

Bradbury's colleagues remember his era

SCIENCE: *Norris Bradbury took over as Director of Los Alamos in October 1945. Would you describe what he faced at that time and what he accomplished?*

ROSEN: I can put it very succinctly. Oppenheimer was the founder of this Laboratory: Bradbury was its savior. After the war many of us had other job offers and many were leaving the Lab. I went to Norris to ask for advice. Norris is a low-key but very effective man. He did an excellent job of helping people decide whether to stay here was, first of all, in the national interest and,

second, perhaps in their own interest as well. This was Bradbury's forte. We tend to forget what management is all about, Management is a tool of leadership. Norris so used it for the country and the Lab.

MARK: With the end of the war, a large number of people who had been important to the Lab's direction and effectiveness could scarcely wait to get back to the place where they really thought of themselves as still being. Most of the well-known scientists were in that group. Bradbury himself wasn't sure about the future of the Lab or his own future.

He was on leave from the Physics Department at Stanford, and he had a house there that his wife liked. But he accepted the assignment of Director for six months, just to give time to decide what was to be done. In addition, the people in the military-scientific group called the Special Engineer Detachment, who had been drafted out of college and graduate school, were very eager to get back and finish their education. So by the end of 1945 the staff of the Lab had fallen by some very large factor, two or perhaps three. It was short of the technical and scientific staff that it needed in order to carry on meaningful activity.

Bradbury turned this process around. He felt that the Laboratory must continue since it was the only place in the country where nuclear weapons could be put together. This is not to say that Bradbury was anxious to use nuclear weapons. But he felt that since the country had put so much effort into these devices and since they were so important, it would be a wrong thing if Los Alamos should not remain capable of producing them. Very shortly it became clear that international agreements on control would not be reached, and it would be necessary for this country to continue nuclear weapons work.

Remember that when Bradbury took over, even the assembly of weapons was a problem because some of the necessary people for that task had already left. The United States was telling the world that we have the atomic bomb, and if you will join us we will throw it open for international control. But the fact was that without this place we didn't have atomic bombs and couldn't acquire more. At the same time the production of fissile materials necessary for weapon production was going through a similar loss of necessary people. The production plants were new and had been run on an emergency basis during wartime. Because they needed all kinds of fixing, their output was slowed



down. That was also a part of the picture at the time that Norris took over the Lab. When Louis said that Norris was the savior of the Lab, he meant just that.

BAKER: If Norris hadn't stayed, or someone like him, I think the Lab would have collapsed. He was so sincere about the need for this Laboratory that he was very convincing when he talked to people about not leaving. And I have always been impressed that he accomplished the task in so short a time. He didn't have much time to save the place, you know.

MARK: Yes. The Lab had been built for a very particular short-range purpose—to build an atomic weapon and bring the war to a close. Some of the buildings and some of the apparatus arrangements were totally temporary. They had to be put on a working basis or else they couldn't be used.

SCIENCE: *What did Bradbury do to get the Lab established on a stable plane?*

MARK: Until the Atomic Energy Commission was established in January 1947, General Groves was the authority, although even his status was unclear. The Manhattan District was formed for wartime and its charter ran out when the war ended, but Groves felt that nuclear weapons development was essential.

As soon as Norris took over he wrote to Groves outlining a proposal for what the Lab should attempt to work on and get done in the coming period. That was the basis on which plans were made and activities were carried out. Almost immediately came up the prospect of a test operation at Bikini Atoll in the Pacific. Simply to get the people, the instruments, the material, and the devices out there and to arrange for all that required a large fraction of the effort that was available.

BAKER: We also have to remember the technical status of the whole business. We had done barely enough, both theoretically and technologically, to get two weapons built. Norris had to get people to do more work on the fission bomb; he was also talked to a great deal at that time about the



thermonuclear weapon. Since he assumed that the Lab would go ahead and continue to develop atomic weapons, he knew that Los Alamos would have to continue to produce a few of the gadgets. But it worried him that Los Alamos was the only place in the country that could build an atomic device. For example, all the fissionable material sent from either Hanford or Oak Ridge had to be purified, changed from a salt to a metal, and then fabricated in order to make a weapon. And we were the only ones who knew how to do it. Norris wanted to get the routine production activities out of the Laboratory as rapidly as possible because there was so



Top left: Richard D. Baker joined the Manhattan Project in 1943 to work on the metallurgy of plutonium and uranium. From 1946 to 1979 he managed the materials research and development for most of the Laboratory's programs and between 1979 and 1981 directed the Laboratory's weapons work. He is now a Laboratory consultant. Top right: William R. Oakes, M.D., came to Los Alamos in 1947 as chief of surgery

at Los Alamos' hospital and consultant to the Laboratory on medical problems related to radiation exposure. Between 1974 and 1981 he was a physician in the Laboratory's Health Division. Bottom: Eugene H. Eyster came to Los Alamos in 1949 from the U. S. Naval Ordnance Laboratory. He managed the Laboratory's work on explosives from 1949 to 1970.



Top left: George A. Cowan returned to Los Alamos in 1949 after an initial short stay at the end of the war. He spent most of his career working on radio-chemical diagnostics for weapons. Later he managed the Laboratory's nuclear chemistry work and directed its basic research activities. He is currently a Senior Fellow of the Laboratory and a member of the White House Science

much work to be done with the materials part of the bomb. We knew very little about plutonium, and we knew very little about its alloys. He used to say that, as the theorists and the designers improved the atomic devices, we were going to require a lot more out of the plutonium and enriched uranium in terms of fabrication, verification of theory, the whole bit.

Council. Top right: Carson Mark came to Los Alamos from Canada in 1945 as part of the British Mission collaborating on the Manhattan Project. He managed the Laboratory's theoretical physics work between 1947 and 1973 and now serves as a Laboratory consultant and a member of the Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards. Bottom: Louis Rosen joined the Manhattan Project in 1944 and continued from that time to work in basic nuclear physics and defense applications. He is head of the Laboratory's Meson Physics Facility and was in large part responsible for its existence.

To show how Bradbury went about things. I want to read part of a letter that he wrote to the Atomic Energy Commission before the Commission officially took office. It was dated November 14, 1946.

The problem of production of atomic weapons has been considered. It is believed that no immediate change can be made in extent of production now being carried out at Los Alamos. However, if the philosophy of maintaining Los Alamos as an atomic weapon research center is carried out, it is suggested that plans be made to remove as much as possible of this routine activity from this site. This has the additional advantage of disseminating the knowledge of necessary technique as well as decreasing the seriousness to the nation of a major accident or catastrophe at Los Alamos.

At that time Norris would say that, as soon as we could get the production out, he wanted to start a great deal of research, applied and basic, on the actinide elements. Soon after, he started that work, and it is still going on. Norris Bradbury, as Louis said, was a very low-key person. He would always qualify his statements about the future by saying, "Look, I don't know where we are going, but if it goes where I think it will go . . ." But when he spoke he was certainly convincing.

MARK: Bake, would you happen to remember when it was possible to build a device any place but here?

BAKER: I guess it was at least five years after the end of the war. Hanford started to fabricate the plutonium parts for us earlier, but then we had to assemble them. We produced only the Trinity-type devices.

COWAN: As Carson mentioned before, in early '46 the Laboratory was committed to go overseas to do the military exercise known as Operation Crossroads, and it occupied the attention of a lot of people. So there was a great deal of ordered activity even as people were coming and going, leaving and returning, and so forth. Opera-

tion Crossroads was sponsored largely by the Navy and was intended to determine the vulnerability of naval vessels to nuclear weapons. It consisted of the detonation of two fission devices, one under the surface of Bikini Lagoon and the other dropped from an airplane. These tests, which took place in July 1946, resulted in some of the classic pictures of the boat perched on top of a bridal veil of water raised by the underwater explosion. I was there when that picture was taken; in fact, I was flying in a B-17 with the photographer. It was right before I left the Laboratory to return to graduate school. At that time there wasn't much question in my mind about whether the Laboratory would continue.

BAKER: Bradbury was doing all this planning and recruiting, and at the same time he had you people over in the Pacific doing those tests. He didn't wait for anyone—a phenomenal man.

MARK: But why they didn't round up a bunch of Japanese ships and use those for the targets at Bikini, I'll never understand. Instead we took some good overage American ships over there and beat them up. We also had to send a large fraction of our scientific staff. Remember that the first bombs almost had to be put together by graduate scientists. For example, although I don't know that Kistiakowsky was absolutely required in the tower at Trinity, he was there. The people who put those pieces together had to really understand what they were doing and why the piece did what it did. They had to be able to say, "It does tit; it's all right."

BAKER: Or, "It fits well enough."

MARK: It was clear in '46 that these weapons, although made at Los Alamos, had to be converted into military equipment that could be handled by people trained to handle them, just as airplanes are flown by guys who know how to fly but don't know how to build a plane. That transition had to be gotten through as fast as possible.

In talking of the great uncertainty



Mushroom cloud and first stages of the base surge from the underwater detonation of a nuclear weapon during Operation Crossroads at Bikini Atoll in the Marshall Islands in 1946. Operation Crossroads also included an atmospheric detonation.

throughout the fall of '45 and the continuing period, we should mention that the future of the Lab had to some extent been resolved by the middle of '46 because the permanent community was already being built.

EYSTER: When I was here at Los Alamos after the Crossroads operation, I remember Max Roy's showing me the first two Western Area houses and his saying, "Now look. We're really going forward—there is going to be a continuing Laboratory and there are even going to be places for people to live!"

BAKER: We were also building DP West at that time. During the war all the fissionable material, especially the plutonium, was handled in D Building. It was decided about the time of Trinity that a new plutonium facility had to be built, but they didn't spend very long designing. As I recall, by the time Bradbury took over, McKee, the contractor, had started construction on the building without a contract. He bought the materials out of his own company's pocket until the government could start reimbursing him.



Planning the Tech Area at Los Alamos in 1946. Seated (left to right) are Bradbury, General Groves, and Eric Jette of the Chemistry and Metallurgy Division. Standing are Colonel Seeman (left) and Colonel Wilhoyt (right).

That site was built in about a year to a year and a half, and it served very well for years and years. It may be true that the Laboratory was floundering as to what to do in '46, but Norris was not acting that way; he was just going ahead making plans to have an atomic weapons laboratory coupled with a lot of research in the areas of nuclear physics, reactors, actinides, and so on. Very far-sighted.

ROSEN: One of the greatest things Norris had a lot to do with from very early on was planning the future of this Laboratory. If this Laboratory was going to serve its function in the application of science to national defense, it had to prepare the way for doing things not only immediately but ten years, twenty years, thirty years hence. The only way to prepare yourself in that context is to develop the knowledge base, and to do so you must

never shortchange the resources available to those in the Laboratory who are dedicated in whole or in part to basic research. That vision more than anything else was important to Bradbury's success.

I remember very well that during the Bradbury years we did not wait for somebody in Washington to decide what we should do. We worried and thought and worked on what our program should be, this was presented to the AEC or whomever, and then we got back something that said, "You shall do such and such," which was in many cases exactly what we told them we would do.

BAKER: Norris decided even before the Commission was formed what he thought the Laboratory should do, and when the Commission was formed, putting it bluntly, he sort of told them what the Lab would do.

MARK: For the first four or five years after the AEC took over, the people in Washington, both on the staff of the Commission and in Congress, knew so little about what the possibilities were, what the options might be, that they either asked for or accepted the planning or proposing that was developed here. They would say, "Please explain why you think such and such is a good thing to do." That was the frame of mind in Washington up until the mid '50s when a large staff, which had to think of something for itself to do, decided it had to direct things. Also, by the mid '50s people in Washington had become more familiar with the nuclear field. Most of them learned for the first time in August 1945 that there were nuclei in atoms and things like that.

