NATIONAL ACADEMY OF SCIENCES

J. ROBERT OPPENHEIMER

1904—1967

A Biographical Memoir by

H. A. BETHE

Any opinions expressed in this memoir are those of the author(s) and do not necessarily reflect the views of the National Academy of Sciences.

Biographical Memoir

NATIONAL ACADEMIES PRESS
WASHINGTON D.C.

J R Oppenheimer

J. ROBERT OPPENHEIMER

April 22, 1904–February 18, 1967

BY H. A. BETHE

J. ROBERT OPPENHEIMER died on 18 February 1967 in Princeton, N. J. More than any other man, he was responsible for raising American theoretical physics from a provincial adjunct of Europe to world leadership.

Robert Oppenheimer was born on 22 April 1904 in New York. His father, who had come to the United States from Germany at the age of 17, was a prosperous textile importer. By inheritance, Robert was well-to-do all his life. The father was quite active in many community affairs, and much interested in art and music. He had a good collection of paintings, including three Van Goghs.

Oppenheimer's mother, Ella Freedman, came from Baltimore. She was a painter who had studied in Paris, and was a very sensitive person. Robert had one younger brother, Frank, who also became a physicist; he is Professor of Experimental Physics at the University of Colorado, Boulder, Colo. Oppenheimer had close ties both with his parents and his brother.

As a boy, Robert was already most interested in matters of

Reprinted from *Biographical Memoirs of Fellows of The Royal Society* (14:391-416) with permission of The Royal Society.

the mind. He attended the Ethical Culture School in New York, one of the best in the city. He was more interested in his homework, in poetry and in science than in mixing with other boys. He has said, ‘It is characteristic that I do not remember any of my classmates.’

Already at the age of 5, Robert collected mineralogical specimens, some of which came from his grandfather in Germany. By the time he was 11 years old his collection was so good and his knowledge so extensive that he was admitted to membership in the Mineralogical Club in New York.

He entered Harvard in 1922 intending to become a chemist, but soon switched to physics. It was characteristic of him not to abandon a subject once he had become interested. Familiarity with chemistry was very useful to him in his Los Alamos days when purification of fissionable materials was one of the main problems of the laboratory. He also retained a lifetime affection for Harvard University, where he was a Member of the Board of Overseers from 1949 to 1955.

At Harvard he was strongly influenced by Professor Percy W. Bridgman, a great and very original experimental physicist. Apart from this, he kept much to himself and devoured knowledge. ‘I had a real chance to learn,’ he said. ‘I loved it. I almost came alive. I took more courses than I was supposed to, lived in the library stacks, just raided the place intellectually.’ In addition to studying physics and chemistry, he learned Latin and Greek and was graduated *summa cum laude* in 1925, having taken three years for the normal four-year course.

His work for the Ph.D. was even more astonishingly rapid: two years sufficed while the present average time required in the United States is four to five years.

After his B.A. degree he travelled for four years to the great centres of physics in Europe. The year 1925 to 1926 he spent at Cambridge University, where he was exposed to the great personality of Lord Rutherford. It was the time when Heisenberg, Born and Schroedinger were developing quantum mechanics. Robert was fascinated and immediately accepted when an invitation came from Max Born to work with him at Göttingen. Here he took his Ph.D. in the spring of 1927.

Next he became a Fellow of the National Research Council, first at Harvard University, then at the California Institute of Technology. In the year 1928 to 1929, he was a Fellow of the International Education Board and visited Leiden and Zürich. He worked with Professor Pauli, an association which greatly influenced his further scientific life.

On his return to the United States in 1929, Oppenheimer received many offers of positions. He accepted two and became an Assistant Professor in Physics, simultaneously at the University of California in Berkeley and at the California Institute of Technology. In the ensuing 13 years, he 'commuted' between the two places, spending the fall and winter in Berkeley, and the spring term, beginning in April, in Pasadena. Many of his associates and students commuted with him.

It was here, in Berkeley, that he created his great School of Theoretical Physics. The majority of the best American theoretical physicists who grew up in those years were trained by Oppenheimer at one stage of their lives. Many were his graduate students, others came to him as Post-doctoral Fellows. They affectionately called him 'Oppie'. His teaching, his style and his example formed the scientific attitude of all of them.

EARLY SCIENTIFIC WORK

Oppenheimer was most fortunate to enter physics in 1925, just when modern quantum mechanics came into being.

While he was too young to take part in its formulation, he was one of the first to use it for the exploration of problems which had been insoluble with the old quantum theory.

In 1927, he wrote with Born a famous paper on the 'Quantum theory of molecules'. In this they showed how to separate the problem into one describing the motion of the electrons around fixed nuclei, and another to describe the motion (vibrations and rotation) of the nuclear skeleton. Their method still forms the basis of any treatment of molecules.

Oppenheimer's main interest until 1929 was the theory of continuous spectra. This was unexplored territory. He had to develop the method to normalize the eigen-functions in the continuous spectrum, and to do calculations of transition probabilities. Here as well as later in his work, his great knowledge of mathematical tools was most useful. He calculated the photoelectric effect for hydrogen and for X-rays. Even today this is a complicated calculation, beyond the scope of most quantum mechanics textbooks. Naturally, his calculations were later improved upon, but he correctly obtained the absorption coefficient at the *K* edge and the frequency dependence in its neighbourhood. It was disturbing that his theory, while agreeing well with measurements of X-ray absorption coefficients, did not seem to be in accord with the opacity of hydrogen in the sun. This, however, was the fault of the limited understanding of the solar atmosphere in 1926. It was then believed that the sun consisted mostly of heavy elements from oxygen on up, like the earth. Many years later, Strömgren suggested that the main constituent was hydrogen. This brought Oppenheimer's calculations of opacity into agreement with astrophysical data. Nowadays the opacity, calculated essentially on the lines of 'Oppie's' theory, is one of the main ingredients of all understanding of stellar interiors. In the course of his calculation of opacity, he also calculated the bremsstrahlung from electrons in the field of nuclei.

His work with the nuclear physicists at Cambridge motivated him to calculate the capture of electrons by ions from other atoms, i.e. such charge exchange processes as

$$He^{2+} + H = He^{+} + H^{+} \quad (1)$$

For this work he had to develop a method for the treatment of collision processes involving non-orthogonal save functions.

This work led him on to a treatment of the ionization of the hydrogen atom by electric fields, probably the first paper describing the penetration of a potential barrier, well before the theory of the alpha disintegration. Discussions with Millikan and Lauritsen at CalTech who had just observed the extraction of electrons from metal surfaces by very strong electric fields, motivated him to extend his theory to a description of this effect (1928).

Studying collisions between electrons and atoms, using the Born approximation, he pointed out that the incident electron can exchange with the atomic electron. This effect is indeed important for the understanding of the scattering of low energy electrons from such atoms as helium, as well as in high energy collisions. He could also make mistakes: he believed that exchange could explain the Ramsauer effect while actually this effect is due to the fact that an integral number of half-waves fit into the atom.

THE BERKELEY PERIOD

Pauli, who all his life emphasized the problems at the very frontier of physics, exerted a lasting influence on Oppenheimer. As the frontier shifted from ordinary quantum mechanics to the relativistic quantum mechanics of

Dirac, and the theory of electromagnetic fields, the work of Oppenheimer and his great school in Berkeley became chiefly devoted to these subjects.

As early as 1930, Oppenheimer wrote a fundamental paper which essentially predicted the positive electron. One year before, Dirac had reinterpreted the negative energy solutions of his relativistic equation for the electron as indicating the existence of positive charges. Dirac had believed that these were protons. Oppenheimer showed, by very cogent arguments involving symmetry, that the positive charges could not have the mass of the proton, but must have the same mass as the electron. This implicitly predicted the existence of the positron which was discovered three years later. Unfortunately Oppie was prevented from drawing this conclusion by his skepticism concerning the validity of the Dirac equation, a skepticism which had been engendered by another calculation (with Harvey Hall, his student) on the photoelectric effect at high energies, which appeared to disagree with experiment.

Also in 1930, Oppenheimer investigated radiative transitions, making use of the newly developed quantum electrodynamics of Pauli and Heisenberg. He had hoped that the infinite perturbations which Heisenberg and Pauli had found in their theory would not occur in observable processes like the scattering of light. To his disappointment they did. Only the mass renormalization of the late 1940's permitted physicists to eliminate these troubles.

