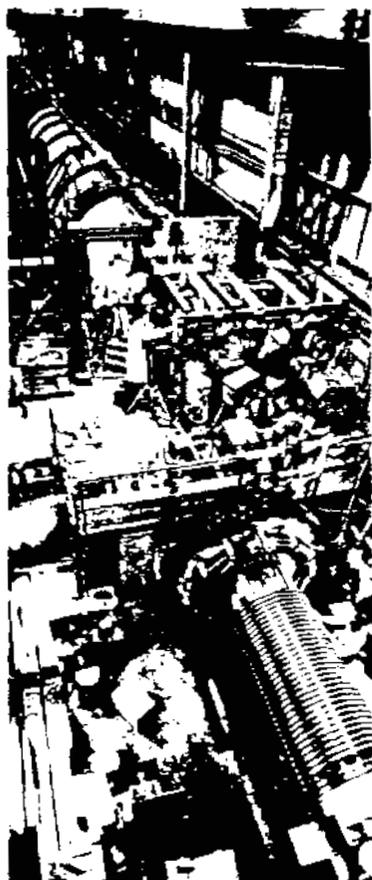
1173540



The Time Projection Chamber (TPC), shown with inventor David Nygren (left), was designed by LBL physicists for use at PEP, the positron-electron colliding beam ring at Stanford.

1173541

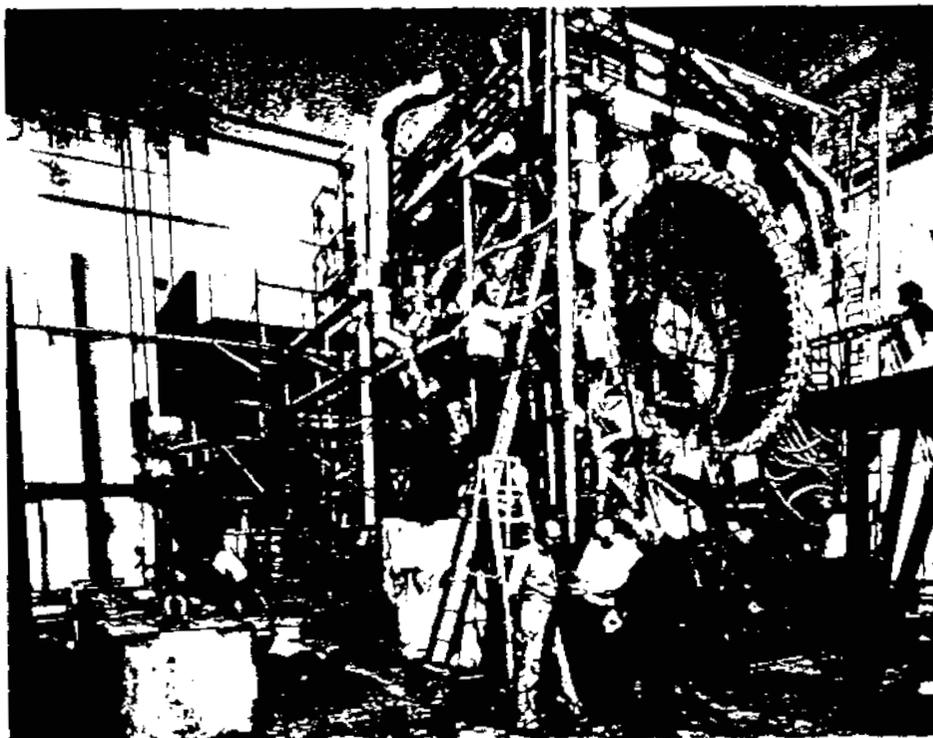
*The HILAC
incorporated
technology
developed for the
MTA*



