Book Reviews

Events Surrounding the Superbomb

Energy and Conflict. The Life and Times of Edward Teller. STANLEY A. BLUMBERG and GWINN OWENS. Putnam, New York, 1976. xviii, 492 pp. + plates. $9.95.

This book consists of three major elements: a biography of Edward Teller up to the time the Manhattan Project started, a description of the initial stages of the U.S. thermonuclear bomb program and Teller's role in it, and the story of Teller and his work in the years since Mike, the first so-called "superbomb," was exploded.

The first of these elements contains much fresh material. It is based in large part on interviews with people who knew Teller as a child and young man in Europe, as well as on his own recollections. I have no reason to doubt the authenticity of this part of the book, nor do I have any way to measure its balance. The rest of the book is a different matter.

The second element of the book, the story of the invention of the hydrogen bomb and Teller's role in it, is a mixture of some fairly well known facts, a long-established major misconception, and some brand-new nonsense.

The most important piece of new nonsense concerns the supposed detonation of a thermonuclear device of some sort by the Soviets in 1951. In fact, the first Soviet explosion involving thermonuclear reactions took place in August 1953. It is known to history as "Joe 4": "Joe" for Joseph Stalin, and "4" because it was the fourth nuclear device of any kind detonated by the Soviets. The authors' notion that there was another Soviet fusion experiment more than two years before that is apparently based on the casual recollections of two men, Teddy Walkowicz and Robert Le Baron. Both of these men were once in positions such that they really did have access to all the facts, but evidently they did not have the opportunity to recheck the data behind their recollections when the authors interviewed them more than 20 years after the purported event, and an error of two years after such a long period of time is not particularly surprising. (Moreover, according to Martin Sherwin in a review of this book in the *New York Times*, Le Baron later "insisted that the report of his interview was badly garbled.") However, when Teller told the authors he knew nothing about such an explosion, they smelled conspiracy against Teller; when the Historian and the Classification Officer of the AEC (now ERDA) each separately denied there was such a Soviet explosion in 1951 the authors simply took that as confirmation of their suspicions. Even the Russians, who are always anxious to claim as much priority as possible in this field, have never made a claim of any thermonuclear bomb experiment before the Joe 4 event of 1953.

The long-established misconception concerns the significance of Joe 4. Blumberg and Owens, like some other authors before them, greatly exaggerate the significance of this experiment. It did involve the first Soviet device to include any thermonuclear fuel, but it was not the world's first experiment to do so, having in fact been preceded by three U.S. thermonuclear tests. Two of these, George and Item, took place only a few weeks apart in the spring of 1951. George was designed to explore some early ideas related to superbombs. Item was designed to check out the idea of "boosting," in which the synergistic interaction of a relatively large fission explosion and a relatively small fusion explosion results in a much enhanced explosive yield. Both were successful. In 1952 came two more major U.S. tests of special interest here: King and Mike. King was a very powerful all-fission device yielding a little more than half a megaton. It was designed and tested in order to demonstrate that, if necessary, very large explosions could easily and cheaply be produced without the use of thermonuclear techniques. Mike was the first thermonuclear superbomb. It was the first physical manifestation of the Teller-Ulam invention of 1951 that won Teller his sobriquet "father of the H-bomb." It fulfilled the ten-year-old prophecy that the H-bomb or superbomb would be "1000 times as large" as the A-bomb. (Mike yielded 10 megatons; the Hiroshima bomb had yielded approximately 13 kilotons). It incorporated design principles that enable a relatively small fission explosion to lead to the production of a relatively large (even arbitrarily large) fusion explosion, which can, but need not, in turn induce the fission of a large mass of ordinary uranium. These new design principles turned out to be very flexible in their application; that is, they can lead to the design of highly efficient smaller bombs (such as the Polaris and Poseidon warheads) as well as bigger ones. For reasons largely having to do with the pioneering nature of the event, the Mike device used the very awkward (from an engineering point of view) liquid deuterium as its thermonuclear fuel and incorporated some quite heavy special experimental equipment that made it unsuitable as it stood for use as a weapon.

Then, in August 1953, came the Soviets' Joe 4. It used a relatively large fission explosion to ignite a relatively small fusion reaction. (Compared to what we achieved with Mike, that is a relatively easy objective, as we had already demonstrated with George and Item.) It used, for the first time anywhere, lithium deuteride as a fuel, and it was, of course, a useful experiment for the Soviets, but it did not involve the Teller-Ulam design principles and hence was not the direct progenitor of the wide variety of large and efficient thermonuclear devices the Soviets now possess. Nor was it particularly large; it yielded only some 400 kilotons, and hence it was probably not even as powerful as the largest prior U.S. all-fission explosion, King. As the Russians always like to say when they have a chance, and as Blumberg and Owens reiterate, the Joe 4 device either already was deliverable by air or could readily have been made so. However, the very same can be said about the earlier U.S. Item and King, and there is probably no military or political sense in which Joe 4 was superior to the latter of these two.

The first tests of fully practical, readily deliverable superbombs based on the Teller-Ulam invention were made in the spring of 1954, when the United States tested five different successful versions of such devices, ranging in yield from 1.7 to 15 megatons. A year and a half later, in November 1955, the Soviets exploded one such device, roughly comparable in yield to the smallest of the U.S. 1954 devices but probably quite inferior to them in its yield-to-weight ratio. It was not until 1957 that the Soviet test program, even in cumulative terms, included a number and variety of bombs roughly equivalent to those the United States had tested in the single test series in 1954. By that time, of course, we had tested many more. Since the programs of the two countries started in somewhat different ways, it is, to be sure, difficult to give an exact figure for how far the United States was ahead in the development of the H-bomb, but it seems to me that one should say we were a year and a half ahead (that is, from March 1954 to November 1955) or, better, that we were three years ahead (from 1954 to 1957). The contrary notion that the Russians won the first heats of the H-bomb race and that we barely eked out a tie in the finals is common; it delights Russian chauvinists, it pleases American hawks, but it is false. Moreover, this false notion does not relate merely to national pride; it involves an important political matter. The idea held in some circles that the Oppenheimer security hearings of 1954 may have been in an important sense "justified" because Oppenheimer almost caused us to lose this vitally important race is based on the assumption that the race was very close.

