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How Ulam set the stage

History has not given enough credit to the main man behind the H-bomb. **by Bengt Carlson**

SOME CONTROVERSIES NEVER DIE; THEY don't even fade. Case in point: Who invented the hydrogen bomb? Was it Edward Teller, the physicist, or Stan Ulam, the mathematician? Having been on the scene at Los Alamos Laboratory in New Mexico during the crucial time period, I have hardly a doubt. It was Stan Ulam.

I knew both Ulam and Teller well—we worked in the same building under J. Carson Mark, leader of the Theoretical Division, one of about 10 divisions that comprised the lab, directed by Norris Bradbury. I was transferred to Los Alamos in 1945, and in 1946 I took over a small group, part of the Theoretical Division. Our main assignment was to solve physics problems as they arose.

Los Alamos Laboratory (then called Site Y) had opened its doors in the spring of 1943, with J. Robert Oppenheimer and Hans Bethe as the respective predecessors to Bradbury and Mark. Wartime Los Alamos's crowning achievement had been the implosion-type atomic bomb. Talk of a possible hydrogen bomb surfaced in 1941; real work on it began in the fall of 1943 at Los Alamos, by a group supervised by Teller.

One thing was clear. Ulam and Teller, although both well-educated scientists of similar backgrounds (both came to the United States in the mid-1930s from Poland and Hungary, respectively), were otherwise totally different. Ulam was the archetypal research man, Teller the self-promoting engineer. As a complement to the many fine reviews of Teller's recent memoirs, here I shall add my own two cents' worth of impressions and recollections of my former colleagues.

As a mathematician, Ulam was above all a thinker and an explorer. He played with new and old ideas in mathematics, explored novel pathways and structures, and tackled interesting problems no matter the source or the field. If an idea jelled, he usually found someone interested in the subject who was willing to write a "joint" paper. This left him free to do what he liked most: brainstorm with colleagues and look for new challenges.

Ulam could be unconventional. Take, for example, his "Orion Project," which proposed using nuclear explosions to power intragalactic expeditions. But he could be practical, too—witness his

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Stan Ulam.

statistical method called Monte Carlo.

Teller was ingenious and energetic, like Ulam, but much more goal-oriented. Clever, opportunistic, and hard-driving, he became an effective salesman for atomic weapons and explosives, striving for a full line of merchandise as well as for wider applications and larger markets. Teller was good at manipulating and turning events to his advantage—as evident in his push for a second weapons laboratory and in his prominence in the Oppenheimer affair. He was generally friendly and protective toward those who followed his lead, but quite the opposite toward those who didn't.

Ulam had come to Los Alamos in January 1944 and was assigned to Teller's group. Problems between them developed early. In an employee rating dated February 13, 1945, sent by Teller to the War Department, he rated Ulam highly in two out of four categories, commenting that "he might conceivably turn up most interesting results," but labeling him unsatisfactory in "personality" and "success in recent work."

Teller wanted very much to succeed at Los Alamos and earn a top position there, but few people understood his personality and he tended to be impatient.

Los Alamos was the most inspiring and hard-working place I ever knew. The post-war teamwork was exemplary, but Teller seemed unable to fall in step. Everybody sensed the importance of the H-bomb effort and its immediate objective, though there were divided opinions about its ultimate role.

In December 1950, Ulam surprised me with a novel idea: repeated implosion to get much higher compressions, which he called "supercompression."