His association with the CalTech experimenters stimulated him to calculate the energy loss of relativistic electrons (1932, with his student Carlson). Their result has proved correct but, at the time, it was believed in contradiction with the evidence from cosmic radiation. In 1933, cosmic radiation yielded the first new particle: Carl Anderson at CalTech discovered the positron which Oppie had almost predicted three years earlier. Oppie immediately proceeded to calculate the cross section for production of positrons at low energy, with his student Milton Plesset. His great knowledge of the continuous spectrum wave functions in the Coulomb field was most useful for this purpose. A more thorough theory with Nedelsky followed.

A little later, he extended the theory of electron pair production to a theory of the showers which are such a prominent phenomenon in cosmic radiation. It had been pointed out by Nordheim, Heitler and Bhabha that these showers could be explained as follows: electrons emit electromagnetic radiation (gamma rays) and these gamma rays in turn produce electron pairs in the electric field of atomic nuclei. Oppenheimer, with his associates Carlson and H. Snyder, developed a most elegant mathematical theory of the multiplicity of air showers, a masterpiece of mathematical treatment of a physical phenomena.

All the time, however, Oppenheimer was worried about the likely breakdown of quantum electrodynamics at energies above 137 mc^2. Indeed, laboratory experiments on the penetration of cosmic ray particles through slabs of lead and similar substances seemed to indicate this breakdown very clearly, provided the particles were electrons. It was only in 1937 that it was discovered that the particles were in fact not electrons but mesons. While most physicists were troubled by the supposed breakdown, it dominated Oppie's thoughts, more than anybody else's and he impressed his worries on his students. A number of his papers deal with this problem. We know now that there is no such breakdown and that in fact quantum electrodynamics holds at least up to about a hundred times this energy, probably higher.

Oppie was also very active in other aspects of fundamental quantum theory. In 1931, he attempted to get a first-order differential equation for light quanta, similar to Dirac's equation for the electron. He failed, but in the process recognized the fundamental difference between particles of spin one-half and of integral spin. This was later a basis of Pauli's theory of the relation between spin and statistics.

In 1934, with Furry, he developed a field theory of the Dirac equation, treating electrons and positrons as of equal status. This paper contains essentially the modern form of the electron-positron theory. He was much concerned with other consequences of the existence of the positron. He and his collaborators found that the observable charge of the electron is not the true charge, foreshadowing charge renormalization. They pointed out the effect of vacuum polarization by virtual pairs of electrons and positrons being formed in strong electric fields. Similar ideas were simultaneously discussed by Dirac and others, but the most explicit calculation of vacuum polarization was made by Oppenheimer's student, Uehling.

In 1937 Anderson and others discovered the meson which had been predicted two years earlier by Yukawa in an effort to explain nuclear forces. Making use of Yukawa's theory, Oppie had suggested that the 'hard component' of cosmic rays, i.e. that which penetrates to sea-level, might consist of mesons which, being much heavier than electrons, would have greater penetrating power, while the soft component was interpreted as electrons and positrons, on the basis of the success of shower theory. Now, after Anderson's discovery, he immediately turned his attention to the properties of mesons. Oppenheimer and two of his students, Christy and Kusaka, showed that the meson could not have a spin of 1 or greater, because otherwise it would radiate too fast when penetrating underground. Oppie carefully discussed why he believed the theory of radiation to be valid in this case.

With Serber, he discussed the production of mesons from primary cosmic rays in the upper atmosphere. With Christy, he postulated that together with the penetrating, charged mesons, other particles should be produced in the upper atmosphere which have a short life and then decay into gamma rays, thus giving rise to the soft component of cosmic rays. In 1947 he postulated that these intermediate particles are neutral mesons (π^0), well before the discovery of that particle.

Both at Berkeley under Ernest Lawrence, and at Pasadena under Lauritsen, experimental nuclear physics was developing rapidly. Oppenheimer and his students turned their attention to this field from 1933 on. He calculated the excitation function for collisions between protons and nuclei, thus helping much in the interpretation of experiments. His most important contribution was the 'Oppenheimer-Phillips process' in which a deuteron, entering a heavy nucleus, is split into proton and neutron, one of these particles being retained by the nucleus while the other is re-emitted. He gave the first quantitative description of this very prominent process which after the war became an important tool in the study of nucleon energy levels and their properties. He also calculated the density of nuclear energy levels, the nuclear photo-effect and the properties of nuclear resonances. When Lauritsen observed that fluorine, bombarded with protons, gave electron pairs, Oppenheimer contributed much to the explanation: the nuclear reaction is

$$^{19}\mathrm{F} + \mathrm{H} = {}^{16}\mathrm{O}^* + {}^{4}\mathrm{H} \qquad (2)$$

^{16}O is formed in an excited state of angular momentum 0. By selection rules a transition from such a state to the ground state can most easily be accomplished by converting a virtual gamma ray into a pair of electrons.

At Pasadena one of the most important activities was astronomy, through the Mount Wilson Observatory. Richard Tolman worked on general relativity. Oppenheimer became interested in neutron stars, and with Snyder, in the gravitational contraction of massive stars until they disappear from observability.

In 1940 and 1941, Oppenheimer's attention was turned to meson theory and the attempt to explain nuclear forces by mesons. He attempted to deal with strong coupling, using his own theories as well as that of Wentzel. He predicted the existence of nucleon isobars with an excitation energy slightly below the rest energy of the meson.

In addition to this massive scientific work, Oppenheimer created the greatest school of theoretical physics that the United States has ever known. Before him, theoretical physics in America was a fairly modest enterprise, although there were a few outstanding representatives. Probably the most important ingredient he brought to his teaching was his exquisite taste. He always knew what were the important problems, as shown by his choice of subjects. He truly lived with these problems, struggling for a solution, and he communicated his concern to his group. In its heyday, there were about eight or ten graduate students in his group and about six Post-doctoral Fellows. He met this group once a day in his office, and discussed with one after another the status of the student's research problem. He was interested in everything, and in one afternoon they might discuss quantum electrodynamics, cosmic rays, electron pair production and nuclear physics.

In his classroom teaching he always applied the highest standards. He was much influenced by Pauli's article in the *Handbuch de Physik*, which provided the deepest understanding of quantum mechanics then and even now. Among his students was Leonard Schiff who incorporated much of Oppenheimer's spirit into his excellent textbook on quantum mechanics. New problems were constantly introduced into the quantum mechanics lectures. The lectures were never easy but they gave his students a feeling of the beauty of the subject and conveyed his excitement about its development. Almost every student went through his course more than once.

Oppie saw much of his students and associates after working hours. He would frequently treat them to an exquisite dinner in San Francisco, or to a less ambitious one in a Mexican restaurant in Oakland. His most constant collaborator of these years, Serber, writes of these excursions: 'One should remember that these were post-depression 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 Arnold 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 chilli that social example required them to eat.'

The magnetism and force of his personality was such that many of his students copied his gestures and mannerisms. Among his students, in addition to those already mentioned, were Fritz Kalckar, George Volkoff, Sid Dancoff, Phil Morrison, Joe Keller, Willis Lamb, Bernard Peters, Bill Rarita, and many others. As Oppenheimer himself has written: 'As the number of students increased, so in general did their quality. The men who worked with me during those years held chairs in many of the great centers of physics in the

United States; they have made important contributions to science, and in many cases to the atomic energy project.'

During his Berkeley time, Oppie had also many friends in the faculty, scientists, classicists and artists. He studied and read Sanskrit with a colleague, and his private reading ranged over the classics, novels, plays and poetry.

Most of the time he was indifferent to the events around him; he never read a newspaper, he had no radio or telephone, he learned of the stock market crash in 1929 only long after the event.

His interest in politics began in 1936. He had been much disturbed by the treatment of the Jews of Germany, including some of his relatives. He saw the effect of the American depression on his students, and had great compassion with them and others who could not find any jobs.

In these days, Oppie's sympathies were quite left-wing. He contributed to a strike fund of the Longshoremen's Union and to various committees helping the Spanish Loyalists in the Civil War. His brother and his sister-in-law were members of the Communist Party for some time; he himself apparently never joined. As far as I can tell, he moved away from the party in 1939 and 1940. He was disgusted by the pact between Stalin and Hitler which permitted Hitler to start the Second World War. He was deeply distressed by the fall of France in 1940. I saw him shortly thereafter at an evening party when he spoke long and eloquently about the terrible tragedy that the fall of France meant to Western civilization. Clearly he entirely disagreed with the Communist slogan that this was 'An imperialist war'.

In 1936 he was promoted to a full professorship at Berkeley and CalTech. In 1941 he was elected to the United States National Academy of Sciences.

In 1940 Oppenheimer married Katherine Harrison. They had one son, Peter, and a daughter Katherine. They lived in a most beautiful house on Eagle Hill, overlooking all of San Francisco Bay, where I (and of course even more his Berkeley friends) spent many happy hours.

