The Early Years

by Robert Serber

THE YEARS 1925 TO 1929 were great years in physics. They saw the development of the quantum theory: the Schrödinger equation, the Dirac equation, field theory and quantum electrodynamics. That it was so completely a European effort illustrates the weakness and provincialism of theoretical physics in the United States at the time. Within fifteen years the situation was drastically changed and American theoretical physics was becoming comparable to the best. A very important element in this change was the influence of Robert Oppenheimer. The alumni of the great school of theoretical physics he established at Berkeley played a large part in the subsequent growth of American physics and also in enabling us to meet the demands of the war years.

Oppenheimer was too young to participate in the original flowering of quantum mechanics. He received his BA from Harvard University in 1925 and spent the next two years at Cambridge University and at the University of Göttingen, receiving his PhD in Göttingen in 1927. In 1928 he was at Harvard and the California Institute of Technology as a National Research Fellow and in 1929 at Leyden and Zurich as a fellow of the International Education Board. Oppenheimer's first paper, in 1926, dealt with molecular energy levels, his second with transitions to continuum states in hydrogenic atoms. [*For a complete bibliography see page 52.*] Then at Göttingen he wrote his famous paper with Max Born on the approximations involved in the theory of molecules. During the next three years he wrote a series of papers mostly depending on his knowledge of the continuum wave functions, which he appears to have been the first to master. He discussed their normalization, calculated the absorption coefficient of x rays near the K-edge, the continuous x-ray spectrum, the elastic and inelastic scattering of electrons including the first treatment of the exchange effects, and he greatly improved the calculation of stellar opacities. His most original contribution, however, was his theory of field emission, the first example of an effect due to barrier penetration (antedating the explanation of radioactive alpha decay). He developed a pertubation theory of nonorthogonal states and used it to calculate the disintegration of a hydrogen atom in an electric field. He then applied his results to the effects observed in metals. This work was done at Pasadena, where Robert A. Millikan and Charles C. Lauritsen were studying the phenomenon, and was the first evidence of a feature later to be so prominent in his work: his close collaboration with his experimental colleagues.

This early work showed power and facility, but after his year with Wolfgang Pauli in 1928–29 his interests changed and thereafter were devoted to the more fundamental questions of physics. At Zurich he learned of Werner Heisenberg's and Pauli's work on quantum electrodynamics, and late in 1929 he published his paper on resonance scattering of light in which he attempted to deal with self-energy difficulties. The hope was that the frequencies would remain finite, even though the energies had infinite shifts, but (for reasons we now understand) this was not the case. Oppenheimer did observe that the leading divergent terms were equal for states of the same energy and pointed out that the applicability of the theory to the fine-structure splitting could be ascribed to this circumstance.

Proton and antiproton

Early in 1930 Oppenheimer showed that the positive particles of the Dirac theory must have the same mass as the electron and thus could not be protons as Paul Dirac had suggested. Oppenheimer's argument was that matrix elements of the current between positive and negative energy states contributed importantly to the scattering of light by both particles and antiparticles. Since the matrix elements were the same, it was impossible to understand how the Thompson formula with the electron mass would hold for the positive energy states and the Thompson formula with the proton mass for the negative energy states. Moreover he calculated the lifetime for annihilation of particles and antiparticles; since the matrix elements involved were similar to those in Thompson scattering, there could be no reason for doubting the resulting short lifetime for annihilation. He concluded that the proton must be an independent elementary particle and have its own antiparticle. Thus he made the first prediction of the antiproton.

Oppenheimer then turned to the problem of the anomalous absorption of ThC″ gamma rays which had first been reported by S. H. Chao, working at Cal Tech, and with Harvey Hall calculated the relativistic photoeffect. An error led them to the conclusion that the Dirac theory must be wrong for energies greater than mc^2 and was probably responsible for Oppenheimer's failure at that time to be convinced of the reality of the positron. In 1931 he attempted to linearize the theory of the photon, as Dirac had done for the electron, and pointed out the different structure of the theory for particles of integral and half-integral spins, the difference that later was the basis of Pauli's proof of the connection between spin and statistics.