OAKES: We often forget that in the early days we really didn't know much about what was what. In the '30s when I was in college and Fermi was in Italy doing his first experiments, plutonium wasn't known. It wasn't discovered until 1940. Cyclotrons had just been built, and the interest in x rays and alpha, beta, and gamma rays were all new things. We knew very little about isotopes. All of these were things we would have studied anyway whether there was a war or not, but the investigations that went on in relation to the bomb accelerated the process.

ROSEN: As these gentlemen are talking and reconstructing some of the flavor of the Bradbury years, one thing comes to my mind. Every year Norris testified before Congress, and one time he was asked by some character, "What have you done recently to save money, cut costs?" Norris said, "A laboratory such as Los Alamos is not established to save money. It is established to spend money."

BAKER: And they answered, "Yes, sir."

ROSEN: That ended that conference. Isn't that a far cry from the way things are now? I should emphasize that Norris didn't make decisions alone. In trying to understand where this Laboratory should go, he in-

volved the staff. There was direct coupling between him and each division leader in the Laboratory.

BAKER: He even worked the group leaders.

ROSEN: He thought he knew everything that was going on in the Laboratory. He wasn't always right. One thing that he understood very well was that this Laboratory must be prepared to solve problems, unknown problems, national problems, when and if they arise. He was always concerned with maintaining that capability, and that reasoning led him to diversify the Laboratory about halfway through his tenure as director.

SCIENCE: *Was there some thought that the Laboratory would be involved in peaceful uses of atomic energy?*

BAKER: Bradbury was moving along, as Louis said, awfully fast. He was looking forward to having research in lots of areas. For example, in August of '46—believe it or not—there was a meeting held here entitled "Conference on Alloys for Breeders." He was already starting to think about using fissionable materials for reactors and getting us in on it.

SCIENCE: *Could we turn now to the problems to be solved in the design and testing of nuclear weapons?*

COWAN: When I left in the fall of '46 it was clear to me that the Laboratory's most immediate and important task was to design smaller fission weapons. I guess the plan for the Sandstone tests was already beginning to take shape in late '46, and those tests took place in the spring of '48. Remember that the Trinity-type devices were heavy and cumbersome and didn't really fit into the standard bomb bay. In fact, after a bomb was dropped, the plane would have to go back for repairs. Also the original devices were overdesigned. They were designed to work well on top of a tower at Alamogordo.

MARK: Let's go back a bit. Certainly, by the end of 1945 we recognized a number of quite obvious, important, first-order facts. One was that the engineering of the weapon device had to be gone over and tremendously



Bradbury (left) and Robert F. Bacher, a member of the Atomic Energy Commission, at Los Alamos in early 1947. During the war Bacher headed the Laboratory's investigations of implosion dynamics.

improved so these weapons didn't have to be actually assembled here. That didn't really require so much design or testing, but it required a great deal of work. That proceeded immediately. Second, we needed weapons whose nuclear parts were of a different pattern than those in the Trinity device. Some calculations and many estimates made during the war indicated that the Trinity device was a conservatively designed weapon and that, if things worked well, other designs could make better use of the fissile materials being produced at Hanford and at Oak Ridge. Enriched uranium from Oak Ridge had been used only in the terribly inefficient gun-assembly pattern at Hiroshima. Plutonium had been used only in the

much more effective implosion assembly pattern. But what would be desirable when you had a stockpile of both materials, either in hand or in the course of becoming, was not determined. A small selection of the very straightforward obvious options in weapons design were tried out at the Sandstone tests in the spring of 1948. These tests gave highly satisfying results that led to essentially immediate plans to make changes in the kinds of weapons for the military stockpile. The Mark 4 was the device anticipated for the stockpile. It would contain standard components that could be made by mass-production methods and could be put together by assembly-line techniques, so the end of routine production at Los Alamos was in sight.



C-54 transport planes carried men and equipment to test operations in the Pacific. (Photo courtesy of the Historical Archives of the U.S. Air Force's Military Airlift Command.)

And most important from the practical point of view, this new implosion weapon would utilize the ample supply of uranium-235 being produced at Oak Ridge.

Another consideration being looked at was the size of the device. It was perhaps more evident to us than to the people in the Department of Defense that it would be convenient to have weapons of smaller physical size so that they would not necessarily require taking the large B-29 up in the air. Most planes were too small to carry a Trinity-type device, so the possibility of size reduction was a very natural line of inquiry. However I don't believe the tests on that point were made as early as the Sandstone tests of 1948, but rather in the tests of '51 and '52.

I might add that the directions in which improvements could be made were easy to picture in '46 but very much harder to realize, particularly when every last piece had to be made here.

SCIENCE: *When did weapons first begin to be stockpiled?*

MARK: About the end of August 1945. To

the extent that the production plants produced material, it was converted, as near as could be managed, into devices that could have been used, had there been the occasion. But, as I mentioned earlier, there was a large slump in production at the end of '45. Consequently we were not making tens of weapons per month or anything of that kind. It was necessary to take two to Bikini Island for Operation Crossroads in the first half of '46, and at that time they were not a trivial fraction of the stockpile.

OAKES: One question that arose during my contact with the Air Force was how does an airplane drop a bomb and get out of the way without getting blown up. This was not a problem for the B-29s carrying the early bombs at 30,000 feet, but one wondered how fast a smaller bomber would have to go. This was a question that changed the size and types of bombs.

SCIENCE: *While we are on design and early testing, can you describe the effort required to do the Sandstone tests?*

MARK: We had only enough manpower and technical capability to run three tests. They

required sending hundreds of people from the Lab out to islands in the Pacific for a couple of months, and some many dozens were there longer than that getting the place ready. Also, before doing other tests one wanted to see how these experiments went, because it was by no means assured how good the results would be. We needed to explore the options of reducing the amount of fissile material or reducing the amount of high explosive. Could one make bombs this small or not? Those were the kinds of things in people's minds in 1948.

OAKES: The 707 wasn't operating in those days, so a good number of people and all the equipment had to go by boat.

COWAN: Some of us went in C-54s, and that was no luxury. There were no seats in them, just canvas slings in which you could sit for the twenty-four hours it took to get out there.

MARK: When I went to the tests in '48, I went sort of first class compared to what Bill is reminding us of. Pan Am actually cancelled a flight on its transpacific route. That flight flew to Japan every day of the year except on this particular day, when it became a special flight to Kwajalein for government-connected people only. They even had female hostesses on that plane, and we had seats. When we landed at Kwajalein, the hostesses were welcomed by a guard of Marines who escorted them to a little hut and stood guard over them all night.

SCIENCE: *Let's move ahead now to August 1949 when the Russians detonated their first atomic weapon. That came as a surprise to President Truman and to many in Washington. Was it a surprise at Los Alamos?*

MARK: The fact of the Russian test was not a total surprise to people who had given it any thought. Sometime they were going to have one, and '49 was not spectacularly early or late.

SCIENCE: *Was the test announced or discovered?*

MARK: It was not announced by the Russians. The American monitoring planes flying between the mainland and Japan

picked up radioactivity in the air, and samples from filter papers were brought back to Los Alamos for analysis. I am not sure whether any other place in the country could have handled the analysis.

COWAN: Not at that time. There were also samples from rain water collected on the roof of the Naval Research Laboratory in Washington, which was set up to do some analyses, but not in the same sense that the filter samples were handled at Los Alamos.

BAKER: There was a monitoring system at that time?

COWAN: It had just been put into effect, perhaps weeks before, through the Air Force.

MARK: Here at the Lab, Rod Spence, George, and their colleagues in radiochemical diagnostics went to work to assess what was in that radioactivity. They concluded that the products had been formed in an explosive event rather than in a production reactor over a long time.

COWAN: The ratios of short-lived fission products to long-lived fission products can provide absolutely definitive information as to whether the event that produced them was drawn out over days, weeks, months, or occurred instantaneously. In this case the ratios said very clearly that all of the fission products were made at the same moment, which is characteristic of an explosion and of nothing else.

MARK: Didn't it take quite a number of days to be really certain of that conclusion?

COWAN: Yes. There were also quite a number of days spent in Washington talking to panels set up to find out whether indeed this evaluation was correct. It was all top secret. I can recall going to Washington where I'd been told I would be picked up at the airport by an intelligence person. I wasn't told what he looked like, and I didn't know how he would find me. When I got off the plane, I saw somebody in a trench coat slouching against the wall, so I walked up to him and said, "Are you waiting for me?" And he said, "Are you Dr. Cowan?" I

picked him out right away.

MARK: I recall that, after the panels were convinced, it took quite a number of days in Washington to persuade President Truman that there was no doubt what the Russians had done. So it was four weeks or a month after the event before he announced that the Russians had made a nuclear explosion. The Russians just sat on their hands and didn't say a word about it.

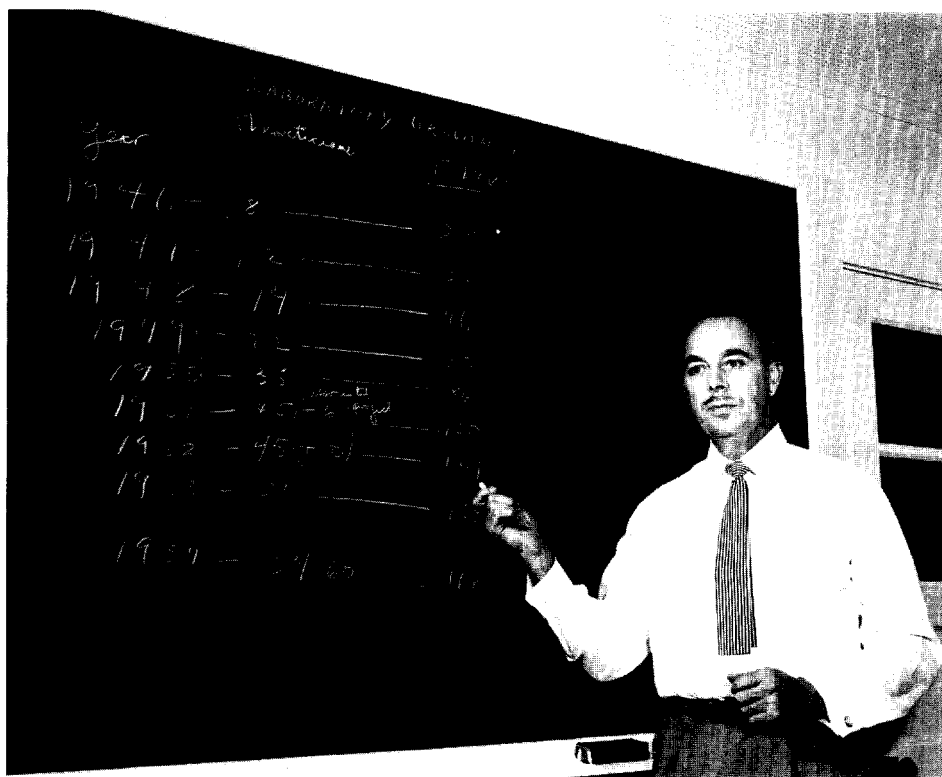
The Russian test caused a number of people, most of them not at Los Alamos, to feel that the nation was now in peril and must make a strong and tremendously impressive response to the terrible misdeed of the Russians. Teller, Lawrence, Alvarez, Lewis Strauss, Senator MacMahon, and Air Force Secretary Finletter were among those who suggested we should go all out to build a thermonuclear bomb that would produce an enormously larger yield than had been achieved with fission bombs. A lot of debate followed, involving many people in Washington with many differences of opinion. Then in January 1950 the President announced we were going to proceed with work on nuclear weapons of all sorts, including the hydrogen bomb. He didn't say we were going to have a crash program to get the hydrogen bomb going, and the Lab had been working on the hydrogen bomb in a secret fashion quite persistently from 1946 on. So Truman's words didn't necessarily mean that we did anything much different from what we had been doing because we didn't really know how to make a gadget that would work as a hydrogen bomb. However, Truman's announcement was regarded as a great victory by those who had been advocating a crash program, and it was taken by the AEC to represent something of that sort. Immediate plans were made to increase the production of nuclear weapons material, and the Los Alamos staff went on a six-day week for the next two and a half years or so—until November 1952 when the Mike test demonstrated that a large thermonuclear explosion was possible.