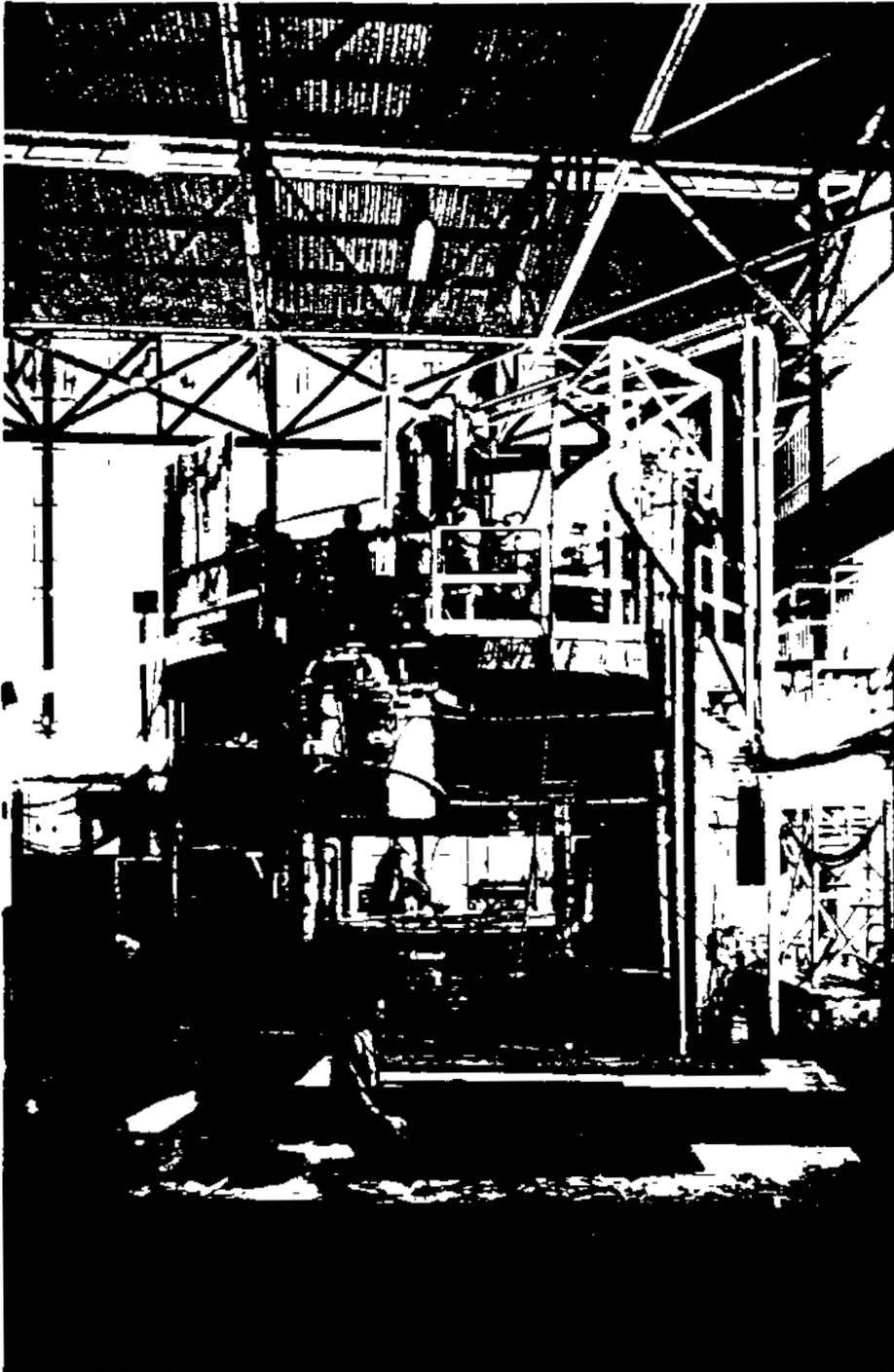
The SuperHilac, successor to the HILAC, accelerates beams of heavy ions.

up work begun just before the war, the Laboratory experimented with beams of carbon atoms accelerated in the 60-inch cyclotron. To obtain intense and uniform beams of more nuclear species, a Heavy Ion Accelerator (HILAC) was built in 1957. It consisted of two Alvarez linacs separated by a narrow space where partially ionized atoms could be stripped of their remaining electrons by collision. The HILAC incorporated technology developed for the MTA Mark I; it made possible acceleration of nuclei as heavy as argon (element 18) to energies up to 10 MeV per nucleon. Among much else, it accomplished the synthesis of nobelium (102) and, in 1961, element 103, named lawrencium in honor of the founder of the Laboratory. A machine that has proven even more productive for nuclear chemistry, the 88-inch sector-focused cyclotron, was authorized in 1958 and completed in 1961. It too profited from MTA designs, in its case application of the Thomas focusing principle developed for Mark III. The machine made possible valuable studies of nucleon scattering, spin-dependent processes, and isotope manufacture.