In 1932, J. Franklin Carlson and Oppenheimer, in an effort to understand the great penetration of cosmic rays, studied the ionization losses of relativistic electrons and Pauli neutrinos. (The latter was a particle suggested by Pauli. They were supposed to have approximately electronic mass, a small magnetic moment and to be a constituent of nuclei.) The conclusion was that neither electrons nor magnetic neutrinos should have the properties of the penetrating component. In 1933, after the discovery of the positron by Carl D. Anderson, Oppenheimer and Milton S. Plesset gave the first correct description of the mechanism of pair production by gamma rays and showed that the theory quantitatively explained the excess absorption of ThC″ gamma rays in heavy elements. However, they pointed out that the theory would predict large deviations from the mass-absorption law for cosmic rays (assuming, as they did, that the sea-level cosmic rays were mostly electrons and positrons) and concluded that, while the theory was applicable in the range of radioactive decay energies, it must fail at energies greater than 137 mc^2. A fundamental barrier to Oppenheimer's success in making progress with the difficulties of quantum electrodynamics must have been this belief in the incorrectness of the theory, a belief which he continually stressed. His appreciation of experimental results and his close association with the experimentalists, a strength in other aspects of his work, in this may have been a weakness.

Field theory

Later in 1933, in a paper with Wendell H. Furry, Oppenheimer formulated the Dirac theory as a field theory, essentially in its modern form. The charge renormalization and the vacuum polarization effects were pointed out although the problem of gauge invariance remained as a difficulty. The vacuum polarization effects were declared to be observable, with a warning that other radiative corrections existed for electrons. Similar considerations were being made by Dirac at about the same time. During 1934 and 1935, Oppenheimer worked on critiques of these and other aspects of electrodynamics. In June 1936, he first discussed the theory of electron–position showers, and an elegant treatment of this important problem was given at the end of the year by Carlson and Oppenheimer and then by Hartland Snyder, one of his students. Oppenheimer concluded that the success of shower theory proved the validity of electron theory and required the existence of a new type of particle in cosmic rays. In June 1937, immediately after the discovery of the meson by Carl D. Anderson, Seth H. Neddermeyer, Jabez C. Street and Stevenson, he wrote with me pointing out the probable connection of the cosmic-ray meson with the particle suggested by Hideki Yukawa. We drew the inference that it was not a primary cosmic ray but was

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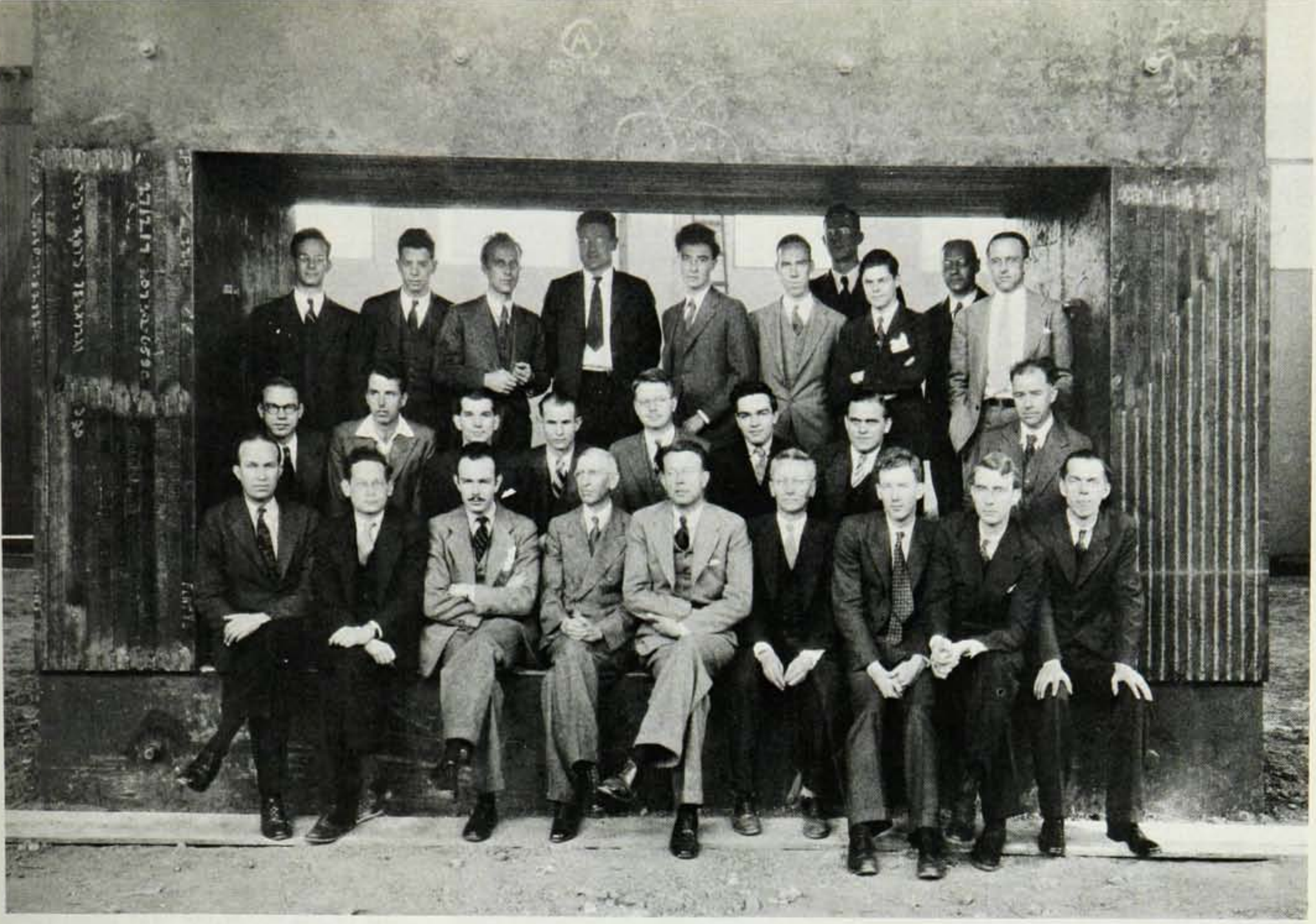
Victor Weisskopf was affiliated with the Manhattan Project from 1943–46. An authority on nuclear structure, he is physics chairman and institute professor at MIT. He was past director-general of CERN.