COWAN: Incidentally, when the first Russian atomic weapon was tested, some people speculated that the Russians produced their plutonium with a heavy-water reactor, or something other than a graphite reactor, and that this reactor, since it produces an excess of neutrons, might be producing the large amounts of tritium needed for one version of a thermonuclear device. That speculation proved to be incorrect. The first Russian reactor was in fact an orthodox graphite reactor. But the notion that it might have been a breeder and that the Russians might be well on their way toward developing a thermonuclear device had something to do with the urgency regarding our own thermonuclear program.

MARK: The fact that Klaus Fuchs had provided information to the Russians also became public within days of the announcement that the United States was going to go ahead with work on hydrogen bombs. The Fuchs business caused additional confusion in Washington. "What could he have told the Russians? No doubt whatever he told them accounts for the fact that the Russians have a bomb now instead of in 1985." Such speculations were of course a great deal of nonsense. In retrospect it is not clear that Fuchs' information really made a large difference in the progress to be expected of the Russians if they started off much as we did.

SCIENCE: *What work needed to be done to make a hydrogen bomb?*

MARK: Well, you might think that when people talked about the hydrogen bomb they had a drawing of a device that simply needed to be built and tested. But in 1950 we didn't have such a drawing because we didn't know how to initiate a large thermonuclear explosion. There were possibilities of small experiments to make sure that we could set off thermonuclear reactions and that we understood how they proceeded. An example of that was the Greenhouse George shot of May 1951. That was the famous shot about which Ernest Lawrence cheerfully handed Edward Teller five dollars after he had



Speaking to reporters in September 1954, Ralph Carlyle Smith (a member of Bradbury's administrative staff) describes growth of the theoretical effort at Los Alamos during the push for the hydrogen bomb.

learned from Louis Rosen that it had worked. The George shot used a very large fission explosion to set off a small thermonuclear one. Those were the first thermonuclear fusion reactions to take place on Earth. Our goal, however, was to produce a very large thermonuclear explosion, and we didn't know how to do that. We were proceeding anyway, and people like Baker and Marshall Holloway had a tremendous materials job on their hands. They rounded up a considerable number of new industrial enterprises to help do the mechanical things that had to be done. American Car and Foundry had been making bomb cases for the blockbuster 10,000-pound high-explosive bombs. They were the only place in the country that had the tooling for pieces of metal of the size that we would need. The A.

D. Little Company knew something about cryogenics on a laboratory scale and was asked to work on a monstrous piece of cryogenic engineering. If we were going to make a thermonuclear device, we would have to have tritium and liquid hydrogen or liquid deuterium, not in a Dewar in a lab but in a container on a tower where it could take part in a nuclear experiment. Although that work had been in progress here, it was possible to increase the attention on it. The Bureau of Standards, which had never attracted tremendously generous funding, was quickly given money to hurry up and complete construction on their cryogenic lab in Boulder that would liquefy hydrogen in massive amounts. We needed it here for testing apparatus, and we needed it for the ultimate purpose. There were many other

people involved too. The Cambridge Corporation was making equipment to get large amounts of hydrogen from Boulder to here and to the Pacific. I am not sure what the metallurgists had to do.

BAKER: They had to do a lot of work on the materials for Dewars. They were always worried about plutonium's getting brittle and stuff like that.

MARK: Never before had the problem of plutonium behavior at liquid hydrogen temperatures been faced. And there were plenty of problems with plutonium even at room temperature. Lots of people got set to work thinking of what should be done if we were to go ahead with what was called Little Edward. That was never carried beyond the conceptual stage, but it certainly required us to do a tremendous number of things, all in a compressed time scale compared to the normal rate.

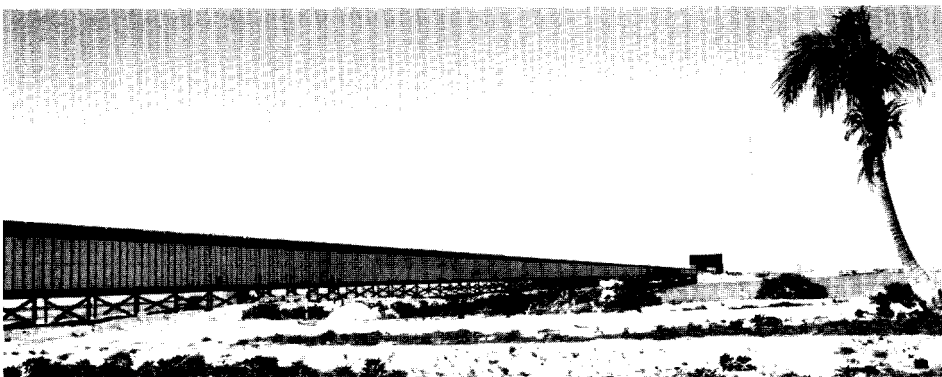
I might also mention that in addition to the design work, which kept us sleepless at night and sleepless by day for a whole year, there were lots of political things happening related to Edward Teller and his campaign for a second lab.

BAKER: Most of the workers didn't pay any attention to those matters.

MARK: Of course, they didn't happen very much here; they happened in the offices of the Secretary of the Air Force and Senator McMahon.

To return to the technical story, on the theoretical side we tried to calculate how thermonuclear reactions might possibly proceed, taking into account this effect or that effect that had been ignored before. There were also gaps in what was known about the neutron and thermonuclear cross sections, and, while that study had never stopped, it could obviously be given more emphasis. And, perhaps as much as in anything, we were engaged in trying to acquire additional people who might be helpful in thinking through what was needed to make the device work.