LOS ALAMOS

In 1942, Oppenheimer felt the deep urge to contribute to the American war effort. The opportunity came soon. He was appointed the leader of a theoretical effort to design the atomic bomb.

By the summer of 1942 it was very likely that Fermi's atomic pile would work, that Dupont would build a production reactor, and that useful quantities of plutonium would be produced. The separation of uranium-235 by the electromagnetic method, though extremely expensive, also seemed very likely to succeed; the separation by gaseous diffusion was less certain. In any case, the committee in charge of the uranium project considered it advisable to begin a serious study of the assembly of a weapon. It proved accurate timing. In 1945, the preparations for the assembly of the weapon were finished just about the same time that the necessary amounts of material became available.

Oppenheimer assembled a small group of theoretical physicists: Teller, who had been working on the atomic pile in Chicago, Van Vleck and myself who had been working on radar, Konopinski, Serber who was then associated with Oppenheimer, and three of his own graduate students. Some members of our group, under the leadership of Serber, did calculations on the actual subject of our study, the neutron diffusion in an atomic bomb and the energy yield obtainable from it. The rest of us, especially Teller, Oppenheimer and I, indulged ourselves in a far-off project—namely, the question of whether and how an atomic bomb could be used to trigger an H-bomb. Grim as the subject was, it was a most exciting enterprise. We were forever inventing new tricks, findings ways to calculate, and rejecting most of the tricks on the basis of the calculations. Now I could see at first-hand the tremendous intellectual power of Oppenheimer who was the unquestioned leader of our group. The ideas we had about triggering an H-bomb later turned out to be all wrong, but the intellectual experience was unforgettable.

In the fall of 1942 plans were started for a more permanent laboratory to investigate the assembly of a nuclear bomb. Oppenheimer chose its location, together with General Groves who was by then in charge of the 'Manhattan Project'. General Groves wanted a remote place in order to keep the secrecy of the project. Oppie knew just the place. He had spent many happy summers in the Pecos Valley in New Mexico, on a ranch, owned by him and his brother. He knew about the Los Alamos Ranch School, an expensive boarding school for boys, which was in bad financial condition. The school was bought out and the Government established its laboratory on one of the most beautiful mesas in New Mexico, with a splendid view of the Sangre de Cristo Mountain Range across 30 miles of the Rio Grande Valley. Pleasant aspen forests rose from Los Alamos to the crest of a minor mountain range, the Jemez, and gave the inhabitants of Los Alamos many opportunities for pleasant hikes, horseback rides and ski expeditions.

Oppenheimer searched the country for the best experimental and theoretical nuclear physicists, for general physicists, chemists and engineers. The task was difficult because many of the best people were already deeply engaged in war work, and some were reluctant to leave this work which promised immediate applicability in World War II, for the remote possibility of an atomic bomb. Nevertheless a magnificent staff was assembled.

Oppenheimer had the great desire to identify with the U.S. war effort, and was quite ready to accept a commission as a Lt.-Colonel in the U.S. Army as was desired by General Groves. The better judgment of some of his colleagues, more experienced in scientific war work, prevented him and the rest of us from becoming integrated into the Army machinery. Of course the Army had charge of guarding the laboratory, of construction of both laboratory and civilian housing, of the civil administration of the town and essentially of all our lives. But in scientific matters the laboratory remained independent.

It was not obvious that Oppenheimer would be its director. He had, after all, no experience in directing a large group of people. The laboratory would be devoted primarily to experiment and to engineering, and Oppenheimer was a theorist. It is greatly to the credit of General Groves that he overruled all these objections and made Oppenheimer the director.

It was a marvellous choice. Los Alamos might have succeeded without him, but certainly only with much greater strain, less enthusiasm, and less speed. As it was, it was an unforgettable experience for all the members of the laboratory. There were other wartime laboratories of high achievement, like the Metallurgical Laboratory at Chicago, the Radiation Laboratory at M.I.T., and others, both here and abroad. But I have never observed in any of these other groups quite the spirit of belonging together, quite the urge to reminisce about the days of the laboratory, quite the feeling that this was really the great time of their lives.

The scientific work at Los Alamos has often been described. I will quote the description by Victor Weisskopf in *Physics Today*:

> 'The task facing Oppenheimer and his collaborators was stupendous. When the work started at Los Alamos not much more was known than the fundamental ideas of a chain reaction. What happens in a nuclear explosion had to be theoretically predicted in all details for the design of the

bomb since there was no time to wait for experiments; no fissionable material was available yet. The details of the fission process had to be understood. The slowing down of neutrons in matter and the theory of explosions and implosions under completely novel conditions had to be investigated. Nuclear physicists had to become experts in fields of technology unknown to them such as shock waves and hydrodynamics. Oppenheimer directed these studies, theoretical and experimental, in the real sense of the words. Here his uncanny speed in grasping the main points of any subject was a decisive factor; he could acquaint himself with the essential details of every part of the work.

'He did not direct from the head office. He was intellectually and even physically present at each decisive step. He was present in the laboratory or in the seminar rooms, when a new effect was measured, when a new idea was conceived. It was not that he contributed so many ideas or suggestions; he did so sometimes, but his main influence came from something else. It was his continuous and intense presence, which produced a sense of direct participation in all of us; it created that unique atmosphere of enthusiasm and challenge that pervaded the place throughout its time.'

He was everywhere at all times, and he worked incredibly long hours. Nevertheless, he still had time for some social life; in fact, the Oppenheimer house with his attractive wife was a social centre. He lived, as far as we could see, on his nervous energy. Always quite thin, he lost another twenty pounds and during a bout with measles reportedly got down to 104 lb., being six feet tall. It is remarkable that his health could stand this pace, because he was never physically strong. The one sport he loved was horseback riding. But in the three years at Los Alamos there was time only for one overnight ride on the two horses his wife fed and groomed for their use. Before Los Alamos, on his ranch, he used to keep five horses for himself and his guests.

One of the factors contributing to the success of the laboratory was its democratic organization. The governing board, where questions of general and technical laboratory policy were discussed, consisted of the division leaders (about eight of them). The coordinating council included all the group leaders, about 50 in number, and kept all of them informed on the most important technical progress and problems of the various groups in the laboratory. All scientists having a B.A. degree were admitted to the colloquium in which specialized talks about laboratory problems were given. Each of these three assemblies met once a week. In this manner everybody in the laboratory felt a part of the whole and felt that he should contribute to the success of the programme. Very often a problem discussed in one of these meetings would intrigue a scientist in a completely different branch of the laboratory, and he would come up with an unexpected solution.

This free interchange of ideas was entirely contrary to the organization of the Manhattan District as a whole. As organized by General Groves, the work was strictly compartmentalized, with one laboratory having little or no knowledge of the problems or progress of the other. Oppenheimer had to fight hard for the free discussion among all qualified members of the Los Alamos Laboratory, but the free flow of information and discussion, together with Oppenheimer's personality, kept morale at its highest throughout the war.

Weisskopf says 'One of the most important factors that kept us at work was the common awareness of the great danger of the bomb in the hands of an irresponsible dictator. After his defeat, it turned out that this danger was in fact not so great; still the work and the spirit continued until the task was accomplished, until in the desert of Alamogordo for the first time a nuclear fire was kindled by man. Every one of us, and Oppenheimer more than anyone, was deeply shaken by this event.'

For his work at Los Alamos, Oppenheimer received the Medal of Merit from President Truman in 1946, 'for his great scientific experience and ability, his inexhaustible energy, his rare capacity as an organizer and executive, his initiative and resourcefulness, and his unswerving devotion to duty. . . .'

HIS PUBLIC LIFE

It was obvious that a community like Los Alamos would be deeply concerned with the ominous implications of the atomic bomb. Oppenheimer was one of the most concerned, and had many discussions about this problem with Niels Bohr. Bohr had come to the United States in 1944 and had been asked to help us at Los Alamos. He was quite interested in our work and gave us some advice. However, his main interest was in talking to statesmen and trying to persuade them that international control of the atom was the only way to avoid a pernicious arms race or worse, atomic war. Bohr did not succeed with statesmen but he greatly impressed Oppenheimer and through him the rest of us at Los Alamos.

After the war the American scientists exerted much pressure in Washington. One of their wishes was civilian control of atomic energy rather than continued control by the Army. The Senate responded to the urging of Szilard, Condon and of the American Federation of Scientists, by setting up the McMahon Committee which after long labour, devised the Atomic Energy Act of 1946. Oppenheimer, although originally in favour of military control because it would provide a smoother transition, was an effective witness before the McMahon Committee.