Glenn T. Seaborg had been associated with the University of California, Berkeley, during the Oppenheimer years. In 1942 he headed plutonium work for the Manhattan Project. He is now chairman of the US AEC.



UNDER A MAGNETIC SPELL. The scientific staff of the University of California Radiation Laboratory are framed within the magnet of the unfinished 60-inch cyclotron in 1938. Left to right and top to bottom: Alexander S. Langsdorf, S. J. Simmons, J. G. Hamilton, David H. Sloan, Oppenheimer, William M. Brobeck, R. Cornog, Robert R. Wilson, E. Viez, John J. Livingood, John Backus, W. B. Mann, Paul C. Aebersold, Edwin M. McMillan, Ernest M. Lyman, M. D. Kamen, D. C. Kalbfell, Winfield W. Salisbury, J. H. Lawrence, Robert Serber, F. N. D. Kurie, Raymond T. Birge, Ernest O. Lawrence, Donald Cooksey, Arthur H. Snell, Luis W. Alvarez, P. H. Abelson.

ejected from nuclei in the upper atmosphere, and we explained the showers below sea level as being produced by knock-on electrons. Another suggestion, that the finite lifetime of the meson would lead to anomalous atmospheric absorption, was eliminated on Millikan's insistence of the validity of the mass-absorption law.

In 1939, in a paper with Snyder and me, Oppenheimer had returned to the question of the soft component below sea level and pointed out that if the mesons had spin one they would radiate too rapidly. This conclusion was conditional on the convergence of electrodynamic theory for spin-one particles. Two of Oppenheimer's students, Robert F. Christy and Siuchi Kusaka, studied the question in more detail and arrived at the same conclusion. In 1941, after Marcel Schein, William P. Jesse and Ernest O. Wollan had shown that the primary cosmic rays were predominantly protons, Oppenheimer and Christy suggested that the soft component at high altitude could be accounted for if, in addition to the penetrating mesons, there were roughly equal numbers of fast decaying mesons with a lifetime of about 10^{-8} seconds that decayed into electrons or positrons. Oppenheimer's later clarification, in 1947, of the role of the π^0 in the generation of the soft component was a natural sequel to this earlier work.

Earlier, Oppenheimer had been involved in the nuclear physics being done by the rapidly growing schools of Ernest O. Lawrence in Berkeley and Lauritsen in Pasadena. His first paper on this subject, in December 1932, accounted for Malcolm Henderson's results on the energy variation of the nuclear reaction produced by bombarding lithium by protons. In 1935, with Melba Phillips, he calculated the yield of protons in deuteron reactions; the "Oppenheimer-Phillips process" explained the experimental results of Lawrence, Edwin M. McMillan and Robert L. Thornton. A series of papers on reactions in light elements discussed observations of the Lauritsen group. In one of them the first evidence for the operation of an isotopic-spin selection rule was pointed out.