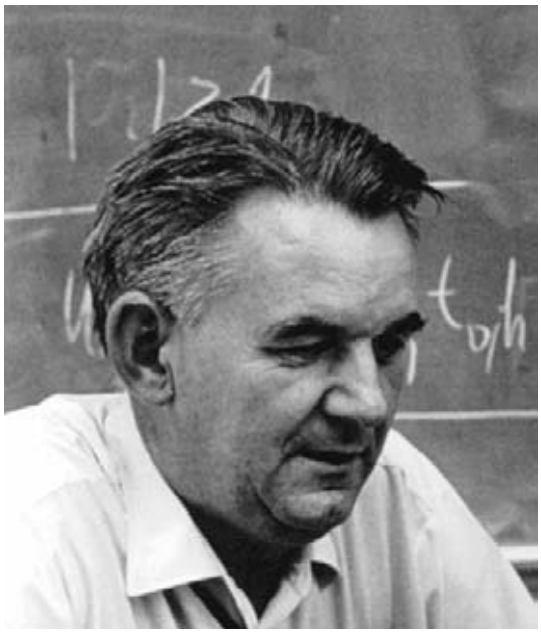
Teller's group initially investigated a design called the "Super." They reported their progress at the end of World War II in October 1945, and at a three-day conference in April 1946. The work seemed encouraging, but eventually the calculations were found to have been based on overly optimistic assumptions. Development on the H-bomb resumed after the first Soviet test of an implosion bomb, a Trinity-test clone, in August 1949. Then, on January 31, 1950, President Harry Truman called for an acceleration of the H-bomb effort.

Ulam was concerned about Teller, especially after the Truman edict, and not just because of the slow progress. He had seen that Teller was prone to fixating on and getting lost in problem details, which Teller admits to in his memoirs. His top fixation was the Super itself. Teller wanted a device that would yield a minimum of 1 megaton and could be built to deliver any desired yield simply by adding more thermonuclear fuel. Fixation number two: He was dead set against compressing the fuel.

Ulam decided to start his own back-up H-bomb program with colleagues Cornelius J. Everett and Josephine Elliott, who was on loan from my group. By June 1950, after two rounds of hand calculations, they discovered that the 1946 Super results had been wrong, a conclusion reinforced by several recalculations. The repetition of negative news settled the matter—the Super had to be abandoned. Naturally, this was a big blow to Teller.

All along, Ulam had urged Teller to consider other approaches. But Teller had pointed to his back-ups: the "Alarm Clock" device, which involved a layering of fission and fusion materials, and fission bombs boosted by placing a capsule of deuterium-tritium mix at the center. Neither could give megaton yields. Nevertheless, Teller began an intensive review of the H-bomb effort and con-

Bengt Carlson circa 1965.



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Edward Teller and Norris Bradbury.

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tinued his unsuccessful efforts to salvage the Super.

In December 1950, Teller finally recognized a flaw in his argument against fuel compression, and was able to accept the idea. He spent the rest of the month lobbying for more support without much satisfaction. He was afraid that with the failure of the Super, the H-bomb program would languish. His protégé, fellow Hungarian and PR man Frederic de Hoffmann, a bright young physicist, was supportive, but neither Teller nor de Hoffmann had the vaguest notion of what step to take next.