More urgent still seemed the problem of international control. By the intervention of some far-sighted statesmen, President Truman was persuaded to appoint a committee to study this problem, under David Lilienthal. Oppenheimer was one of the members. Lilienthal describes the work of the committee impressively in his 'Journal'. All five members were outstanding men in business, engineering or science. But Oppenheimer brought to it the years of experience of creation of the atomic bomb. The work of the committee, although all its members contributed, was primarily that of Oppenheimer. Lilienthal said of him, 'He was the only authentic genius I have ever met.'

The Lilienthal Report which was then endorsed by Under-Secretary of State Dean Acheson called for the creation of an international authority to control all atomic-energy work. The plan emphasized the need for a positive task for the international authority. It should develop atomic reactors for power and other peaceful uses, and also atomic weapons if desired; it should not have merely the function of a policeman preventing individual nations from developing atomic energy and weapons on their own. This wise plan became official U.S. policy. Its presentation to the United Nations was entrusted to Bernard Baruch, a very respected and very conservative elder statesman. Unfortunately Baruch's advisers and Baruch himself, changed the emphasis: instead of pointing to the great joint task of developing peaceful uses of atomic energy, Baruch placed the main emphasis on the 'condign' punishment of violators of the agreement to be concluded. I do not know whether there was ever any chance of acceptance of the plan by the Soviet Union, that country being at the time exclusively concerned with its own national interest. But if there ever was a chance it was lost by the manner of Baruch's presentation.

Oppenheimer was one of the first to see that the plan would be rejected by the U.S.S.R. Most of the members of the Federation of American Scientists held on to hope beyond hope. His realism, as well as his official duties, kept Oppenheimer rather separate from the Federation and other political organizations of the scientists.

His first government appointment was in 1945, as a member of Secretary of War Stimson's Scientific Panel of the War Department's Interim Committee on Atomic Energy. This panel was asked, before Hiroshima, whether there was any technically effective alternative to dropping the bomb on Japan; its answer was negative. Later, an enlarged panel was asked what to do with atomic energy after the war. The members of this enlarged panel were Oppenheimer, members of the other wartime laboratories of the Manhattan District, and several elder-statesmen scientists. One of the committee's meetings took place at Los Alamos, and some other Los Alamos scientists were asked to participate. I remember this meeting very vividly. All of the participants were impressive people who had made great contributions. Nevertheless, whenever Oppenheimer left the room, discussion slid back into fairly routine problems, such as the specific nuclear reactions one should investigate and the kind of research that could be done with a nuclear reactor. On his return, the level of the discussion immediately rose and we all had the feeling that now the meeting had become really worth while.

Oppenheimer's most important Government task was to be Chairman of the Atomic Energy Commission's (AEC) General Advisory Committee (GAC) from 1946 to 1952. This most important body included Fermi, Rabi, Conant, Dubridge, Smythe and Seaborg (both later AEC Commissioners) and two industrialists, Worthington and Rowe. It advised the Commission not only on scientific matters but also on matters of general policy. It was a hard-working committee, having about six sessions a year, of three days each, mostly over week-ends. In the words of Seaborg 'At the conclusion of each session, when the AEC Commissioners came in to review our work, Oppie presented a masterful summary of the proceedings. I know that my fellow members of the GAC remember with me that this was pure

Oppenheimer at his very best. I regret that tape-recordings were not made of these eloquent summations of our deliberations, for I believe that these would provide fascinating historical material.'

The first task of the GAC and AEC was to strengthen the position of the U.S. in the production and military use of fissionable material. The plutonium production plants at Hanford had to be improved and further ones had to be built. Oppenheimer devoted much time to strengthening the Los Alamos Laboratory after many of its members had left at the end of the war, as well as supporting the other AEC laboratories, Oak Ridge and Argonne.

These latter two laboratories were given the specific task of developing nuclear power. Oppenheimer had the great desire to foster peaceful applications but, like most of his colleagues on the GAC, he was overly pessimistic about economic possibilities. In a talk at this time, he thought that the application of isotopes in research would for a long time remain the most important peaceful application of atomic energy. In a sense he was right; it took about ten years before large-scale power reactors were constructed in the United States and only recently have they become economical.

Oppenheimer was deeply devoted to the support of fundamental research in nuclear physics. The Brookhaven National Laboratory was established for this specific purpose, the Radiation Laboratory at Berkeley was generously supported, and many university projects for the construction of high energy accelerators and their use were financed. The AEC was one of the chief contributors to the tremendous expansion in research in physics in the United States, and Oppenheimer and his GAC gave much encouragement to the Commission to do so. Oppenheimer strongly advocated to make fundamental scientific information available to scientists all over the world and distributing special materials, such as radio-isotopes, freely to scientists abroad.

In military applications, Oppenheimer was one of the first advocates of a system to detect foreign nuclear weapons tests. He proposed this while still at Los Alamos. He then supported strongly the programme to develop techniques for detection in 1948 to 1950. This was one of his many functions as Chairman of the Committee on Atomic Energy of the Joint Research and Development Board of the Armed Services. In addition this committee was concerned with the proper application of atomic weapons in warfare. Its membership was half civilian, half military. His efforts to get a detection system established bore fruit on 29 August 1949 when the first Soviet atom bomb was exploded. A panel of the Committee on Atomic Energy including Oppie himself, scrutinized the evidence presented and concluded that indeed a weapons test had taken place in the Soviet Union.

He served the Joint Research and Development Board from 1947 to 1952, also in other capacities. He was a member of the National Research Advisory Committee from 1949 to 1952, and of the Secretary of State's Panel on Disarmament in 1952 and 1953. Most important of these committees was the Science Advisory Committee (1951-1954). It was then part of the Office of Defense Mobilization and later developed into the President's Science Advisory Committee.

More important still, he participated in many summer studies on the effect of nuclear weapons on military tactics and strategy. In particular, in the Vista project, the study group urged that the U.S. should not place its main reliance on strategic atomic weapons and massive retaliation, but should rather develop tactical nuclear weapons to defend Western Europe against possible Russian attack. This advice was very unpopular in many quarters of the Air Force, devoted primarily to strategic bombing.

In 1949, after the U.S.S.R. had exploded its first atomic weapon, the work of the GAC reached a crisis. As a response to the Soviet explosion, Edward Teller and Ernest Lawrence proposed that the U.S. should develop H-bombs. The GAC wrote a strong recommendation against the crash development of the 'super'. All members of the Committee agreed on this (Seaborg did not attend, after writing a letter stating that he was quite undecided).

One important argument of the GAC was that there was, at that time, no sufficient technical basis for this development (the crucial invention was made in 1951, by Teller). Another strong argument was that the U.S. should not deliberately step up the arms race, and should at least first make an effort to discuss with Soviet Russia the possibility of an agreement not to develop hydrogen weapons. A more radical minority report was written by Fermi and Rabi.

For about three months the issue was hotly debated in Washington. The Joint Committee on Atomic Energy of the Congress enthusiastically endorsed the proposal by Teller and Lawrence. Lilienthal, Chairman of the AEC, supported the GAC position and writes in his 'Journal' about the nervous strain of this battle. The decision probably came when Acheson, the Secretary of State, endorsed the H-bomb plan. At the end of January 1950 President Truman decided to pursue with full vigour the design and manufacture of an H-bomb.

He probably could not have decided any other way at the time. However, it is most deplorable that time and again nations have decided in favour of another step in armament without first trying to obtain mutual agreement with other nations to refrain from new escalation of death. The effort of Oppenheimer and the GAC to make the U.S. Government pause and think about this step stands as a most important milestone.

After President Truman had overruled the committee, Oppenheimer tried to resign as Chairman of the Committee, but the resignation was not accepted, probably wrongly.

THE SECURITY INVESTIGATION

1953 was a difficult year in U.S. politics. Senator Joseph McCarthy charged nearly anyone he could think of with being a Communist, and hence a traitor to the United States. Since McCarthy's charges had contributed much to the defeat of the Democrats in the Presidential elections of 1952, the new Republican government let him have free rein for a long time.

That Robert Oppenheimer would be one of the victims was foreshadowed in a scurrilous article in *Fortune* in 1953. The author had collected much material from disgruntled officers of the Air Force who were opposed to Oppenheimer's defence policy. Although they had won the battle for massive retaliation they wanted to defeat the 'enemy' completely. A former employee of the Joint Congressional Committee on Atomic Energy in a nearly paranoic letter, accused Oppenheimer of being a Communist and working against the interest of the U.S. Oppie had also made some personal enemies, and on the basis of all this, in December 1953, President Eisenhower ordered that Oppenheimer's clearance for secret government work be terminated. This was communicated to him by the AEC in December 1953. Oppenheimer answered the charges in a long letter, and both charges and answer were published in the *New York Times,* on 13 April, 1954.