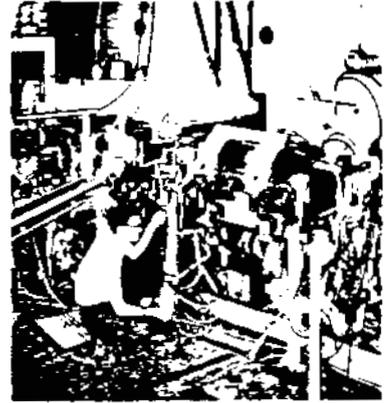
After the lead in high-energy accelerators passed from Berkeley, the Laboratory upgraded the HILAC to the SuperHilac, which, with its new ion source, could accelerate nuclei as heavy as krypton's.



Mark II (no kin to the MTA) is the successor to the Mark I detector in which the new class of particles known as the psi or J particles was found. It is being used at PEP.



HISS, the heavy ion spectrometer system at the Bevatron, permits a number of different heavy-ion experiments to be done at the same time



The 88-inch sector focused cyclotron, completed in 1961, uses the Thomas focusing principle refined by the MTA project

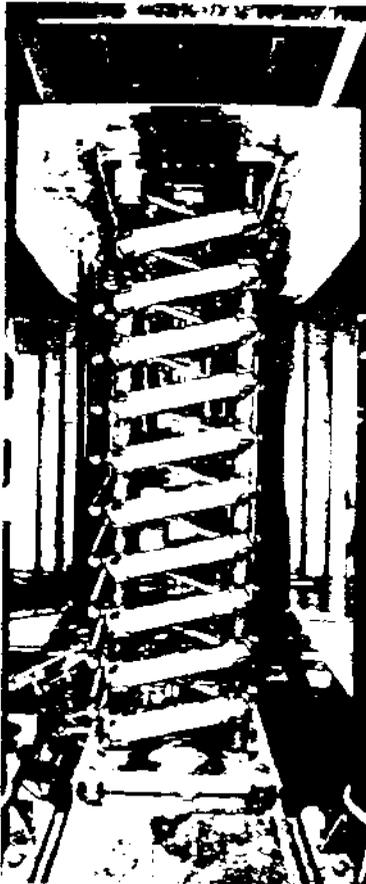


The Bevalac link joins the SuperHilac (top of picture) to the Bevatron, allowing the accelerators to work together in a tandem mode known as the Bevalac