Oppenheimer's connections at Pasadena with the staff of the Mount Wilson Observatory and with Richard Tolman led to an interest in astrophysics and general relativity, to papers on neutron stars in 1938 and 1939, and to his well known work with Snyder on gravitational contraction in 1939.

The years 1940 and 1941 saw intensive work in meson theory including attempts to deal with the strong cou-

Oppenheimer Chronology

1904	Born 22 April in New York City
1925	Received BA from Harvard College, summa cum laude
1926	Studied under Lord Ernest Rutherford at the Cavendish Laboratory in the University of Cambridge
1927	Received his PhD under Max Born at the University of Göttingen
1928	National Research Fellow at Harvard University and at the California Instiute of Technology
1929	Fellow of the International Education Board with Wolfgang Pauli at the Universities of Leyden and Zurich
1929	Joint appointment at the University of California at Berkeley and Cal Tech
1936	Full professor at Berkeley and Cal Tech
1940	Married Katherine Harrison (1 son, 1 daughter)
1941	Elected to the United States National Academy of Sciences
1942	Organized the Los Alamos Scientific Laboratory
1943	Appointed director of the Los Alamos Scientific Laboratory
1946	Received the United States Medal for Merit
1946	Helped prepare the Atomic Energy Act of 1946
1946–52	Chairman of the General Advisory Committee of the United States Atomic Energy Commission
1946–66	Director of the Institute for Advanced Study at Princeton, New Jersey
1948	President of the American Physical Society
1954	Investigated by the Personnel Security Board of the United States Atomic Energy Commission
1958	Awarded the Legion d'honneur by France
1963	Received the Enrico Fermi Award from President Lyndon B. Johnson
1967	Died 18 February in Princeton

pling problem by including inertial and radiative-reaction damping effects. In 1941, in a paper with Julian Schwinger, he applied Gregor Wentzel's strong coupling theory to pseudoscalar mesons and predicted the existence of nucleon isobars with an excitation energy slightly less than the rest energy of the meson. Multiple-meson production was also discussed. These efforts continued until the war.

Oppie as a teacher

Oppenheimer's fascinating personality played a major part in his unique powers as a teacher. I can cite my own experience, the impact of my first meeting with him. In 1934, I received a PhD from John H. Van Vleck at the University of Wisconsin and a National Research Fellowship that I intended to spend at an eastern university. On the way east from Wisconsin I stopped in at Ann Arbor for a month at the summer session. Oppenheimer was there and after I heard him lecture and spent some time with him I reversed my direction and went to Berkeley. Upon arriving I discovered that most of the National Research fellows in theoretical physics were already there.

By this time Oppenheimer's course in quantum mechanics was well established. Oppie (as he was known to his Berkeley students) was quick, impatient and had a sharp tongue, and in the earliest days of his teaching he was reputed to have terrorized the students. But after five years of experience he had mellowed (if his earlier students were to be believed). His course was an inspirational as well as an educational achievement. He transmitted to his students a feeling of the beauty of the logical structure of physics and an excitement about the development of physics. Almost everyone listened to the course more than once: Oppie occasionally had difficulty in dissuading students from coming for a third or fourth time. The basic logic of Oppenheimer's course in quantum mechanics derived from Pauli's article in the *Handbuch der Physik*. Its graduates (Leonard Schiff in particular) carried it, each in his own version, to many campuses.

Oppie's way of working with his research students was also original. His group consisted of eight or ten graduate students and about a half dozen postdoctoral fellows. He met the group once a day in his office. A little before the appointed time the members straggled in and disposed themselves on the tables and about the walls. Oppie came in and discussed with one after another the status of the student's research problem while the others listened and offered comments. All were exposed to a broad range of topics. Oppenheimer was interested in everything; one subject after another was introduced and coexisted with all the others. In an afternoon they might discuss electrodynamics, cosmic rays, astrophysics and nuclear physics.