Oppenheimer chose to have a security investigation which was organized essentially like a Court of Law with a Board of three judges, and lawyers both for the government and for the defence. He chose to face this investigation in spite of the fact that he was quite convinced from the beginning that he would lose his case.

The ensuing, long-protracted security investigations became a *cause célèbre*. Many of his scientist friends came out in his defence, a few against him. The *Proceedings*, published by the AEC, give a vivid story of the discussions within the U.S. Government on defence policy between 1947 and 1953. They have been avidly read by friend and foe, at home and abroad.

Both the Security Hearing Board, by a vote of 2 to 1, and the AEC, by a vote of 4 to 1 decided to withhold security clearance from Oppenheimer. In the final majority opinion by the Commission the only real argument against granting him clearance was the grotesque story involving Haakon Chevalier in 1942. Intrinsically this 'espionage attempt' was of no importance whatever; the counter-intelligence corps did not even bother to investigate the lead until May 1946. But apparently Oppenheimer, in an effort to shield his friend Chevalier, and at the same time not to endanger his position as Head of the Los Alamos Laboratory, had first invented a very foolish 'cock-and-bull-story' and then later denied it.

The importance attached to this incident is the more astonishing as (1) these facts had all been known to General Groves who had cleared him for wartime work; (2) the same facts were scrutinized by the whole AEC in 1947 and again clearance was granted for the most delicate atomic energy work. One of the members of the AEC in 1947 was Lewis Strauss who, in 1954, wrote the majority opinion of the AEC against him. It is hard to imagine that this old story could have attained so much greater importance between 1947 and 1954.

The scientific community, with few exceptions, was deeply shocked by the decision of the AEC. An eloquent discussion was given by Bush, the wartime leader of the U.S. Science Defence effort, in the New York *Times Magazine*, 13 June, 1954. Personally I felt that the AEC which I had always regarded as our, the scientists', agency in the government, had become a hostile body.

The AEC soon made efforts to reconcile the scientific community. Perhaps most important was the appointment of John Von Neumann, the noted mathematician, as a second scientific member of the Commission. He was universally respected, by the friends of Oppenheimer as well as those of Teller. Soon afterwards Joseph McCarthy's agitation ended when a Senate Committee investigated his own behaviour as a committee chairman, and this led to McCarthy's censure by the Senate. The political climate generally improved.

But it took until 1961 for the Government to make amends to Oppenheimer, President John F. Kennedy invited Oppenheimer to a White House dinner given in honour of Nobel Prize Winners. The most important recognition, however, was the presentation to him of the Fermi Award of the AEC, the highest honour that body can bestow. It carries a prize of $50 000.

THE FERMI AWARD

The decision to present the Award was made by President Kennedy, the actual presentation by President Johnson in December 1963. On the presentation President Johnson said in part: 'Dr. Oppenheimer, I am pleased that you are here today to receive formal recognition for your many contributions to theoretical physics and to the advancement of science in our nation. Your leadership in the development of an outstanding school of theoretical physics in the

United States and your contributions to our basic knowledge make your achievements unique in the scientific world.'

In his acceptance remarks Oppenheimer said, 'I think it is just possible, Mr President, that it has taken some charity and some courage for you to make this award today.'

THE PRINCETON PERIOD

In 1947, Oppenheimer was appointed Director of the Institute for Advanced Study in Princeton. The Institute had always included prominent physicists: Albert Einstein had been one of its Charter Members appointed in 1933. Bohr and Dirac had been frequent visitors, and Pauli spent the war years there. A number of other well-known physicists had worked at the Institute at one time or another.

But on Oppenheimer's arrival, the physics department of the Institute changed. While its emphasis had been on well-established professors before, it now became a centre for young physicists. Five research associates from Berkeley came with him in 1947. Thereafter the Institute was open to dozens of post-doctoral fellows, from the United States and abroad. Even more than Berkeley in the 1930's, the Princeton Institute became the centre for physics. Nearly everybody who was anybody passed through its stimulating atmosphere. Murray Gell-Mann, Marvin L. Goldberger, Geoffrey F. Chew, Frances E. Low, Yoichira Nambu, were among the American visitors, Maurice Levy came from France, Lehman and Symancik from Germany, and countless visitors from Great Britain, Italy, Japan and other countries. There was a distinguished permanent staff including Pais, Dyson, Placzek, T. D. Lee and C. N. Yang. The distinguished visitors of old times continued to come.

Oppenheimer brought to the Institute his whole method of inspired teaching. He no longer did much research of his own, but he constantly inspired his collaborators. The seminars which he directed were always very lively. In 1948 I gave one of these seminars, on some calculations concerning the Lamb shift. I spoke for less than half the time and this, I was told, was a much larger fraction of the time than was customary in the seminar. The rest was discussion by the many bright young physicists, and especially by Oppenheimer himself. Ideas developed fast in this atmosphere of intense discussion and stimulation.

Vigourous discussion as well as emphasis on fundamental problems remained Oppenheimer's style. All through his life he was able to convey to all around him a sense of excitement in the quest of science.

He could also irritate the people who worked with him. His great mind was able to read and digest physics much faster than the minds of his less gifted colleagues. In scientific conversation he always assumed that others knew as much as he. This being seldom the case and few persons being willing to admit their ignorance, his partner often felt at a disadvantage. Yet, when asked directly, he explained willingly.

Abraham Pais writes of his influence at the Institute: 'He could convey to young men a sense of extraordinary relevance of the physics of their day and give them a sense of their participation in a great adventure, as for example in the Richtmyer lecture: "There are rich days ahead for physics; we may hope, I think, to be living in one of the heroic ages of physical science, whereas, in the past, a vast new field of experience has taught us its new lessons and its new order."

'He could define and thereby enhance their dedication, by words such as these: "People who practice science, who try to learn, believe that knowledge is good. They have a sense of guilt when they try to acquire it. This keeps them busy . . . It seems hard to live any other way than thinking that it was better to know something than not to know it; and that the more you know the better, provided you know it honestly."

'To an unusual degree, Oppenheimer possessed the ability to instill such attitudes in the young physicists around him, to urge them not to let up. He could be critical, sharply critical at times, of their efforts. But there was no greater satisfaction for him than to see such efforts bear fruit and then to tell others of the work that someone had done.'

In addition to his work at the Institute, he was a leading spirit for many years at the Conferences on Physics which started from a small basis and then expanded into international scope.

Pais writes: 'The first such conference in physics took place on 2-4 June 1947, on Shelter Island, New York. For this meeting Oppenheimer wrote the outline of topics for discussion entitled "The foundations of quantum mechanics". As was to happen so often in the following years, Oppenheimer showed himself to be the three-fold master: by stressing the important problems, by directing the discussion and by summarizing the findings.

'In his outline he discussed the copiousness of meson production in cosmic radiation in terms of meson theories then current and concluded that "no reasonable formulation along this line will satisfactorily account for the smallness of the subsequent interaction of mesons with nuclear matter". In the discussion of this point, Robert Marshak got up to propose that there should be two kinds of mesons. It was, one may recall, in September of that year that Cecil F. Powell reported the discovery of π decay at a Copenhagen conference.'

The Shelter Island Conference witnessed the opening of a new chapter in quantum electrodynamics. Willis Lamb, one of Oppenheimer's Ph.D students (1938), gave an account of his experiment on the upward energy shift of the 2-*S* state of hydrogen. Rabi reported on a deviation in the hyper-fine structure of hydrogen and deuterium from theory.

Immediately Oppenheimer emphasized that here one might be faced with self-energy effects. This subject was close to his mind: as early as 1930 he had been concerned with atomic level displacements due to radiative effects. Oppenheimer's remarks, and a talk by Kramers, stimulated me, immediately after the Shelter Island Conference, to explain the Lamb shift as a residual self-energy effect due to the interaction of the electron with the electromagnetic field. My theory was only non-relativistic. But at the next conference, at Pocono Manor in April 1948, Schwinger and Feynman discussed their different, relativistic solutions for the self-energy effects. The Old-Stone-On-Hudson meeting, a year later, discussed further development of the theory.

At these conferences Oppenheimer was the undisputed leader. Pais found some comments in old notes from the Pocono Conference. By Oppenheimer: 'Now it doesn't matter that things are infinite.' By Rabi: 'What the hell should I measure now?' Pais remarks: 'They reflect the sense of optimism of the late forties, especially the expectation that with the new theoretical tools other than electromagnetic interactions would soon give sensible results.'