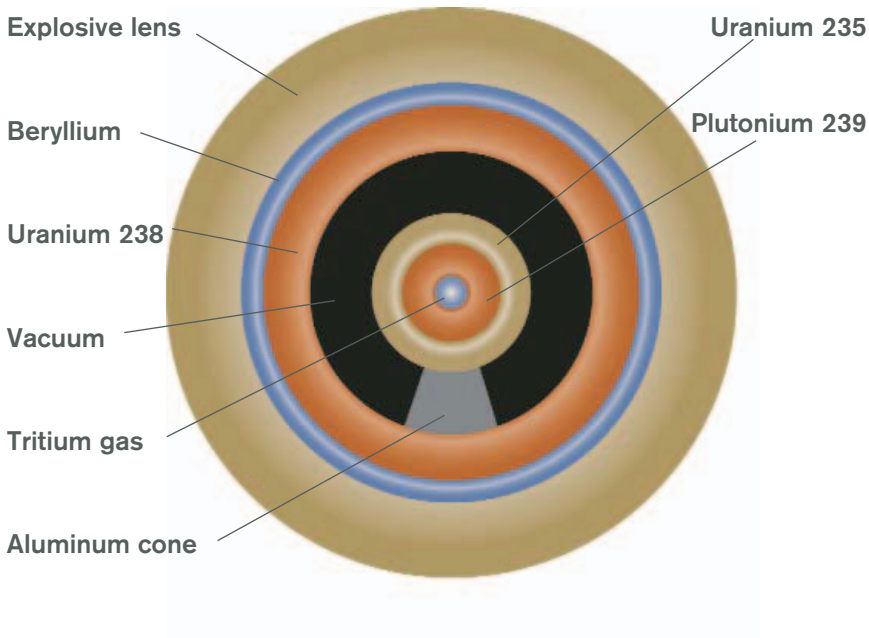
Ulam would stop by my office every now and again to update me on the goings-on. And in December 1950, he surprised me with a novel idea: repeated implosion to get much higher compressions. He called his idea of enhanced compression "supercompression." Instead of using high explosives to implode the principal bomb, he would use the energy output from an auxiliary fission bomb, one near the principal bomb. This sequencing or "staging" of an auxiliary bomb (the primary) with a principal bomb (the secondary) was Ulam's ingenious invention. Even if only a fraction of the energy were utilized, its punch would still be far superior to that of high explosives.

Ulam had been looking for more efficient ways to use fissile materials. By mid-January 1951, he had developed his "iterative scheme" for creating a superbomb. In this scheme, the secondary would now contain thermonuclear fuel. To make it work, the primary and secondary stages would be placed in a capsule-like container, which would also serve to contain the energy release and enable the directing of it. The Ulam basics were staging, containing, and directing the energy to get optimum compression. It seems ironic to me that Teller apparently missed these basics because of his long-held objection to compression.

A new H-bomb configuration, the Staged Super (also called the Teller–Ulam configuration), came out of a subsequent meeting and follow-ups between Ulam and Teller. The Staged Super was based on Ulam's three ideas plus an important contribution from Teller: radiation implosion, the quickest and surest way to get compression.

On January 24, 1951, Ulam briefed Mark and Bradbury on his scheme for superbombs. Both responded positively; Ulam later wrote that Bradbury "quickly grasped its possibilities and showed interest in having it pursued." The next day, Ulam conferred with Teller in a lengthy but productive meeting—a meeting as dramatic, perhaps, as the infamous 1941 Bohr–Heisenberg meetings in Copenhagen. It was a turning point in the H-bomb program. In his autobiography, Ulam wrote about his meeting with Teller:

"At once Edward took up my suggestions, hesitantly at first, but enthusiastically after a few hours. He had seen not only the novel elements, but had found a parallel version, an alternative to what I had said, perhaps more convenient and generalized. From then on pessimism gave way to hope.



An implosion device, similar to that dropped on Nagasaki in 1945, which is the basis for a thermonuclear weapon's primary.

"In the following days I saw Edward several times. We discussed the problem for about half an hour each time. I wrote a first sketch of the proposal. Teller made some changes and additions, and we wrote a joint report quickly. It contained the first engineering sketches of the new possibilities of starting thermonuclear explosions. We wrote about two parallel schemes based on these principles. The report became the fundamental basis for the design of the first successful thermonuclear reactions and the test in the Pacific called 'Mike.' A flurry of activity ensued.

"Teller lost no time in presenting these ideas, perhaps with most of the emphasis on the second half of our paper, at a General Advisory Committee meeting in Princeton which was to become quite famous because it marked the turning point in the development of the H-bomb. A more detailed follow-up report was written by Teller and de Hoffmann. New physicists were brought to Los Alamos, and work toward experimental verification started in earnest." (*Adventures of a Mathematician*, University of California Press, 1991.)