Dinner in Frisco

Oppie's relations with his students were not confined to office and classroom. He was a bachelor then and a part of his social life was intertwined with ours. Often we worked late, continued the discussion through dinner and then later at his apartment on Shasta Road. When we tired of our problems or cleared up the point at issue, the talk would turn to wider realms of the intellect, of art, music, literature and politics. If the problems were going badly we might give up and go to a movie. Sometimes we took a night off and had a Mexican dinner in Oakland or went to a good restaurant in San Francisco. In the early days this meant taking the Berkeley ferry and the ride across the bay. The ferries back to Berkeley did not run often late at night, and this required passing the time waiting for them at the bars and night clubs near the ferry dock. Frequently we missed several ferries. Ed McMillan was often our companion then.

We held regular joint seminars with Felix Bloch and his students from Stanford University. After the seminars Oppie would frequently treat the whole entourage to dinner at Jack's in San Francisco. One should remember that these were postdepression days when students were poor. The world of good food, good wines and gracious living was far from the experience of many of them, and Oppie was introducing them to an unfamiliar way of life. We acquired something of his tastes. We went to concerts together and listened to chamber music. Oppie and Arn Nordsieck read Plato in the original Greek. During many evening parties we drank, talked and danced until late, and, when Oppie was supplying the food, the novices suffered from the hot chile that social example required them to eat.

During this time Oppie was a professor at both Berkeley and Cal Tech (where he metamorphized into Robert). The arrangement was made possible because the Berkeley spring semester ended early in April, allowing Robert to teach the spring quarter in Pasadena. Many of his students made the annual trek with him. Some things were easier in those days. We thought nothing of giving up our house or apartment in Berkeley, confident that we could find a garden cottage in Pasadena for 25 dollars a month. We did not own more than could be packed into the back of a car. In Pasadena, in addition to being exposed to new information on physics, we led an active social life. The Tolmans were good friends and we had very warm relations with Charlie Lauritsen and his group. Willy Fowler was a graduate student then and Tommy Lauritsen was still in high school. We spent many evenings at the Mexican restaurants on Olvera Street and many nights partying in Charlie Lauritsen's garden.

One feature of the times that contrasts with present customs was the relatively little personal contact we had with the outer world of physics. The meetings we went to were the West Coast meetings of the American Physical Society. The first conference I can recall was a cosmic-ray symposium in Chicago that Oppie and I drove to from his New Mexico ranch in the early summer of 1939. We had a few visitors, however. Bohr and Dirac and Pauli made short visits to Berkeley or Pasadena, and I met Victor Weisskopf, Hans Bethe, George Placzek, George Gamow and Walter Elsasser at the ranch.

WIDE WORLD PHOTOS

POINTING TO PERFECTION. **E. O. Lawrence and Oppenheimer examine diffusion pumps for creating an almost perfect vacuum in an accelerating chamber between poles of the 184-inch cyclotron.**

There were many facets of Oppenheimer's character that contributed to his greatness as a teacher: his great capacity as a physicist, his wide intellectual interests, his astonishing quickness of mind, his great gift for expression, his sensitive perception, his social presence which made him the center of every gathering. His students emulated him as best they could. They copied his gestures, his mannerisms, his intonations. He truly influenced their lives. Among his prewar students (besides some I have already mentioned) were Leo Nedelsky, Glenn Camp, Ed Uehling, Fritz Kalckar, George Volkoff, Sid Dancoff, Phil Morrison, Joe Keller, Willis Lamb, Bernard Peters, Bill Rarita, Eldred Nelson, Stan Frankel and Chaim Richman. All of us owe him more than we can say, for his instruction, friendship and affection. For us his death was a great blow and a great loss. □

The Los Alamos Years

by Victor F. Weisskopf

THE YEAR 1939 changed many things. It witnessed the beginning of the most destructive war in history. It has also changed science. Many physicists who never were interested in applications of science devoted their skills to the necessities of war and became applied physicists. They faced new problems, new experiences, different from the accustomed academic environment. But the deepest change in the character of our science came from the discovery of fission. Many of us hoped at that time—and Oppenheimer was one of them—that the number of neutrons released would have been small enough to prevent a chain reaction. But soon enough it was clear that, on the forefront of the most esoteric and basic part of our science, a phenomenon was discovered, full of tremendous destructive and constructive potentialities. It was not yet ready for exploitation; many staggering problems had to be solved, but the way was clearly indicated. Many physicists were drawn into this work, by fate and destiny rather than enthusiasm. A threat hung over us, the frightening possibility of finding this