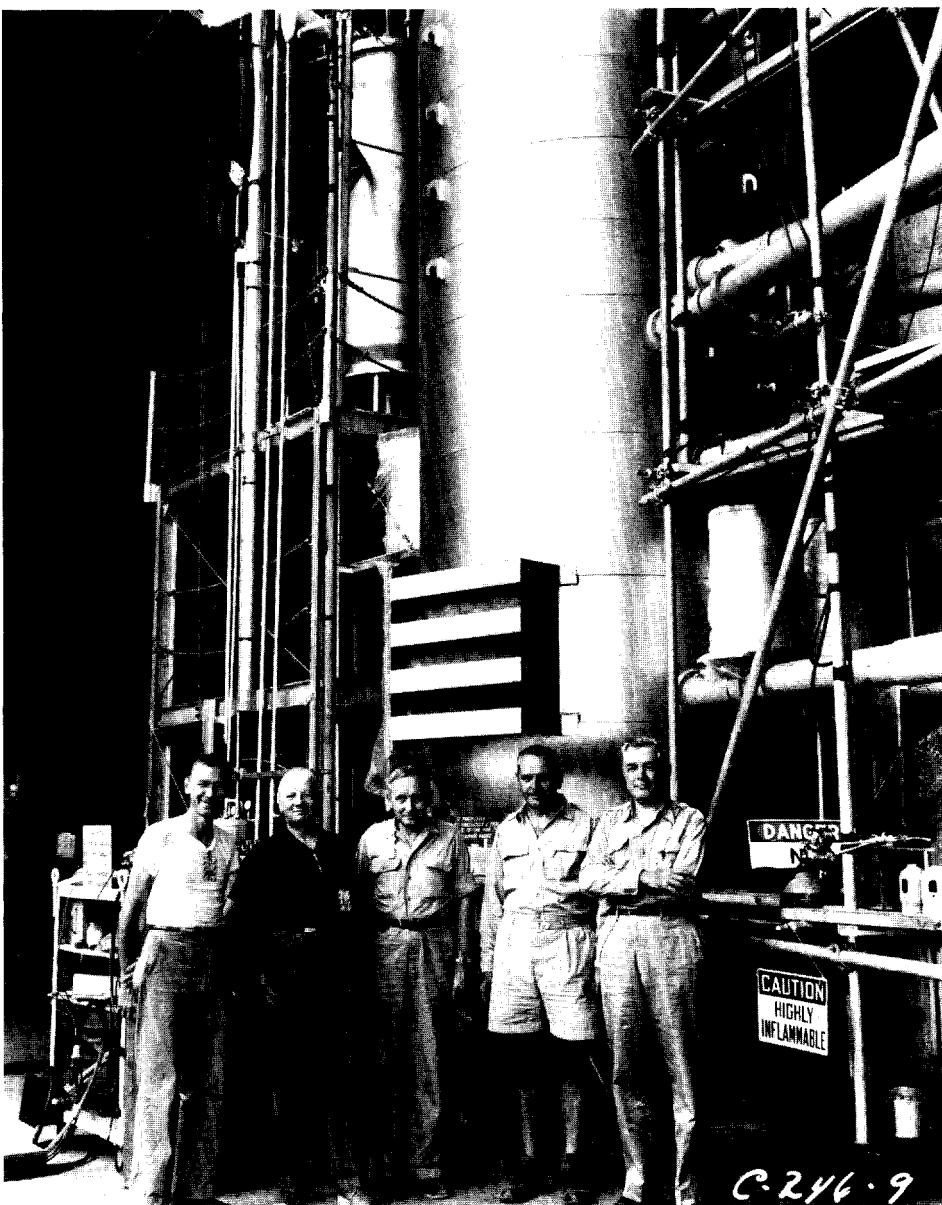
Between January 1950 until the end of



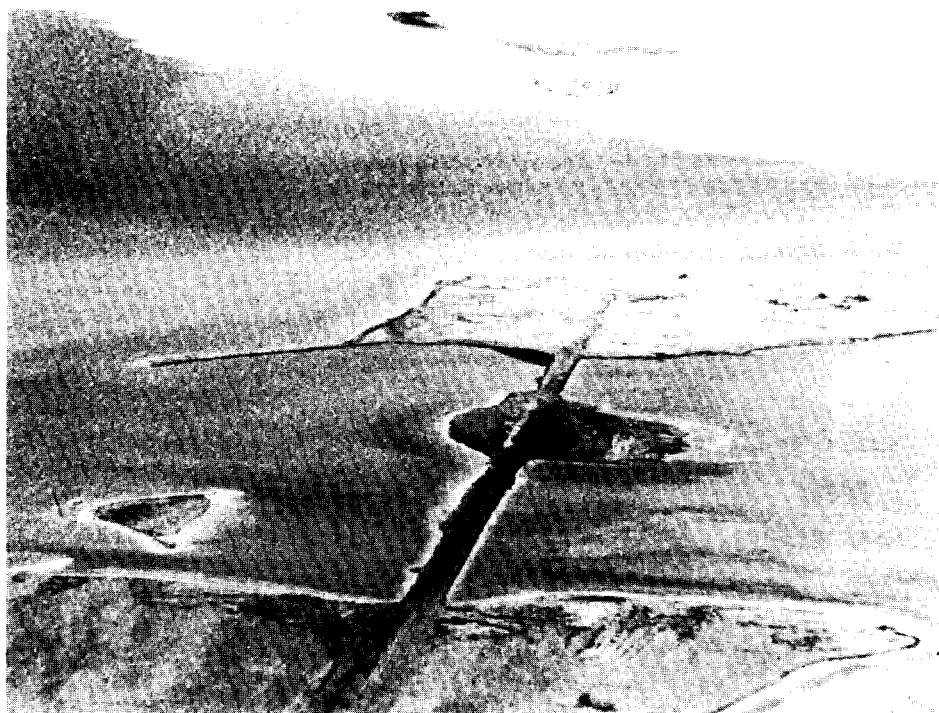
January of 1951, our work carried in mind a pattern of device that has often been referred to as the Classical Super. However, as described in the GAC [General Advisory Committee] report and in many other places, the prospects for its working were uncertain. Then in February or March 1951 the Teller-Ulam concept came in sight, and that immediately struck people as something that could be put together and would work. It was then that the whole point of the studies shifted. This was before the Greenhouse George shot. Greenhouse George had been planned and, in fact, preparations for it were under way out in the Pacific when the Teller-Ulam concept was invented. The new concept led to the big powwow in Princeton in June of 1951 at which the AEC and the GAC responded by saying, "Please tell us how quickly you can move on it." A year and a half before the GAC had said, "We don't think you should start a crash program on the ideas you have now." They got overruled. But in June 1951 they said, "That's something on which a crash program is warranted. Go ahead," and. "What do you need?" It was from that point on that we went out and made this really monstrous experiment in the form of Mike, which weighed about 140,000 pounds not counting the cryostat, the liquefaction plant, and the other stuff attached to it. And indeed it was a great success from the point of view of working about as well as the calculations had indicated it might. Mike wasn't a weapon, but it brought in sight the feasibility of weapons in which a fission explosion sets off a large thermonuclear explosion. That has been the main line of work ever since with tremendous variations to make the devices weigh less than 140,000 pounds and make them fit into missiles.

COWAN: During this period following the Russian test, we were also involved in an accelerated program for testing small fission devices, which, by the way, was done at the Nevada Test Site in 1951.

SCIENCE: Why did we begin testing in the



Top: The helium tunnel, a diagnostic line of sight, transmitted gamma rays from the Mike shot on Elugelab Island to recording equipment in a massive blockhouse a couple of miles away. The tunnel contained steel and plastic collimators and was filled with helium rather than air to prevent absorption of the gamma rays. Bottom: The Mike device clothed in its cryogenic plumbing on the island of Elugelab at Eniwetok Atoll in 1952. George Grover (left) and Marshall Holloway (center), who was in charge of the Mike shot, are shown with high-ranking officials of American Car and Foundry, the company responsible for most of Mike's fabrication.



Eniwetok Atoll before and after the Mike shot. Elugelab, the island on which Mike was detonated, disappeared completely as a result of the test.

continental United States?

COWAN: In order to do things faster and more conveniently than overseas, This additional test site was justified by the urgency of having to do certain things preparatory to the overseas tests. and the work there contributed significantly, I think, to the success in '52 of the Mike device, I remember one particular event in Nevada. whose name I can't recall, that demonstrated that certain aspects of the principles involved in the design of Mike were presumably correct.

MARK: A test in the Pacific had to be scheduled and planned for something like a year in advance. It required a construction crew of several thousand people going half-way around the world with all the sanitary and whatever facilities were needed. It took a group from the Lab, some going by boat, some by plane, to get out there and unpack their equipment, to see if it was still working or had broken on the way out, to string the wires and put them up, and so on. In Nevada you didn't need anything like the task force that was necessary when working outside the continental limits. In Nevada people could actually use hotel rooms in Las Vegas and go to work in the morning.

EYSTER: Al Graves had an arrangement whereby he could leave Los Alamos in the morning and return in the evening and still spend a useful fraction of the day out in Nevada. He had to leave home in the dark, and one morning he arrived there with one black shoe and one brown shoe.

ROSEN: Actually it was during the tests of '51 and '52 that Bradbury's policy of encouraging basic research paid off in large measure. Those tests brought to bear instruments that were developed not to do the tests but to do quite different things in fundamental nuclear physics, electronic and nonelectronic instruments for measuring neutron spectra.

COWAN: There were also new radiochemical detectors incorporated in Greenhouse George. They were first suggested by Dick Garwin, at that time a consultant and a

summer student at the Laboratory. Those detectors have since been used routinely in weapons testing. They came out of the basic research program in nuclear physics and nuclear chemistry and are a highly important diagnostic technique.

ROSEN: We could fill a book with examples of the symbiosis between basic and applied research just from the experiences here over the past forty years.

MARK: Louis and his colleagues had been attempting to measure cross sections for various nuclear reactions at the Los Alamos accelerators, and they had devised instruments to get the best recording of the neutron energies and fluxes involved in those experiments. In the Pacific we also wanted to measure the neutron flux and neutron energies, and we wanted those measurements as a function of time during the explosions. The problem was by no means the same as in the accelerator experiments but was closely related. Louis and his group took their equipment, which was delicately mounted on glass and tripods and stuff in the lab, and boxed it up in such a way that it could sit close to many kilotons of explosion and still record the data.

BAKER: Electronics was in its infancy then, and it was a tremendous job to make those detectors work under those conditions.

COWAN: Detectors and the electronics for them developed very fast during that period. We were moving away from particle detection with the old Geiger-Muller tube to detection with sodium iodide crystals. That was an enormous advance. Then multichannel analyzers came along; the first crude ones were a tremendous step forward because we could easily separate particle counts into energy bins and quickly determine the spectrum. Many of these new instruments were homegrown. Every three months the situation seemed to change as a tremendous amount of new stuff was designed and tested. Of course a very important aspect of this work was that money was no object. We could afford whatever we were able to do.

ROSEN: All that had to be decided was what did we need to measure. Then the resources for accomplishing the measurement were available without further question.

COWAN: And we worked furiously to get the job done. We were on a six-day week and Sunday was supposed to be the day off, but that wasn't the case either. Nor did people necessarily go home to sleep at night; people sometimes slept in their offices.

MARK: One improvement Louis didn't mention relates to the fact that for many years he maintained a corps of housewives working four hours a day ruining their eyes peering into microscopes to get the data he was anxious to see. The mechanization of that work was a tremendous breakthrough.

ROSEN: Those women did an enormous amount of important and demanding work. They were looking at nuclear particle patterns through microscopes. We were often able to hire a young lady because she had decided she just couldn't have any children, but after she worked for about a year—we helped with the fertility problem in Los Alamos.

COWAN: During this same period our need for large-scale electronic computing in connection with calculations for thermonuclear devices had an important stimulating effect on the development of computers. Many of the calculations in '51 were carried out elsewhere because of our limited computing facilities.

MARK: They were carried out on the UNIVAC at Philadelphia and the SEAC at Washington and the Western Bureau of Standards machine and I think the ENIAC also.

COWAN: When did our computing capability start to exceed that at other places in the country?

MARK: It was probably around '52. Our own MANIAC began to work then, and we were also getting a 701 from IBM. As soon as IBM made further improvements, we switched to those and our computing capability became impressive very rapidly. We

acquired the first samples of two or three successive generations of IBM machines.

COWAN: We were the first customer for everything.

MARK: So a stream of salesmen from all the computing manufacturers began to beat a track to the door.

SCIENCE: *You mentioned that knowledge of Fuchs' betrayal came at just about the same time that we initiated the big push for the hydrogen bomb. What was the reaction of Los Alamos to that revelation?*

BAKER: I had known Fuchs quite well because he and I lived in the Big House during the war. He certainly was a charming fellow. Boy, was I mad when I found out he was spying for the Russians! But I doubt if he helped them by more than six months or so.

MARK: Reading the biography of Kurchatov by Golovin, I got the impression that Fuchs' information didn't bring them a great deal of news. They had an idea of what we were doing and had already started their own work on a fission device before Fuchs came to Los Alamos. Remember Flerov's paper on the spontaneous fission rate of uranium-238 in 1940. That was a tremendous bit of work for that time because the number of spontaneous fissions in uranium-238 is really very low. He reported his work in the *Physical Review* and didn't get a rise out of any American physicist because we had all been told this work is secret. He then said, "Gee, the Americans didn't comment on this. That's the kind of thing they would have gotten very excited about six months ago. They must be working on something secret."

BAKER: I always felt that Fuchs helped them to go directly to the implosion system for plutonium rather than worrying as we did about obtaining extremely pure plutonium for gun-type devices. Fuchs surely knew that plutonium-240 underwent spontaneous fission and fouled up the gun device. Don't forget how great a turmoil there was here when we discovered plutonium-240 in the



Klaus Fuchs at Los Alamos. (Photo: The Bettman Archives.)

Hanford plutonium. For some reason we didn't expect it. We were going gun-wise at that time.

MARK: My reference to Flerov's work is not totally irrelevant because the Russians were tremendously well prepared to spot spontaneous fission. If they could see it in uranium-238, they could certainly see it in plutonium-240).

COWAN: Flerov's colleague Petrzhak told me that in 1943, when the Germans were advancing against the Russians and Russia was fighting for its life, he was called back from the Russian-German front to Moscow to join Kurchatov's group. 1943 was after the first chain reaction at Stagg Field in Chicago, and I suppose that might have had something to do with setting up the Russian group at a time when the country was in great danger of falling to the Nazis,

MARK: That was before Fuchs was here. He didn't come until '44.

SCIENCE: *What were other impacts of Fuchs' betrayal?*

EYSTER: After the discovery of what he had been up to, our relations with the British in the field of nuclear weapons were abruptly

and pretty completely cut off for some time.

MARK: They were in the soup before that because of difficulties with the Quebec Agreement between Roosevelt and Churchill.

EYSTER: Considerably later we went back to talking to the British, and it was fairly instructive to us in the explosives business to see the course that the British had taken in the intervening years. We were surprised to learn that, in the main, British developments were very similar to ours.

SCIENCE: *When did you go back to working with the British?*

MARK: "58.

SCIENCE: *Were there any changes in security regulations following the Fuchs affair?*

MARK: I don't remember any change. The security regulations that came in with the Atomic Energy Act of 1946 were in some respects troublesome because everybody on board had to be reinvestigated. A number of people were dropped who had previously been thought to be all right, but that happened quite independently of Fuchs. The McCarthy hearings, which raised the specter of the government's being full of spies, intensified the security work somewhat, but I don't think Fuchs' betrayal in itself had any effect.

BAKER: But when it was first known what Fuchs had done, there was a lot of clatter about poor security, poor clearance procedures, on and on.

COWAN: We didn't independently investigate Fuchs. He came to us as a loyal citizen who had been cleared by the British for access to this kind of institution.

BAKER: One of the criticisms was, "Why didn't we clear him too"?

COWAN: That would have required going to Great Britain and conducting a security investigation, and besides that he was a German emigre.

MARK: Remember, the wartime clearance procedure was totally different from the clearance procedure that came into effect in 1947. During the war a guy might have

associated with anybody at all, but if someone decided he was all right, he was all right.

COWAN: The security clearance after that took into account your wife's politics, her family's politics, your friends' and family's politics. This emphasis increased as a result of the McCarthy era so that in effect you weren't innocent until proved guilty, but instead you were almost guilty until proved innocent. Some people were unjustly denied clearances at that time.

