Rewriting the History of the H-Bomb

Nobel laureate Hans Bethe says technical errors by Edward Teller, not political opposition by Robert Oppenheimer, hindered work on the superbomb

A once-secret history of the building of the hydrogen bomb takes issue with the popular notion that J. Robert Oppenheimer and Los Alamos Laboratory, the top-secret birthplace of the bomb in New Mexico, resisted for purely political reasons a crash program aimed at building an H-bomb. Instead it reveals long struggles with technical problems. Chief among these were faulty calculations that misled the U.S. nuclear weapons program between the years 1946 and 1950. The errors, according to the history, were the work of Edward Teller, "father" of the H-bomb.

The recently declassified account, written in 1954, also says the size and intensity of the H-bomb effort at Los Alamos meant there was no need for the founding of the Lawrence Livermore Laboratory in California, Teller's rival lab for the design of nuclear weapons.

The sweeping revision of H-bomb history was penned by Hans A. Bethe, a physicist at Cornell University who is widely respected in the weapons community. It appears in the current issue of *Los Alamos Science*, a quarterly publication of the laboratory.

Bethe's history "is the most revealing and authentic that I've ever seen in the open literature," says Richard L. Garwin, a physicist who worked on the development of the H-bomb and became a top Pentagon consultant. Its most striking revelation, he says, is that "Teller did a number of things wrong which were highly misleading to the laboratory."

A pivotal figure in thermonuclear research, Bethe in 1938 deduced that fusion powers the sun and stars. That discovery, along with work on other nuclear puzzles, eventually won him the Nobel Prize. After the war Bethe helped with the development of fusion weapons, a project that culminated in 1952 with the 10-megaton rumble of the world's first hydrogen bomb. Bethe wrote his account in 1954 after a U.S. inquiry found that Oppenheimer had hindered a crash program on the "super." Oppenheimer, who directed Los Alamos and the building of the first atomic bomb, lost his security clearance and fell from power.

"My impression," says J. Carson Mark, head of the Los Alamos theoretical division between 1947 and 1973, "is that Bethe's article is factually accurate. And it's not badly colored, especially considering that Bethe was quite angry with Teller over his weasel-worded testimony in the Oppenheimer hearing."

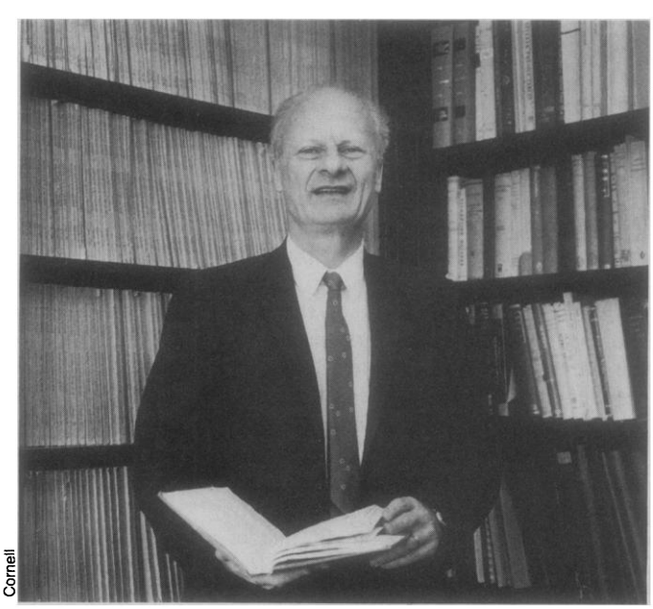
At Teller's Hoover Institution office in California, an administrative assistant said Teller had not seen the article and could not comment.

In *Los Alamos Science*, Bethe says he had intended to put his 1954 account "into the Laboratory's archives and not to publish it, in order not to stir up old controversies." That he decided otherwise was due to the continued appearance of inaccurate articles and books, most recently *J. Robert Oppenheimer, Shatterer of Worlds*, published in connection with a recent PBS television series (*1*). "While this book is excellent in most respects," Bethe writes, "it gives a very wrong impression of the development of the H-bomb."