Oppenheimer continued to play a leading role in the Conferences, which from then on developed into the Rochester Conferences. The latter soon became international. They were among the first conferences which brought together the scientists from East and West. And they have continued to do so, through easy and difficult political times. This role of science to bring together people of different countries and different political opinions, was very much Oppie's desire.

Oppenheimer had become widely known as a principal representative figure of the natural sciences. Thus, when in 1948 the American Institute of Physics inaugurated a new journal, *Physics Today*, the dialogue between theory and experiment was symbolized on the cover of its first issue by a picture of a pork-pie sombrero, Oppenheimer's well-known symbol, tossed on a cyclotron. When in 1950 the *Scientific American* devoted an issue to summarize that incredibly full half-century in science, 1900-1950, it was fitting that Oppenheimer should write its general introduction.

In the 1950's, the Institute at Princeton continued to play its leading role. One of the main problems was the profusion of new particles which had been discovered. Pais was one of the men who brought some order into this chaos. Later on Yang & Lee solved a great puzzle in the behaviour of the *K*-meson by postulating that parity need not be conserved in weak decays. Astrophysics and statistical mechanics were also successfully pursued at the Institute. Oppenheimer was always there to stimulate, criticize, encourage and clarify. Even to the last days (I saw him a few months before his death, when he was already very ill) he followed all of particle and theoretical physics with avidity, and discussed the problems with profundity, and with curiosity about the next step.

WRITINGS ON GENERAL TOPICS

Ever since the Second World War, Oppenheimer's own writings and talks were concerned with general subjects rather than with physics. There is an impressive list of them, about 125. He was invited to give lectures at many universities, and in other distinguished settings, like the Reith Lectures of the B.B.C. In his lectures he cast a spell over his audience with his marvellous command of the English language. It was a pleasure just to listen to him and watch how he formulated his thoughts. He added much wit and a store of good anecdotes, and most importantly, the signs of deep concern about humanity.

Probably his greatest concern was the relation between modern science and the general culture of our time. He was troubled that the tremendous increase of knowledge makes it impossible for an intelligent, educated man to cope with even the more important parts of knowledge. His concern resembled that of C. P. Snow about the 'two cultures', but was more profound, partly I think because Oppie himself was a creative scientist. He worried about the increasing gap between specialized knowledge and common sense, the increasing gap between neighbouring sciences, and even between different branches within his own science of physics. He said: 'Even in physics we do not entirely succeed in spite of a passion for unity which is very strong.'

This activity has again been well summarized by Pais: 'Briefly, then, what Oppenheimer had in mind was this. First, he addressed himself to what is loosely called the intellectual community. He wished to foster a common understanding primarily within this community. Second, as a example of what in his opinion could profitably be shared, he mentions the lesson of quantum theory which we call complementarity. He wished and in fact tried to explain this lesson to the biologist, the statesman and the artist because he believed that what to the physicist is a technique represents at the same time a general way of thinking that could be liberating to all. Third, he saw a two-fold duty for our education system. In the face of increasing demands on education we should continue to stress that the cultural life of sciences lies almost entirely in the intimate view of the professional. At the same time, "no man should escape our universities without . . . some sense of the fact that not through his fault, but in the nature of things, he is going to be an ignorant man, and so is everyone else".

'Of the great effort needed to achieve these aims he said the following: "I think that, with the growing wealth of the world, and the possibility that it will not all be used to make new committees, there may indeed be genuine leisure, and that a high commitment on this leisure is that we re-knit the discourse and the understanding between the members of our community. As a start, we must learn again, without contempt and with great patience, to talk to one another; and we must hear."'

As a move toward bridging the gap between various disciplines he invited many psychologists and historians for temporary visits to the Institute. He talked enthusiastically of the progress psychologists were making in understanding the process of learning.

Another subject of great concern to him was atomic power and the politics related to it. He gave many lectures on this, before colleges, general audiences and to young people. He wrote about it in the prestigious journal *Foreign Affairs*. He discussed the decision to drop the atomic bomb, international control of atomic energy, and Secretary Stimson's role in the development of the bomb. His opinion was always moderate; he thought that the development of the bomb and its drop had been inevitable, but that the world should make every effort that the bomb should not be used again. He also wrote about specific subjects, such as the functions of the International Agency on Atomic Energy to which he was much devoted.

Some of his writings are in response to the many honours he received, and the many interviews he was asked to give. Others are personal tributes to other scientists: he was a very good friend who would not forget his friends.

Other writings are predictions of the development of physics in the future, summaries of conferences and of developments in physics such as 'symmetries of forces', and '30 years of mesons'.

His reputation as a scientist and a symbol was at least as great in Great Britain and France as it was in the United States. He paid frequent visits to both countries, and was much honoured in both.

Again I would like to quote Pais: 'Any single one of the following contributions would have marked Oppenheimer out as a pre-eminent scientist: his own research work in physics; his influence as a teacher; his leadership at Los Alamos; the growth of the Institute for Advanced Study to a leading centre of theoretical physics under his directorship; and his efforts to promote a more common understanding of science. When all is combined, we honour Oppenheimer as a great leader of science in our time. When all is interwoven with the dramatic events that centred around him, we remember Oppenheimer as one of the most remarkable personalities of this century.'

Oppenheimer will be remembered by the world and by his country. He will leave a lasting memory in all the scientists who have worked with him, and in the many who have passed through his school and whose taste in physics was formed by him. His was a truly brilliant mind, best described by his long-time associate Charles Lauritsen: 'This man was unbelievable. He always gave you the answer before you had time to formulate the question.'

The photograph of Oppenheimer was taken by Ulli Steltzer.

BIBLIOGRAPHY

SCIENTIFIC PAPERS

1926. Quantum theory and intensity distribution in continuous spectra. *Nature, Lond.* **118**, 771.

1925-27. On the quantum theory of vibration-rotation bands. *Proc. Camb. Phil. Soc.* **23**, 327-335.

1925-27. On the quantum theory of the problem of the two bodies. *Proc. Camb. Phil. Soc.* **23**, 422-431.

1926. Quantentheorie des kontinuierlichen Absorptionsspektrums. *Naturwissenschaften,* **14**, 1282.

1927. On the quantum theory of the polarization of impact radiation. *Proc. Nat. Acad. Sci. Wash.* **13**, 800-805.

1927. Bemerkung zur Zerstreuung der α = Teilchen. *Z. Phys.* **43**, 413-415.

1927. Zur Quantentheorie kontinuierlicher Spektren. *Z. Phys.* **41**, 268-293.

1927. Zur Quantenmechanik der Richtungsentartung. *Z. Phys.* **43**, 27-46.

1927. (With M. Born.) Zur Quantentheorie der Molekeln. *Annln Phys.* **84**, 457-484.

1928. Three notes on the quantum theory of aperiodic effects. *Phys. Rev.* **31**, 66-81.

1928. On the quantum theory of the capture of electrons. *Phys. Rev.* **31**, 349-356.

1928. On the quantum theory of field currents. *Phys. Rev.* **31**, 914.

1928. On the quantum theory of electronic impacts. *Phys. Rev.* **32**, 361-376

1928. On the quantum theory of the Ramsauer effect. *Proc. Nat. Acad. Sci. Wash.* **14**, 261-262.

1928. On the quantum theory of the autoelectric field currents. *Proc. Nat. Acad. Sci. Wash.* **14**, 363-365.

1929. Uber die Strahlung der Freien Elektronen im Coulombfeld. *Z. Phys.* **55**, 725-737.

1903 (With Harvey Hall.) Why does molecular hydrogen reach equilibrium so slowly? *Phys. Rev.* **35**, 132-133.