The facts suggest that there were no spies around in the early '50s in spite of McCarthyism-type comments to the contrary, or at least there was nobody at a high level with an open channel of communication to the Russians to pass on the Teller-Ulam idea. In developing their fission bomb, the Russians demonstrated their technical competence to do things in about the same length of time that we required, but they nevertheless took three times as long to do something equivalent to our first real thermonuclear test. It took us a year and a half after the Teller-Ulam concept to go to a test, and it took the Russians four and a half years from that time,

MARK: I don't entirely accept your point, George. Their first thermonuclear device was six years after their first fission bomb; ours was seven.

COWAN: But Carson, the Russians paid enormous attention to the significance of our thermonuclear event. The Kurchatov biography says that he was in effect given a blank check. He didn't get it to develop the fission weapon, but after Mike went off he had the resources of Mother Russia at his disposal. And nine months later the Russians tested a thermonuclear device. That was a tour de force, but it didn't imply any covert information about the new concept,

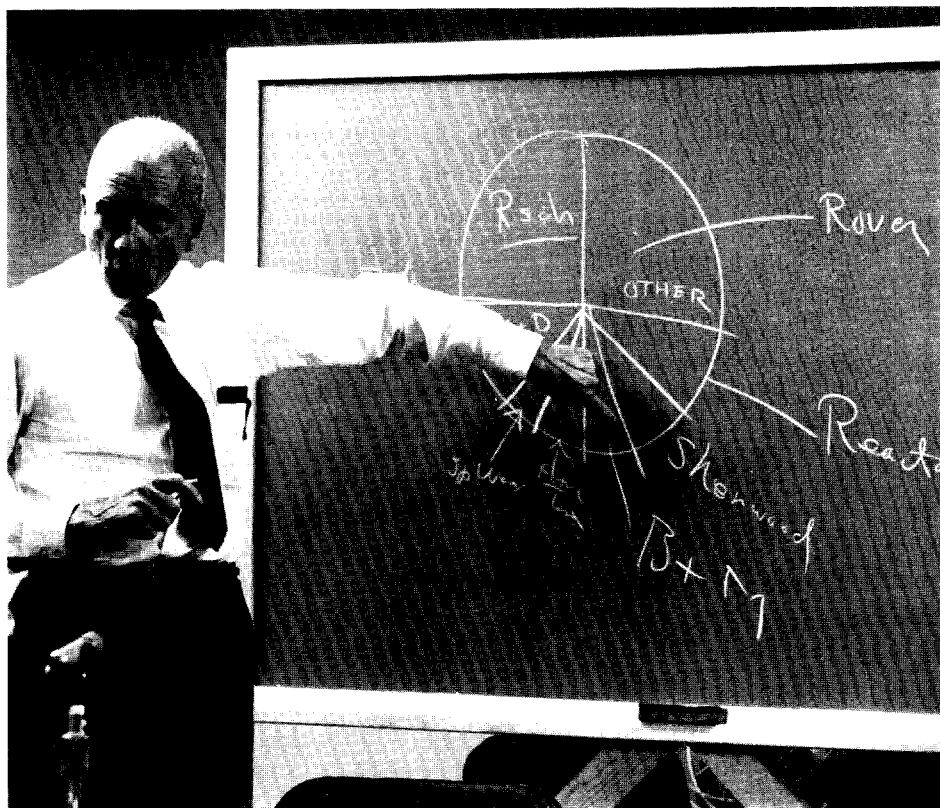
MARK: It suggests that information wasn't flowing, but, even if it had been, their development of a thermonuclear device would have required a longer time than ours. When we started toward Mike in '51, it took about a year and a half, but by that time we

had tested fission devices in Nevada and in the Greenhouse tests that were important to the success of Mike. In other words, we had a great deal more experience with fission bombs than the Russians had at the start of the four and a half years or so it took them to develop something equivalent. I don't know how to compare the times. But I agree that there is no evidence that they were speeded up by exchange of information. If there is any place where information might have had that effect, it was in China. They took two and a half years from their first fission bomb to their first thermonuclear.

SCIENCE: During the summer of 1952 prior to the Mike shot, a second weapons laboratory was being formed at Livermore. Did Los Alamos feel competitive toward the second weapons laboratory?

COWAN: It is hard to recall how tolerant our views were at that time. I recall collaboration much more vividly than I do the notion of competition, although competition probably existed right from the beginning. On the other hand, it seems clear to me in retrospect that it was appropriate to set up a second weapons laboratory. There was too much at stake for the nation to rely entirely on one laboratory.

EYSTER: There has been over the years a great deal of collaboration. When Livermore first started, we made explosives for them because they had not yet gotten any local facilities going. In many areas in explosives we would have meetings and say, "You think this thing is very important, but we don't. So why don't you work on it and tell us what you are doing and vice versa." We used to send them slightly censored monthly reports, censored only in the sense that administrative and local things were cut. The Livermore people quickly got hung up and could only send formal laboratory reports. We said, "Oh, to hell with it; we'll send ours to you anyway." Sure, Livermore developed silly things, but you can't really fault the institution of marriage just because it doesn't always work.



Bradbury discusses the Laboratory's budget in July 1953.

COWAN: I once asked Rabi about this, and he said he felt the relationship between the two labs was that of big brother and little brother. Little brother was the guy who always felt he was overlooked and unappreciated. Big brother was not aware of it. That stuck in my mind because it explained some of the things that were going on at that time.

MARK: There was no well-spelled-out arrangement on sharing work. It was necessary to know all of the same things whether you were working on a design that originated there or here. Sharing the work meant exchanging information either place might have, or both. For example, cross sections had been measured there and measured here, and the answers were different. Collaboration was necessary to find out which was the better measurement or

how to reconcile the discrepancy. The same was true ultimately with respect to computing techniques. The competition that is sometimes referred to—and was real—occurred during the past dozen years when a number of new weapons were scheduled for stockpile and it had to be decided whether a warhead of the Los Alamos model or the Livermore model would be used.

But to return to George's statement that the country could make sense of two labs and maybe even had a requirement for two, it was nevertheless started in a rather unpleasant way. It grew out of rather unfair and vicious criticism of Los Alamos. From the moment Teller left here in October of '51—or perhaps even before—there was behind-the-scenes fomenting for a second lab. For a time it was even threatened that the Air Force would set up a second lab in



Bradbury and Oppenheimer at Los Alamos in May 1964.

Chicago because that was where Edward was. The AEC had to head that off.

BAKER: Frankly, the split almost happened before the war ended because there was so much dissatisfaction.

MARK: The timing was also questionable because in the summer of '52 Los Alamos was strained to an incredible extent preparing for the tests coming on in November. But except for the unpleasant beginning, which has nothing to do with the Livermore people, the relationship was a good one.

SCIENCE: *As you mentioned earlier, McCarthyism was in full swing in the early '50s. Did the McCarthy hearings affect the Los Alamos staff?*

MARK: They didn't bear very hard on individuals here, but they made everybody somewhere between nervous and disgusted. But that atmosphere quite possibly had

something to do with the fact of the Oppenheimer hearing. The administration, the AEC, the Secretary of State, and so forth, had word that McCarthy was showing interest in the Oppenheimer file. They felt that they had to prove somehow that this had been looked after and everything was all right before they turned it loose for a side-show such as McCarthy was so fond of—not that they came off much better.

SCIENCE: *What was known about the Oppenheimer case at Los Alamos?*

MARK: Well, almost nothing was known, except the fact that he was under investigation, until after the public announcement that his clearance had been revoked. In December 1953 I was to go on an excursion to Washington, and, as usual, I planned to go by Princeton to talk to Johnny von Neumann. Norris, aware that I was going to

Princeton, called me aside and said, "I am sorry to have to tell you that you shouldn't continue to discuss programs with Oppenheimer." That was the first word I had that there was anything under discussion at all. The hearings occurred in the spring of '54, and the AEC decided to lift his clearance about the end of June 1954, two days before Oppie's consultant contract ran out.

SCIENCE: *Had he been a frequent visitor to the Laboratory during this period?*

MARK: Not a very frequent but a very natural one. He had been Chairman of the General Advisory Committee. Norris and others on the staff would appear before the GAC to tell them what we were doing. So he was very frequently in touch with the work, although he wasn't a terribly frequent visitor to the Laboratory.

COWAN: Why was Oppenheimer brought before a hearing?

MARK: It was at Oppenheimer's insistence. He was offered in December the opportunity to resign. He said he couldn't accept that because it would be resigning under a cloud, and he wanted to clear it up.

SCIENCE: *What was the response at Los Alamos when you heard the results of the hearing?*

MARK: There were certainly a number of people here and in other parts of the country who attached a very strong feeling to it. There was the famous event of Bob Christie's not shaking hands with Edward at breakfast at the Lodge here the day after he heard about the situation. There were people who wouldn't associate socially with Edward for years. There were a mixture of responses. It didn't affect the Lab's work; it did affect many personal relationships, but that's now thirty years ago and some of the bad feelings have been softened or been forgotten.

COWAN: There was no official response from the Lab, but a chapter of the Federation of Atomic Scientists at Los Alamos met and drafted written comments concerning the security procedures and practices of the

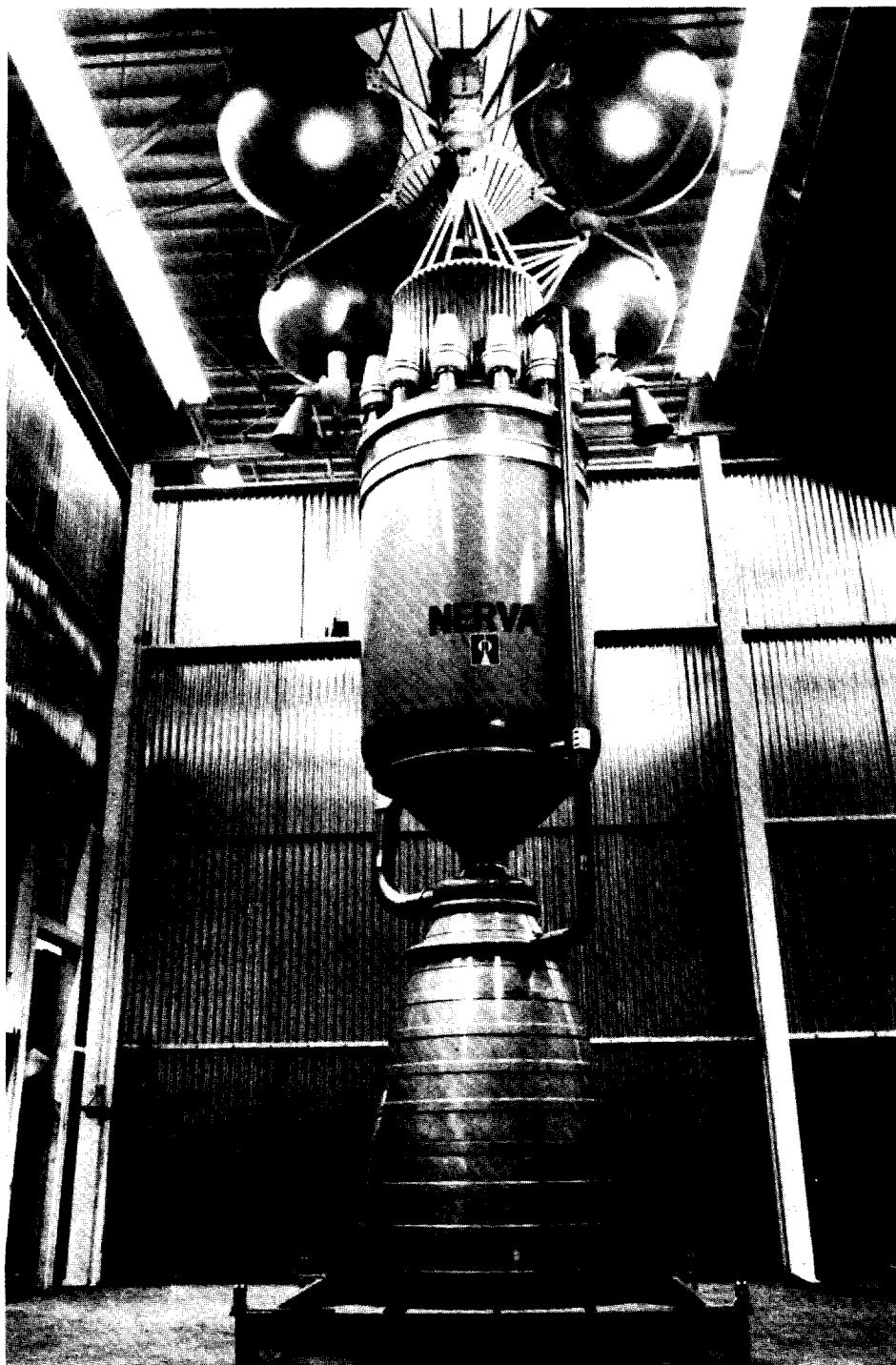
Atomic Energy Commission. These were all inspired by the reaction to the Oppenheimer hearing. The comments were pretty caustic and highly critical, particularly of the guilt-by-association aspect. Lewis Strauss visited at that time, and an indignant group of scientists went to see him at the height of their indignation. He was so skillful in flattering everybody that he had us eating out of his hand in about ten minutes. As soon as he left, people turned to each other and said, "What happened?"