1173543

*An electron
microscope standing
three stories tall*

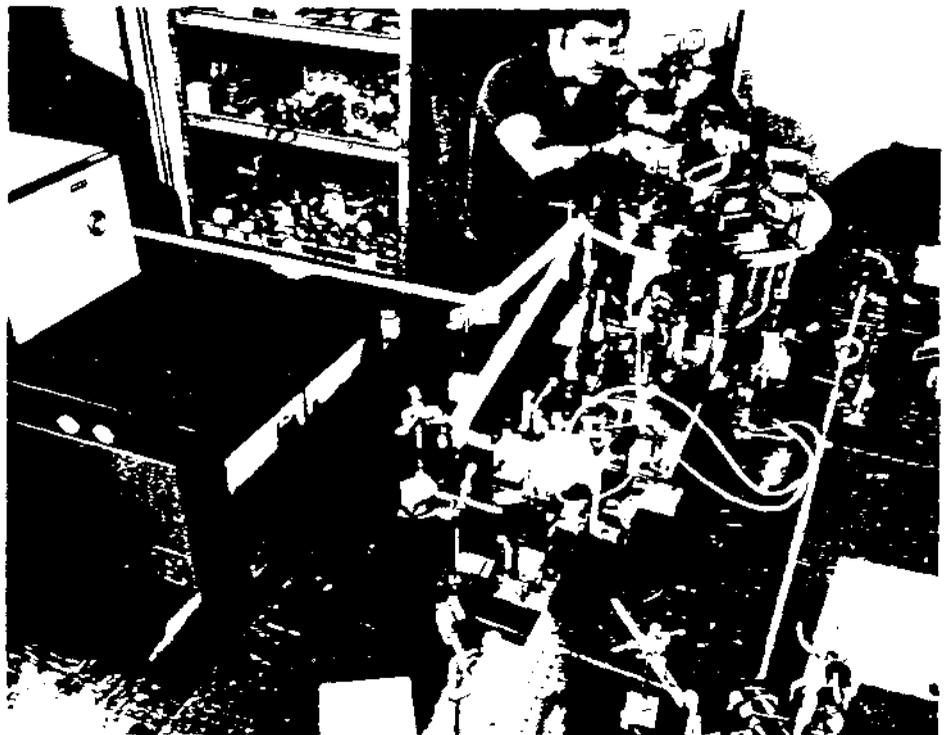
102



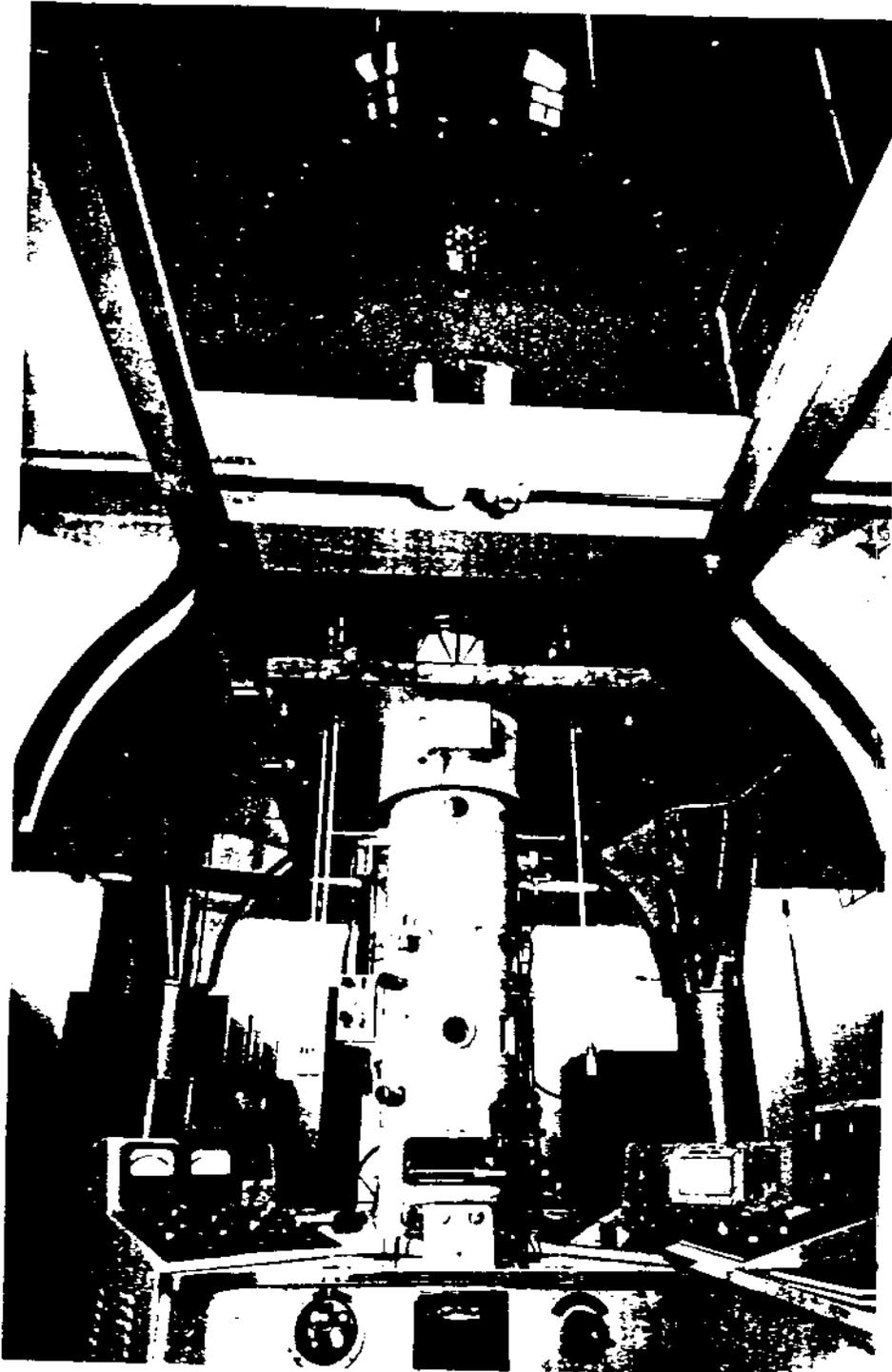
Rectifier decks are part of the high voltage system for Abei, the new injector at the SuperHilac, which permits acceleration of ions as heavy as uranium

Another source, capable of producing partially ionized atoms as heavy as uranium, has recently been introduced. Heavy ions with energies of 8.5 MeV per nucleon from the SuperHilac may now be sent either directly into research areas for nuclear chemistry or shunted into a long pipe for injection into the Bevatron, where they can be accelerated to 2.1 BeV per nucleon for applications in nuclear medicine or nuclear physics. The Bevalac, as the complex of SuperHilac and Bevatron is known, and the 88-inch cyclotron together constitute the nation's principal facility for heavy-ion work.