1930. Note on the theory of the interaction of field and matter. *Phys. Rev.* **35**, 461-477.
1930. On the theory of electrons and protons. *Phys. Rev.* **35**, 562-563.
1930. Two notes on the probability of radiative transitions. *Phys. Rev.* **35**, 939-947.
1931. Selection rules and the angular momentum of light quanta. *Phys. Rev.* **37**, 231.
1931. Note on the statistics of nuclei. *Phys. Rev.* **37**, 232-233.
1931. (With P. Ehrenfest.) Note on the statistics of nuclei. *Phys. Rev.* **37**, 333-338.
1931. (With Harvey Hall.) Relativistic theory of the photoelectric effect by Harvey Hall: Part II—Photoelectric absorption of ultragamma radiation. *Phys. Rev.* **38**, 57-79.
1931. Note on light quanta and the electromagnetic field. *Phys. Rev.* **38**, 725-746.
1931. (With J. F. Carlson.) On the range of fast electrons and neutrons. *Phys. Rev.* **38**, 1787-1788 (1931); (Abstract) *Phys. Rev.* **39**, 864-865. (1932.)
1932. (With J. F. Carlson.) Impacts of fast electrons and magnetic neutrons. *Phys. Rev.* **41**, 763-792.
1933. Disintegration of lithium by protons. *Phys. Rev.* **43**, 380.
1933. (With M. S. Plesset.) The production of the positive electron. *Phys. Rev.* **44**, 53-55.
1933. (With Leo Nedelsky.) The production of positives by nuclear gamma-rays. *Phys. Rev.* **44**, 948-949; (Abstract) *Phys. Rev.* **45**, 136. (1934); (Errata) *Phys. Rev.* **45**, 283. (1934.)
1934. (With W. H. Furry.) On the theory of the electron and positive. *Phys. Rev.* **45**, 245-262: (Letter) *Phys. Rev.* **45**, 343-344.
1934. The theory of the electron and positives. *Phys. Rev.* **45**, 290.
1934. (With W. H. Furry.) On the limitation of the theory of the positron. *Phys. Rev.* **45**, 903-904.
1934. (With C. C. Lauritsen.) On the scattering of Th C” gamma-rays. *Phys. Rev.* **46**, 80-81.
1935. Are the formulae for the absorption of high energy radiation valid? *Phys. Rev.* **47**, 44-52.

1935. Note on charge and field fluctuations. *Phys. Rev.* **47**, 144-145.

1935. Notes on the production of pairs by charged particles. *Phys. Rev.* **47**, 146-147.

1935. The disintegration of the deuteron by impact. *Phys. Rev.* **47**, 845-846.

1935. (With M. Phillips.) Note on the transmutation function for deuterons. *Phys. Rev.* **48**, 500-502.

1936. On the elementary interpretation of showers and bursts. *Phys. Rev.* **50**, 389.

1936. (With Robert Serber.) The density of nuclear levels. *Phys. Rev.* **50**, 391.

1937. (With J. F. Carlson.) On multiplicative showers. *Phys. Rev.* **51**, 220-231.

1937. (With G. Nordheim, L. W. Nordheim & R. Serber.) The disintegration of high energy protons. *Phys. Rev.* **51**, 1037-1045.

1937. (With R. Serber.) Note on the nature of cosmic ray particles. *Phys. Rev.* **51**, 1113.

1937. (With F. Kalckar & R. Serber.) Note on nuclear photoeffect at high energies. *Phys. Rev.* **45**, 273-278.

1937. (With F. Kalckar & R. Serber.) Note on resonances in transmutations of light nuclei. *Phys. Rev.* **52**, 279-282.

1938. (With R. Serber.) Note on boron plus proton reactions. *Phys. Rev.* **53**, 636-638.

1938. (With R. Serber.) On the stability of stellar neutron cores. *Phys. Rev.* **54**, 540.

1939. (With G. M. Volkoff.) On massive neutron cores.*Phys. Rev.* **55**, 374-381.

1939. (With H. Snyder.) On continued gravitational contraction. *Phys. Rev.* **56**, 455-459.

1939. (With J. S. Schwinger.) On pair emission in the proton bombardment of fluorine. *Phys. Rev.* **56**, 1066-1067.

1939. In behaviour of high energy electrons in cosmic radiation by C. G. Montgomery and D. C. Montgomery; *Discussion* by J. R. Oppenheimer. *Rev. Mod. Phys.* **11**, 264-266.

1939. Celebration of the sixtieth birthday of Albert Einstein. *Science* **89**, 335.

1940. (With H. Snyder & R. Serber.) The production of soft secondaries by mesotrons. *Phys. Rev.* **57**, 75-81.
1940. On the applicability of quantum theory to mesotron collisions. *Phys. Rev.* **57**, 353.
1941. On the spin of the mesotron. *Phys. Rev.* **59**, 462.
1941. On the selection rules in beta-decay. *Phys. Rev.* **59**, 908.
1941. (With J. Schwinger.) On the interaction of mesotrons and nuclei. *Phys. Rev.* **60**, 150-152.
1941. Internal conversion in photosynthesis. *Phys. Rev.* **60**, 158.
1941. (With R. Christy.) The high energy soft component of cosmic rays. *Phys. Rev.* **60**, 159.
1941. (With E. C. Nelson.) Multiple production of mesotrons by protons. *Phys. Rev.* **60**, 159-160.
1941. On the internal pairs from oxygen. *Phys. Rev.* **60**, 164.
1941. The mesotron and the quantum theory of fields. In: Enrico Fermi *et al., Nuclear physics*, Philadelphia, University of Pennsylvania Press, pp. 39-50.
1942. (With E. C. Nelson.) Pair theory of meson scattering. *Phys. Rev.* **61**, 202.
1946. (With H. A. Bethe.) Reaction of radiation on electron scattering and Heitler's theory of radiation damping. *Phys. Rev.* **70**, 451-457.
1948. (With H. W. Lewis & S. A. Wouthuysen.) The multiple production of mesons. *Phys. Rev.* **73**, 127-140.
1948. (With S. T. Epstein & R. J. Finkelstein.) Note on stimulated decay of negative mesons. *Phys. Rev.* **73**, 1140-1141.
1949. Discussions on the disintegration and nuclear absorption of mesons. Remarks on μ-decay. *Rev. Mod. Phys.* **21**, 34-35.
1950. (With William Arnold.) Internal conversion in the photosynthetic mechanism of blue green algae. *J. Gen. Physiology* **33**, 423-435.

LECTURES, SPEECHES, BROADCASTS AND NEWSPAPER ARTICLES

1944a. Cosmic rays: Report of recent progress. Univ. of California.
1945a. The atomic age. N.Y. Philharmonic Symphony Hour.
1945b. Atomic weapons. American Phil. Society and National Academy of Sciences.
1945c. The bomb and the world. National Policy Comm. Conference.

1946a. The turn of the screw. F.A.S. Book, *One World or None.*
1946b. The atom bomb and college education. University of Pennsylvania.
1946c. Atomic explosives. Westinghouse Century Forum, pubd. *N.Y. Times.*
1946d. Scientific information to USAEC, UNAEC, Bibliography.
1946e. The scientist in contemporary society. Princeton Univ. Bicentennial Broadcast.
1946f. The new weapon. *One World or None.* (F.A.S.)
1946g. International control of atomic energy. Bulletin of Atomic Scientists; Foreign Affairs; *Seven Minutes to midnight,* pubd. Basic Books, Inc., N.Y.
1947a. Richtmeyer Lecture, APS and AA Physics Teachers' Meeting, pubd. Science Service Wire Report.
1947b. Scientific foundations for world order. Denver Univ. pubd. pamphlet form and in book, *Foundations for world order,* Univ. of Denver.
1947c. Functions of International Agency in Research and Development. Condensed version in *Bulletin of Atomic Scientists.*
1947d. Atomic energy as a contemporary problem. National War College.
1947e. Physics in the contemporary world. M.I.T.
1948a. Some aspects of the problems of atomic energy. N.Y. Bar Association.
1948b. Physical research in the near future. Cooper Union, N.Y.
1948c. The growth of understanding of the atomic world. Princeton University.
1948d. Multiple production of meson. Lewis-Oppenheimer-Wouthuysen, P.R. **73**, 127.
1948e. Concluding remarks to cosmic ray symposium. CalTech.
1948f. Notes on science and practice. Harvard University, Lawrence Science School.
1949a. Some thoughts on the place of science in today's world. Smith College Lecture.
1949b. Statements for *March of time* (Movies).
1949c. Letter to Senator McMahon. *Bulletin of Atomic Scientists.*
1949d. Discovery and application of sources of nuclear energy. Johns Hopkins Univ.