Popular history tends to stress Teller's positive role. It is true that Teller suggested the idea of an H-bomb early in 1942, and that Oppenheimer, Bethe, and others studied the possibility that summer. The work was soon interrupted by the Manhattan Project. After the war, atomic scientists argued over the wisdom of embarking on a high-priority program for the development of a superbomb. Nobody knew if such a weapon was possible. Many hoped it was not. Teller, a foe of the Soviets, urged a crash program. Bethe, head of the Los Alamos theoretical division during the war and Teller's former boss, called for a more cautious approach, as did other physicists. Oppenheimer for one was deeply troubled by what he had wrought at Los Alamos, and was repulsed by the notion of bombs of unlimited power. The question is whether his personal aversion led to technical bias. Superficial histories hold that Teller overcame political resistance to the super and founded a rival laboratory at Livermore. The facts are more interesting.

Bethe divides the decade-long quest for thermonuclear weapons into the evolution of four distinct methods: A, B, C and D. Method A was the "classical super" proposed by Teller in 1942. After the war Teller also invented methods B and C. However, by 1947 it became clear that method B would fail and that method C would work only for weapons of small yield. Research at Los Alamos thus focused on method A. "New plans for calculations were made frequently," writes Bethe, "mostly by consultation between Teller and the senior staff of the theoretical division."

The calculations on the feasibility of method A were so complex that they



Cornell

Hans A. Bethe

"Nobody will blame Teller because the calculations of 1946 were wrong, especially because adequate computing machines were not then available."

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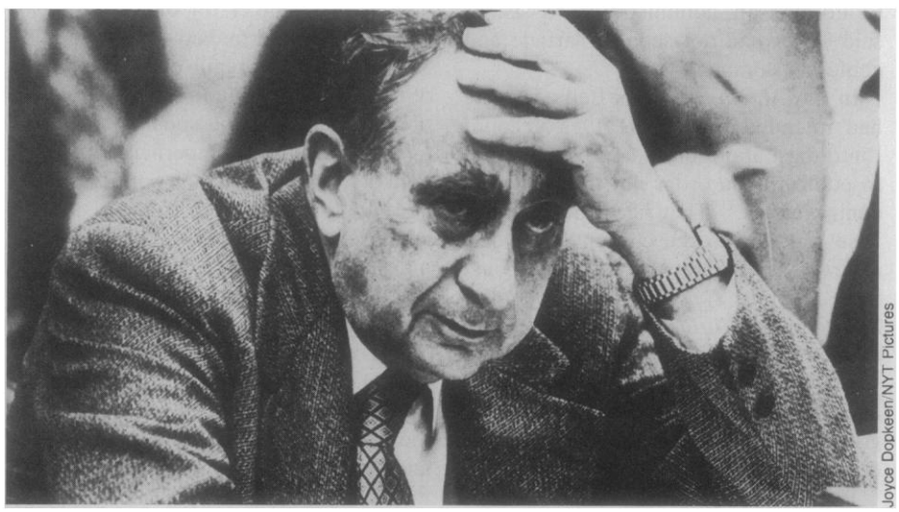
quickly overpowered the crude computers then available. "Some greatly simplified calculations were done but it was realized that they left out many important factors and were quite unreliable. Work therefore concentrated on preparing full-scale calculations 'for the time when adequate fast computing machines become available'—a phrase which recurs in many of the theoretical reports of this period."

The calculations, given low priority, focused on what Bethe calls problem 1. However, the perceived stumbling block to the success of method A was seen to be problem 2. Teller came up with a possible solution, one that required a test. Plans were made for verification of Teller's idea at Eniwetok Atoll in the Pacific, a series of bursts known as Greenhouse.



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Edward Teller

Lobbied passionately for a crash program on the building of the hydrogen bomb

Oppenheimer, chairman of the Atomic Energy Commission's (AEC's) general advisory committee from 1947 to 1952, initially had successfully argued for a slow approach to the thermonuclear question. But early in 1950, President Truman, prompted by the detonation of the first Russian A-bomb and by the revelation that a spy had delivered H-bomb secrets to the Soviets, ordered the superbomb developed with all possible speed.

"While Teller and most of the Los Alamos Laboratory were busy preparing the Greenhouse tests," writes Bethe, "a number of persons in the theoretical division had continued to consider the various problems posed by Part 1. In particular Dr. Stanislaw Ulam on his own initiative had decided to check the feasibility of aspects of Part 1 without the aid of high-speed computing equipment. He, and Dr. Cornelius J. Everrett who assisted him, soon found that the calculations of Teller's group of 1946 were wrong. Ulam's calculations showed that an extraordinarily large amount of tritium would be necessary." Tritium, a heavy form of hydrogen, does not occur in nature and is difficult to produce. Ulam's calculations of 1950 meant the cost of method A would be prohibitive. When Teller heard the news he went "pale with fury," according to Ulam (*2*).