SCIENCE: *The Laboratory became involved in a number of nonweapon research projects during Bradbury's tenure. Can you describe how they got started?*

MARK: The fast reactor Clementine was approved in late '45 to investigate plutonium as a possible reactor fuel. It had never been used in a reactor, and the only place in the country, or for that matter in the world, that was prepared to handle plutonium was Los Alamos. Also, it was known then that a successful breeder process would most likely use plutonium as a fuel. After Clementine there were LAPRE and LAMPRE. These were also experimental plutonium reactors.

BAKER: Most interesting to me was that the country, and particularly people at this Laboratory, started to think about using plutonium as a reactor fuel so early in the game. Programs that would generate knowledge on plutonium alloys and the like were set up with a view toward reactor fuels. So in addition to all the development work and intense effort on fission and thermonuclear weapons, there was other thinking going on in the Lab on research and reactors. To a great extent this was precipitated by Norris Bradbury's attitude toward research.

MARK: The plutonium reactor work doesn't deserve to be called a major nonweapon program. But it started very early and it took a lot of work. The country was going in all directions in reactors. Argonne Lab was thinking of two or three kinds, Clinton Lab was thinking of some others, Monsanto was thinking of a different one, and so on. The



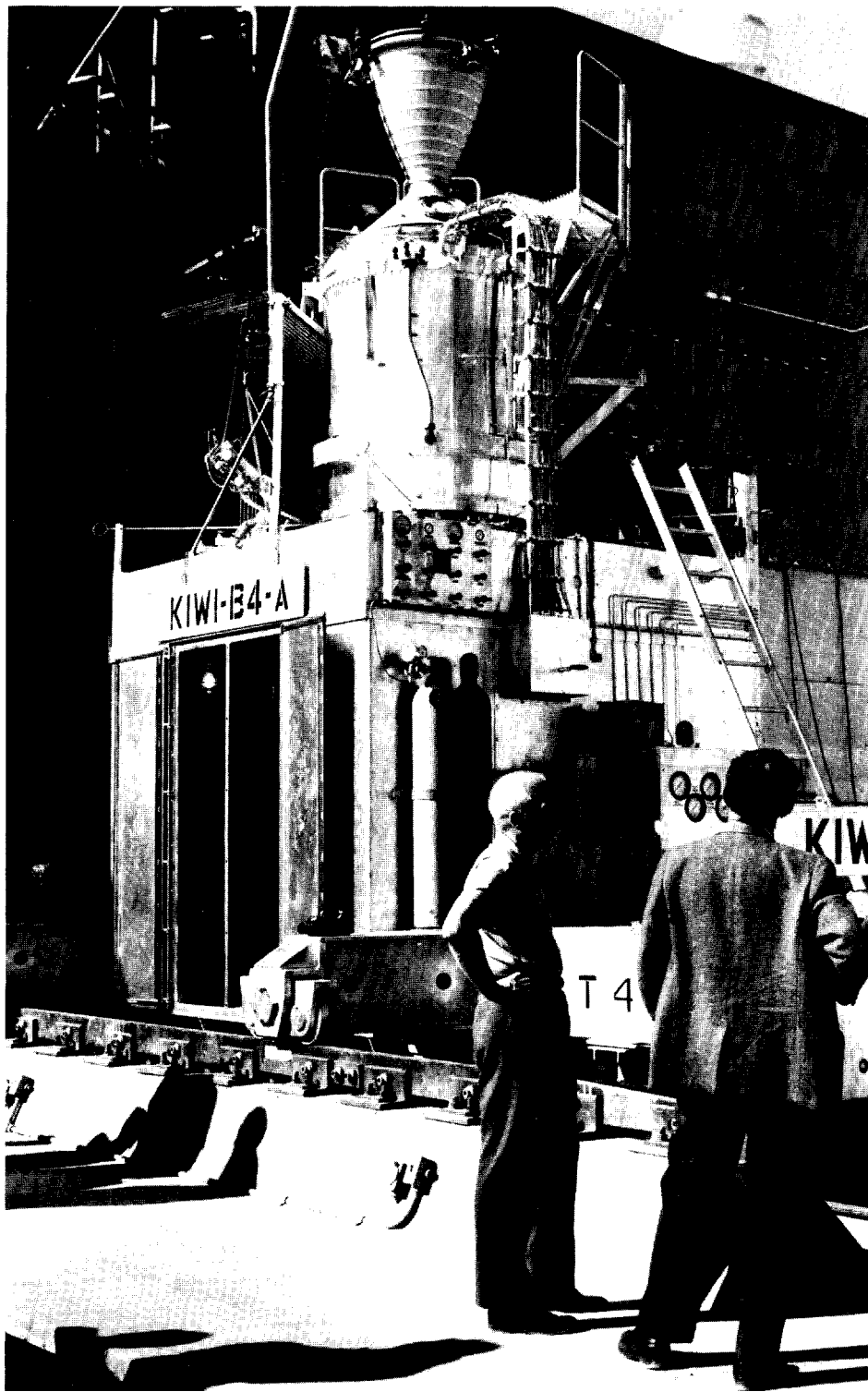
The Rover nuclear reactor was designed to power rockets. Compressed hydrogen in the spheres at the top flowed through the reactor core (center) and formed a jet as it exited the nozzle at the bottom.

Air Force was thinking of going around the world in their nuclear plane, and there was no point to our getting into that business. If there was a point to our being in the reactor business, it was by the plutonium route. People wanted to do it because it would be related to weapon problems, but it never became a program to the extent that Project Sherwood did. Project Sherwood was the first research effort devoted to fusion. Jim

Tuck was its main protagonist at the start and for some time after that. He thought that there was a way to get thermonuclear reactions to proceed in a controlled way. So he set up experiments to explore this possibility and immediately perceived difficulties that neither he nor anybody else had ever thought of. Controlled fusion is still full of difficulties.

SCIENCE: *How was it funded?*

MARK: At first it was probably funded from



Norris Bradbury (left) and Stan Ulam (right) at the site of a Rover reactor test.

general research funds because it didn't spend much money. But it soon became a serious, separately funded activity. And of course it grew up in other places in the country and so became an official AEC program,

COWAN: One of the major contributors to the theory of controlled thermonuclear reactions was Marshall Rosenbluth, who came to Los Alamos and worked on it rather early in

the game.

MARK: One summer in the early '50s I had a really distinguished, tremendously capable bunch of consultants, and I thought how good it would be if they would work on weapons. Much to my disgust the whole crowd of them went off and worked instead on Sherwood.

COWAN: Project Sherwood was, in fact, the first major nonweapon program. Then in '55

we began work on a nuclear rocket—that was the Rover Project—and in '59 or thereabout we started UHTREX, the ultra-high-temperature reactor experiment.

MARK: We are forgetting to mention an even earlier program that had to do with health physics.

BAKER: We are. Norris Bradbury was very adamant on starting a health physics program and research on radiation effects.

COWAN: Much of it was concerned with the physiological problems produced by exposure to plutonium and tritium and then to fallout from nuclear explosions, fission-product fallout.

SCIENCE: *Bill, you were part of the health physics effort. Can you describe some of what went on?*

OAKES: Yes. But first let me say how I came to be here. Louis Hemplemann, who headed the medical health program at Los Alamos, came to Washington University, where I was a physician, and talked to me about the exciting things that could be done at Los Alamos. Among them was the possibility of studying molecules and their metabolism by tagging them with radioactive carbon produced at Los Alamos. I had spent much of my career worrying about the problems of radioactive materials, and the idea of using these materials for research seemed to me to be one of the great new viewpoints. I should mention that I had had quite enough of the military function during the war as a member of the Air Force, and the fact that Los Alamos was now under the civilian Atomic Energy Commission was an important factor in my deciding to come here.

SCIENCE: *What was known at that time about radiation hazards?*

OAKES: Physicians and people in general had learned from World War I that the handling of radium was a very dangerous thing. At that time watch-dial painters had become seriously ill from putting the brushes in their mouths. We knew that plutonium, being a heavy metal, deposited in the bones and caused destruction and eventual bone

tumors. Plutonium is an alpha emitter and is not dangerous on the outside of your body, but if you breathe it in or swallow it you are probably in trouble. We knew that people who were exposed to plutonium and the other actinide elements should be protected. Hemplemann came to Los Alamos to get this job done. Special air-handling areas were set up where people worked with plutonium, so that the plutonium would travel away from the worker in case of an accident. The nice thing during wartime was that the technicians handling plutonium knew the basic facts and thus understood the problems.

Attempts were also made, primarily with film badges, to determine whether or not people had been exposed to radiation.

MARK: And colonies of mice and even some expensive dogs were exposed to air containing plutonium and then studied.

EYSTER: I can remember we devoted a lot of time on the first electron microscope to studying beryllium oxide samples.

COWAN: Yes. Beryllium was used in the atomic energy program. It was recognized shortly after the war that exposure to this element caused berylliosis, and that was one of the health concerns.

BAKER: Louis Hemplemann was dedicated to protecting the staff and so was Norris. But they didn't frighten us. Health and safety were really sold to us, not imposed.

MARK: They had a lot of things to watch, and they knew what they were doing, at least qualitatively. They had a very good record of keeping bad things from happening to people.

COWAN: I can't resist mentioning some experiments to find out the rate of elimination of tritium from the body. These experiments involved inhaling a whiff of tritium gas and then setting up a diuresis by consuming so much beer per hour, free government beer. All the output was measured.

ROSEN: I took part in those experiments and was one of those who got more tritium than was allowed at the time. My problem was

that I didn't like beer.

