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NEWS AND COMMENT

Oppenheimer: "Where He Was There Was Always Life and Excitement"

Hans A. Bethe

The author, professor of physics at Cornell University, was a longtime colleague of the late Dr. Oppenheimer. During World War II he served as director of the theoretical physics division at Los Alamos Scientific Laboratory, where Oppenheimer was director.

I. The Scientist

J. Robert Oppenheimer, who died 18 February, did more than any other man to make American theoretical physics great.

His mind was all the time concerned with the most fundamental questions in physics. This attitude of concentrating on the fundamental difficulties and ignoring the easy problems he communicated to his students. "What we don't understand we explain to each other," he once said in describing the activities of the physics group at the Institute for Advanced Studies, at Princeton. There was always a burning question which had to be discussed from all aspects, a solution to be found, to be rejected, and another solution attempted. Where he was, there was always life and excitement, and the expectation of excitement in physics for generations to come.

Oppenheimer started in physics at the most opportune time, taking his B.A. at Harvard in 1925. In 1926 Schroedinger discovered his equation, and already that year Oppenheimer had written his Ph.D. thesis in Göttingen on an important application of that just-invented theory. He calculated the photoelectric effect in hydrogen and for x-rays. Even today this is a complicated calculation, beyond the scope of most quantum mechanics textbooks. In 1926 Oppenheimer had to develop all the methods himself, including the normalization of wave functions in the continuum. Naturally, his calculations were later improved upon, but he correctly obtained the absorption coefficient at the K edge and the frequency dependence in its neighborhood. He was disturbed by the fact that his theory, while agreeing well with measurements of x-ray absorption coefficients, did not seem to be in accord with the absorption of hydrogen in the sun. This, however, was the fault of the limited understanding of the solar atmosphere in 1926, not of Oppenheimer's theory.

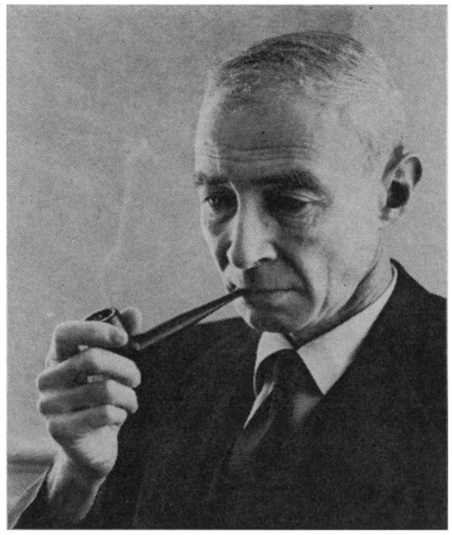
For 4 years, 1925 to 1929, Oppenheimer traveled from one center of physics to another—Cambridge University and Göttingen as a Ph.D. student, Harvard and California Institute of Technology as a National Research Fellow, then Leyden and Zurich as a fellow of the International Education Board. In Zurich he was influenced by Pauli, probably the man with the deepest understanding of quantum mechanics. In Göttingen, after completing his Ph.D., Oppenheimer worked with Max Born, one of the inventors of the then new quantum mechanics. Their paper on the structure of molecules is still the basis of our understanding of molecular spectra.

In 1929 Oppenheimer accepted a position as assistant professor at the University of California, Berkeley. Simultaneously he held an appointment at California Institute of Technology in Pasadena, where he regularly spent part of the year. This was the beginning of his great school of theoretical physics. In the 14 years before Los Alamos, a large number of the best theoretical physicists in the United States, including Christy and Schiff, did their work for the Ph.D. with him. Soon his school became famous and attracted postdoctoral fellows like Serber and Schwinger. His lectures were a great experience, for experimental as well as theoretical physicists. In addition to a superb literary style, he brought to them a degree of sophistication in physics previously unknown in the United States. Here was a man who obviously understood all the deep secrets of quantum mechanics and who yet made it clear that the most important questions were unanswered. His earnestness and deep involvement gave his research students the same sense of challenge. He never gave his students the easy and superficial answers but trained them to appreciate and work on the deep problems. Many of them migrated with him between Berkeley and Pasadena every year.

The problems of nonrelativistic quantum mechanics had been pretty well solved by 1929. Now Dirac's relativistic wave equation of the electron became the great challenge. In 1930 Dirac advanced the hypothesis that the vexing negative-energy states in his equation were all normally occupied except for a few "holes," which he assumed corresponded to protons. Oppenheimer quickly showed that this last hypothesis was untenable, and that the holes must have the same mass as an electron. This led to the theoretical prediction of the positron, discovered 2 years later by Anderson in cosmic radiation, that great laboratory of nature which revealed to us so many new particles in the 1930's and 1940's.