The part of the book dealing with Teller in the years since Mike presents a different problem. Here there are also some misconceptions, but the main fault involves what is omitted. These particular misconceptions and omissions all derive from the authors' evident failure to discuss the most important events of the last half of Teller's professional life, the years at Livermore (1952–1975), with anyone who really knew much about them, save Teller himself. Judging from the names of sources given in the preface, and from the text itself, the story of the Livermore years is based mainly on prior books, plus interviews with Teller, Ferdinand Brickwedde, and Lowell Wood. Contrary to what Blumberg and Owens say, Brickwedde never was on the staff at Livermore; he visited during the first year for a month or so only, he was involved in strictly peripheral matters, and he had relatively little personal knowledge either of the H-bomb program itself or of Teller's interaction with it. Wood only became involved in this stream of events some ten or so years after the period began. In sum, there is no evidence in the book that any members of the scientific or administrative leadership of the laboratory during its formative years, except Teller, were interviewed or otherwise consulted by the authors.

The most important misconception involves the first nuclear weapons tests made by the Lawrence Livermore Laboratory, a laboratory that was created in 1952 in part because of Teller's dissatisfaction with the leadership at Los Alamos and in part because Ernest O. Lawrence and others (including me) thought the U.S. nuclear program should be stepped up in response to the first Soviet A-test, Korea, and other Cold War events. The first Livermore tests in Nevada in 1953 gave somewhat lower than expected yields, the first Livermore test in the Pacific in 1954 was a fizzle, and what was to have been the second was canceled. Blumberg and Owens present several possible reasons for these problems and for the cancellation. They discuss the idea that the canceled bomb may have been "too big"; they suggest that the fizzle occurred because it involved especially radical or imaginative ideas, or because it was trying to achieve "too much." In fact, these first tests did not involve especially radical or bold designs. Rather, the Livermore Laboratory was trying too hard to do something that would differ substantially from what Los Alamos was doing (a general policy that was not, as the book claims it was, due primarily to Teller), and as a result Teller and the rest of us worked out a design containing a serious unanticipated fault. We canceled the second test when we realized it had the same problem. Contrary to the quotation taken from Teller's own writings, in no sense did Teller have difficulty in persuading the rest of us that the second Pacific test should be canceled.

The most serious of the many omissions involves what happened next. Despite those early poor showings, in the mid-1950's the Livermore Laboratory did manage to pull up its socks and generate some important contributions to nuclear weapons design. This was accomplished almost entirely by two groups of young men mostly in their late 20's and early 30's, just as had been the case at Los Alamos ten years earlier during the war. One of these groups was led by Harold Brown, the other by John S. Foster, Jr. Each of these groups continued to follow the Livermore precept of "doing something different from Los Alamos," but this time they were successful. They did make use of a particular suggestion by John von Neumann, and, of course, of the basic 1951 Teller-Ulam invention, but in the main the new elements were based on their own ideas. These ideas worked out very well, and while the Polaris warhead is the best-known instance, an important fraction of the current U.S. warhead designs are based on ideas that came out of the Brown and Foster groups during those early years. The failure of Blumberg and Owens to mention either of these men or any of the other laboratory leaders except in one or two relatively trivial connections (Brown as an originator of Project Plowshare, Foster in connection with helping to hasten the preparations for a 1958 test series) creates the totally false picture that the intellectual content of the Livermore program was mainly or even entirely due to Edward Teller. Moreover, this error evidently is not happenstance; it seems rather to be what the authors indeed came to believe on the basis of their interviews. They say that "Teller, by 1960, felt that Brown and a new generation of young scientists were ready to take over the responsibility of running the laboratory." In fact, this new generation had been fully responsible from the beginning, more than eight years before.

One last point. This book contains a number of references to this reviewer. I have ignored them here, but that should not be interpreted as a case of "silence gives consent."

Edward Teller is one of the most important figures in the development of 20th-century technology. The world needs a good biography of this complex and exceptionally influential scientist, and he deserves one. Unfortunately, *Energy and Conflict* does not even begin to fill the need.

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Background for a Mission

The Study of Comets. Proceedings of a colloquium, Greenbelt, Md., Oct. 1974. B. DONN, M. MUMMA, W. JACKSON, M. A'HEARN, and R. HARRINGTON, Eds. National Aeronautics and Space Administration, Washington, D.C., 1975 (available from the Superintendent of Documents, Washington, D.C.). In two parts. xxxiv + 1084 pp., illus. $11.25. NASA SP-393.

In the introduction to this book, Bertram Donn writes that the prospect of space missions to comets led members of the staff of Goddard Space Flight Center to propose an International Astronomical Union colloquium at which the cometary physics essential for mission planning would be examined. The great brightness predicted for the perihelion passage of comet Kohoutek led to some modification of the program, but the original objective has been realized in the publication reviewed here. Of the 153 participants, 39 were from countries other than the United States; it is to be regretted that there is only one contribution from the U.S.S.R.

Part 1 of *The Study of Comets* is devoted principally to observational pro-