LETTERS

Hydrogen Bomb History

In the 19 November issue of *Science*, William J. Broad (News and Comment, p. 769) attempts "Rewriting the history of the hydrogen bomb," using as his starting point an article by my friend Hans Bethe, published in *Los Alamos Science*. Broad's article is not new, not accurate, and not constructive.

The valid parts of Broad's article are hardly novel. They were published in *Science* (volume 121, 25 February 1955, p. 267) under the title "The work of many people."

The inaccurate parts of the argument are much harder to discuss. On the one hand, we are dealing with accusations based on partially declassified information. How can a scientific reader judge the connections and comparisons between methods A, B, C, and D, when none of these can be described? A general scientific judgment as to what contributed to and what obstructed progress on the hydrogen bomb must await the release of complete information. Furthermore, as a participant I am necessarily restrained from evaluating my own contribution.

The atmosphere in which work on the hydrogen bomb proceeded was characterized by the Atomic Energy Commission (AEC) General Advisory Committee meeting at Princeton on 16 June 1951, which Bethe describes incorrectly in his article. The meeting was arranged by Norris Bradbury, then director of Los Alamos. The report on the hydrogen bomb did not mention method D. When I asked to speak, Bradbury denied me the opportunity. Although there were several people present who knew about method D, including Bethe and Oppenheimer, none chose to speak about it. However, AEC Commissioner Smythe, who believed that the other side of the argument should be heard, made my presentation possible. In the developments that immediately followed, this proved to be decisive.

While Bethe's article in *Los Alamos Science* primarily offered a criticism of Shepley and Blair's book, Broad neither mentions nor quotes these authors. Concerning that book, Bethe and I are in agreement. In fact, at our first meeting in 1958, Jack Kennedy offered a compliment on the basis of "the nice things said about me" by Shepley and Blair. I could find no better reply than to recite a Gilbert and Sullivan ditty which includes the phrase, "scarce a word of it is true."

There are two essential points on which I must comment. One is my advocacy of the hydrogen bomb. The result of our developing the hydrogen bomb was that we maintained our strength and could guarantee the stability of the world up to the mid-1970's. Herb York has argued [*The Advisors: Oppenheimer, Teller and the Superbomb* (Freeman, San Francisco, 1976)] that, had the Soviets developed the hydrogen bomb first, we would have matched them in a very short time. Recent events illustrate our capacity for self-delusion. Even when our President (disregarding his own political interests which would persuade him to offer an optimistic picture to the American people) was courageous enough to tell us the bitter truth—that Soviet military preparations have outstripped our own—a surprisingly large number of people (including York) are unwilling to acknowledge any danger.

The second point is that Broad calls into question the importance of the second weapons laboratory, which was established at Livermore. Actually Bethe in his article points out in a note added in 1982: "In the intervening 28 years, Livermore has contributed greatly to nuclear weapons development."

This is indeed true. Livermore led the way toward establishing a submarine-based nuclear force that today is considered the most secure aspect of our defense. At present, Livermore is spearheading the development of purely defensive weapons. Most fortunately—since the development of such weapons may replace the dubious security of the "balance of terror" with a more stable protective basis for peace, this trend has been generally adopted by both Los Alamos and Sandia Laboratory. Due to the variety in research made possible by the existence of several separate laboratories, such constructive plans receive emphasis.

Actually part of Bethe's intention in writing his comments was connected with a defense of Oppenheimer's role. Here, of course, our opinions are not in agreement, but perhaps we are less far apart than is generally assumed. I am submitting comments on the BBC television drama *J. Robert Oppenheimer* to the *Los Alamos Science*, and these should clarify this point.

In historic perspective, the Oppenheimer case, which is the underlying theme in both Bethe's and Broad's articles, was most unfortunate. It introduced a division in the scientific community that has weakened national defense and our ability to make peace secure.

Very recently Bethe wrote me a short, pleasant letter stating that he had wanted to present his opinion within a very limited community and was very unhappy about the broader use being made of his article. In this respect, Bethe and I again agree, but Broad does not. What we need is not to emphasize past disagreements but to bring about as much agreement as possible in order to face the future with its obviously great dangers.

EDWARD TELLER
Hoover Institution on War, Revolution and Peace, Stanford, California 94305

I agree with many points in Teller's letter, for example, with the statement that the Livermore laboratory contributed decisively to submarine-based nuclear weapons which today are the most secure components of our defense. But he brings in two points which were not in Broad's article, and with which I strongly disagree.

The first concerns the General Advisory Committee (GAC) meeting at Princeton on 16 June 1951. My recollection differs from Teller's. The main purpose of this meeting was to discuss method D, and I specially postponed a trip to Europe because method D was to be discussed. I had been informed of method D about a month earlier and was immediately persuaded that this was the correct solution to the problem. I was asked beforehand to participate with Teller in presenting the method to the GAC and the Atomic Energy Commission, and to the best of my recollection this was done after reports on the most recent test series had been given.

The second point concerns the development of "purely defensive" nuclear weapons at Livermore and other weapons laboratories. If reliable defensive weapons were feasible, I would welcome this escape from the balance of terror. But I remain convinced that, in the nuclear field, the offense will continue to have the advantage and can negate any defensive weapons with relatively little effort. Defensive nuclear weapons will at best remain wishful thinking.

HANS A. BETHE
Floyd R. Newman Laboratory of Nuclear Studies, Cornell University, Ithaca, New York 14853

Correction

The full-color map reproduced on page 987 of the 3 December 1982 issue is a preliminary rendition of the Composite Magnetic Anomaly Map of the United States, not the final version. Only the color schemes differ; the magnetic contours are identical.