Teller quickly accepted Ulam's new ideas. Not just that, he also realized that the radiation could be appropriately directed into a compression scheme, what he called radiation implosion. This was an important contribution; it made quite a splash at the time. It was good news that had been long in coming. Ulam's suggestion had been something less manageable, "hydrodynamic shock." Still, it seems strange to write about two parallel schemes when they were both based on the same principles: Ulam's staging and supercompression. Was Teller trying to elevate radiation implosion to the crux of the matter? Quite likely. Teller did, however, immediately direct de Hoffmann to write a description of the Staged Super and to check on some of its basic features by calculation. Ulam's new ideas had indeed led to action.

It is interesting to compare Teller's account of the meeting with Ulam's. Teller, with apparent condescension, put it this way:

"Not long after my visit to Nevada, Stan Ulam came to my office. He announced that he had an idea: Use a fission explosion to compress the deuterium and it would burn. His suggestion was far from original: Compression had been suggested by various people on innumerable occasions in the past. But this was the first time that I did not object to it. Stan then proceeded to describe how an atomic explosive should compress several enclosures of deuterium through hydrodynamic shock. His statement excluded my realization of why compression was important, and it also included details that were impractical.

"I told him that I had thought of something that might work even better: It would be much more effective to compress the deuterium with the help of radiation. To be strong, compression must be appropriately symmetrical. Continued symmetrical compression can be much more easily obtained if the energy is first distributed with the help of radiation. But Stan was not interested in my proposal and refused to listen.

"Finally, to put an end to the discussion, I told him that I would write up both proposals, and we would sign it as a joint report. I have no idea whether Stan ever considered the extent to which compression would or would not help." (*Memoirs*, Perseus Press, 2001.)

In what Teller wrote, there is no mention at all of Ulam's iterative scheme—not the staging, supercompression, or the rest—nothing positive, just Teller's impatience with Ulam. An astonishing breakthrough was at hand, but where were the signs of his elation?

Teller's skewed version of events is quite understandable. He had been the top man in the H-bomb effort from the very beginning—instigator, promoter, contributor—all along. Then Ulam came on stage, spearheaded the demolition of the Super, and upstaged him in finding a new plan. Teller understandably had a difficult time accepting this.

At the June 1951 General Advisory Committee meeting at Princeton,

Atomic Energy Commissioner Gordon Dean remarked to Teller that he was astonished at how Teller "had produced a brand-new idea on the spur of the moment!" This was an opportunity for Teller to acknowledge Ulam's role. He could have said: "Well, Ulam had some novel ideas, and I did, too. We put our heads together and there it was—a case of teamwork at Los Alamos." He didn't. (A few years later in 1955, in "Work of Many People" in *Science* magazine, Teller relented slightly, saying, "Two signs of hope came within a few weeks: One was an imaginative suggestion by Ulam, another a fine calculation by de Hoffmann.")

