

LETTERS

Debate on Oppenheimer's Role in Manhattan Project Reaches Critical Stage

As an industrial historian, I was disappointed to read Lawrence Cranberg's letter in the September 1999 issue of *PHYSICS TODAY* (page 78). In light of much written history on this topic, it is apparent that he incorrectly summarizes J. Robert Oppenheimer's participation in the Manhattan Project as being simply that of having served as the director of the laboratory that produced the atomic bomb. He also seems to subtly suggest that a lab directorship was only an ivory tower administrative role, easily filled by any novice.

To begin, Cranberg states that "it could be argued that Oppenheimer had little to do with the scientific leadership that produced the A-bomb." In fact, published history points to the opposite. To clarify, Oppenheimer was involved with the A-bomb project as early as October 1941, when he lectured to General Electric Corp scientists in Schenectady, New York, on the physics of uranium. In 1942, at Arthur Compton's urging, he brought together, in a small attic room in Le Conte Hall on the University of California's Berkeley campus, a group of physicists he nicknamed the "Luminaries." There, they answered some of the basic questions regarding A-bomb design and the weapon's potential effects. The substance of their work later became the "Los Alamos Primer," the first A-bomb design guide at Los Alamos.

While Oppenheimer was at Los Alamos, his directorship style was one of close participation with his department heads and demonstrating a knack for grasping all aspects of the bomb research program. Cranberg's claim that Oppenheimer was "not even remotely an engineer" is a moot point and serves only to force the incorrect perception that scientists don't understand basic engi-

neering principles.

Cranberg also suggests that after Enrico Fermi tested the first self-sustaining reactor (CP-1 in Chicago), only engineering scale-up and design were required. This interpretation is overly simplistic and bears no resemblance to the actual development of the first A-bombs. In fact, Manhattan Project scientists pursued critical physics research during all bomb "engineering" phases, until final weapon assembly and shipment.¹

I also fail to understand Cranberg's comparison between Oppenheimer and Fermi. Although an outstanding scientist and leader in the physics community, Fermi never directed Los Alamos, and therefore cannot be judged in terms of a role he never played. All one could assume is that if he had directed Los Alamos, an A-bomb may have been developed under his leadership too. In fact, the successful development of the bomb serves to give credit to Oppenheimer for his leadership qualities under the strain of war, time constraints, and a new system of scientific research. Much like Ernest O. Lawrence at the Radiation Laboratory at Berkeley, Oppenheimer was learning how to manage some of the first big science projects, a daunting task that was quite new during that turbulent period in history.

Today, many nonscientist, but voting, citizens understand little about large-scale scientific and engineering endeavors, and fewer understand basic scientific principles. In my opinion, by printing Cranberg's letter without factual corrections, or even an editorial response, *PHYSICS TODAY* may have indirectly led some individuals to believe his unsupported rhetoric.

Reference

1. See, for example, L. Hodeson, P. W. Henriksen, R. A. Meade, C. Westfall, *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years*, Cambridge U. P., New York (1993), and R. Rhodes, *The Making of the Atomic Bomb*, Simon and Schuster, New York (1986).

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It is difficult to believe that Lawrence Cranberg's assessment of two great nuclear scientists derives from direct personal knowledge. As someone who was there, I think that, not unlike those who tried to rob Oppie of his dignity during the McCarthy era, Cranberg is greatly misinformed.

I would not take away from Enrico Fermi his massive contribution in Chicago and at Los Alamos, where he and his group continued with two reactor-type experiments—namely, Lopo, which was a low-power, water-moderated neutron source, and Hypo, which was an upgrade of Lopo. Although dedicated to the advancement of postwar reactor technology, Enrico achieved wartime results that led to the specific geometric details of the ^{235}U -fueled "Dragon," which was the next major step toward a solid bomblike model. Enrico was a shy person, a perfectionist, and what has come to be called a workaholic, and therefore he could be said to have had personality traits not necessarily well suited to his being the superpragmatic, risk taking leader of the atomic bomb project.

On the other hand, Oppie, who had never coveted the leadership role, was aware of and could deal with two leadership factors that were both obvious and critical. One was that General Leslie Groves, the administrative head of the project, would not tolerate a leader who was not an American. How well would anyone other than Oppie have fared when having to face powerful military leaders (Groves and General Carl Spaatz, to name a couple) who initially thought of the Manhattan Project as a boondoggle and a waste of \$2 billion that could be better spent on traditional weaponry? The other factor was that the Manhattan Project very much needed the scientific contributions of Jewish refugees from Nazi-dominated Europe and of American Jews, and questions were raised about how willing they would be to participate and cooperate. Here, too, Oppie was successful.

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Lawrence Cranberg compares Oppenheimer with Fermi. As postdoc of the former's and student of the latter's, I feel that some thing should be said. Certainly Fermi was a marvelous model for a physicist, and I don't know who could stand the comparison. Cranberg blames Oppie for not being, as Fermi was, successful in experiment as well as theory. But who else was? Einstein? Feynman? Schwinger? Von Neumann? In this, Fermi was probably unique in our century. Cranberg credits development of the A-bomb to President Roosevelt, and its use to President Truman, and he takes Oppie to task for not having made any technical contributions.