"That Ulam's calculations had to be done at all was proof," writes Bethe, "that the H-bomb was not ready for a 'crash' program when Teller first advocated such an idea in the fall of 1949. Nobody will blame Teller because the calculations of 1946 were wrong, especially because adequate computing machines were not then available. But he was blamed at Los Alamos for leading the laboratory, and indeed the whole country, into an adventurous program on the basis of calculations which he himself must have known to have been very incomplete. . . ."

"Teller himself was desperate between October 1950 and January 1951. He proposed a number of complicated schemes to save method A, none of which seemed to show much promise."

By early 1951, however, Teller and mathematician Ulam conceived an entirely new, ingenious method for a practical hydrogen bomb of unlimited power, a plan Bethe refers to as method D. After the *Progressive* case, in which the government tried and failed to keep a magazine from publishing H-bomb secrets, a key feature of method D was revealed to be the power of x-rays, traveling at the speed of light, to ignite thermonuclear fuel (*3*). The whole idea was so "technically sweet," according to Oppenheimer, that he could no longer raise objections to a crash program.

"Concentrated work on Method A would never have led to Method D," writes Bethe. "By a misappraisal of the facts many persons not closely connected with the development have concluded that the scientists who had shown good judgment concerning the technical feasibility of Method A were now suddenly proved wrong, whereas Teller, who had been wrong in interpreting his own calculations, was suddenly right. The fact was that the new concept had created an entirely new technical situation. Such miracles do happen occasionally in scientific history but it would be folly to count on their occurrence. One of the dangerous consequences of the H-bomb history may well be that government administrators, and perhaps some scientists, too, will imagine that similar miracles should be expected in other developments."

Despite the tortuous path to the development of a superbomb, the scientists at Los Alamos worked enthusiastically on the project. Bethe estimates that between 1946 and 1949 the theoretical division worked about equally on the design of fission weapons and on the solution of the H-bomb problem. After Truman's call to arms in 1950, the effort expanded as more than a dozen scientists were added to the division's staff. Bethe's account is clearly at odds with popular history. In *Shatterer of Worlds*, author Peter Goodchild writes that "In spite of the President's directive, the work on the Super was given little priority and was overall at a low ebb."

In fact, ardor at Los Alamos was such that even the failure of method A did not dampen spirits. Bethe paid a visit in April 1950. "The entire Laboratory seemed enthusiastic about the project and was working at high speed," he writes. "That they continued to work with full energy on Teller's Greenhouse Test, even after Ulam's calculations had made the success of the whole program very doubtful, shows how far they were willing to go in following Teller's lead."

Atomic history from an insider's point of view, writes Bethe, not only refutes the notion that Los Alamos dragged its feet, but casts new light on reasons for the founding of Lawrence Livermore. "Certainly the events of the year 1950 would hardly seem to have given Teller any justification to ask the AEC, in the spring of 1951, to establish a second weapons laboratory."

Nonetheless, Teller continued to lobby passionately for a new lab. In December of that year he addressed the AEC

advisory committee and cited the need for urgency because the Russians might already be ahead (*4*). Los Alamos had not made good use of its limited resources, he said, and had been inflexible in its approach to the super.

In fact, as Los Alamos sped ahead with work on method D, relations between Teller and others on the staff cooled. Teller, among other demands, wanted the thermonuclear test moved up. Teller and Norris Bradbury, the postwar director of the lab, could not agree on who should direct the actual building of the bomb. "Bradbury had great experience in administrative matters like these," writes Bethe. "Teller had no experience and in the past had shown no talent for administration." Teller had abandoned programs in midstream. He had injected modifications

No Fraud Found in Alcoholism Study

A 5-month inquiry into allegations of fraud and other improprieties in an influential research project concerned with the treatment of alcoholics has concluded that there is "no reasonable cause to doubt the scientific or personal integrity" of the two researchers who conducted the study. The inquiry found that the researchers, Mark and Linda Sobell, did not fabricate data and, except for one lapse—ascribed to carelessness—that they accurately reported their procedures and results. Although the report of the inquiry, published on 5 November, vindicates the Sobells' integrity, it is unlikely to still the controversy that has swirled around the interpretation of their research.