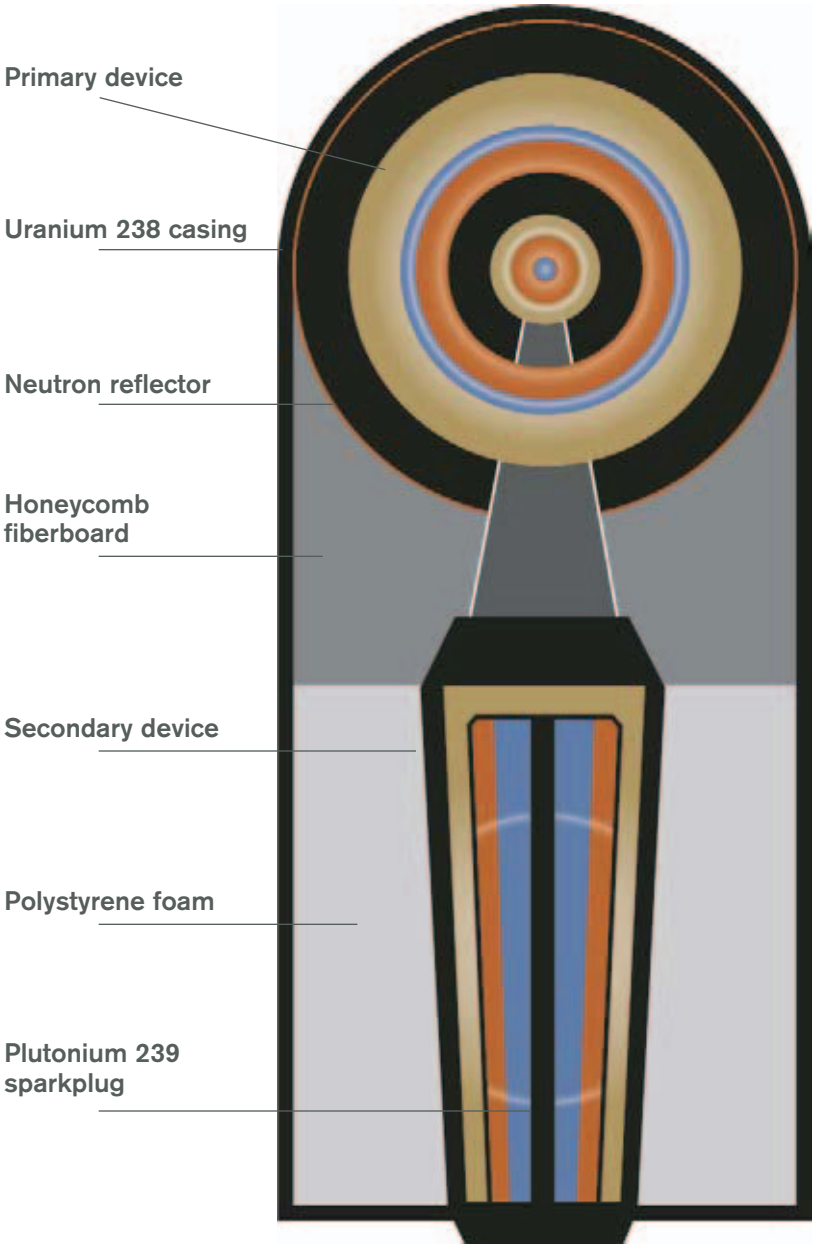
How did Ulam react? He was very disappointed in Teller. He wrote a letter to John von Neumann on February 23, 1951, to explain in his own words his ideas and the new scheme it implied—something totally different from Teller's Super. He was happy to have affirmatively answered the question about whether an H-bomb in the high-megaton range could be built. He was also relieved, I am quite sure, not to become involved in the events that were bound to follow.

It is not difficult to explain Teller's behavior; his memoirs are quite revealing. First, his ambitions were far-reaching, surpassing even his considerable talent. The work on the Super had gone bad, and he saw Los Alamos moving at a leisurely pace and lacking in interest. Second, after the Super was abandoned because of calculations done by Ulam's group, Ulam came up with the iterative scheme, a replacement for Teller's failed Super. But Teller noticed that Ulam had overlooked a detail. By turning the tables, Teller managed to downplay Ulam's great innovation.

Did all this hurt Ulam? Yes, Ulam was deprived of full recognition for his work and possibly also of the prestigious Fermi Award. And did it help Teller? Surely. Teller made Gordon Dean believe that, although the Super was gone, he was onto something brand new. Funds for the Livermore Lab, the second weapons lab Teller had been pushing for, were released in July 1952.

Teller, taking credit for the H-bomb (and testifying damagingly at Oppenheimer's hearings), began to emerge as the top nuclear man. Ulam, on the other hand, kept a low profile and remained active in the Theoretical Division at Los Alamos until the mid-1960s.

Many people familiar with the matter seem to agree (or at least suspect) that Ulam had more to do with the success of the H-bomb project than did Teller. I know he did. We owe much to the genius of Stan Ulam, the man with the back-up plan. ⚛



A thermonuclear weapon. Both diagrams adapted from *Weapons of Tomorrow* by Brian Beckett (Plenum Press, 1983).