Cosmic radiation was the chief interest of Millikan, then president of Caltec and its chief physicist. A very peculiar phenomenon, the electron showers, had been observed, both in the atmosphere and in pieces of solid material. After the theory of the production of positrons and electrons (to which Oppenheimer and M. Plesset contributed the first paper) had been published, Oppenheimer and his school developed a most elegant theory of shower production which accounted for most of the observed phenomena and which has remained fundamentally unchanged. Other components of cosmic radiation were known to penetrate deep into the earth; these were recognized as mu mesons, after the discovery of that particle by Anderson and Neddermeyer, and these were known, in turn, to produce showers, though rarely. Oppenheimer's students Christy and Kusaka found this an indication that the meson had spin of 0 or ½. Particles of higher spin would give much too strong radiation.

At this point cosmic ray research tied in with Oppenheimer's other chief concern at the time—the fact that the theory gave divergent integrals for the self-energy and for the probability of certain processes at high energies. His struggle with this problem was intense, but he rejected all facile solutions. Concerning one theory by a prominent colleague which attempted to explain some showers of particularly rapid development, he said wryly, "What a shameless exploitation of divergent integrals." In the midst of these researches came the war, making a break in the lives of most American physicists, but in Oppenheimer's perhaps more than in any other. After the war, the divergence of field theory and the internal contradictions of meson physics were still with us. As much as his official duties permitted, Oppenheimer returned to physics, which was entering a time of rapid and exciting development.



—photo by Ulli Steltzer

J. Robert Oppenheimer, 1904–1967

His influence on physics was greatly enhanced when, in 1947, he was offered the position of director of the Institute for Advanced Studies, at Princeton. As head of its physics group, Oppenheimer realized, probably more fully than had ever been done before, the full possibilities of the Institute. Here was a place where dozens of the best and most active young theoretical physicists could assemble and could discuss the most interesting ideas of physics, which kept streaming in faster than they could be digested. The physics department of the Institute became the world's center for the development of high-energy physics and field theory. It is probably no exaggeration to say that, for the next 10 years, it was the mecca of theoretical physics, as Copenhagen had been in the 1920's and 1930's.

Physics was now much more mature than it had been in the 1930's at Berkeley. So were the physicists who flocked to Princeton. They were all of postdoctoral status, and many of them were of established prominence. Pauli was a frequent guest until his death, and so were Dirac and Yukawa, who first proposed the theory of the meson. A large number of postdoctoral fellows received their final training and taste in physics at this great center. Among them were Gell-Mann, Goldberger, Chew, Low, Nambu, and others in this country who were leading in the development of modern theory. There were almost equally many young visitors from abroad—France, Italy, England, Germany, and many other countries. And then there was the superb, almost-permanent staff, including Dyson and Pais, as well as Lee and Yang, who did their revolutionary work on the breaking of parity in weak interactions at the Institute.

Oppenheimer was always there to stimulate, to discuss, to listen to ideas. Even when he was busiest with public affairs, he knew what was most important in physics. It was forever astonishing how quickly he could absorb new ideas and single out the most important point.

In 1948 I gave a seminar at the Institute on some calculations concerning the Lamb shift. I spoke for less than half the time; 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. Incidentally, I was told that I had been allowed to speak a much larger fraction of the time than was customary in the seminar.

Vigorous discussion as well as emphasis on fundamental problems was Oppenheimer's style. Perhaps this originated during his time at Göttingen in 1926, the formative year of quantum mechanics and of his scientific life; perhaps he wanted to perpetuate that feeling of continuous discovery which must have pervaded Göttingen. 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.

Aside from his work at the Institute in Princeton, Oppenheimer played a leading role in the high-energy conferences which annually brought together theoretical and experimental physicists. The first such conference, organized by the Rockefeller Institute, was held in 1947 at Shelter Island. Some rather remarkable experimental results were presented—the Lamb shift and the anomalous magnetic moment of the electron. This stimulated theorists to develop modern quantum electrodynamics and renormalization theory, which eliminated, to a large extent, the unpleasant divergences which had plagued prewar theory. Oppenheimer, most active at the first conference, organized the next two, giving an opportunity to Schwinger and Feynman to present their diverse solutions to this problem. Later, Marshak established a regular annual conference at Rochester, which soon became international and now is held alternately in Russia, Western Europe, and the United States. To the end, Oppenheimer was much involved in the organization of these meetings, and was a regular participant.