BAKER: Some have given the impression that when we started working with tritium, plutonium, and enriched uranium, we just barged around without paying any attention to the health or safety aspects. That was just not true. Hemplemann convinced all the people working with the material to be careful, and so we all worked with him. We built enclosures for handling plutonium, they gave us nose counts, and we had monitoring instruments, which didn't go down to as low a level as one might want now but did tick if there were alphas around. It was pretty well handled and I think quite a plus for Louis Hemplemann. He didn't come around and try to scare anybody. He just told us we had to get off the dime.

MARK: I think he had a team with him who shared his ideas and made the effort effective.

BAKER: We didn't take chances either in the processing or storage of materials. Everyone knew all about the dangers of accumulating critical masses.

MARK: Also the group of forty people or so who had more than the prescribed exposure to plutonium have been followed; Hemplemann is still involved in following that group.

To summarize, health physics was a separate program. Although it was necessary in connection with weapons it really went into a much broader field.

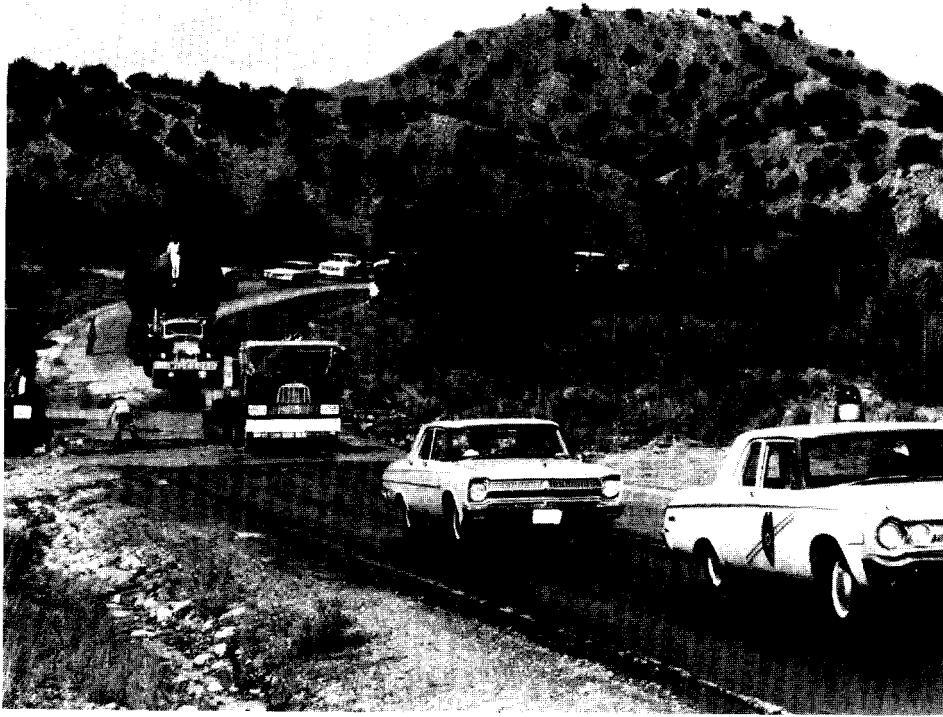
BAKER: Norris, even in the early days, did not limit what people did with so-called weapons money to just weapons problems. In the case of health physics, if it was related to radiation and the like, his attitude was "Fine, let's get on with it." Of course if there was something red-hot in weapons you had better do that first.

COWAN: An example, not of a program but of the scientific spin-offs, was in radiochemistry. Radiochemists had the freedom to investigate the debris from the Mike explosion, and the result was the discovery of two new elements, einsteinium and

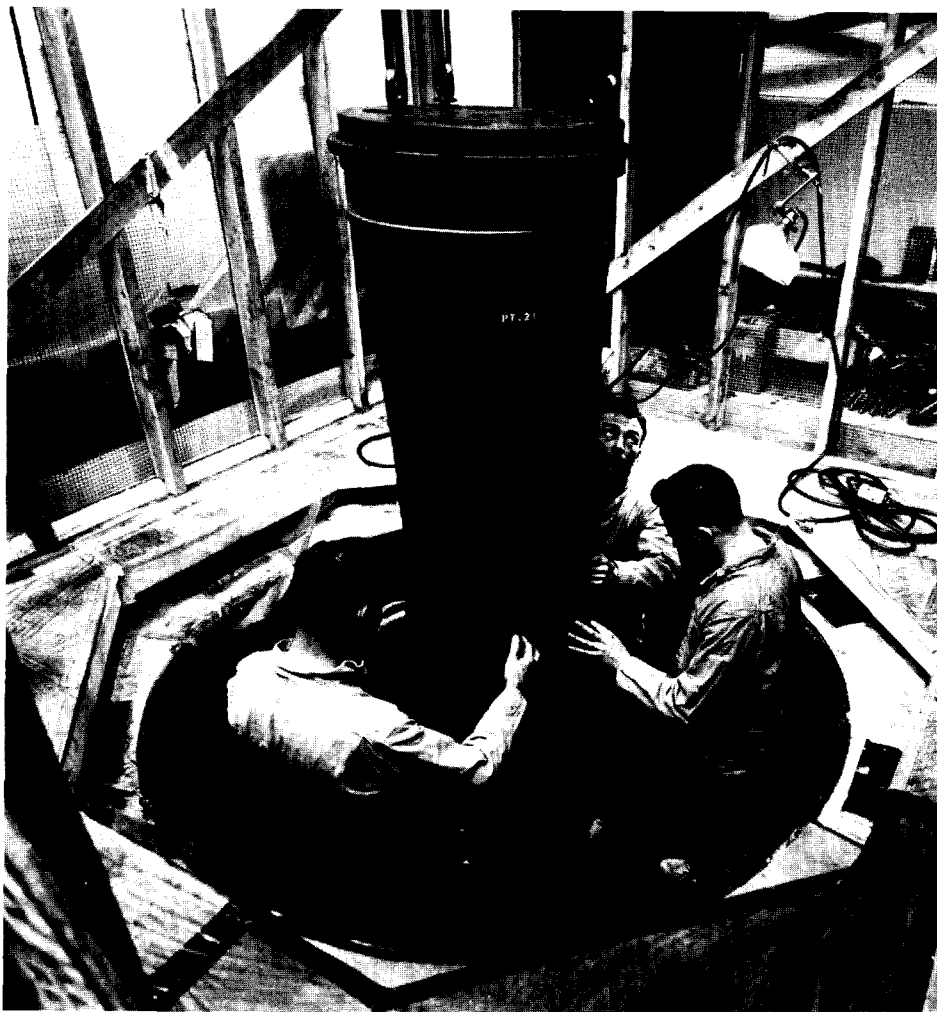
fermium, and of all the heavy isotopes of plutonium including plutonium-244, which was later found to exist in nature because it is so long-lived. In one very intensive period of activity following Mike, we extended what was known about the transplutonic elements by almost as much as what has been learned since. The neutron flux in that explosion was so intense that it produced everything up to mass 255. All of these products were identified and characterized. Previously there had been no way to make these things or even to know they existed. Later on, in '59, a symposium on scientific applications of nuclear explosions was held here. We discussed applications of nuclear explosions to basic scientific research that could in turn feed back into our diagnostic techniques, such as the use of neutrons from explosions for time-of-flight cross-section measurements. The effort to produce new heavy elements beyond einsteinium and fermium dated from that time and resulted in a spectacular improvement in the neutron flux produced in thermonuclear devices. However, it failed to produce new elements because of what might be called an accident of nuclear physics: the excess neutrons in the nucleus produce a catastrophic shortening of the lifetimes of the products due to spontaneous fission. They become so short-lived that there is no time to dig the products out of the ground and identify them after an explosion. We discovered that afterward. But at any rate the technical feats accomplished at that time—Livermore was also involved with these experiments—were really quite spectacular.

SCIENCE: *Did these efforts help weapons development?*

COWAN: It certainly helped to improve the diagnostic techniques. For example, the desire to identify a few atoms of new heavy elements in the radiochemical samples from an explosion inspired the acquisition of one of the first mass separators. Having been brought in to look for new heavy elements, it was very quickly pre-empted by the diag-



The containment vessel for the UHTREX reactor had a difficult journey to Los Alamos over flooded terrain.



The UHTREX reactor core being lowered into its containment vessel,

nostic people who found it so useful that they took it over full-time. The people who were looking for heavy elements had to go off and negotiate for a second one.

MARK: The capability and experience with ion-exchange columns was also increased.

COWAN: Yes. I can still recall the decision to process a kilogram of dirt from Nevada at a time when people were used to processing gram amounts. Everyone involved rose to the occasion and found it was possible. Then there was no reason not to do all sorts of new things with diagnostic detectors that had never been thought of before. These new techniques became fairly standard. So the freedom at Los Alamos to pursue new ideas helped to stimulate all sorts of new technology. It led to excitement, to intellectual challenges, and to all sorts of things that are very easy to lose in its absence.

BAKER: Such an enlightened attitude was also very important to recruiting, whether we realized it or not.

SCIENCE: *How did the Rover program get started?*

COWAN: I associate it with Bussard and the notion that the country needed an intercontinental ballistic missile for security purposes and that the only way it could be done was with nuclear power. Once Bussard introduced that idea, it excited a lot of interest. The reactor design involved passing hydrogen gas through a fission reactor core, thereby cooling the core and heating the hydrogen to the extremely high temperatures necessary to propel a rocket. The hydrogen thus served as the reactor moderator, coolant, and propellant.

BAKER: Norris Bradbury thought the whole idea was interesting and simply started it up without separate funding. That's the way we used to work. We had to come up with a fuel that was compatible with very high temperatures and compatible with what the designers thought they could do relative to the size, weight, and power requirements of the reactor. We worked on two types of fuels. One was a uranium dioxide cermet, a

fuel made by mixing uranium dioxide with a metal like molybdenum and forming it into a solid piece. The second was a mixture of uranium carbide and graphite formed by graphitizing a mixture of uranium dioxide, carbon, and a binder. Eventually we developed a graphite fuel consisting of coated particles of uranium carbide in a graphite matrix. These were made by mixing the particles with graphite and a resin. The mixture was extruded into the form of the fuel elements and graphitized at high temperature. The designers worked on reactor designs for both types of fuel. We worked for a fair time using Norris' money and then very rapidly acquired separate funding. We went right ahead and developed the reactor and both fuels, but then the cermet-fueled reactor, Dumbo, was turned over to Argonne. Then Westinghouse was brought in because it was visualized that while we were doing the reactor testing, industry should get ready to do the flight testing and start the production of reactors for space application.

MARK: Is it true that UHTREX was almost a spin-off from the Rover work since techniques for living with high temperatures had been developed for that project?

BAKER: UHTREX was a direct spin-off, I always felt, in idea and fuel. It used extruded graphite fuel elements that retained fission products. And there were holes in them for the gas to flow through. It had a gas scrubber and all that; it was a pretty neat reactor.

SCIENCE: *What happened to UHTREX?*

BAKER: Milt Shaw of the AEC was taken with fast breeder reactors. He often said he didn't want to divert money to UHTREX; he wanted it all to go to the breeder. It was a shame that the UHTREX work was cut off.

MARK: I remember that the breeder was costing more and more above expectation. In order to keep it going Milt took money from many projects, not only from UHTREX.

SCIENCE: *What problems did you have to solve in developing high-temperature fuel elements?*



Bradbury visiting the site of Livermore's Gnome shot in a salt dome near Carlsbad, New Mexico. This 1961 shot was part of Plowshare, a program on peaceful uses of nuclear explosions. From left to right are an unidentified guide, John Orndoff, Bradbury, Al Graves, and Carson Mark.