I am not happy that the bomb was developed, and much less so that it was used, and I do not admire Oppie for having been the director of the project. But I have only heard good things about his wartime direction of Los Alamos, never any criticism. In fact, from all that I have read, Oppie was an excellent director. And before the war, he had been the outstanding leader and teacher of theoretical physics in the US. He brought into existence the first American school of theoretical physics. As a student just after the war, I still studied quantum mechanics from prewar mimeographed notes of an Oppenheimer course (the teacher of my course was Edward Teller). As a young postdoc at the Institute for Advanced Study in Princeton in 1948–49, where Oppie was the director, I—like others interested in field theory and in particle physics—eagerly attended the weekly seminars he organized.

In short, denying Oppenheimer's leading role in physics, especially in US physics, is hardly correct.

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CRANBERG REPLIES: I welcome the responses of Timothy Karpin, James Osborn, and Jack Steinberger. To add useful evidence and analysis to the A-bomb story, though, I too think it best to cite sources. My reading of Richard Rhodes, for example, is that he attributes the "Los Alamos Primer" to lectures given by Robert Serber and compiled by associate lab director Edward Condon.¹ J. Robert Oppenheimer's role was evidently to convene the lectures.

Further, Rhodes states that implosion, the key development beyond the "Los Alamos Primer" phase, is attributable to Seth Neddermeyer, and Rhodes quotes John Manley, who was there, as saying that Neddermeyer faced "stiff opposition" from Oppenheimer and others.²

I stand by my original letter, but that letter will have served a higher purpose than evaluating Oppenheimer's role in the A-bomb project if it focuses attention on the underlying and recurring general questions about the requirements for leadership of large-scale scientific-engineering endeavors. And I hope that both the letter and this exchange will continue to stimulate constructive discussion of those requirements—surely a topic worthy of further discussion in the pages of both *PHYSICS TODAY* and *APS News*.

References

1. R. Rhodes, *The Making of the Atomic Bomb*, Simon and Schuster, New York (1986), p. 460.
2. Rhodes, pp. 466–67.

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Does H_0 Play Role in Universe Like $\hbar$ Does in Atomic Domain?

Of the many redshift studies that have been done over the years, one of the most interesting has to be that of William Tift of the Steward Observatory in Arizona. He has been studying and reporting on redshift data for over two decades now, and has repeatedly found a bunching of the data around certain values.¹ When interpreted in terms of recession velocities in the usual way, these values are integral multiples of a certain basic value—namely, 72 km/s. Although somewhat controversial initially, these basic results were later confirmed by Bruce Guthrie and William Napier of the Royal Observatory in the UK.²

Furthermore, these results have also proved to be very close to the latest value reported for the Hubble constant, as announced by the Hubble Space Telescope H_0 Key Project team: 71 km/s per megaparsec (see *PHYSICS TODAY*, August 1999, page 19). Here it is useful to note that, in her 1992 survey,³ team coleader Wendy Freedman gave the most probable value of H_0 as 73 km/(s Mpc).

The closeness of all of the above

results suggests that the recession velocities measured by Tift could be written as integral multiples of H_0 , so that $v = n \cdot H_0 d_0$, where n is an integer and d_0 is a basic unit of distance (1 Mpc). This equation is basically a quantized form of Hubble's law, and it implies that galaxies are located only at certain distances $d = nd_0$ away from us, at least in the near universe. Just how far out this equation would apply is not clear, but it does hold for our nearest galactic neighbor, M31 (the Andromeda galaxy), which is known to be approximately 1 Mpc away (corresponding to $n = 1$ in the above formula).

A quantized Hubble's law might be masked by other effects farther out, but it does suggest that the Hubble constant may play a role in the large-scale universe similar to that played by Planck's constant in the atomic domain—that is, in giving rise to structure in the universe.

References

1. For a review of his work on redshift quantization since 1976, see W. G. Tift, *Astrophys. Space Sci.* **227**, 25 (1995).
2. For a review of their work on the problem, see B. N. C. Guthrie, W. M. Napier, *Astron. Astrophys.* **310**, 353 (1996).
3. W. Freedman, *Sci. Am.*, November 1992, p. 54.

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Materials Science Needs and Is Getting Quantitative Methods

The following comment is prompted by my having read Jerzy Bernholc's article, "Computational Materials Science: The Era of Applied Quantum Mechanics," in your September 1999 issue (page 30). Although we must be impressed by the ingenuity that is often displayed in large-scale *ab initio* simulations, the road from breaking a solid or molecule in a simulation to the engineering concept of "strength" is a long one, and unlikely to be traversed by using simulations only. Similarly, other relevant engineering properties, such as corrosion and fracture resistance, phase (meta)stability, microstructure formation, and macroscopic transport, are often a complex (and unknown) combination of microscopic phenomena.

What is the problem? Due to the