Among the new domains of interdisciplinary research opened at the Laboratory in response to national needs, the Materials and Molecular Research Division (MMRD) has perhaps the closest ties to earlier work. It is the successor of the Inorganic Materials Research Division founded in 1960 to extend to space technology and other spheres the studies of reactor materials begun at Berkeley during the war. By drawing upon many disciplines, MMRD has developed outstanding instrumentation for its purposes. One such instrument, a 1.5 MeV electron microscope standing three stories tall and costing \$1.7 million, carries forward the Laboratory's tradition of big machines. With an atomic resolution microscope scheduled for construction in 1982,



Lasers have a variety of uses in the Laboratory today; here, a postdoc in the Materials and Molecular Research Division uses a nitrogen-pumped dye-tuned laser to separate a compound into its constituents



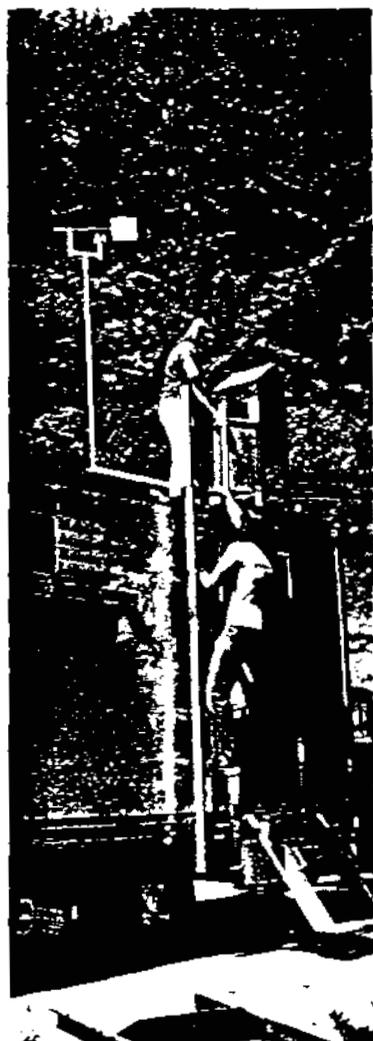
LBL's national center for electron microscopy features the new 1.5 MeV high-voltage electron microscope, the most powerful in the U.S. today



An abandoned iron mine in Stripa, Sweden, is the site of nuclear-waste disposal studies under the direction of LBL geologists and the Swedish government

1173545

Overly specialized institutions, like overly specialized organisms, do not long survive



An atmospheric research laboratory on wheels is part of LBL's air-pollution research program.

MMRD's National Electron Facility will be the leading center of microscopy in the country. By bringing together both on campus and in the Laboratory researchers in various branches of chemistry, physics, ceramics, and engineering, MMRD proceeds without the duplication of effort that might occur were disciplinary boundaries scrupulously observed. Like chemical biodynamics and nuclear science, materials and molecular research benefits from the opportunities for wide and varied collaboration unique to the Laboratory with its great resources and close campus ties.

A similar flexibility characterized the Laboratory's response to the reorganization of AEC with a mandate to support programs in non-nuclear energy development and environmental conservation. McMillan formed an Environmental Research Office to promote the new field; the 70 or so research projects it had on hand or in preparation in 1970 included ones on water desalination, atmospheric aerosols, disease induced by pollution, and the effects of the supersonic transport on the earth's ozone budget. Many others have been added since the Office rose to a Division—in fact to the largest division in the Laboratory—under Sessler. In 1977 he split it into two, one for Energy and Environment and one for Earth Sciences, which includes research in geothermal energy and on disposal of nuclear wastes. In recent years these two divisions together have spent almost a quarter of the Laboratory's budget.