BAKER: A graphite-based fuel element was in existence when we began the Rover project. It consisted of little pellets of graphite containing coated particles that retained the fission products. The pellets were made by molding and not by extrusion. For the Rover reactor we wanted long fuel elements with holes for the hydrogen to pass through. But it was impossible to mold the many holes in these long fuel elements to very precise dimensions. We found a way to do it by extrusion. I always thought that was quite a technological feat. Another really terrific technological development was the coating of those holes with high-temperature carbides so you could buzz hydrogen through those fuel elements at something like 2000 degrees Centigrade without chewing them all up.

In the Bradbury years we also started a very vigorous program with Milt Shaw on uranium and plutonium carbide fuels for

breeder reactors. That program has an old heart now and is barely breathing, but it survived Milt Shaw. And we worked for Argonne on uranium alloy fuels for fast reactors.

MARK: The Lab also built some of the fuel elements for the SNAP reactors; that work anticipated the work on heart pacers.

BAKER: We got into the SNAP fuels under Norris. They were plutonium-238 fuels for space power sources. Then during the last Year or two of Norris' stewardship, we developed plutonium-238 power sources for heart pacers. I want to say again that a lot of this work came because of Norris' attitude that we should look into whatever we thought we could do. Once we had looked into it, we would go to Washington and discuss it as a possible separate program, but we always had a fair amount of discretionary money to try out our bright ideas. I am not criticizing the present Laboratory ad-



Raemer Schreiber (left) and Norris Bradbury (right) at the Trinity site in 1950.

ministration because I know things are different now, but, gee, it was great.

MARK: There has also been a change in Washington. The present attitude goes something like this, "Here is the project you are to be working on; how much does it cost? A hundred thousand dollars? OK. When will it be finished? Tell us right now what the results are going to be!"

BAKER: We went into that regime under Norris.

MARK: It began to move that way.

EYSTER: I can remember very early in the game when the notion got around here that there ought to be some tactical weapons that were essentially free rockets, rockets without a lot of guidance. There was no one in the Army who felt this project truly came within

their mission, so Norris convinced Captain Tyler, the AEC Area Manager, to engage in a ploy. I remember going with Norris and Tyler in the big Carco airplane out to the Naval Ordnance Test Station at Inyokern. It was arranged for the AEC to give them some money to work on a two-stage free rocket for tactical uses. Finally the Army heard about it; they got so mad that they did indeed develop Honest John, a single-stage free rocket. I think it just recently went out of service.

COWAN: This comment may be a little facetious but not entirely. We were done in by the development of large computers, which permitted the identification and so the cost control of every so-called cost center down to five thousand dollars. In the '50s

McNamara and his whiz kids came into the Department of Defense and brought in this revolutionary idea of controlling all that went on by setting up this accounting system. That spread like a malicious disease and it has led to so-called micromanagement. It couldn't have been done without modern computer technology.

SCIENCE: *The weapons program was going strong in the late '50s with the rapid development of more convenient versions of hydrogen bombs. Then in 1958 and '59 the United States participated in the test ban conference, and in 1959 we agreed to a moratorium on testing. What was the impact of these events on the Laboratory?*

COWAN: I think it provided impetus to diversification of the Lab's programs.

MARK: A diverse program had already been built up at Los Alamos, but the moratorium added a strong talking point to the LAMPF project, which got itself recognized and put into gear along about '62 or '63. LAMPF was to be a linear accelerator that would serve as both a meson factory and an intense source of neutrons. It would interest a lot of the weapons people in case we got out of the testing business, and it had its own value as well. Diversification of the Lab meant that if a sudden test ban came on you wouldn't suddenly have to dismantle the whole Lab's budget and personnel.

SCIENCE: *What happened to testing after 1959?*

MARK: There was a moratorium during which no tests were done. Then they were resumed in 1961 and '62. Then in '63 under the Limited Test Ban Treaty, tests were all to be conducted underground. We have had more tests underground than we ever had in the air.

BAKER: Two other areas that Norris recognized from early on and that have since blossomed into large efforts at the Laboratory are waste disposal and the safeguarding of nuclear fuels. From the beginning we were working on safeguards, that is, systems that could detect gross diversions of nuclear

materials. We were doing, to the best of our ability, complete accountability, which is a safeguards buzz word for keeping track of where it all is. We were also doing neutron interrogation to measure these materials very early in the game.

MARK: The work on safeguards was partly promoted by Senator Hickenlooper's hearings on where those 4 grams of uranium went.

COWAN: We should point out another significant change in the weapons program that occurred after 1959. The emphasis changed from qualitative new concepts in weapons design to systems engineering because the delivery system had changed from airplanes to transcontinental missiles. There came to be an increasing emphasis on the engineering aspects of weapons, their weights, the way they were configured, the way they could fit into a certain geometry, and so forth. The present emphasis is on the application of the very large energy outputs and short pulses produced by nuclear weapons. If there is a challenging field associated with weapons today, it is the exploitation of these special features of nuclear explosions. Today the weapons business has a different set of emphases, a different set of talents, and in many respects a different set of people.

MARK: To a large extent the ingredients of weapons haven't changed that much, but the modes of application have forced a tremendous change in the way you approach the problem of drawing up a weapon. If it is to go into a Minuteman, that is where you start; if the weapon doesn't fit the delivery vehicle, it doesn't have any significance.

EYSTER: I would say that there have been about three red-hot ideas or concepts in nuclear weapons development. These worked and were attractive because they were simple.

COWAN: There were some other red-hot ideas that haven't been successful but presumably could be. For example, if it were possible to initiate a thermonuclear explosion with nothing but high explosives, I think that



Senator Clinton P. Anderson, a member of the Joint Congressional Committee on Atomic Energy and a good friend of the Laboratory, touring the Sherwood project in November 1962. Present are Keith Boyer (far left), Anderson (left foreground), Bradbury (center), and Jim Phillips far right).



Vice President Hubert Humphrey (left) and Norris Bradbury (center) being shown the first complete prototype accelerating tank assembly of the proposed Los Alamos meson factory by Louis Rosen (right) in September 1966.

would have had a militarily significant impact.

MARK: That idea has been pursued; it just turned out, like Sherwood, to be very sticky.

BAKER: You have to understand the physics first on that one.

MARK: It's a materials problem, like all of our problems.

SCIENCE: *How do you view the direction of the Lab now, and where do you think it should go?*

MARK: The Laboratory has been responding with the techniques, capabilities, and support that it can find to a broadening range of important national problems, and I imagine that direction will persist if it continues to be supported. However, the tremendous elaboration, growth, and detail of management by administrators in Washington is going to make progress along such lines much harder than it was during the times we have been speaking of here. Although you had to check with Norris before you spent anything important, if you aroused his conviction that something should be looked into, you could go out and do it. That is how most of the things we have talked about got started.

The Lab will have a dull future unless it can find a way to use the best scientists from here and outside to sort out those things that would be worthwhile trying, whether they are approved programs or not. These people must also have enough influence and authority to assure that the work be directed not by the Bureau of the Budget but rather by the ideas themselves. If these are good ideas, some of them will succeed. But to find out you have to spend some man-years of work and perhaps quite a few.

SCIENCE: *Does Los Alamos have a role in arms control?*

COWAN: I think it would have been rather remarkable if the place in which the nuclear weapons expertise resided had itself taken on the advocacy of suspension of nuclear weapons development. It might have been entirely admirable, but it is not to be expected and it

wasn't the role in which we were cast. Therefore we have been the advocates of weapons development. When a description of our position is leveled at the Laboratory as an accusation, I would say that is totally unfair.

EYSTER: Winston Churchill once said that he did not intend to preside over the dismemberment of the British Empire.

COWAN: To somebody who says with a sense of indignation that the Laboratory has gone to Washington and argued for the continuation of weapons testing, I would respond, "So what else is new? That is the Los Alamos role."

MARK: Los Alamos doesn't properly have a role in arms control. It shouldn't perhaps argue against it, but you can't expect it to be a front-line proponent saying we should get rid of weapons.

SCIENCE: *Have we provided technological assistance for arms control?*

MARK: That we have. The Vela satellite program to detect nuclear explosions in space is one instance.

COWAN: We have also participated in seismological developments for the detection of weapons tests underground.

BAKER: The Laboratory has always sent representatives and advisors to Geneva and to other arms-control conferences.

MARK: So if there ever is a complete test ban treaty, the Lab might still have a role in the monitoring. We could advise on what things to look out for and how those things could be detected.

SCIENCE: *The administration is encouraging industry to increase its effort in research and development of new technology. How does that affect the Laboratory?*

COWAN: Historically we have always interfaced very, very closely with academia. That is where we have looked for our top staff people, where we try to maintain our credentials, and where we get most of our consultants. But we haven't interfaced much with industry except through purchase requests and contracts. We have generally been the

customer and they the supplier. In the present environment we are looking much harder at our interface with industry and identifying cadres of people in industry with whom we can have scientific exchanges comparable to those we have had with academia. This may very well pay off in terms of accelerated diffusion of ideas to the marketplace. It still is a hypothesis rather than a demonstrated fact, although there are individual instances one can point to. But my own feeling is that these scientific exchanges with industry will pay off and will become a much more significant aspect of the Laboratory's contributions to national programs.

BAKER: Isn't the government making it somewhat easier to interface with industry?

COWAN: Yes. They are now permitting patent rights to revert to the individual laboratories rather than remain government property. So now, if we have a brilliant idea, industry may negotiate on the basis, for example, of an exclusive manufacturing right. Under the previous policy all our ideas were available in the general marketplace, and that ran contrary to all the rules of a commercial enterprise. A businessman does not enter a new field in which the same technology is available to everybody because he runs the risk of making an investment, advancing the technology, and then watching his competitor take it over because it is government property.

EYSTER: Well, Bake, you and I surely have had a long-continuing business with industry that wasn't entirely on a purchase basis. We worked very closely with industry to improve the design of numerically controlled machining tools so they could achieve the precision required in weapons manufacturing.

COWAN: I suspect you can say similar things about our relationships with the computer industry, with IBM, Control Data, Cray, and so forth. These were interactive relationships.

MARK: They certainly were, because some of their machines were built with suggestions

and information from us. We said, "This is what we would like you to do rather than that."

EYSTER: Industry did not always appear in the role of consultant because it had another way of being paid—the expectation of business, or the purchase of other types of machines, and so on. Academia doesn't usually have such prospects.

COWAN: Let me modify what I said. This relationship with industry has existed but it is being much more intensely pursued.

BAKER: We probably gave the people who manufactured induction heaters one of the biggest boosts in their business. We would buy their high-frequency induction heaters, and an electronics buff here would fiddle around with them and make them better. Then we would tell the manufacturers, and they would go back and incorporate the new features.

COWAN: Industry has picked up cell sorters and other sorts of interesting spin-offs. But now this business of technology transfer is becoming a more defined activity. We have a defined relationship with academia through, for example, our consultantships. I think there is something to be learned in pursuing somewhat the same kind of thing with industry.



BAKER: There is a great deal to be learned with this deal on the patents. And if DOE lawyers weren't so plentiful, we could go faster with it. But the thing I still don't see is how we are going to completely overcome the problem of proprietary information. A couple of us approached the carbon com-

panics about what they could tell us. They replied, "We're not going to tell you a hell of a lot of anything because what we have is proprietary information. Even though it gives us an edge over our competitors for only about two or three years, that's better than no edge. So run along." ■