II. The Wartime Leader

To the world outside physics, Oppenheimer is best known as the director of the Los Alamos Scientific Laboratory during the war. I had the good fortune to participate in an activity preparatory to the work at Los Alamos. In the summer of 1942 a small group met under Oppenheimer's leadership to discuss theoretical methods of assembling an atomic weapon. By that time 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 turned out to be accurate timing. 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 interesting enterprise. We were forever inventing new tricks, finding ways to calculate, and rejecting most of the tricks on the basis of the calculations. It was one of the best scientific collaborations I have ever experienced.

Life soon became more serious. After the summer study we all went home to our respective tasks of war research, but in the fall plans were started which led to the founding of the Los Alamos Scientific Laboratory in March 1943. It was not at all clear 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, by then in charge of the "Manhattan Project," that he overruled all these objections and made Oppenheimer the director.

It was a marvelous 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.

That this was true of Los Alamos was mainly due to Oppenheimer. He was a leader. It was clear to all of us, whenever he spoke, that he knew everything that was important to know about the technical problems of the laboratory, and he somehow had it well organized in his head. But he was not domineering, he never dictated what should be done. He brought out the best in all of us, like a good host with his guests. And because he clearly did his job very well, in a manner all could see, we all strove to do our job as best we could.

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 program. 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 free discussion among all qualified members of the laboratory. But the free flow of information and discussion, together with Oppenheimer's personality, kept morale at its highest throughout the war.

As the war was coming to an end and the problem arose of what to do with atomic energy, the government appointed an interim committee to discuss the problem. The members 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 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 worthwhile.

III. The Public Figure

With the end of the war, political problems came to the fore. Oppenheimer has often been blamed for his initial support of the May-Johnson Bill, which provided for continued military control and severe penalties for any infraction of the rules. Oppenheimer supported it because he thought it was the only way to get atomic energy organized quickly. But he soon joined the mainstream of scientists and of Congress supporting the McMahon bill, which in the end became law.

An even greater concern was the international treatment of atomic energy. During the war Oppenheimer had listened carefully to Niels Bohr, who had very clear ideas about what an atomic armaments race would mean and had a plan to avoid it by making atomic energy international. 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, but the combined efforts of statesmen and scientists after the war did result in some progress.

One result was the Acheson-Lilienthal Report (1946). Oppenheimer played the leading role in the Lilienthal Committee. The report 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 was endorsed by a State Department committee under Acheson and became official U.S. policy. It was presented to the United Nations by Baruch, but unfortunately was totally rejected by the U.S.S.R. Oppenheimer was one of the first to see that the plan would be rejected by Russia. 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.

—UPI Photo

Oppenheimer receives Fermi Award from President Johnson in 1963. "I think it is just possible, Mr. President," Oppenheimer remarked, "that it has taken some charity and some courage for you to make this award today."

From 1947 to 1953 Oppenheimer was a familiar figure in Washington. His main function was that of chairman of the General Advisory Committee of the Atomic Energy Commission, created in early 1947. But he also consulted with the Department of Defense on atomic weapons and on the general strategic policy of the United States. He was an important member of many *ad hoc* study groups on military matters. In all this he resisted, to the extent possible, the prevalent philosophy that atomic weapons give us "more bang for a buck." He, and others with him, advocated that more emphasis be put on atomic weapons for tactical use (so as to avoid a wholesale conflagration) and on conventional armaments. This earned him the hostility of some elements of the Air Force.

The General Advisory Committee of the AEC was a group of extremely high-grade scientists and businessmen. In its early years it recommended an extensive research effort by the AEC, which contributed greatly to the present preeminence of the U.S. in high-energy and nuclear physics. National laboratories like Brookhaven, Oak Ridge, and Argonne were established during this period, and the Berkeley Radiation Laboratory was strongly supported. In these years the groundwork was laid for the development of nuclear power reactors by the AEC. The main task of the AEC and its General Advisory Committee was to ensure an ample supply of fissionable material for reactors, as well as atomic weapons, by constructing production facilities. Thanks to this effort we are now living in an age of atomic plenty.

IV. Security Charges

The work of the General Advisory Committee came to a crisis in the fall of 1949, after the U.S.S.R. had exploded its first atomic weapon. In response, Edward Teller proposed that the U.S. should develop H-bombs. The committee wrote a strong recommendation against the development of the "super." One important argument 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. This advice was overruled by President Truman, after several months of heated debate behind the scenes. But, happily, a course similar to that recommended by the General Advisory Committee is now being pursued by President Johnson with regard to antiballistic missiles. The U.S. has asked the Soviet Union to enter into an agreement to stop the deployment of such missiles on both sides. Evidently antiballistic missiles are different from H-bombs and, more important, the present Russian Government is very different from that of Stalin in 1950. Even so, the U.S. may not succeed. But it is good to think that this idea, proposed by the General Advisory Committee in 1949, has now been adopted by our government as official policy.