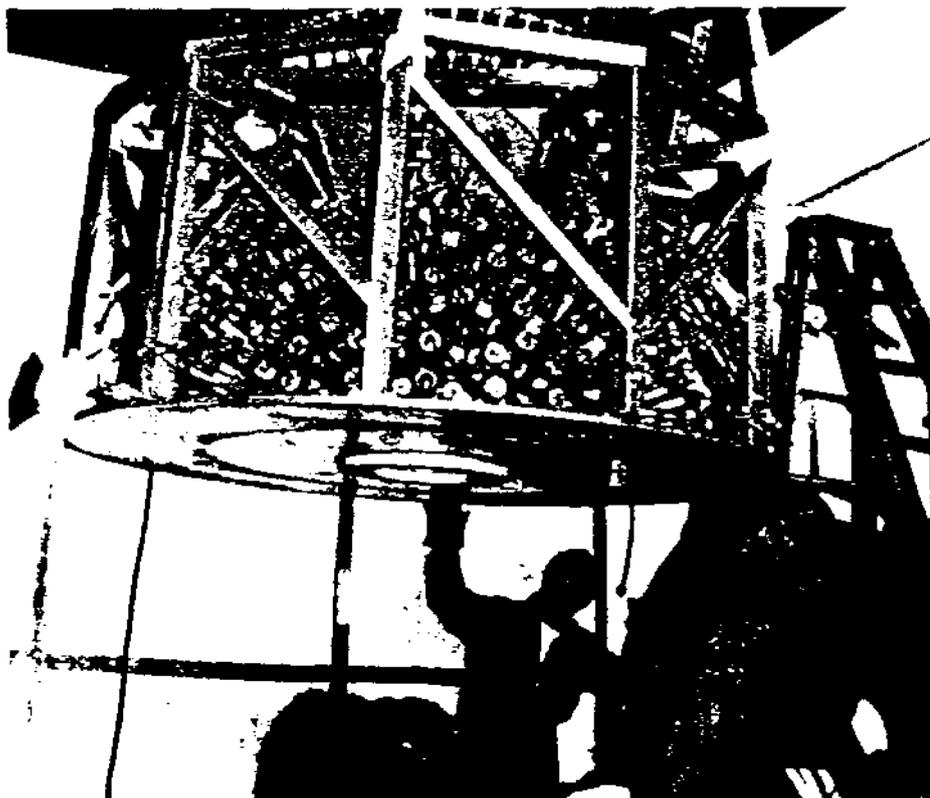
Overly specialized institutions, like overly specialized organisms, do not long survive major changes in their environments. The



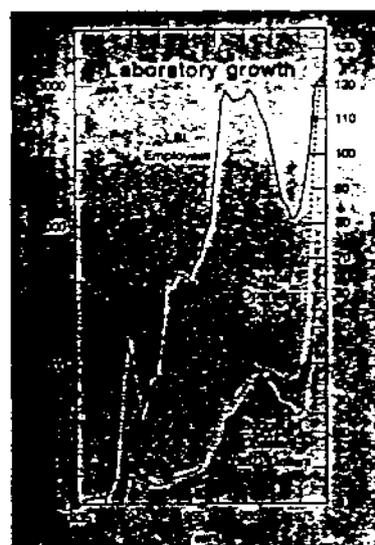
Temperature and radioactivity of a hot geothermal pool in Ruby Valley, Nevada, are measured by LBL scientists in a comprehensive study of geothermal energy sources

Laboratory's main principle of adaptation has been the creation of interdisciplinary teams that dissolve ordinary institutional boundaries in order to develop a machine, a research project, or a research program. It was on this principle that Lawrence established his laboratory. To demonstrate the wide promise of his machine and its products to his patrons, he recruited biologists, physicians, and chemists as well as physicists and engineers to work on and around the cyclotron. After the war he reaffirmed the principle by promoting hybrids like Calvin's bio-organic chemistry. Materials research, the first big interdisciplinary program initiated after Lawrence's death, drew on institutional mechanisms already firmly in place. The divisions of energy and environment and earth sciences are new variations on the successful principle of growth through diversification into interdisciplinary research program.

If our biological metaphor has any validity—and, perhaps, even if it does not—the Laboratory is well placed to continue the course of high achievement and successful adaptation of its first fifty years into its next half century.



The Plastic Ball, developed by a collaboration between scientists from LBL and West Germany, is the first detector system that records electronically the products of high-energy nuclear collisions simultaneously from all angles. It is being used in Bevalac experiments.



Sources. National Defense Project Contract W-7405 Report, UCRL Budget Estimates FY 1951, Pro Forma Berkeley Financial Summary Fiscal Years 1951-1960, and UCRL Annual Reports. Data for 1954 and 1955 are estimates, little attempt then being made to segregate Berkeley and Livermore operations costs.