The focus of the controversy is a project conducted by the Sobells at Patton State Hospital in San Diego in the early 1970's. Their research suggested that some people who are physically dependent on alcohol can be taught to moderate their drinking. Conventional wisdom held (and to a large extent still holds) that abstinence is the only cure for physically dependent alcoholics, but the Sobells reported that a group of 20 alcoholics taught to control their drinking fared better than a similar group whose treatment was geared toward total abstinence.

These findings were widely publicized and sparked a raging controversy among alcoholism researchers. The controversy hit the headlines earlier this year with the publication of a follow-up study, conducted by an independent team of researchers, that found only one of the 20 experimental subjects successfully controlled his drinking over several years, and there was some doubt whether he had ever been physically dependent on alcohol. (Eight continued to drink heavily, six abandoned their efforts to engage in controlled drinking and became abstinent, four died, and one was missing, the study found.) The follow-up study also emphasized that most of the subjects who took part in the Sobells' controlled drinking program had a high incidence of rehospitalization, alcohol-related arrests, or bouts of heavy drinking even during the 2 years they were studied by the Sobells (M. L. Pendery, I. M. Maltzman, and L. J. West, *Science*, 9 July, p. 169).

The team that conducted the follow-up, headed by Mary Pendery of the Veterans Administration Medical Center at San Diego, and Irving Maltzman, professor of psychology at the University of California at Los Angeles, interviewed people who took part in the Sobells' project and examined hospital and law enforcement records. The Sobells tried to block the Pendery group's study on the grounds that the confidentiality of the subjects might be compromised.

Although the published version of the Pendery group's paper contained no allegations of misconduct by the Sobells, an unpublished draft suggested that the research was not conducted in the way the Sobells had claimed and that the reported results were inaccurate. Also, in June, Maltzman was quoted by the *New York Times* as saying, "Beyond any reasonable doubt, it's fraud." The Toronto-based Addiction Research Foundation, where the Sobells are now working, appointed a four-person committee of enquiry to look into the allegations early in June. It was headed by Bernard Dickens, professor of law at the University of Toronto.

The committee interviewed the Sobells, looked at much of their raw data, listened to some of the tape-recorded interviews they had conducted with the experimental and control subjects, and received sworn affidavits from research assistants who took part in the study. Pendery, however, declined invitations to participate, and Maltzman says he was never approached directly by the committee.

In essence, the committee concluded that the Sobells had carried out the research in the way they reported, and that they had not misrepresented the results. But, although the committee refuted a variety of unpublished allegations against the Sobells, it explicitly avoided judging the central issue raised by the Pendery team's published findings: whether alcoholics can be taught to moderate their drinking. The committee did not contest the Pendery team's detailed findings about how badly the controlled drinking group fared, for example, but said that these findings should have been put in context by looking at what happened to the group given more conventional therapy. (The Pendery team's published paper said, however, that the groups were not compared because "we are addressing the question of whether controlled drinking is itself a desirable treatment goal, not the question of whether the patients directed toward that goal fared better or worse than a control group that all agree fared badly.")

The committee did criticize the Sobells on one point. They had reported that they interviewed research subjects and people connected with them "every 3–4 weeks throughout the entire follow-up period." But most subjects were contacted less frequently, and in some cases there were gaps of 3 to 6 months between interviews, the committee said. It concluded that the Sobells were "careless" in estimating a statistic they never calculated.

The committee's findings have clearly failed to convince the Sobells' critics. Maltzman told *Science* that he considers the report "an outrageous whitewash." He maintains that the committee should have reinterviewed the experimental subjects rather than rely on the Sobells' records, and says that in misreporting the frequency of contacts, the Sobells "committed fraud." Dickens says that the committee decided not to reinterview the subjects in part because recollections by alcoholics of events many years ago may not have been reliable.—**COLIN NORMAN**

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into projects already under construction. "Everybody recognizes that Teller more than anyone else contributed ideas at every stage of the H-bomb program, and this fact should never be obscured," writes Bethe. However, as a journalistic profile of the period put it, "nine out of ten of Teller's ideas are useless. He needs men with more judgment, even if they be less gifted, to select the tenth idea which often is a stroke of genius."

Bethe's account adds up to a recasting of cold war history. The key issue in the 1954 inquiry was whether pure political pressure had slowed work on the super, although Oppenheimer was charged with additional indiscretions. As Teller told the inquiry: "If it is a question of wisdom and judgment, as demonstrated by actions since 1945, then I would say one would be wiser not to grant clearance."