After President Truman had overruled the committee, it would probably have been right for Oppenheimer to resign as chairman. He tried to, but the resignation was not accepted. This fact, together with the hostility he had incurred in the Air Force for his opposition to strategic bombing, brought about his troubles in 1953 and 1954. They were introduced by a strange article attacking him in *Fortune*. In 1953, on the basis of a denunciation, President Eisenhower ordered that Oppenheimer's security clearance be terminated. The ensuing, long-protracted security investigation became a *cause célèbre*. Many of his scientist friends came out in his defense, a few came out against him. The Proceedings, published by the AEC, give a vivid story of the discussions within the U.S. Government on defense policy between 1947 and 1953. They have been avidly read by friend and foe 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 of Haakon Chevalier in 1942. Intrinsically this "espionage attempt" was of no importance whatever (the counterintelligence corps did not even bother to investigate the lead), but apparently Oppenheimer, under stress and overwork at Los Alamos, had invented a rather foolish cock-and-bull story to shield his friend, and had then denied it.

It was not until April 1962 that the government made amends. Then President Kennedy invited him to a White House dinner for Nobel prize winners. And in 1963, just after taking office, President Johnson gave Oppenheimer the highest honor given by the AEC, the $50,000 Fermi award. 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."

V. A Changed Person

Oppenheimer took the outcome of the security hearing very quietly, but he was a changed person; much of his previous spirit and liveliness had left him. Excluded from government work, he apparently did not have the strength to return to active work in physics. He was as interested and well-informed on physics as ever before, still a leading figure at international conferences. But his main activity was now along more general lines.

He was deeply concerned, both before and after 1954, with the public understanding of science. His Reith lectures over the BBC, given in 1953 and published under the title *Science and the Common Understanding*, are among the most lucid and, at the same time, most profound popular expositions of atomic and quantum theory. Here, again, he never took the easy way of explaining just the facts, and he carefully avoided any facile analogies between the uncertainty principle and biological processes.

He was much aware of, and troubled by, the inability of the modern scientist to communicate his exhilarating experience of discovery, and also the contents of his discoveries, to the educated layman, in contrast to the close communication between science and society two centuries earlier [see, for example, "Some Reflections on Science and Culture" (1960)]. In still other lectures ["The Open Mind" (1955)] he discusses the relation of scientists to society, and many facets of the atomic policy of the United States. He always gives the impression of having long wrestled with the problem; he always raises a great many penetrating questions; and he gives few concrete answers.

If this left his audience only partly satisfied, they were compensated by the beauty of his style. I have seldom heard a speaker, scientist or otherwise, who had such a command of the English language, and who could so well fit words to the depth of the thought. There was wit also, and a store of good anecdotes, but, most of all, the signs of a deeply concerned human being.

Oppenheimer 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."

Post-Apollo: NASA's Plans Get Boost from LBJ and PSAC

Some answers are now available to the long-standing question of what will be the major goals of the U.S. space program following completion of the initial Apollo moon voyages. The program's longer-range goals must remain a matter of speculation because, if for no other reason, man's fitness for long-duration space flight is still to be determined. Nevertheless, the National Aeronautics and Space Administration has charted a course of sorts for the post-Apollo era and is pursuing it with the encouragement of President Johnson and his scientific advisers. Manned planetary expeditions and orbital space stations are among the space agency's ultimate goals.

In his budget message to Congress in January the President, alluding to NASA's post-Apollo plans, observed that the country would now have to look beyond Apollo "unless we wish to abandon the manned space capability we have created. . . . This budget," Johnson said, "provides for the initiation of an effective follow-on to the manned lunar landing. We will explore the moon. We will learn to live in space for months at a time. Our astronauts will conduct scientific and engineering experiments in space to enhance man's mastery of that environment."

The new NASA budget reflects a decision for NASA to pursue what the agency has called a "balanced program," involving the use of much of the Apollo technology. The meaning of this bland and ingratiating label is that, after Apollo, the space program will not again focus on a single overriding objective, such as a manned flight to Mars. The era of the balanced program should begin late in this decade, provided the timetable for the first lunar landings is not upset by mishaps such as the recent Apollo spacecraft fire.

NASA hopes to lay the foundation for the new program over the next few years by undertaking a variety of manned and unmanned space activities. Some would test man's ability to survive and perform effectively during prolonged space flight. Others would be