1950a. Response. In *Fateful decision*, NBC Program, pubd. *Bulletin of Atomic Scientists.*
1950b. The atomic age. National War College.
1950c. The age of science. *Scientific American.*
1950d. The encouragement of science. Westinghouse Science Talent Search.
1951a. Contemporary problems of atomic energy. N.Y. Bar Association.
1953a. The scientist in society. Princeton University Graduate Council Talk.
1953b. Contributions of computers in research. IBM Seminar.
1953c. Atomic weapons and American policy. *Foreign Affairs.*
1953d. Science and the common understanding, Reith Lectures. BBC, Nov. 1953, pubd. Simon & Schuster, 1953, Oxford University Press, 1954; Paper edition, 1966; Translations—French, Spanish, German, Danish.
1954a. The world we live in. *Life Magazine* Radio Broadcast.
1954b. Remarks at Pyramid Club Award.
1954c. A career in science. Princeton University, Career Forum.
1955a. Comments by Robert Oppenheimer. Hiroshima Diary.
1955b. Analogy in science. American Psych. Assoc. Meeting.
1955c. Science and the good old days. Princeton Old Guard Talk.
1955d. Science and public affairs. Princeton University, Woodrow Wilson School.
1955e. *The open mind* (Book), pubd. New York: Simon and Schuster.
1955f. The constitution of matter, Lecture—Oregon State, 1955; Goucher College, 1956; Northwestern Univ., 1956; Naval Research Lab, 1956; Wayne University, 1959.
1956a. Einstein article. *Reviews of Modern Physics.*
1956b. Atomic energy for peaceful uses. Daily Princetonian.
1956c. Physics tonight. American Institute of Physics.
1956d. Where is science taking us? *Saturday Review.*
1956e. Dignity of Man award. Kessler Institute.
1956f. Science and our times, Roosevelt University, pubd. excerpt 'Science and modern society' in *New Republic.*
1956g. The growth of science and the structure of culture. American Academy of Arts and Sciences Conf.
1956h. Comment for quotation in leaflet. World Universities Service.
1956i. A study of thinking. Sewanee Review of Bruner Book.

1956j. Cosmic breakthrough and a human problem. Princeton University, Graduate College Forum.
1957a. The hope of order. Harvard University, James Lecture.
1957b. Theory versus practice in American values and performance. M.I.T., American Project Conf.
1957c. Impossible choices, pubd. *Science.*
1957d. Science, values and the human community. Fulbright Conference on Higher Education, Sarah Lawrence College.
1957e. The environs of atomic power. American Assembly, Arden House.
1957f. Tolman, Richard Chase (article on), *Encyclopaedia Britannica.*
1957g. Nuclear power and international relations. Princeton Univ., NATO Conf.
1957h. Engineers and scientists. Drexel Institute of Technology.
1958a. The tree of knowledge. International Press Institute, pubd. *Harper's Magazine,* Oct. 1958.
1958b. L'Arbre de La Science. University of Paris.
1958c. Concluding remarks. Rochester/CERN Conference.
1958d. The mystery of matter. *Saturday Evening Post*; pubd. 1960 in *Adventures of the mind*, Vintage Books.
1958e. La science moderne et la raison. Société Française de Philosophie.
1958f. Science and the structure of culture. Rutgers University.
1958g. Knowledge and the structure of culture. Vassar College.
1958h. Science and the world today. Princeton Theological Seminary.
1958i. Knowledge and culture. Hampton Institute.
1958j. L'espoir de L'ordre. *Science.*
1958k. Science and statecraft. Weizmann Institute.
1958l. An inward look, foreign affairs; reprinted in *Second-Rate Brains,* Doubledays News Book.
1958m. Description des particles et interactions élémentaires. University of Paris.
1959a. Tradition and discovery. ACLS, Rochester.
1959b. The great challenge. CBS/TV.
1959c. Freedom and necessity in the sciences. Dartmouth College.
1959d. Contemporary developments in the field of science. Lawrenceville Herodotus Club.
1959e. Remarks. Dinner for Harold Taylor.

1959f. In the keeping of unreason. *Congress for cultural freedom,* pubd. Prospective.
1959g. The role of the big accelerators. *Think magazine.*
1959h. Reflections on science and philosophy. Yale, Hoyt Lecture.
1959i. NATO and the ideal of unity. Princeton University, NATO Conf.
1959j. The need for new knowledge, Weaver Symposium; pubd. in translation in *Revista de Occidente,* March 1963.
1960a. Some thoughts on science and politics. Princeton Univ. Woodrow Wilson School.
1960b. Leprince-Ringuet's 'Des Atomes et Des Hommes'. Univ. of Chicago Press.
1960c. Common knowledge. Reed College.
1960d. The house of science. American Institute of Architects.
1960e. Science, culture et expression, prospective, Nr. 5. Translated abbreviated version of 'In the keeping of unreason' (see 1959f).
1960f. Sorrow and renewal. Speech at Congress for Cultural Freedom, Berlin: pubd. in *Encounter.*
1960g. An afternoon with Professor Oppenheimer. Society of Science and Man, Tokyo.
1960h. Speaking to one another. Univ. of Pennsylvania, Franklin Lecture.
1960i. Some reflections on science and culture. Chapel Hill, University of North Carolina.
1960j. Science and culture, International House of Japan; variation of 'Reflections on science and culture' (1962b).
1960k. Knowledge as science, action, culture. Queen's University, Canada.
1961a. Science and converse. Princeton University Graduate College Forum.
1961b. Secretary Stimson and the atomic bomb. Phillips Academy, Andover, pubd. *Andover Bulletin.*
1961c. Some human problems of our scientific age. Tribune Libre Universitaire, Brussels; text reprinted in review of Tribune Libre Universitaire.
1961d. Reflections on science and culture, pubd. *University of Colorado Quarterly; The Mexico Quarterly Review,* Spring 1962.

1962a. Freedom as an attribute of human life. Congress for Cultural Freedom, pubd. London, *History and hope.*

1962b. On science and culture, pubd. *Encounter,* 1962 (Geneva Talk); variations entitled 'Some reflections on science and culture' widely pubd.

1962c. The flyer trapeze. *Whidden Lectures, McMaster University,* pubd. Oxford University Press.

1963a. The added cubit. National Book Awards. Pubd. *Encounter.*

1963b. The scientific revolution and its effect on democratic institutions. Fund for the Republic, 10th Anniv.; pubd. *Bulletin of Atomic Scientists* under title 'A Talk in Chicago'.

1963c. Niels Bohr memoir for Year Book, American Philosophical Society; Niels Bohr and his times, Pegram Lectures, Brookhaven.

1963d. Communication and comprehension of scientific knowledge. National Academy of Sciences Centennial, pubd. in *Science,* Nov. 1963, pubd. in *The Scientific Endeavour* for National Academy of Sciences by Rockefeller Institute Press.

1963e. Dawn of a new age, by E. Rabinowitch, *N.Y. Times,* Review of Books.

1964a. Hope and foreknowledge. California Institute of Technology.

1964b. Prospects in the arts and sciences. Columbia University Bicentennial; reprinted in *Man's Right to Knowledge,* 2nd series 1954; reprinted in *Fifty Great Essays* collected by Professor Edward Huberman, Bantam Books.

1964c. Our times as Galillean times. Essay for 'Saggi su Galileo Galilei'.

1964d. L'intime et le commun. Rencontres Internationales de Genève.

1964e. The fraternal dialogue. Université de Paix, pubd. in *From Heart to Heart.*

1965a. Science in the making. U.S. Army National Junior Science and Humanities Symposium West Point, pubd. in proceedings of Symposium.

1965b. Alpha or Omega. *Washington Post.*

1965c. The 20th Anniversary of Trinity. *Washington Post,* Outlook Section.

1965d. Listen to leaders in science. Chapter, pubd. Tupper and Love, Inc.

1965e. Decision to drop the bomb. NBC White Paper, Books by Coward McCann.

1965f. Foreword to 'Nature of matter—purpose of high energy physics', Current foreword, pubd. Brookhaven National Laboratory.

1965g. The symmetries of forces and states. Contribution to volume presented to V. Weisskopf, North Holland Pub. Co. Amsterdam.

1965h. Remarks on symmetry principles, High Energy Physics Conference, University of Miami, pubd. in Conference Proceedings.

1965i. On Albert Einstein. UNESCO, Paris, pubd. *N.Y. Review,* March 1966.

1966a. Physics and man's understanding. Smithsonian Institution Bicentennial; reprinted as *Knowledge among men.* Encounter.

1966b. Thirty years of mesons. American Physical Society, pubd. *Physics Today.*

1966c. Perspectives in modern physics. Topic in volume dedicated to Hans A. Bethe.

1966d. The forbearance of nations. *Herald Tribune Paris-Washington Post.*

LITERATURE ABOUT OPPENHEIMER

Nuel Pharr Davis. 1968. *Lawrence and Oppenheimer.* New York: Simon and Schuster.

R. Serber, V. F. Weisskopf, A. Pais and G. T. Seaborg. 1967. *A Memorial to Oppenheimer.* Talks at a Washington meeting of the American Physical Society, April 1967. Published *Physics Today,* **20**, No. 10, October 1967.

David E. Lilienthal. 1964. *The journals of David E. Lilienthal.* The Atomic Energy Years 1945-1950, Vol II. New York: Harper and Row.