What Bethe's revision of H-bomb history makes understandable is why atomic scientists of the inner circle often hold Teller in such contempt. In 1954, the laws of classification made it impossible for them to come to Oppenheimer's defense in public, to explain the technical reasons for a cautious approach to the super. They were barred from revealing the blind alleys, the mistaken calculations. Bethe tried, and his attempt was promptly seen as a potential breach of security, one that might jeopardize the U.S. lead in atomic weapons. After all, designs based on Teller's faulty calculations were among the H-bomb secrets that had been stolen by the Soviets. Why untangle the mess in public? Such restraint, moreover, may have been wise in some ways. The Soviets did not detonate an H-bomb built on the Teller-Ulam principle until late 1955, more than a year after the Oppenheimer inquiry and 3 years after the first such U.S. detonation (*5*). It has long been known that Oppenheimer urged a slow approach to the super. Bethe's account now reveals the extent to which Oppenheimer's opposition to a crash program was based on technical as well as political reasons. But in the fanaticism of the McCarthy era, any opposition was enough to ensure his demise.—**WILLIAM J. BROAD**

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4. R. G. Hewlett and F. Duncan, *Atomic Shield 1947–1952* (Pennsylvania State Univ. Press, University Park, 1969), pp. 569–570.
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Deaths Halt Interferon Trials in France

Paris. The French government announced last week that it was halting a broad program of clinical trials of human interferon in public hospitals, following the deaths of four cancer patients who were being treated with the drug as part of an investigation of its potential antitumor properties. The patients died of heart attacks.

The suspension of the trials is the second setback within a few months for an ambitious program to test the potential effectiveness of human interferon, launched by the Ministry of Health in December 1980. Last June, production of alpha interferon by the company L'Institut Pasteur Productions (IPP), a subsidiary of the Sanofi/Elf-Aquitaine group, was suspended after delays in getting the clinical trials off the ground had led to a much smaller demand for interferon than initially anticipated.

The trials are being organized by a scientific committee which has now decided that they should not proceed until the reasons for the deaths of the patients, each of whom was suffering from an advanced form of cancer, are known in more detail. In particular, tests are being carried out by IPP to discover whether the toxic effects were the result of the interferon itself, or whether they were caused by impurities in the blood samples from which the interferon was taken.

IPP, which is producing the interferon in collaboration with the National Center for Blood Transfusion, is now expected to carry out additional purification of the 50 billion units it has been holding in stock since production was halted in June.

Meanwhile, the deaths of the four patients have helped stir a growing debate over whether it is ethical to permit experimentation on human subjects on such a large scale before more detailed information is known about the therapeutic action of interferon and its toxic side effects. In addition, there are concerns that hospitals may have been using unregistered batches of interferon offered by foreign companies for experimental use, a practice which, if confirmed, raises yet more ethical questions since questionable financial dealings may also have been involved. Under the terms of the government-sponsored trials, clinicians who carried out experiments with cancer patients according to an approved protocol were reimbursed for the full costs of the interferon that they had used.

American researchers have not observed heart attack problems with cancer patients in interferon trials, according to Robert Oldham, director of the National Cancer Institute's program overseeing clinical trials with interferon. Interferon supplies for these studies are domestically produced. Oldham said that interferon can produce a high fever in some patients. This effect, he said, could harm a patient with a history of severe heart problems.—**DAVID DICKSON**

DOE to Gut Solar and Conservation Programs

The Department of Energy is considering a fiscal year 1984 budget that would virtually eliminate all its energy conservation programs and slash the current solar energy budget by two-thirds.

According to internal documents, the department plans to cut the present energy conservation budget of $384 million to $51 million. The proposal would terminate nearly all projects, but create one new program in basic research, allotting it $48 million. Programs that would not survive include research and information projects in energy efficiency. The department also wants to phase out all grants to state and local governments at a time when they are being squeezed by other federal cutbacks.

The solar energy budget would be reduced from its current appropriation of $268 million to $87 million. The biggest cut would hit the solar thermal energy program, reducing it from $78 million to $22 million.

The Reagan Administration has consistently tried to cut these programs drastically, but Congress has restored the funds each year. Nevertheless, the Administration seems determined to keep trying.

—**MARJORIE SUN**

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